# THE FEDERAL EFFORT TO DESEGREGATE SOUTHERN HOSPITALS AND THE BLACK-WHITE INFANT MORTALITY GAP

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**Abstract.** In 1966, Southern hospitals were barred from participating in Medicare unless they discontinued their long-standing practice of racial segregation. Using data from five Deep South states and exploiting county-level variation in Medicare certification dates, we find that gaining access to an ostensibly integrated hospital had no effect on Black infant mortality. Our estimates are sufficiently precise to reject the hypothesis that the federal hospital desegregation campaign contributed meaningfully to the narrowing of the Black-White infant mortality gap in the Deep South. Similarly, there is little evidence that the campaign contributed to the trend towards in-hospital births among Southern Black mothers. These results are consistent with descriptions of the campaign as producing only cosmetic changes and illustrate the limits of anti-discrimination policies imposed upon reluctant actors.

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*Of all the forms of inequality, injustice in health care is the most shocking and inhumane.*

--Martin Luther King Jr.

## 1. INTRODUCTION

In the Jim Crow Era, Southern hospitals were racially segregated. Hospitals that focused on providing care to Black patients (i.e., "Black hospitals") were, with a few exceptions, understaffed and lacked the latest medical technology (Thomas 2006; McBride 2018, pp. 49-50). White-run hospitals could be "biracial" but Black patients were physically separated from their White counterparts and did not receive equal care (Reynolds 2004; Thomas 2006). Eradicating this entrenched system of racial discrimination and exclusion was a key objective of the American Civil Rights Movement (Washington et al. 2009).

Under political and legal pressure, a handful of hospitals in the South desegregated between 1962 and 1965 (Brown-Nagin 2011, p. 207; Smith 2016). Most, however, remained racially segregated until 1966, when the Johnson administration threatened to withhold Medicare funding from hospitals not in compliance with the Civil Rights Act. The newly created Office of Equal Health Opportunity (OEHO) was tasked with determining whether hospitals were in compliance. Beginning in April of 1966, OEHO investigators, working closely with civil rights activists, visited hospitals across the country with the goal of identifying, and correcting, discriminatory practices (Nash 1968; Reynolds 1997). Six months later, more than 7,000 hospitals had been certified by the OEHO as eligible to receive Medicare funds; 214 Southern hospitals opted to remain racially segregated, forgoing all federal funding (Nash 1968; Reynolds 1997).

The federal campaign to desegregate Southern hospitals has been described as a "powerful force for equal treatment" (Smith 2016, p. 181) and "among the most important Civil Rights achievements in U.S. history" (Sternberg 2015, para. 2), yet, with one prominent exception, its effects on Black health are woefully understudied. Using data from Mississippi, Almond et al. (2006)

document a strong negative association between gaining access to a Medicare-eligible hospital and the Black postneonatal mortality rate (equal to deaths among one- through 11-month-olds per 1,000 live births). Almond et al. (2006) conclude that hospital desegregation saved thousands of Black lives and contributed substantially to the dramatic narrowing of the national Black-White postneonatal mortality gap during the 1960s and early 1970s, but these authors do not account for the strong downward trend in Black postneonatal mortality.

In this paper, we use data from the National Vital Statistics System (NVSS) for the period 1959-1973 to examine the effects of the hospital desegregation campaign on infant mortality by race and the decision of whether to give birth at home or in the hospital. Focusing on five states in which support for segregationist policies and practices was especially staunch (Alabama, Georgia, Louisiana, Mississippi, and South Carolina), we find no evidence that gaining access to a Medicare-eligible hospital affected Black (or White) infant mortality. Our estimates are precise, indicating that we are leveraging sufficient cross-county variation in Medicare certification dates to distinguish between the effects of hospital desegregation and secular trends. Similarly, we find little evidence that the hospital desegregation campaign contributed to the trend towards in-hospital births among Southern Black mothers originally documented by Chay and Greenstone (2000).

The litmus test for Medicare eligibility was race-blind assignment. OEHO investigators required that patients be assigned to physicians and hospital beds without regard to race, color, or national origin (Smith 2016, pp. 110-111). OEHO investigators could not, and were not asked to, address fundamental structural barriers that prevented minority patients from accessing high-quality healthcare (Smith 1998; Sarrazin et al. 2009; Chandra et al. 2017). Nor could they expunge difficult-to-observe racial attitudes and modes of communication that, to this day, shape the delivery of healthcare in the United States (Cooper et al. 2003; Alsan et al. 2019; Greenwood et al. 2020; Hill et al. 2020). In 1972, the Government Accounting Office (GAO) released an assessment of hospital

compliance with the Civil Rights Act. The report concluded that the hospital desegregation campaign had virtually eliminated "overt" racial discrimination, while more "subtle" forms of racial discrimination persisted (Comptroller General 1972, p. 10). Our results suggest that correcting overtly discriminatory practices on the part of Southern hospitals was simply not enough to ensure that Black infants experienced the same health outcomes as their White counterparts. More generally, our results are consistent with an argument from the anti-discrimination literature that punitive actions against employers are of limited effectiveness because they do not address underlying biases and prejudices (Valfort 2018).

The remainder of the paper is organized as follows. In Section 2, we provide historical context, discuss previous studies, and briefly describe some of the challenges faced by minorities today when trying to gain access to high-quality hospital care. In addition, we show that the Black-White infant mortality gap declined faster in the South than in other regions of the country, and we discuss several potential drivers of this phenomenon. In Section 3, we test the extent to which hospital desegregation contributed to the narrowing of the Black-White infant mortality gap in the Deep South, and we document why our estimates are so different from those reported by Almond et al. (2006). In Section 4, we estimate the effect of the hospital desegregation campaign on the choice of where to give birth. Section 5 concludes.

## 2. BACKGROUND

### 2.1. Hospital desegregation

Information on the extent of segregation in Southern hospitals is available from a 1955 survey coordinated by Cornely (1956, 1957). Sixty-seven urban general hospitals in seven Southern states were assessed by local affiliates of the National Urban League. Only four of the 67 hospitals admitted Black patients without restrictions. The remaining hospitals either refused to admit Black

patients, had segregated wards, or had other "modifications of established segregated or discriminatory practices" (Cornely 1956, p. 1079).[1]

When the Civil Rights Act passed in 1964, nobody was certain how it would impact the health care system. The amendments to the Social Security Act that would eventually establish the Medicare and Medicaid programs had yet to be taken up by Congress, and most observers believed that the process of hospital desegregation would proceed slowly, dependent upon private initiative, voluntary compliance, and lawsuits brought by the victims of discrimination (Kenny 1965; Smith 2016, pp. 84-86; Largent 2018).[2]

Initially, the Johnson administration focused on encouraging hospitals to voluntarily comply with Title VI of the Civil Rights Act, which banned the allocation of federal funds to entities that discriminated on the basis of race (Largent 2018). This strategy, however, was only partially successful. In July-October of 1965, the U.S. Civil Rights Commission surveyed 39 hospitals in Southern and border states with the goal of determining whether they were in compliance. The Commission found that two hospitals, both of which were located in Maryland, had "desegregated substantially" before the passage of the Civil Rights Act; 11 of the 39 hospitals had made "significant changes in their discriminatory patterns of patient assignments, staff assignments, and

[1] See Cornely (1956, pp. 1078-1079) and Cornely (1957, pp. 8-9) for more details on how the survey was conducted and its results. Seventy-two hospitals in 3 border states (Maryland, Missouri, and Oklahoma) were also included in the study. Ten of these 72 hospitals admitted Black patients without restrictions (Cornely 1957, p. 9). Thomas (2006) describes the availability of North Carolina hospital beds in 1960: there were 919 hospital beds in Black hospitals and 2,905 beds in White-only hospitals; in "biracial hospitals," there were 1,758 beds reserved for Black patients and 8,822 beds reserved for White patients. According to Thomas (2006, p. 854), "[b]y 1960 the vast majority of both black and white patients were being treated in biracial hospitals, whereas nearly half of white hospitals had either opened their doors to black patients or shut down since the end of World War II."

[2] Joseph Califano Jr., who served as special assistant to President Johnson, was interviewed by Smith (2016, p. 82). According to Califano,

> No one understood Johnson's plan. The Civil Rights Act was passed in 1964 before the enactment of all the domestic programs the following year. No one anticipated the massive flow of federal funding that would begin in 1965. If the civil rights bill had been pushed after all of that subsequent legislation, it would have never passed.

access to public facilities" since its passage, while the other 26 still engaged in discriminatory practices (U.S. Civil Rights Commission 1966, p. 6). For instance,

> At the time of the Commission investigation, James Walker Memorial Hospital in Wilmington, North Carolina, which had been involved in a decade of litigation over its segregated facilities, continued to maintain a building for Negro patients at the rear of the main facility. Negro patients were wheeled from the separate structure into the main facility for surgery and other services. Some Negro patients were housed in segregated wards in the main building. The hospital also made staff assignments according to race although the administrator said some Negro nurses had been assigned to care for white patients since the passage of the Civil Rights Act (U.S. Civil Rights Commission 1966, p. 8).[3]

The Commission concluded that the pace of hospital desegregation was largely determined by local factors (e.g., the hospital "administrator or board") as opposed to efforts on the part of the Public Health Service (PHS), which was, in theory, responsible for ensuring Title VI compliance (U.S. Civil Rights Commission 1966, p. 14).[4]

Under increasing pressure from civil rights groups, a profound shift in the federal government's strategy occurred in the beginning of 1966: hospitals that wanted to participate in the soon-to-be launched Medicare program would have to be certified as Title VI compliant and the OEHO (which was created in February of 1966 and was under the auspices of the PHS) would be in charge of the certification process. The OEHO quickly promulgated a detailed set of guidelines for participating hospitals. The litmus test of Title VI compliance was race-blind assignment: the new

[3] To take another example of discriminatory practices documented by the Commission, the

> Macon Hospital in Georgia had made only minimal changes to comply with Title VI provisions. After passage of the Civil Rights Act, the hospital converted its formerly all-Negro building into a facility for welfare patients only. Negros account for 60 to 70 percent of the welfare patient load. No Negro and white patient occupied the same room or ward in this building at the time of the Commission staff visit (U. S. Civil Rights Commission 1966, pp. 8-9).

[4] As late as April of 1966, the OEHO estimated that only 25 percent of hospitals in the South (and only 11 percent of hospital beds) were in compliance with Title VI (Reynolds 1997).

OEHO guidelines stipulated that patients were to be assigned to hospital rooms, wards, and buildings "without regard to race, color, or national origin"; likewise, medical staff were to be matched with patients "without regard to race, color, or national origin" (Smith 2016, pp. 110-111).[5]

The OEHO trained hundreds of investigators to conduct on-site inspections (Griffin 1966; Smith 2016). Working closely with local civil rights activists and Black hospital workers, they identified, and tried to correct, discriminatory practices. Although there are accounts of medical staff and administrators adopting the Title VI guidelines with little fanfare (Nash 1968; Brown-Nagin 2011; Smith 2005, 2016, pp. 120-128), the demands of the OEHO investigators were occasionally met with fierce resistance (Smith 2005, 2016, pp. 120-128). In fact, 214 Southern hospitals decided that they would forgo federal funding rather than integrate their facilities (Nash 1968; Reynolds 1997).

By November of 1966, the OEHO had certified more than 7,000 hospitals as Medicare-eligible (Nash 1968; Reynolds 1997).[6] A few months later, the OEHO was dismantled and enforcement of the Title VI guidelines fell to the Office of Civil Rights (OCR). Under the OCR, enforcement was not nearly as strict as it had been under the OEHO (U.S. House of Representatives 1973; Quadagno 2000). The OCR relied heavily on complaints of discrimination made by Medicare and Medicaid beneficiaries to identify non-compliant hospitals; on-site reviews were rare and there is anecdotal evidence of ostensibly desegregated hospitals having made only

[5] Hospitals were also required to notify employees and persons previously excluded from services that they were in compliance with the Civil Rights Act (Reynolds 2004).

[6] For more information on the history of hospital desegregation and the efforts of the OEHO, see Nash (1968), Reynolds (1997, 2004) and Smith (2016).

cosmetic changes, blatantly flouting Title VI guidelines (U.S. House of Representatives 1973; Quadagno 2000; Smith 2005).[7]

In July of 1972, the GAO released an assessment of hospital Title VI compliance. Focusing on four metropolitan areas (Atlanta, Birmingham, Detroit, and Los Angeles), it described the federal hospital desegregation campaign as having all but eliminated "overt" discrimination (Comptroller General 1972). GAO investigators did, however, observe that a "disproportionately large share of minority patients received their healthcare at government-owned hospitals" and that private hospitals routinely denied staff privileges to Black physicians (Comptroller General 1972, p. 10). The practice of denying privileges to Black physicians effectively barred their patients, who were themselves predominantly Black, from being admitted to private hospitals.[8] According to GAO investigators, public hospitals "attracted" minorities because they provided low-cost care to indigent patients, were easily accessible, and had a history of treating minority patients.[9]

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[7] There were only 300 on-site hospital reviews conducted by the OCR in 1971 (Comptroller General 1972). In hearings before the Civil Rights and Constitutional Rights Subcommittee (U.S. House of Representatives 1973), Jeffrey Merrill, a representative of the American Public Health Association (APHA), testified that:

> Last year the APHA was involved in a study in Mississippi where we had the opportunity to examine a lot of hospitals within the delta area. There were two things we found out through this study. One is…that, on a given day when the hospital is forewarned of a visit by some sort of enforcement agency, beds are shifted and it is very simple to shift a bed on wheels. And so coincidentally, on that and maybe for a couple of days, the hospital appears totally integrated.

[8] Only 93 Black physicians were practicing medicine in Atlanta and Birmingham. Black patients in these cities relied heavily on outpatient clinics run by public hospitals, and the Black physicians who worked at these clinics and public hospitals did not want (or did not use) staff privileges at private hospitals because "of (1) loyalty to predominantly black-patient hospitals…, (2) the desire to have their patients near their offices, or (3) the time and expense of making rounds at several hospitals" (Comptroller General 1972, pp. 32-33).

[9] There were 24 Medicare-certified hospitals in Atlanta and Birmingham when the GAO conducted its investigation, all but two of which were private. In Atlanta, the county-run Grady Memorial Hospital served fully 58 percent of all Black patients during the period July 19-26, 1971. In Birmingham, University Hospital served 49 percent of Black patients. Five of the 22 private hospitals in Atlanta and Birmingham admitted a combined total of 12 Black patients and 442 White patients. According to the report, most Black patients in Atlanta and Birmingham knew that Medicare and Medicaid would cover their costs at private hospitals (Comptroller General 1972). Quadagno (2000) describes Black patients as fearing that they would be refused care at hospitals with a long tradition of exclusively serving White patients despite the fact that these hospitals were certified and ostensibly desegregated. Bledsoe (1968) provides additional accounts of Black patients being reluctant to use formerly White-only hospitals.

### 2.2. Hospital desegregation and the Black-White infant mortality gap

Over the past century, the Black infant mortality rate (IMR) in the United States has fallen at an average annual rate of 2.6 percent (Singh and Yu 2019). However, because the White IMR has fallen at a faster rate over the past century (3.1 percent per year), the Black-White IMR ratio actually increased (Singh and Yu 2019).[10] Intriguingly, the Black-White IMR ratio fell by 13 percent during the period 1965-1971, from 1.9 to 1.65 (Chay and Greenstone 2000). Although there had been smaller, one- and two-year reductions in the Black-White IMR ratio since the end of World War II, this was the first sustained decline for 20 years.

Almond et al. (2006) attribute much of this decline to the hospital desegregation campaign in the South. They reach this conclusion by estimating the effects of the hospital desegregation campaign on postneonatal mortality rates (PNMRs) by race in Mississippi. Almond et al. (2006) show that the Black PNMR fell much faster in counties served by at least one Medicare-eligible hospital as compared to counties served by hospitals that refused federal funding. The estimated effect on the Black PNMR is large enough to explain the entire convergence between Black and White IMRs in Mississippi over the period 1965-1971.[11]

Opposition to the hospital desegregation campaign was particularly fierce in Mississippi (Reynolds 1997; Smith 2005). Despite this opposition, most Black mothers in Mississippi had the option of delivering their baby at a Medicare-eligible hospital by the end of 1967.[12] By the end of

[10] In 1916, the Black IMR was 184.9 per 1,000 live births and the White IMR was 99.0; by 2017, the Black IMR had fallen to 10.8 per 1,000 live births and the White IMR had fallen to 4.9 (Singh and Yu 2019).

[11] Almond et al. (2006, p. 18) conclude that the "integration of hospitals in the rural South accounted for…25% (based on all post-neonatal fatalities) of the national 7.5 per 1,000 decline in the national black PNMR between 1965 and 1975." See also Finkelstein and McKnight (2008), who use data from the Mississippi Delta to examine the effect of having access to a Medicare-eligible hospital on mortality among the elderly. They find that access to a Medicare-eligible hospital was associated with a 35 percent reduction in non-White elderly pneumonia mortality.

[12] See Appendix Table A1. In 1967, 45 out of 82 Mississippi counties were served by a Medicare-certified hospital, while 12 counties had no hospital (certified or otherwise) but their residents had the option of receiving care at a certified hospital in a bordering county. During the period 1955-1975, 64 percent of Black births occurred in these 57 counties.

1969, all but five counties in the state were served by at least one Medicare-eligible hospital (or their residents had the option of receiving care at a certified hospital in a bordering county). This "modest variation" in Medicare certification dates precluded Almond et al. (2006, p. 15) from including year fixed effects in their regressions.[13] One of the advantages of using data from five Deep South states, as opposed to only Mississippi, is that we observe more than twice as many counties whose residents gained access to a Medicare-eligible hospital after 1967, which allows us to distinguish the effects of access from the secular Black IMR trend.

Aside from the pioneering work of Almond et al. (2006), we do not know a great deal about the relationship between hospital desegregation and health.[14] Researchers have, however, attempted to gauge the effects of school desegregation on various educational outcomes. These effects are, *a priori*, difficult to sign (Reber 2007; Reardon and Owens 2014), although several studies provide evidence that Black students benefit from attending integrated schools (Guryan 2004; Reber 2010, 2011; Johnson 2011).[15] There is also a large literature on anti-discrimination policies and labor market outcomes. In a review of this literature, Valfort (2018) concludes that the "punitive approach" (i.e., imposing sanctions on employers who discriminate) is not particularly effective because it does not counter prejudice or limit the expression of cognitive biases.

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[13] Almond et al. (2006, p. 15) note that their estimates become statistically insignificant when year fixed effects are included on the right-hand side of their regression "due to the difficulty of separately identifying the year and event time fixed effects with the modest variation in Medicare certification dates."

[14] Building on the work of Almond et al. (2006), Chay et al. (2009, 2014) argue that hospital desegregation led to a narrowing of the Black-White PNMR gap in the South, ultimately leading to improvements in test scores, educational attainment, and earnings among Blacks. See also Thompson (2019), who provides evidence that selective fertility during the period 1965-1971 drove these improvements.

[15] For instance, Guryan (2004) finds that desegregation led to an almost three percentage-point decline in the dropout rate of Black students but had no appreciable effect on White dropout rates. Reber (2010, 2011) finds that, after desegregation, predominantly Black school districts in Louisiana received more funding and were able to reduce class sizes, while funding and class sizes in predominantly White school districts did not change. Johnson (2011) finds that an additional year of court-ordered desegregation led to a one percentage-point increase in Black high school graduation rates but had no effect on White high school graduation rates. For more information on the relationship between school desegregation and student outcomes, see Ashenfelter et al. (2006), Billings et al. (2014), Bergman (2016), and Gamoran and An (2016). Relatedly, Thompson (2021) finds that school desegregation led to a 42 percent reduction in Black teacher employment.

### 2.3. Other determinants of the Black-White infant mortality gap

The Black-White infant mortality gap has existed in the United States since at least the turn of the 20th century, when reliable mortality data by race first became available (Ewbank 1987). Its causes are still being researched, and debated, today (Elder et al. 2016; Wallace et al. 2017; Smith et al. 2018; Taylor et al. 2019; Anderson et al. 2021a).[16]

Relatively crude measures of socioeconomic status (e.g., mother's age, education, and marital status) can explain approximately one-third of the Black-White infant mortality gap (Elder et al. 2014; Elder et al. 2016). The other two-thirds of the gap are attributable to myriad factors, including, but not limited to, local geography, hospital quality, physician behavior and characteristics, and government policies.[17] Isolating the effects of racial discrimination has proven to be especially challenging (Wallace et al. 2017).

Greenwood et al. (2020) examine newborns delivered in Florida hospitals during the period 1992-2015. Matching the race of the attending physician to that of the newborns, these authors find that Black newborns cared for by a Black physician are more likely to survive than those cared for by a White physician. By contrast, physician race is essentially unrelated to White newborn mortality. Because differences in hospital and physician quality do not explain these results (the authors control for hospital and physician fixed effects), Black physicians appear to be systematically outperforming their White colleagues for other, difficult-to-measure reasons.

Racial prejudice could explain why Black physicians are better at treating Black newborns than their White counterparts. It is also possible that the estimates in Greenwood et al. (2020) are

[16] Abramitzky (2015, pp. 1246-1247) presents an excellent discussion of other important open questions with deep historical roots, or what he calls "big think" questions in economic history.

[17] Elder et al. (2014, 2016) include state indicators on the right-hand side of their regressions, but local geography can be an important obstacle to accessing high-quality healthcare. As noted by Chandra and Skinner (2004) and Chandra et al. (2017), providing health insurance and strictly enforcing Title VI does not make it any easier for minorities to access hospitals that are located far from where they live and work.

driven by mistrust between Black patients and their White physicians. Cooper et al. (2003) provide evidence that healthcare visits are shorter when Black patients are assigned to a White, as opposed to a Black, physician. Alsan et al. (2019) find that Black men are more likely to demand preventative care when randomly assigned to a Black physician.[18] These and similar results have prompted present-day observers to argue that Title VI of the 1964 Civil Rights Act "has not yet had its intended effect" (Frakt 2020, para. 24).

### 2.4. Trends in the Black-White infant mortality gap

Figure 1 shows the evolution of the Black-White infant mortality gap during the period 1959-1973 for three regions of the United States: the Deep South (Alabama, Georgia, Louisiana, Mississippi, and South Carolina), other Southern states, and non-Southern states.[19] Counts of infant deaths by race come from the Multiple Cause-of-Death Files, published by the National Vital Statistics System and available through the National Bureau of Economic Research's Public Use Data Archive.[20] From 1963 to 1973, the Black-White infant mortality gap in the Deep South fell by

[18] See also Hill et al. (2020) and Alsan and Wanamaker (2018). Hill et al. (2020) find that being attended by a same-race physician in the emergency department is associated with a 13 percent reduction in mortality. Alsan and Wanamaker (2018, p. 412) find that proximity to the Tuskegee study had long-lasting effects on whether Blacks "trust a doctor's judgement and whether they suspect that the medical establishment will deny them necessary treatment or services." Yoder and Hardy (2018, p. 4) cite "mistrust of medical providers" as a factor that has led to decreased prenatal care among Black mothers.

[19] The Black-White infant mortality gap is defined as the difference between Black infant deaths per 1,000 Black live births and White infant deaths per 1,000 White live births. It should be noted that our definition of Black infant mortality includes "other" non-Whites. However, during the period under study, infant deaths among "other" non-Whites never exceeded 0.8 percent of total infant deaths in the South and 1.9 percent of total infant deaths in non-Southern States.

[20] In 1972, due to personnel and budgetary restrictions, the mortality counts from the Multiple Cause-of-Death Files were based on information obtained from a 50-percent sample of death records, as opposed to the complete census of death records as in other years (U.S. Department of Health, Education, and Welfare 1976). In practice, this requires aggregating the mortality counts to the county level and multiplying by two.

more than 50 percent, from 25.7 to 12.5. Although the Black-White infant mortality gap also fell in other Southern states and non-Southern states, the trend was most pronounced in the Deep South.[21]

Figure 2 focuses on the evolution of the Black-White infant mortality gap in the Deep South. The Black and White IMRs are both trending downward during the period under study, but the Black IMR trend is steeper. In theory, the narrowing of the Black-White infant mortality gap in the Deep South could have been caused by any number of factors. There was, for instance, a sharp reduction in Black fertility immediately after the passage of the Civil Rights Act (Thompson 2019), and Black Southerners made significant economic progress throughout the 1960s (Freeman 1981; Donohue and Heckman 1991; Wright 1999, 2013).[22] Other government interventions, including the rollout of Community Health Centers (CHCs) and the implementation of state Medicaid programs, could have also contributed to the observed trends (Goldman and Grossman 1988; Bailey and Goodman-Bacon 2015; Goodman-Bacon 2018).[23]

## 3. CERTIFIED HOSPITAL ACCESS AND INFANT MORTALITY

Did the hospital desegregation campaign, which was launched in 1966, contribute to the narrowing of the Black-White infant mortality gap in the Deep South? To distinguish the effect of

[21] Other Southern states include Arkansas, Delaware, the District of Columbia, Florida, Kentucky, Maryland, North Carolina, Oklahoma, Tennessee, Texas, Virginia, and West Virginia. The non-Southern region excludes Alaska, Hawaii, and New Jersey. New Jersey was dropped because information by race was not available for 1962 and 1963.

[22] See Wright (2013, pp. 105-149) for an excellent description of Southern labor markets in the 1960s and the effects of the Civil Rights movement.

[23] The rollout of CHCs across the South began in 1965. Bailey and Goodman-Bacon (2015, p. 1075) note that "CHCs could have facilitated the diagnosis of potentially lethal diseases and afford medications for treatment, but they were not substitutes for hospitals' acute care for sick infants." In 1966, Louisiana became the first state in the Deep South to implement Medicaid. It was followed shortly thereafter by Georgia (1967), South Carolina (1968), Mississippi (1969), and Alabama (1970). Goodman-Bacon (2018) provides evidence that the introduction of state Medicaid programs reduced non-White infant mortality. Below, we report separate pre- and post-Medicaid estimates of the effect of having access to a desegregated (i.e., Title VI-compliant) hospital. Hospitals that were certified as Title VI-compliant could participate in both programs.

the hospital desegregation campaign from the effects of Black economic progress and other government interventions, we begin by estimating an event-study regression model by race:

$$(1) \qquad IMR_{ct} = a_0 + v_c + \sum_{y=-4}^{-2} \pi_y 1\,(t - T_c^* = y) + \sum_{y=0}^{4} \pi_y 1\,(t - T_c^* = y) + \varepsilon_{ct},$$

where $IMR_{ct}$ is the infant mortality rate in county $c$ and year $t$ = 1959…1973.[24] County fixed effects, $v_c$, account for determinants of infant mortality that were constant over time. Following Almond et al. (2006), year fixed effects, which would account for shared shocks to infant mortality across the Deep South, are not included on the right-hand side of equation (1).

The event-year dummies, $y$, are equal to 1 when the year of observation, $t$, is $y$ = −4,…,0,…,4 years from $T_c^*$, the year in which the first hospital in county $c$ was listed as Medicare-eligible in "Guide Issues," published by the *Journal of the American Hospital Association* (*JAHA*).[25] If there were no hospitals in county $c$, then treatment status was determined based on whether there was a certified hospital operating in a bordering county.

[24] Information on county-level live births comes from several sources, which are provided in Appendix Table B1. While we report estimates based on the IMR throughout, results are similar if we take the natural log of one plus the IMR or the quartic root of the IMR. The quartic root function has been used by other researchers to account for zeros (Thomas et al. 2006; Tarozzi et al. 2014; Ashraf et al. 2015; Anderson et al. 2021b).

[25] The Medicare program was officially launched on July 1, 1966. The 1967 *JAHA* Guide Issue lists Medicare-eligible hospitals through February of 1967. If any hospital in county $c$ was certified as Medicare-eligible in 1966 or in January/February of 1967, then the $y$ = 0 event-year dummy is equal to 1 in $t$ = 1967, and equal to 0 otherwise. The $y$ = 1 event-year dummy is equal to 1 in $t$ = 1968, and equal to 0 otherwise, and so forth. The 1968 *JAHA* Guide Issue lists Medicare-eligible hospitals through February of 1968. If the 1967 Guide Issue did not list any hospital in county $c$ as certified but the 1968 Guide Issue did, then the $y$ = 0 event-year dummy is equal to 1 in $t$ = 1968, the $y$ = 1 event-year dummy is equal to 1 in $t$ = 1969, and so forth. See the notes to Appendix Table B2 in Anderson et al. (2021c) for more information on Medicare certification listings. In an effort to confirm the accuracy of the certification dates in the Guide Issues, we spot checked them against information available in contemporary local newspaper articles. Almost without exception, the certification dates listed in the Guide Issues were consistent with contemporary accounts. Note that the $y$ = -4 event-year dummy is equal to 1 if $t$ is 4 or more years before $T_c^*$. Likewise, the $y$ = 4 event-year dummy is equal to 1 if $t$ is 4 or more years after $T_c^*$. The results presented below are similar if we alternatively define the event-year dummies, $y$, as equal to 1 when the year of observation, $t$, is $y$ = −5,…,0,…,5 years from $T_c^*$.

The estimates of $\pi_y$ (i.e., the coefficients of the event-year dummies) characterize the effects of having access to a general or maternity hospital that was certified as Medicare-eligible (and therefore ostensibly desegregated). Note that $y = -1$ is omitted, which normalizes the estimates of $\pi_y$ to 0 in that year. The pre-treatment estimates of $\pi_y$ can be thought of as falsification tests—their patterns and statistical significance allow us to investigate the parallel trends assumption.

Estimates of (1) for Black infants are presented in panel A of Figure 3. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level (Bertrand et al. 2004).[26] Consistent with the results of Almond et al. (2006), the estimates of $\pi_y$ are consistently negative in the post-treatment period. For instance, in the first year of treatment (i.e., $y = 0$) having access to a Medicare-eligible hospital is associated with a 3.21 reduction in the Black IMR, which is 6.7 percent of the pre-treatment mean; four or more years later, having access to a Medicare-eligible hospital is associated with 13.3 fewer Black infant deaths per 1,000 births, which is very close to the largest event-study estimates reported by Almond et al. (2006). The obvious downward trend in the pre-treatment period, however, casts doubt on whether these estimates should be given a causal interpretation.

Because year fixed effects are not included on the right-hand side of equation (1), we cannot distinguish between the effects of having access to a Medicare-eligible hospital and common shocks to IMRs across the Deep South. Exploiting cross-county variation in Medicare-certification dates, we estimate a more standard event-study regression by race:

$$(2) \qquad IMR_{ct} = a_0 + v_c + \lambda_t + \sum_{y=-4}^{-2} \pi_y 1\,(t - T_c^* = y) + \sum_{y=0}^{4} \pi_y 1\,(t - T_c^* = y) + \varepsilon_{ct},$$

[26] We report 90 percent confidence intervals in the event-study figures.

where the year fixed effects are represented by $\lambda_t$.[27] With the inclusion of year fixed effects, the difference-in-difference (DD) analogue of equation (2) is:

$$(3) \qquad IMR_{ct} = a_0 + v_c + \lambda_t + a_1 Medicare_{ct} + \varepsilon_{ct},$$

where $Medicare_{ct}$ is equal to 1 if Black mothers in county $c$ and year $t$ had access to a general or maternity hospital that was certified as Medicare-eligible (and is equal to 0 otherwise).

Estimates of equations (2) and (3) are reported in panel B of Figure 3. They provide little evidence that the hospital desegregation campaign reduced the Black IMR. In fact, four out of five post-treatment estimates of $\pi_y$ are positive, although they are small in magnitude and statistically insignificant.

Including a vector of county-level controls, $\boldsymbol{X}_{ct}$, on the right-hand side of the estimating equation does not appreciably affect this basic pattern of results (panel C, Figure 3).[28] According to the DD estimate from the fully specified regression model, which summarizes the event-study estimates, access to a certified hospital is associated with a 1.63 increase in the Black IMR. Although statistically insignificant at conventional levels, the DD estimate is precise, indicating that we have sufficient cross-county variation in Medicare certification dates to distinguish between the effects of gaining access and secular Black IMR trends. Based on the lower bound of the 90 percent

[27] Appendix Table A1 shows the number of counties in the Deep South in which Black mothers had access to a Medicare-eligible hospital by state and year. In 1967, Black mothers had access in 335 out of the 403 Deep South counties in our sample; by 1968, they had access in 378 of these counties; and by 1969 they had access in 385. Black mothers in the remaining 18 out of 403 counties gained access to Medicare-certified hospitals between 1970 and 1973. Fifteen Deep South counties are missing from our sample. Nine Alabama counties are excluded due to missing data on live Black births. Six Georgia counties are excluded because no live Black births were recorded during the sample period.

[28] The controls are listed in Appendix Table A3, along with descriptive statistics, definitions, and sources. They include the percent of the county population that was 25 years of age or older with a high school diploma, direct health and hospital expenditures by the county government, and the county employment to population ratio. Missing values were calculated using linear interpolation and extrapolation.

confidence interval of this estimate, gaining access to a Medicare-eligible hospital did not reduce the Black IMR by more than -.407, or less than 3 percent of the actual change in the Black IMR between 1965 and 1973.[29]

Event-study estimates for White infant mortality, which are reported in panels A-C of Figure 4, are similar to the Black infant mortality estimates. Without controlling for year fixed effects, the post-treatment estimates of $\pi_y$ are negative and statistically significant, but clearly represent the continuation of a pre-treatment trend. With year fixed effects and $\boldsymbol{X_{ct}}$ on the right-hand side of the regression, the pre-treatment trend flattens out and the post-treatment estimates of $\pi_y$ lose significance. In general, we view the results reported in Figures 3 and 4 as evidence that the negative post-treatment estimates of $\pi_y$ produced using equation (1) simply reflect common determinants of Black and White infant health as opposed to the effects of the hospital desegregation campaign.[30]

### 3.1. Postneonatal mortality and infant mortality by cause

Almond et al. (2006) focus on the Black-White postneonatal mortality gap, arguing that "medical care was most successful in preventing deaths during the post-neonatal period" (p. 7).[31] In Figure 5, we report event-study estimates of the effect of gaining access to a Medicare-eligible hospital on Black neonatal (panel A) and postneonatal mortality (panel B), controlling for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects.

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[29] During the period 1965-1973, the Black IMR in the Deep South fell from 45.7 to 31.3.

[30] In Appendix Figure A1, we report event-study estimates based on modified versions of equations (1) and (2), in which the $IMR_{ct}$ is replaced with the difference between Black and White infant mortality rates (i.e., the Black-White infant mortality gap). Not surprisingly, given the results reported in Figures 3 and 4, there is no evidence that gaining access to a Medicare-eligible hospital affected the Black-White infant mortality gap.

[31] Medical technologies benefitting premature and low-weight infants (e.g., improvements in mechanical ventilation and local neonatal intensive care units) were not developed and diffused widely until the 1970s and 1980s (Almond et al. 2006). Appendix Figure A2 shows the narrowing of the Black-White postneonatal mortality gap, which was most pronounced in the Deep South. From 1959 to 1973, the Black PNMR in the Deep South fell from 23.1 to 12.8, or 45 percent (Appendix Figure A3).

Regardless of which outcome is used, there is little evidence that gaining access to a Medicare-eligible hospital improved the health of Black infants. The post-treatment estimates of $\pi_y$ are small, often positive and, without exception, statistically insignificant.[32]

Pneumonia/influenza and diarrhea were two of the leading causes of mortality among U.S. infants during the sample period (U.S. Department of Health, Education, and Welfare 1963).[33] Event-study estimates of the effect of gaining access to a Medicare-eligible hospital on Black infant mortality due to pneumonia/influenza is reported in panel A of Figure 6; the corresponding event-study estimates for diarrhea mortality are reported in panel B. With one exception, which is positive and statistically significant, the post-treatment estimates of $\pi_y$ are precise nulls. For instance, based on the lower bound of the 90 percent confidence interval, gaining access to a Medicare-eligible hospital did not reduce Black infant deaths due to diarrhea by more than -.508 per 1,000 live Black births in $y = 3$.[34]

### 3.2. Extensions and robustness checks

We begin this section by exploring alternative methods of controlling for underlying trends in Black IMRs. In panel A of Figure 7, we report estimates of the effect of gaining access to a Medicare-eligible hospital on the Black IMR controlling for county-specific linear time trends.

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[32] In Appendix Figure A4, we report event-study estimates of the effect of the hospital desegregation campaign on the Black-White difference in neonatal (panel A) and postneonatal (panel B) mortality. In panel B, the post-treatment estimates of $\pi_y$ are uniformly positive and statistically insignificant.

[33] Appendix Figures A5 and A6 show the evolution of pneumonia/influenza and diarrhea mortality rates, respectively, for infants in the Deep South. Pneumonia and gastroenteritis "were readily and necessarily" treated in hospitals, often with antibiotics, during the 1960s (Almond et al. 2006, p. 12). Appendix Table B2 lists the International Classification of Disease (ICD) codes that were used to generate infant mortality counts by cause. During the period under study, the ICD underwent its 8th revision, taking effect in 1968. Because these changes applied to all counties in our sample, they are captured by the year fixed effects.

[34] In Appendix Figure A7, we report event-study estimates of the effect of the hospital desegregation campaign on the Black-White difference in infant mortality due to pneumonia/influenza (panel A) and diarrhea (panel B). The post-treatment estimates of $\pi_y$ are generally small and statistically insignificant.

Using this specification, there is no evidence of a trend break at $y = 0$ and all the estimates of $\pi_y$ are statistically insignificant, but it is worth noting that the standard errors of the post-treatment indicators become larger. If we control for state-by-year fixed effects (panel B, Figure 7) or detrend the dependent variable (panel C, Figure 7) as suggested by Goodman-Bacon (2021), the results are similar (i.e., there is little evidence of a trend break in the post-treatment period).[35]

If the effect of gaining access to a Medicare-eligible hospital on Black infant mortality is heterogeneous and dynamic, then estimates of equation (2) can be biased (Sun and Abraham 2021). To address this issue, we produce alternative event-study estimates restricting the counterfactuals to "not-yet-treated" counties (Callaway and Sant'Anna 2021).[36] Each event-study estimate in Figure 8 represents the average effect of gaining access to a Medicare-eligible hospital $y$ years from $T_c^*$. Again, we find little evidence that gaining access to a Medicare-eligible hospital reduced Black infant mortality. The overall average treatment effect on the treated (ATT), which is the average of the estimated post-treatment ATTs, is positive and statistically indistinguishable from zero.[37]

In Appendix Table A4, we report DD regression estimates from additional robustness checks.[38] Up to this point in the analysis, we have defined treatment based on when the first

[35] Goodman-Bacon (2021) suggests estimating separate pre-treatment trends based on the timing of treatment (e.g., estimating a pre-treatment trend for counties that became Medicare eligible in 1967, another pre-treatment trend for counties that became Medicare eligible in 1968, and so forth). These pre-treatment trends are then projected onto the post-treatment period and used to detrend the dependent variable. For details on this procedure, see Appendix C in Goodman-Bacon (2021). Although the post-treatment estimates shown in panel C of Figure 7 at $y = 3$ and $y \geq 4$ are negative and significant, this procedure makes the downward pre-treatment trend in Black infant mortality more pronounced.

[36] "Never-treated" counties are also included as counterfactuals, but there are only three never-treated counties in our sample.

[37] For computational purposes, the sample is restricted to a balanced panel of counties and, consequently, is slightly smaller (N = 5,955 as opposed to N = 6,033). Appendix Figure A8 shows estimated ATTs for Black postneonatal mortality and Black infant mortality by cause. An introduction to the R package used to estimate the treatment effects in Callaway and Sant'Anna (2021) is available at: https://bcallaway11.github.io/did/articles/index.html.

[38] We report DD regression estimates for the sake of brevity, but the corresponding event-study figures are available from the authors upon request.

hospital in county *c* was certified as Medicare-eligible. Using information available in the *JAHA* Guide Issues, we explore alternative measures of treatment in columns (1)-(3). Specifically, we define treatment based on the following thresholds: whether 25 percent (or more) of the hospital beds in county *c* belonged to Medicare-eligible hospitals, whether 50 percent of the hospital beds belonged to Medicare-eligible hospitals, and whether 75 percent of the hospital beds belonged to Medicare-eligible hospitals. DD regression estimates based on these thresholds provide no evidence that the campaign to desegregate hospitals reduced the Black IMR.[39]

Next, we distinguish between counties in which the first hospital was certified as Medicare-eligible in 1966-1967 and those in which the first hospital was certified in 1968 or later. Presumably, non-compliance after 1967 is indicative of greater racial animus.[40] The DD regression estimates, which are reported in column (4) of Appendix Table A4, suggest that, regardless of when certification occurred, Title VI compliance had no effect on the Black IMR.

Seventy-five out of the 403 counties in our analysis were not served by a general or maternity hospital prior to 1967. Up to this point in the analysis, treatment for these counties has been based on whether there was a Medicare-eligible hospital operating in a bordering county. If we drop these no-hospital counties from the sample, the resulting DD estimate is small, positive, and statistically insignificant (column 5 of Appendix Table A4). In column (6), we control for the number of

[39] Appendix Table A5 lists the proportion of treated counties by state and year based on these alternative thresholds.

[40] Smith (2005) describes the process of desegregating the two private hospitals in Jackson, Mississippi. St. Dominic Hospital was part of a hospital system based in Springfield, Illinois. It quickly complied with Title VI guidelines and began receiving Medicare payments in July of 1966. By contrast, Baptist Hospital's board of trustees was composed of "white Mississippians" (Smith 2005, p. 262). Baptist Hospital remained segregated through April of 1969, when, under intense financial pressure, the board voted "to take steps to develop a plan for qualifying the hospital for Medicare and Medicaid patients" ("Hospital to Seek CR Okay" 1969). In response to this vote, the state field director of the National Association for the Advancement of Colored People (NAACP), Charles Evers, sent a telegram to Robert Finch, the Secretary of the U.S. Department of Health, Education, and Welfare, describing Baptist as "the most segregated hospital in Mississippi" (Associated Press 1969). Descriptions of hospitals certifying after 1967 in Alabama, Georgia, Louisiana, and South Carolina are available from contemporary newspaper articles. For instance, see: "Monroe Hospital Approved for Medicare" (1968), Constitution State News Service (1968), "Federal Examiner Orders HEW To Certify P&S Hospital Here" (1969), and "Tuomey Now on Medicare" (1969).

bordering counties with at least one Medicare-eligible hospital. Including this variable on the right-hand side of our regression model has little effect on the estimated coefficient of $Medicare_{ct}$.[41] Similarly, not weighting by live births produces little evidence that gaining access to a Medicare-eligible hospital reduced the Black IMR (column 7 of Appendix Table A4).

The Louisiana Medicaid program began issuing payments to hospitals as of July 1, 1966, which means that we cannot distinguish between the effects of gaining access to a Medicaid- vs. Medicare-eligible hospital in Louisiana. Participation in both of these programs required Title VI compliance. In the other Deep South states, Medicaid implementation came after 1966, allowing us to estimate a regression that includes an interaction between $Medicare_{ct}$ and $Medicaid_{st}$ (i.e., an indicator for whether state $s$ had implemented Medicaid by year $t$).[42] The results of this exercise are reported in column (8) of Appendix Table A4. The coefficient estimate on $Medicare_{ct}$ is small, positive, and statistically insignificant. The coefficient estimate of the interaction term, while negative, is also small and statistically insignificant, suggesting that the Medicaid program had little, if any, effect on Black infant mortality.[43]

Finally, in an effort to study the role of Black hospitals, we used data from Rice and Jones (1994), Wesley (2010), and contemporary sources to distinguish between counties that were served by a Black hospital in the pre-treatment period and those that were not. In column (9) of Appendix Table A4, we show DD estimates obtained by interacting $Medicare_{ct}$ with an indicator, $Black\ Hospital_c$,

[41] We also replaced $Medicare_{ct}$ with an indicator equal to 1 if county $c$ or any of its bordering counties had at least one Medicare-eligible hospital during year $t$ (and equal to 0 otherwise). The estimated coefficient of this indicator was positive and statistically significant.

[42] The Georgia Medicaid program was implemented in October of 1967; the South Carolina program was implemented in July of 1968; the Mississippi program was implemented in October of 1969; and the Alabama program was implemented in January of 1970. Estimating a regression model that also controls for the direct effect of Medicaid produces qualitatively similar results.

[43] Exploiting cross-state variation in categorical eligibility, Goodman-Bacon (2018) documents a sharp reduction in non-White infant mortality after the implementation of state Medicaid programs. Dropping Southern states did not appreciably affect his estimates.

equal to 1 if a county was served by at least one Black hospital in 1966 (and equal to 0 otherwise). These estimates suggest that, regardless of whether county *c* was served by a Black hospital, gaining access to a Medicare-eligible hospital had no effect on Black infant mortality.

### 3.3. Focusing on Mississippi

Black IMRs were steadily falling across the Deep South during the 1960s and early 1970s (Figure 9). However, for several years after the passage of the Civil Rights Act, Black IMRs were highest in Mississippi, where opposition to the hospital desegregation campaign was particularly fierce (Reynolds 1997; Smith 2005).

In Figure 10, we report estimates of equation (2) restricting our sample to counties in Mississippi. These estimates provide little evidence that the effect of gaining access to a Medicare-eligible hospital on the Black IMR was more (or less) pronounced in Mississippi than in the rest of the Deep South.[44]

As noted above, Almond et al.'s (2006) event-study analysis is focused on explaining the Black-White PNMR gap in Mississippi. In the first two columns of Table 1, we show event-study estimates taken directly from Almond et al. (2006, Table 4, columns 1a and 1b). Their fully specified model includes county-level controls, county fixed effects, and county-specific linear trends. Their event-study estimates are negative, significant, and large enough to explain the narrowing of the Black-White infant mortality gap in Mississippi from 1965 to 1971. For instance, gaining access to a Medicare-eligible hospital for four years is associated with a 10.3 reduction in the Black PNMR.

[44] Separate event-study estimates for Alabama, Georgia, Louisiana, and South Carolina are reported in Appendix Figure A9. DD estimates by state are reported in Appendix Table A6. Estimates based on models that do not include year fixed effects are reported in the first column and are uniformly negative, large, and statistically significant at the one percent level. Tellingly, the largest negative estimate is for South Carolina, the state that experienced the largest decline in its Black IMR from 1959 to 1973. With the inclusion of year fixed effects, these estimates lose statistical significance and four out of five become positive.

Although we do not have the information necessary to perfectly reconstruct Almond et al.'s (2006) county-level controls, we are able to come reasonably close to reproducing their event-study estimates in columns (3) and (4) of Table 1.[45] In column (5), we omit the county-specific linear trends and instead add year fixed effects to their regression model. The estimated effects of gaining access to a Medicare-eligible hospital become much smaller and lose statistical significance. In column (6), we include both the county-specific linear trends and year fixed effects on the right-hand side of Almond et al.'s (2006) regression model. The standard errors become sufficiently large that we cannot reject the hypothesis that the estimates reported in column (6) are equal to those reported in either column (4) or (5). Similarly, we cannot reject the hypothesis that the estimated effects on the Black PNMR are equal to the estimated effects on the White PNMR, which are also consistently negative and of roughly comparable magnitude (panel II of Table 1).[46]

## 4. In-Hospital Births and Maternal Mortality

During the period 1955-1965, out-of-hospital births to Southern Black mothers were declining steadily (Chay and Greenstone 2000). By 1965, less than 10 percent of Black infants in urban Southern counties were delivered at home, attended by a doctor or midwife; less than 30

[45] Specifically, Almond et al. (2006) control for the fraction of mothers across five age categories, the fraction of unmarried mothers, per-capita income, and per-capita transfer payments.

[46] In Appendix Table A7, we report the event-study estimates produced by Almond et al. (2006, Table 4, columns 2a and 2b) for postneonatal mortality due to pneumonia, influenza, and diarrhea. The results are similar to those reported in Table 1. In the top panel of Appendix Figure A10, we reproduce the event-study estimates in Almond et al. (2006, Figure 3), which are from a specification that does not include county fixed effects, year fixed effects, or county-level covariates. In the bottom panel of Appendix Figure A10, we illustrate what happens to these estimates when county and year fixed effects are included in the regression model. Finally, we adopt Almond et al.'s (2006) specification in Appendix Table A8 and estimate the effect of the hospital desegregation campaign on Black postneonatal mortality in all five Deep South states. When we include year fixed effects and county-specific linear trends, the estimated effects of having access to a Medicare-eligible hospital on Black postneonatal mortality due to pneumonia, influenza, and diarrhea become positive and significant (panel II, column 4), allowing us to formally reject the hypothesis that they are equal to those produced without controlling for year fixed effects (panel II, columns 1 and 2).

percent of Black infants in rural Southern counties were delivered at home (Chay and Greenstone 2000).

Giving birth in the hospital did not, however, guarantee receipt of high-quality care for Black mothers and their babies. "Biracial hospitals" discriminated against Black patients, assigning them to separate wards or buildings, while Black hospitals were, with only a few exceptions, understaffed and under-resourced (Reynolds 2004; Thomas 2006; McBride 2018, pp. 49-50).[47] After the introduction of Medicare and Medicaid, Black hospitals struggled financially and most eventually closed (Beardsley 1996; Odum 1992; Smith 2016, pp. 168-169). Black mothers living in the South were left with the choice of delivering in a formerly discriminatory (i.e., White-only or segregated) hospital or at home, attended by a midwife or physician, neither of whom was necessarily well trained in obstetrics (Mongeau et al. 1961; Houde et al. 1982; Ward 2003).[48]

Figure 11 shows the in-hospital birth rate (i.e., the number of in-hospital births per 1,000 live births) by race in the Deep South for the years 1959-1973. In-hospital Black births were rising steadily throughout this period. In 1959, the first year of our analysis, 57 percent of Black births took place in the hospital. By 1973, 94 percent of Black births were in-hospital.[49]

Did federal efforts to desegregate hospitals contribute to the trends shown in Figure 11? To answer this question, we use data from state vital statistics reports and the NVSS for the period

[47] The practice of assigning Black patients to their own wards, floors, or buildings also had the effect of limiting access. To avoid overcrowding, Black patients could be refused admission despite the availability of "white beds" (U.S. Civil Rights Commission 1966, pp. 12-13). Black hospitals were often White-run but focused on providing care to Black patients (Rice and Jones 1994).

[48] During the period under study, midwives across the Deep South were required to be licensed (Anderson et al. 2020). According to contemporary accounts, licensing greatly improved the quality of midwifery services provided (South Carolina 1960; Bonaparte 2014). Nonetheless, midwives were not trained to attend complicated pregnancies (Dodd 1920; Mongeau et al. 1961).

[49] Appendix Figures A11 and A12 show out-of-hospital physician-attended birth rates by race and out-of-hospital midwife-attended birth rates by race, respectively. In Appendix Figure A13, we use data from the National Health Interview Survey to examine past-year hospital admissions among infants. The Black-White gap in hospital admission rates began trending upward in 1964, peaked in 1966, and then leveled off.

1959-1973.[50] In panel A of Figure 12, we report event-study estimates of the effect of gaining access to a Medicare-eligible hospital on *In-Hospital Black Births*$_{ct}$, equal to the number of in-hospital Black births per 1,000 live Black births in county *c* and year *t*. The results provide no evidence that the hospital desegregation campaign was responsible for the increase in the in-hospital birth rate among Black mothers during the period under study. In fact, gaining access to a Medicare-eligible hospital is associated with a statistically significant decrease in the Black in-hospital birth rate four or more years after treatment.

In the remaining panels of Figure 12, we explore whether gaining access to a Medicare-eligible hospital affected Black out-of-hospital births by attendant type (i.e., physician versus midwife).[51] The event-study estimates provide evidence that, three or four years after treatment, gaining access to a Medicare-eligible hospital led to fewer out-of-hospital Black births attended by physicians and more out-of-hospital Black births attended midwives (panels B and C, Figure 12). We should note, however, that these estimates are quite sensitive to model specification.[52]

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[50] Because the Natality Files, published by the NVSS, did not make information available on birth attendant type for the years 1960-1967, we transcribed records from individual state vital statistics reports. Appendix Table B1 lists the sources used to collect information on county-level live births by location (i.e., in- versus out-of-hospital) and attendant (i.e., physician versus midwife). Another advantage to using state vital statistics records is that the NVSS is missing data on live births by race for roughly 10 percent of the counties in our sample. Information on live births by attendant is unavailable at the county level from Alabama vital statistics records. As a result, Alabama is excluded from all birth location/attendant analyses in this section. Prior to 1975, information on whether a hospital birth was attended by a physician or a midwife was not included on the birth certificate. Presumably, however, the vast majority of these births were attended by a physician. In 1975, 97 percent of in-hospital births in the United States were attended by a physician. Of the remainder, 0.6 percent were delivered by midwives and 2.3 percent were delivered by "other" persons or persons for whom no status was specified (U.S. Department of Health, Education, and Welfare 1978)

[51] During this period, birth certificates recorded whether an out-of-hospital birth was attended by a physician, midwife, or other/not specified attendant (U.S. Department of Health, Education, and Welfare 1978). We included out-of-hospital births by "other/not specified" in the count of out-of-hospital births attended by a midwife. Based on our own calculations, over 90 percent of births in this combined category were attended by midwives.

[52] For instance, controlling for county-specific linear time trends, the post-treatment estimates of $\pi_y$ for in-hospital Black births become positive and significant, while the post-treatment estimates of $\pi_y$ for out-of-hospital Black births attended by midwives become negative and significant (Appendix Figure A14). Both sets of estimates, however, are clearly extensions of trends that begin in the pre-treatment period.

Finally, we report event-study estimates of the effect of gaining access to a Medicare-eligible hospital on the Black maternal mortality rate, defined as deaths due to complications from pregnancy or childbirth per 1,000 live births (panel D, Figure 12). These estimates provide little support for the notion that the hospital desegregation campaign improved the maternal health of Black mothers in the Deep South during the period 1959-1973.[53]

## 5. CONCLUSION

According to Title VI of the Civil Rights Act of 1964, no person "on the ground of race, color, or national origin" should be denied the benefits of any program receiving federal financial assistance (U.S. Department of Labor n.d.). When the Civil Rights Act was passed, most U.S. hospitals did not rely on federal money and the discriminatory practices of Southern hospitals continued largely unabated. The landscape radically changed, however, when President Johnson signed Medicare into law on July 30, 1965, promising to generously pay for the health care of millions of people. The threat of withholding Medicare funding, coupled with the efforts of investigators from the newly created Office of Equal Health Opportunity who were tasked with identifying hospitals that were not in compliance with Title VI, led to the eventual desegregation of even the most notoriously segregated hospitals in the South (Associated Press 1969).

Focusing on one Deep South state, Mississippi, Almond et al. (2006) find that access to a Medicare-eligible hospital is associated with sharp reductions in Black postneonatal mortality, especially mortality due to causes considered preventable with timely hospital treatment. They conclude that, by prohibiting institutions that received federal funds from discriminating, the

[53] Maternal mortality counts included women in the "puerperal state," which lasted through pregnancy and continued for 42 days after delivery (Guyer et al. 2000). Because the maternal mortality rate is equal to 0 for 78 percent of our county-year observations, we also estimated a Poisson regression model. The results were qualitatively similar to those reported in panel D of Figure 12.

hospital desegregation campaign saved tens of thousands of Black infant lives. Aside from Almond et al. (2006), we know very little about the relationship between hospital desegregation and health.

Using county-level mortality data from five Deep South states and exploiting considerably more variation in Medicare certification dates than was available to Almond et al. (2006), we revisit the relationship between the federal hospital desegregation campaign and the Black IMR for the period 1959-1973. Our results suggest that gaining access to a Medicare-eligible hospital had little, if any, effect on Black infant mortality. Specifically, we find that having access to a certified hospital is associated with 1.63 additional Black infant deaths per 1,000 live Black births. Although this estimate is not statistically significant, it is sufficiently precise to reject the hypothesis that the hospital desegregation campaign contributed meaningfully to the narrowing of the Black-White infant mortality gap. Likewise, we find that gaining access to a Medicare-eligible hospital had no appreciable effect on Black postneonatal mortality, nor did it affect Black infant deaths due to preventable causes (i.e., pneumonia/influenza and diarrhea). Again, by leveraging considerably more cross-county variation in Medicare certification dates than did Almond et al. (2006), we are able to measure these effects with precision, clearly distinguishing them from secular trends.

Using newly transcribed data on live births by race, location (in-hospital versus out-of-hospital), and attendant type (physician versus midwife) available from annual state vital statistics reports, we find no evidence that the hospital desegregation campaign accelerated the trend towards in-hospital births among Southern Black mothers, which was originally documented by Chay and Greenstone (2000). There is, however, some evidence—albeit sensitive to model specification—that gaining access to a Medicare-eligible hospital led to fewer Black births attended by a physician and more out-of-hospital Black births attended by a midwife.

The effort to desegregate Southern hospitals was met with considerable resistance. Two hundred and fourteen hospitals in the South initially refused to integrate their facilities, forgoing all

federal funding (Nash 1968; Reynolds 1997); ostensibly integrated hospitals openly flouted Title VI guidelines (U.S. House of Representatives 1973; Quadagno 2000); and Black patients were reticent to seek care at private hospitals that had, for generations, segregated or excluded them altogether (Comptroller General 1972; Quadagno 2000). Had the goals of the federal effort been embraced by White-run hospital administrators and physicians, its short-run effects on Black infant and maternal health may well have been different from those we document above.

Our empirical strategy prevents us from exploring the longer-run effects of the hospital desegregation campaign. It is, however, worth noting that the Black-White infant mortality gap has been closing steadily in several Southern states over the past two decades (Speights et al. 2017). Identifying the factors that contributed—and continue to contribute—to the narrowing of the Black-White infant mortality gap will depend on identifying well-defined natural experiments and carefully accounting for secular trends.

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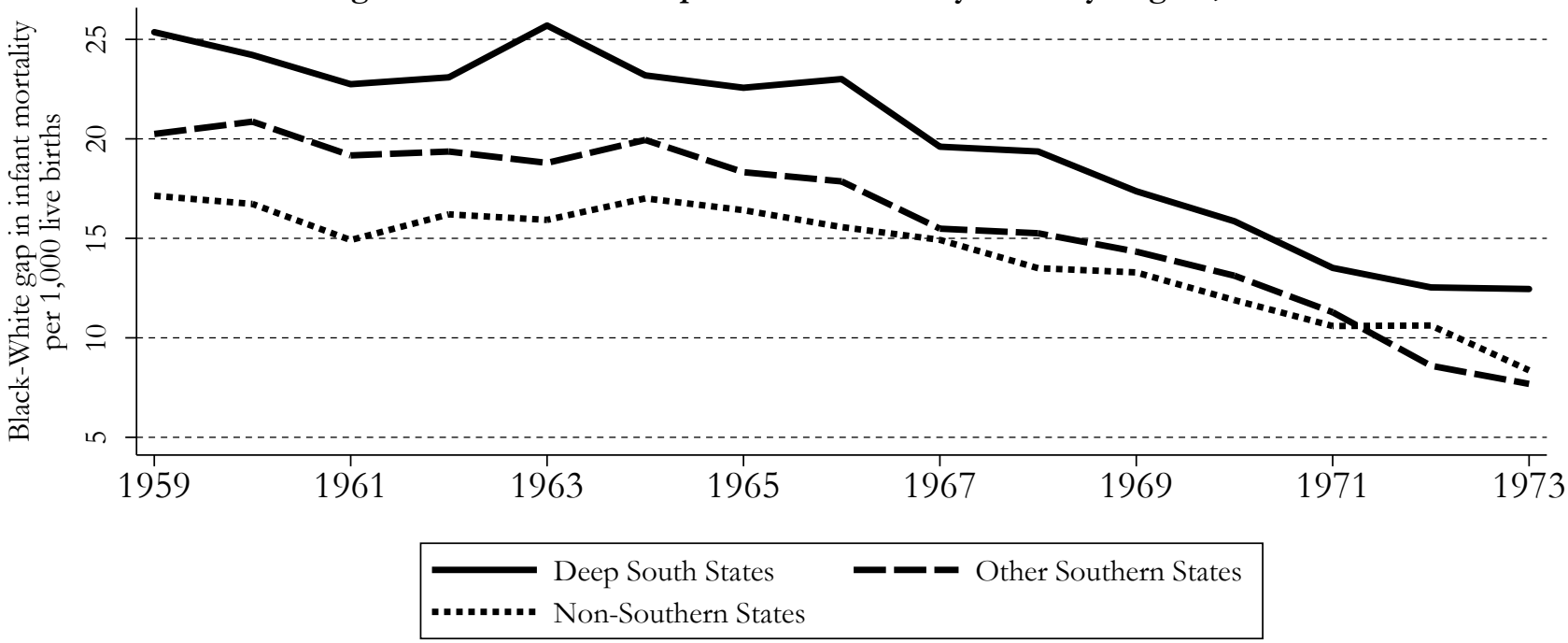
**Figure 1. Black-White Gap in Infant Mortality Rates by Region, 1959-1973**




Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

**Figure 2. Infant Mortality Rates by Race in the Deep South, 1959-1973**

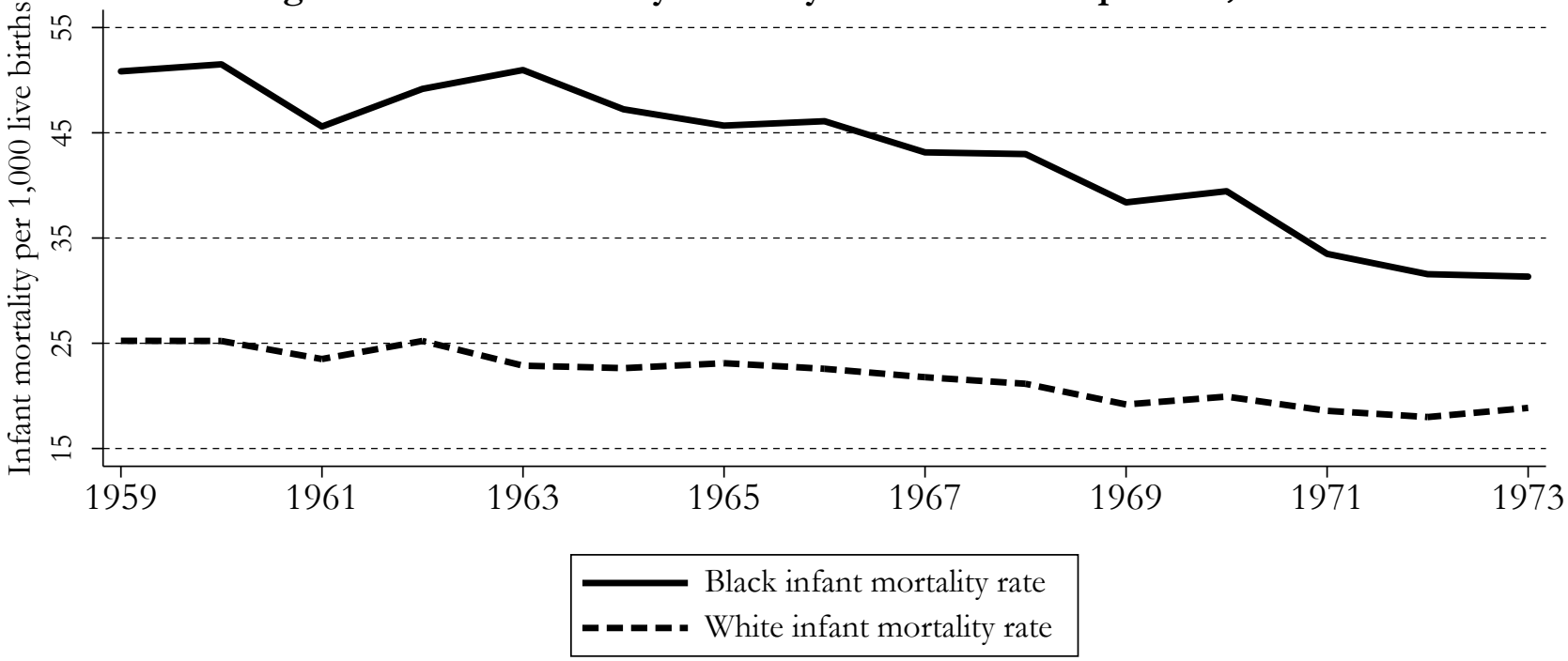



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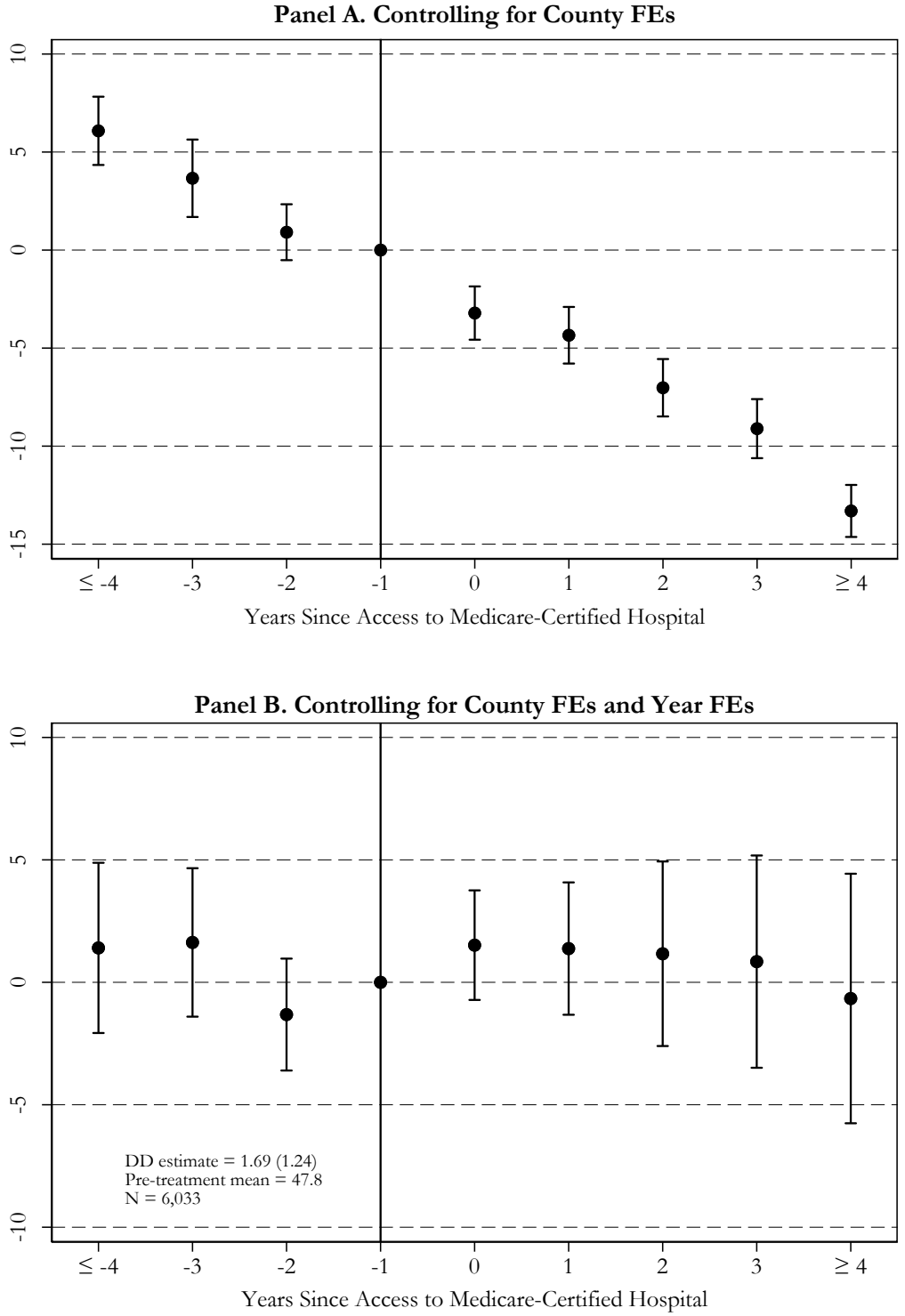
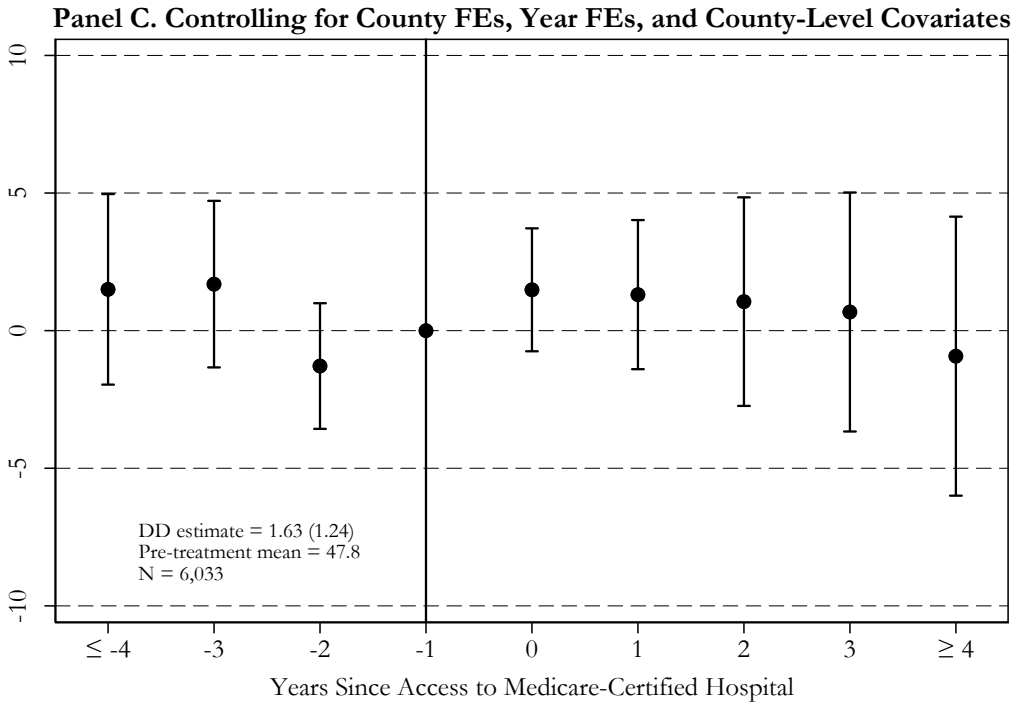
**Figure 3. The Effect of the Hospital Desegregation Campaign on Black Infant Mortality, 1959-1973**

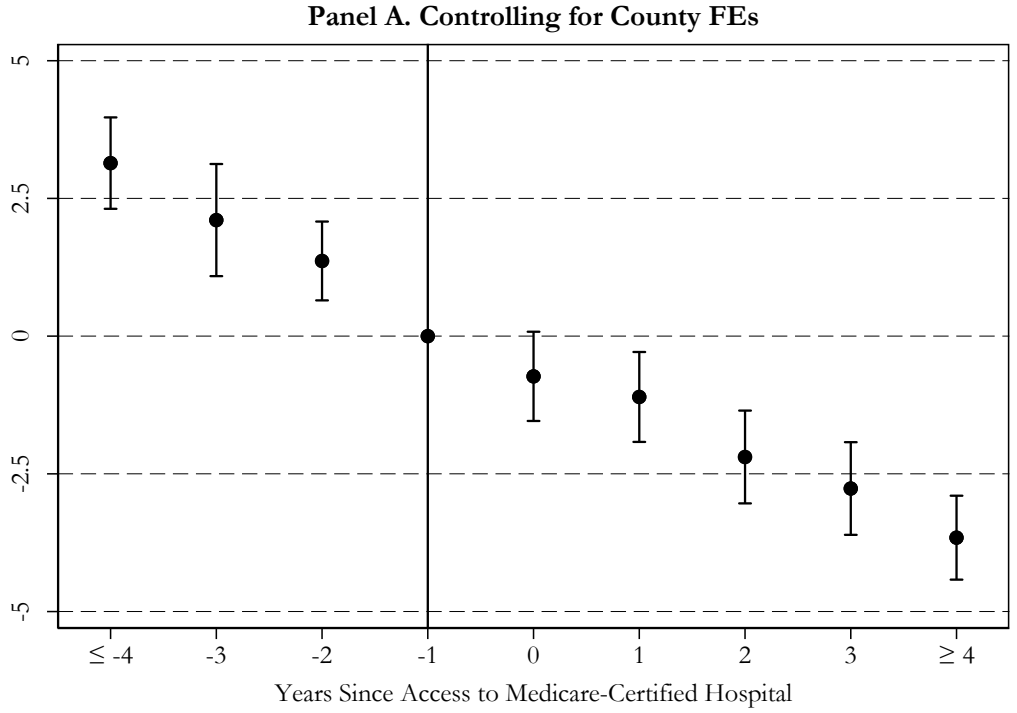


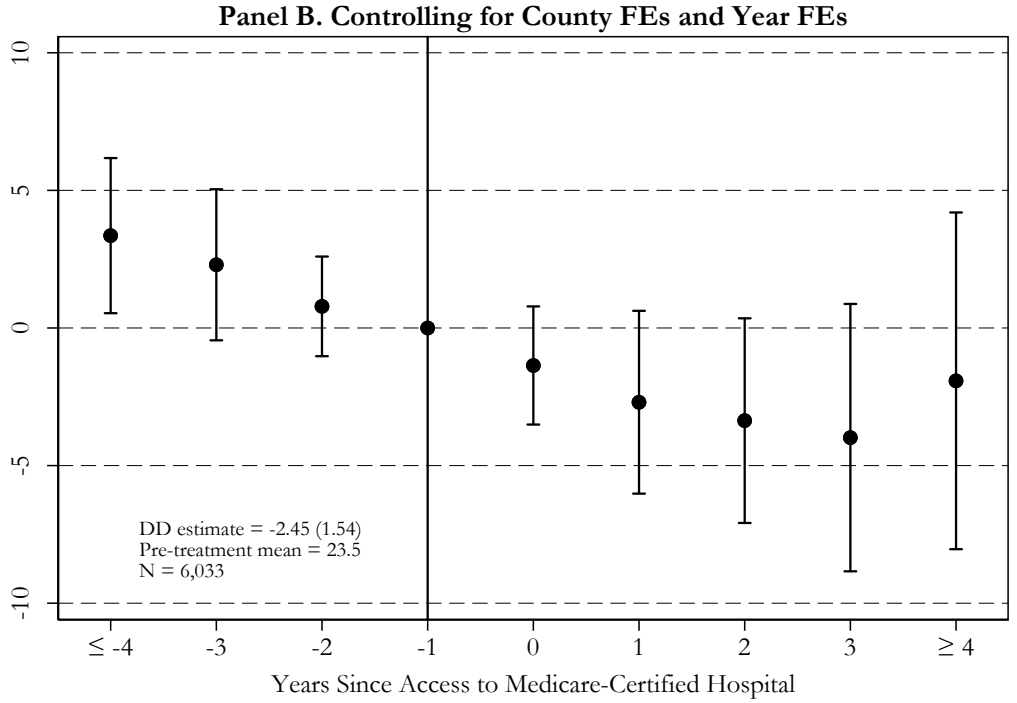


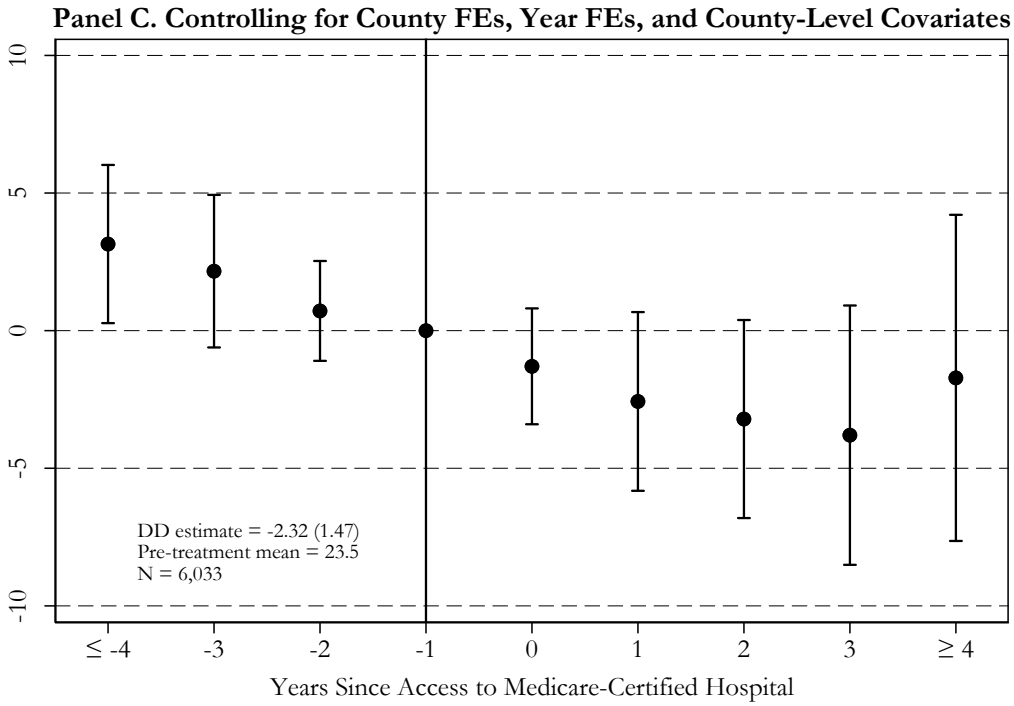

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. All models control for county fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise). Columns (1)-(3) of Appendix Table A2 present the estimates shown above.

**Figure 4. The Effect of the Hospital Desegregation Campaign on White Infant Mortality, 1959-1973**



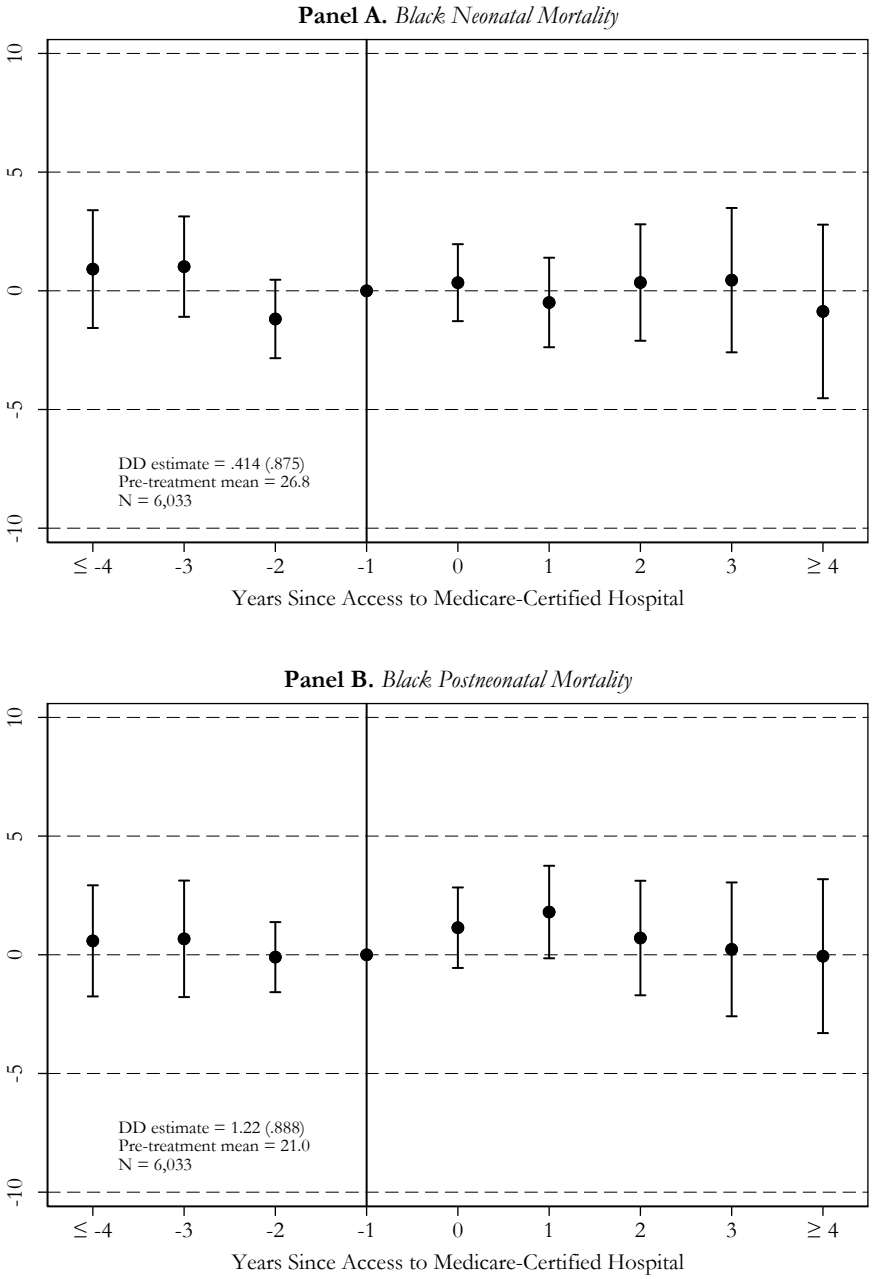






*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of White infant deaths per 1,000 live White births in county *c* and year *t*. All models control for county fixed effects. Regressions are weighted by live White births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if White mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise). Columns (4)-(6) of Appendix Table A2 present the estimates shown above.

**Figure 5. The Effect of the Hospital Desegregation Campaign on Black Neonatal and Postneonatal Mortality, 1959-1973**

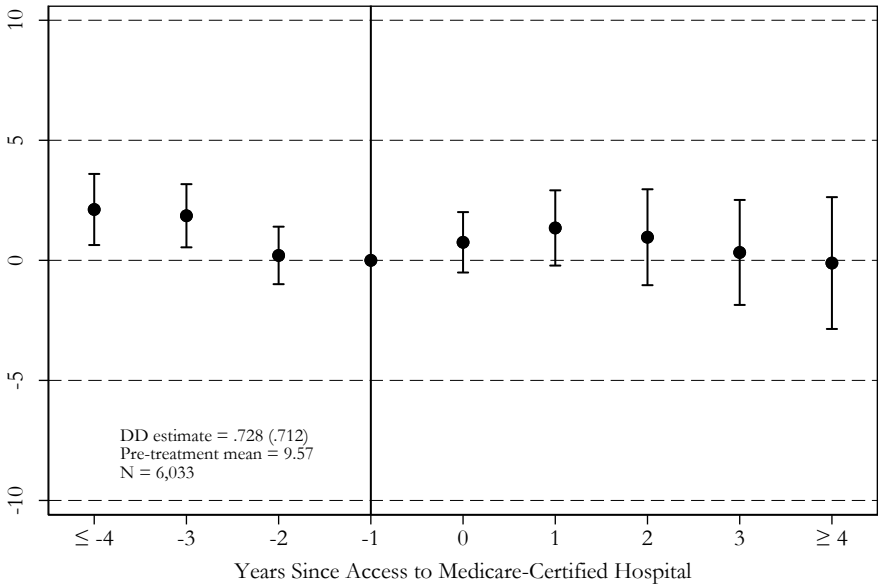



*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

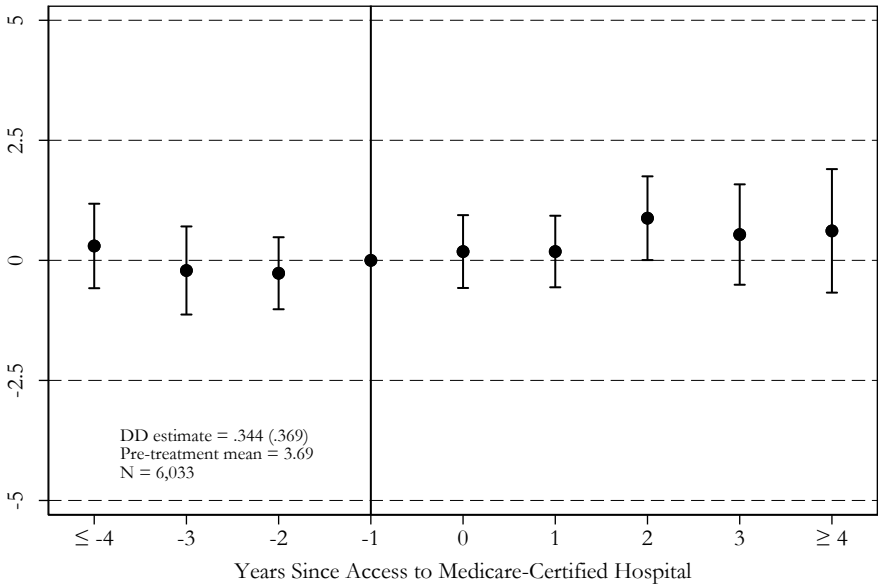
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of specified Black deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Figure 6. The Effect of the Hospital Desegregation Campaign on Black Infant Mortality by Cause, 1959-1973**

**Panel A.** *Black Infant Mortality Due to Pneumonia/Influenza*



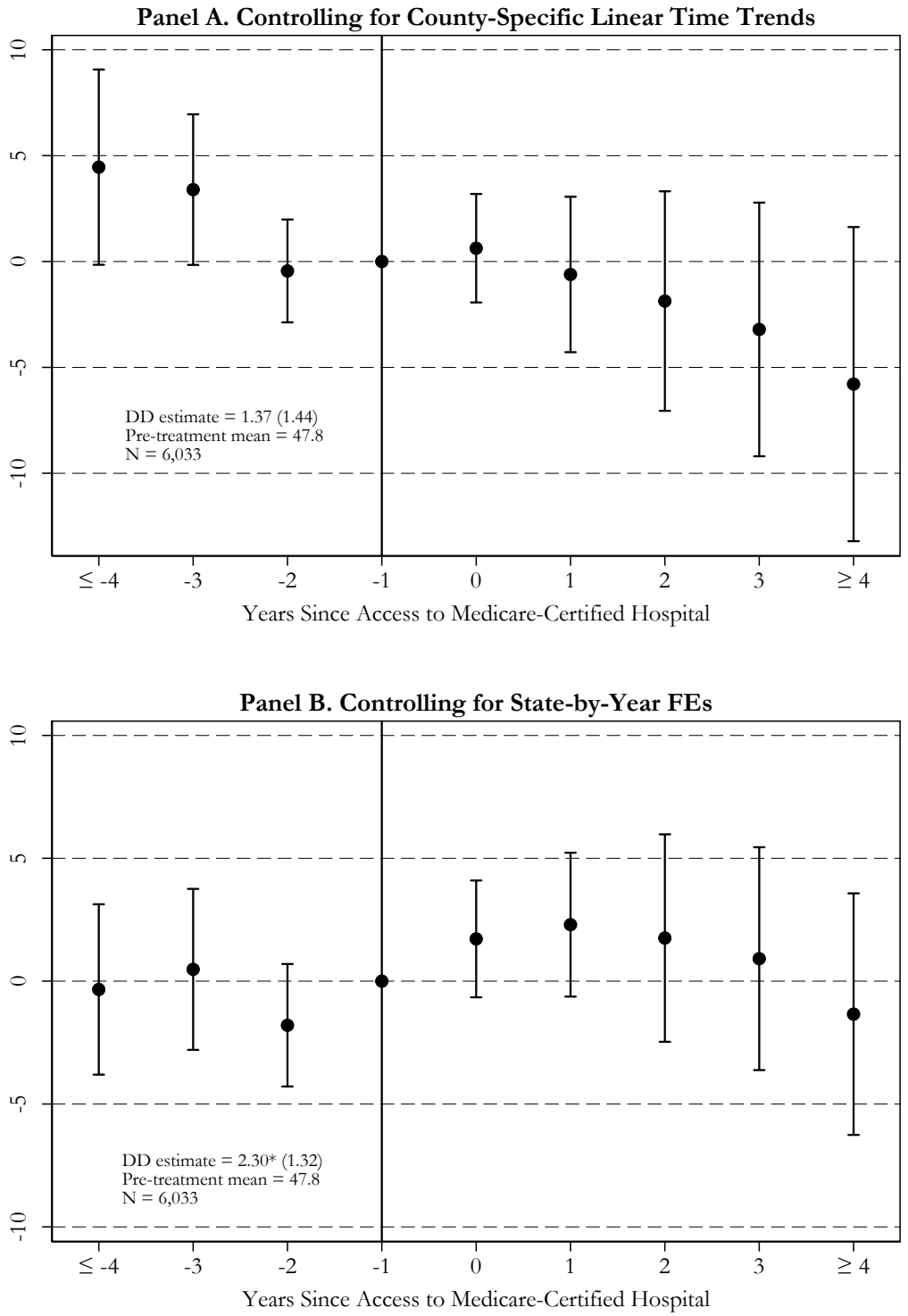

**Panel B.** *Black Infant Mortality Due to Diarrhea*

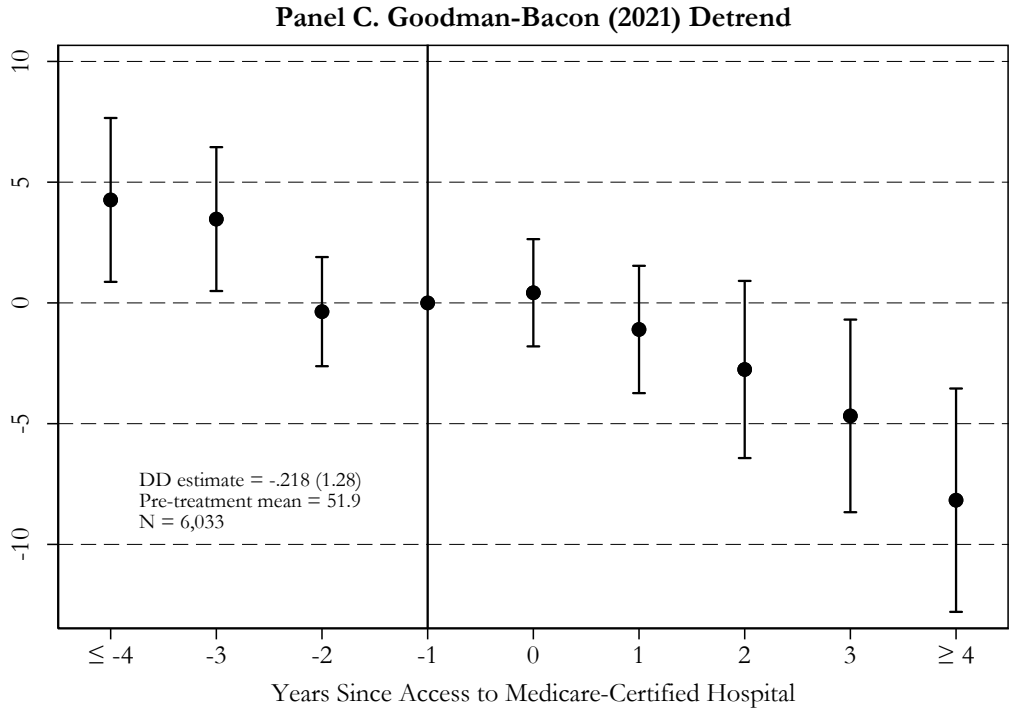



*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of specified Black deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Figure 7. Robustness Checks: The Effect of the Hospital Desegregation Campaign on Black Infant Mortality, 1959-1973**

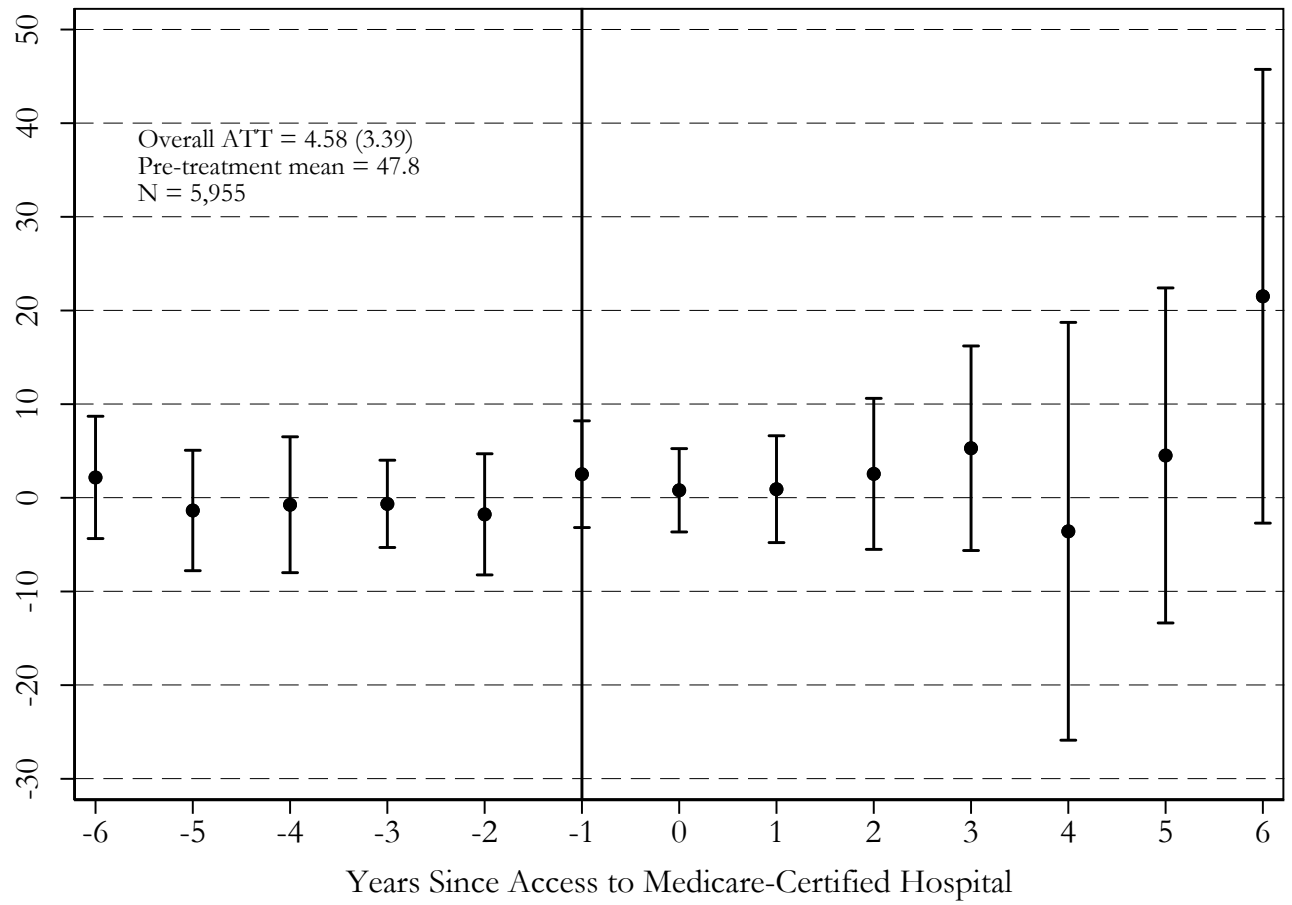





*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

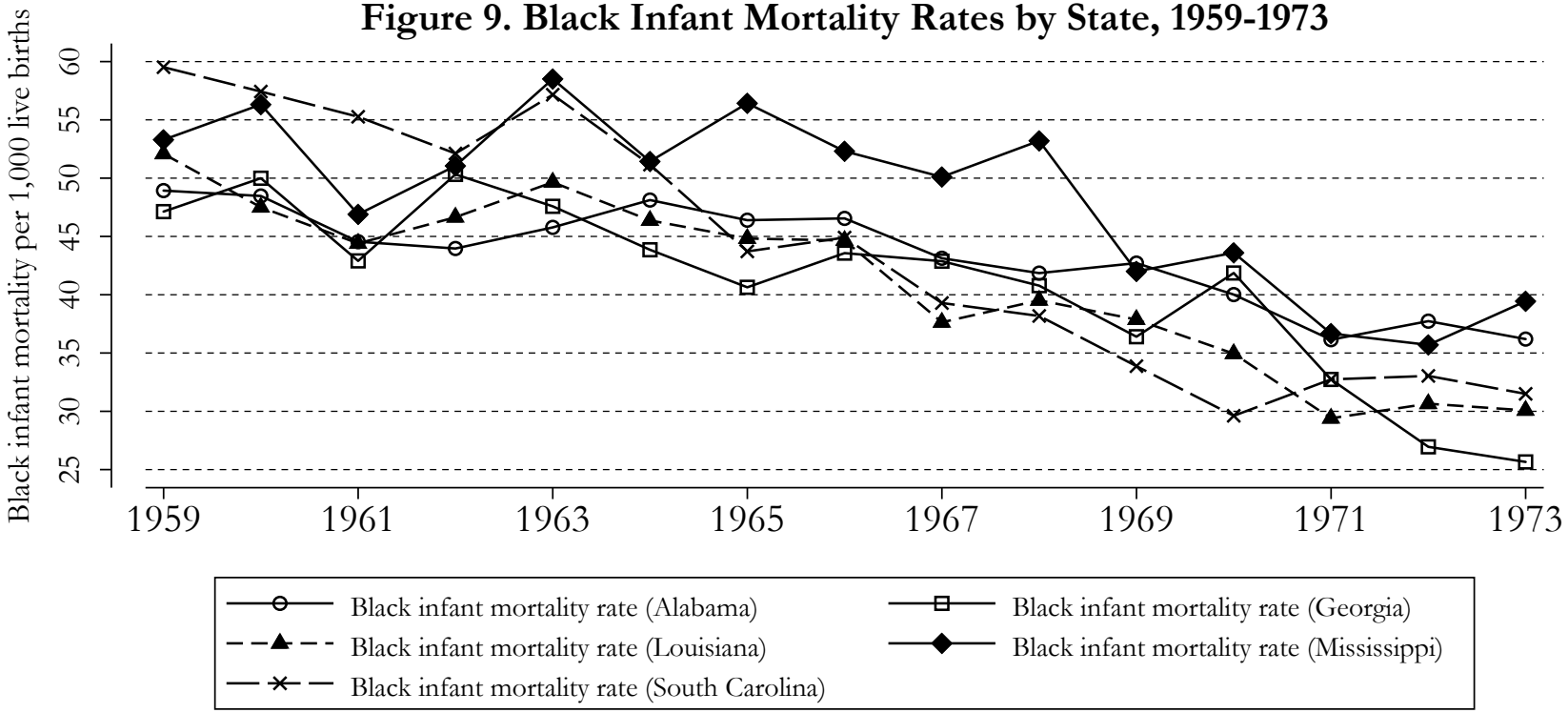
**Figure 8. Callaway and Sant'Anna (2021) Event Study: The Effect of the Hospital Desegregation Campaign on Black Infant Mortality, 1959-1973**




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

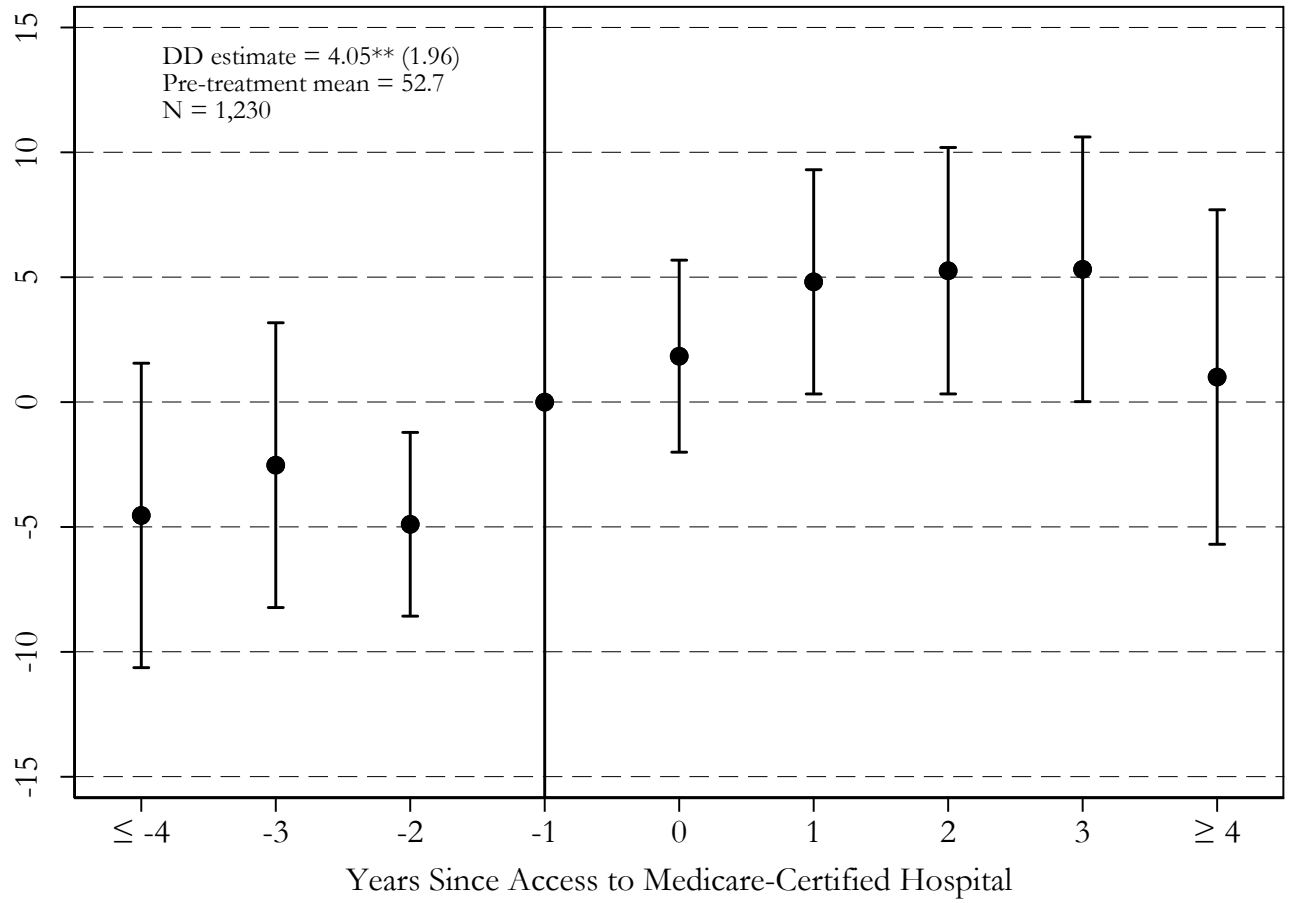
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Estimates of group-time average treatment effects on the treated (ATTs) and their 90% confidence intervals are reported. ATT estimates are from equation (3.4) in Callaway and Sant'Anna (2021). The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. Estimated ATTs are weighted by live Black births and standard errors are corrected for clustering at the county level. The overall ATT is the average of the estimated ATTs in the post-treatment period and is from equation (3.12) in Callaway and Sant'Anna (2021).

## Figure 9. Black Infant Mortality Rates by State, 1959-1973




Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

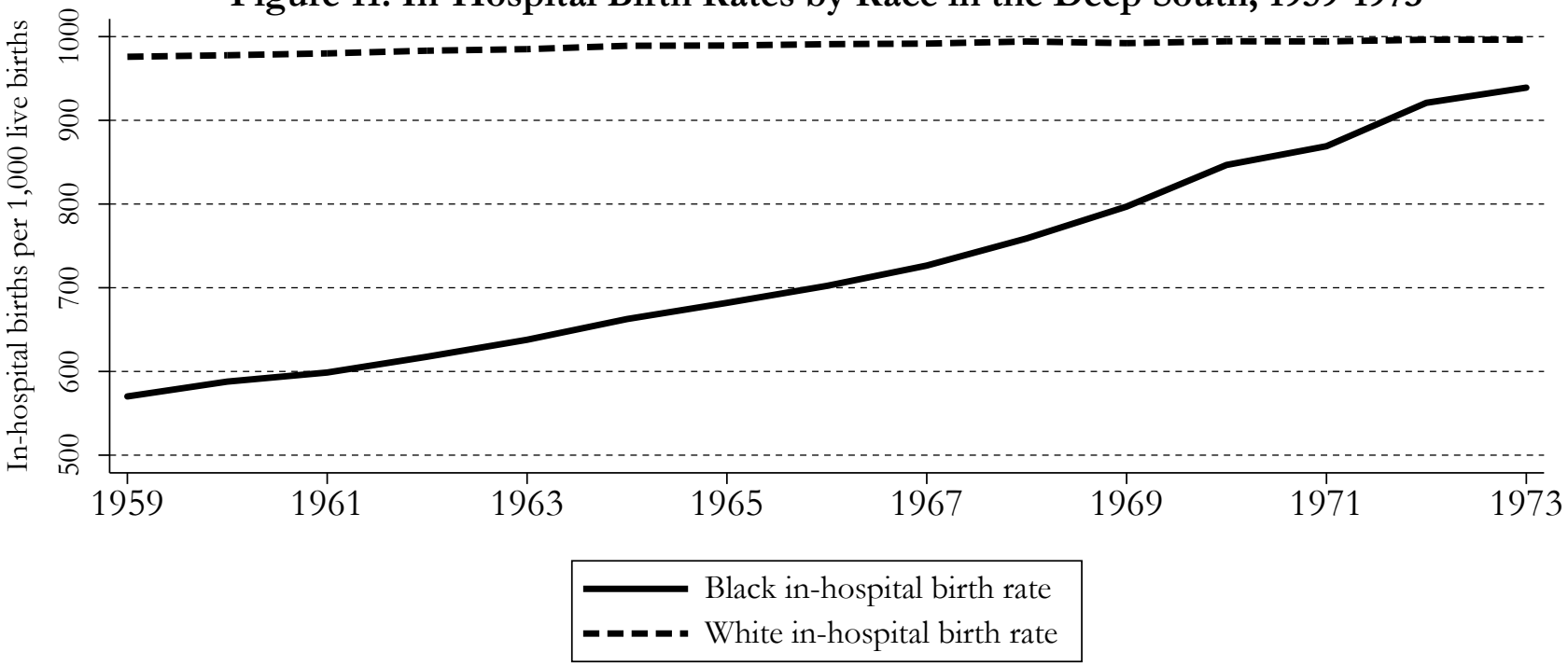
**Figure 10. The Effect of the Hospital Desegregation Campaign on Black Infant Mortality in Mississippi, 1959-1973**




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. Controls include the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. The regression is weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimate (and its standard error) comes from a regression where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

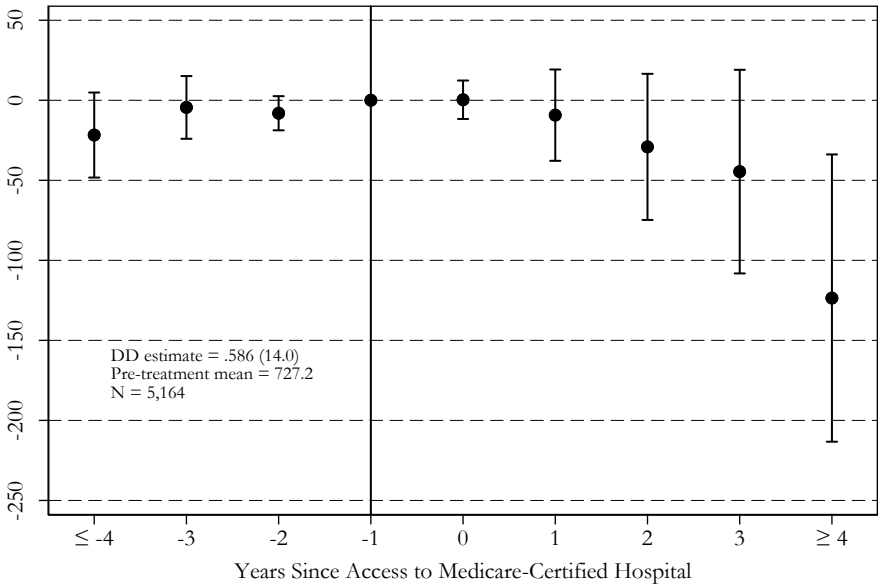
**Figure 11. In-Hospital Birth Rates by Race in the Deep South, 1959-1973**



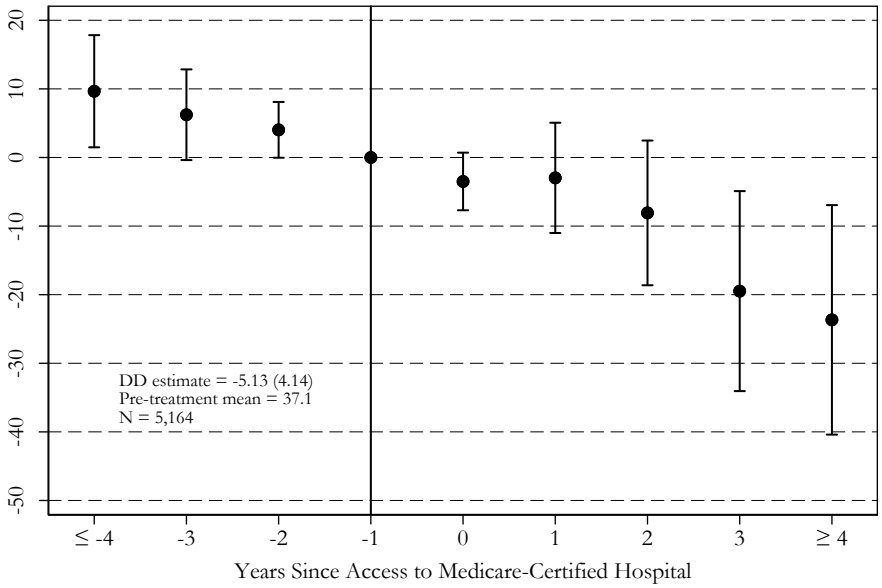

Notes: Based on annual data from the Natality Files, published by the National Vital Statistics System.

**Figure 12. Black Births by Location/Attendant and Maternal Mortality, 1959-1973**

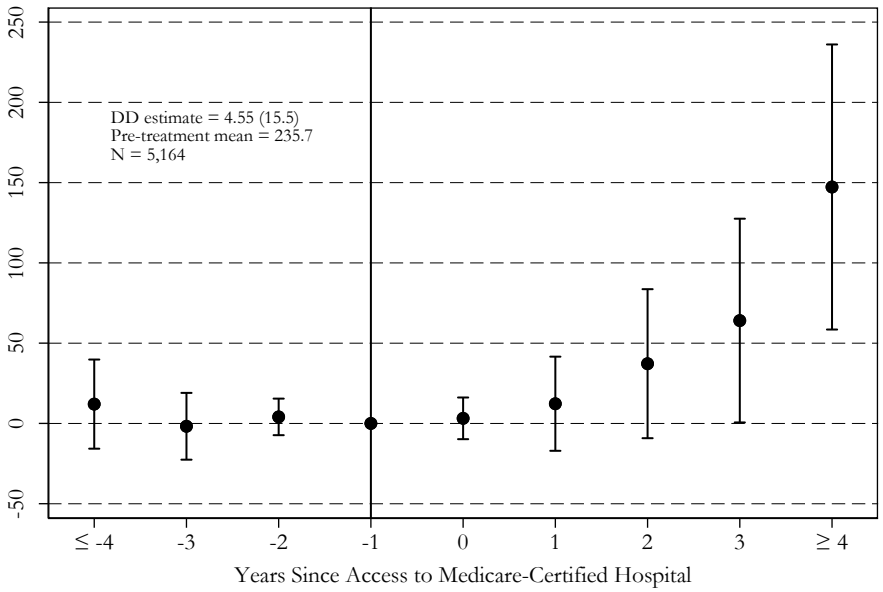
**Panel A.** *In-Hospital Black Births*



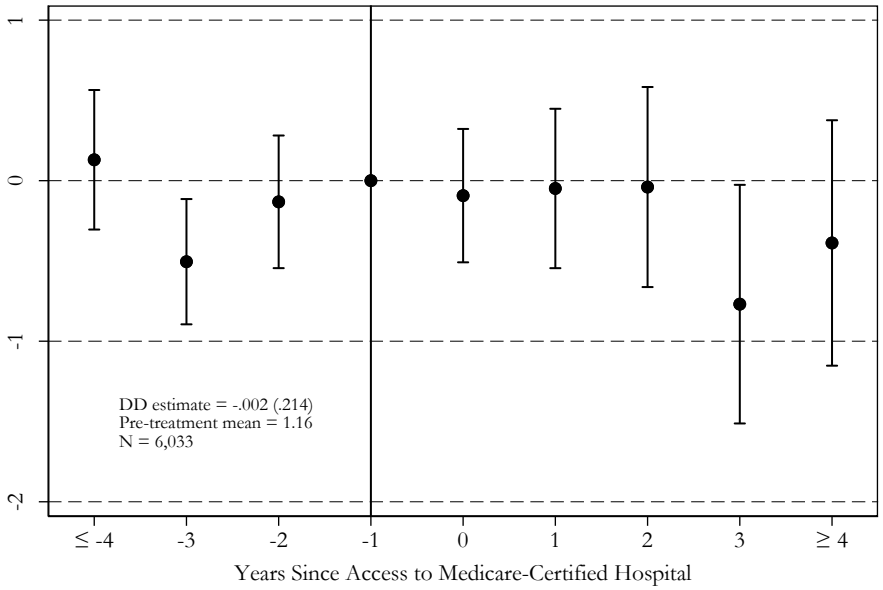

**Panel B.** *Out-of-Hospital Black Births by Physicians*



**Panel C.** *Out-of-Hospital Black Births by Midwives*




**Panel D.** *Black Maternal Mortality*




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The results in panels A-C are based on annual county-level data from individual state vital statistics reports and the Natality Files, published by the National Vital Statistics System. The results in panel D are based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. In panels A-C, the dependent variable is equal to the number of live Black births by location and attendant per 1,000 live Black births in county *c* and year *t*. In panel D, the dependent variable is equal to the number of Black maternal deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Table 1. Replicating and Extending Estimates from Almond et al. (2006) on the Effect of the Hospital Desegregation Campaign on Postneonatal Mortality in Mississippi by Race**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Estimates reported in Almond et al. (2006) | | Replicating and extending estimates reported in Almond et al. (2006) | | | |
| **Panel I. Black postneonatal mortality** | | | | | | |
| *1 Year After Medicare* | -5.74*** | -5.84*** | -6.46*** | -5.34*** | -1.64 | -2.64 |
| | (1.36) | (1.46) | (1.20) | (1.16) | (1.46) | (2.08) |
| *2 Years After Medicare* | -8.00*** | -8.09*** | -8.15*** | -6.64*** | -1.62 | -3.35 |
| | (1.37) | (1.50) | (1.01) | (1.15) | (1.65) | (2.79) |
| *3 Years After Medicare* | -9.79*** | -9.81*** | -8.39*** | -6.59*** | -.751 | -3.30 |
| | (1.54) | (1.83) | (.899) | (1.32) | (1.84) | (3.79) |
| *4 Years After Medicare* | -10.2*** | -10.3*** | -10.6*** | -9.05*** | -1.84 | -5.54 |
| | (1.82) | (2.17) | (1.16) | (1.39) | (2.34) | (4.72) |
| *5 Years After Medicare* | -11.5*** | -11.9*** | -10.4*** | -9.19*** | -.258 | -5.17 |
| | (2.06) | (2.46) | (1.48) | (1.82) | (3.14) | (6.38) |
| *6 Years After Medicare* | -12.6*** | -12.9*** | -9.10*** | -7.63*** | 1.67 | -4.20 |
| | (2.19) | (2.78) | (1.25) | (1.74) | (3.51) | (7.65) |
| N | 1,022 | 1,022 | 1,200 | 1,200 | 1,200 | 1,200 |
| $R^2$ | .20 | .42 | .19 | .41 | .38 | .43 |
| **Panel II. White postneonatal mortality** | | | | | | |
| *1 Year After Medicare* | .377 | .600 | -1.08** | -.472 | 1.01 | -.093 |
| | (.589) | (.660) | (.456) | (.631) | (.749) | (1.01) |
| *2 Years After Medicare* | -.660 | -.302 | -1.52*** | -.762 | 1.41 | -.393 |
| | (.607) | (.691) | (.504) | (.681) | (.792) | (1.44) |
| *3 Years After Medicare* | -1.03 | -.556 | -2.27*** | -1.42* | -.177 | -2.90 |
| | (.667) | (.749) | (.431) | (.755) | (.849) | (1.86) |
| *4 Years After Medicare* | -1.76** | -1.36 | -2.23*** | -1.49** | .588 | -3.04 |
| | (.716) | (.840) | (.449) | (.737) | (.947) | (2.36) |
| *5 Years After Medicare* | -1.52** | -1.21 | -1.83*** | -1.03 | 1.18 | -3.66 |
| | (.729) | (.840) | (.628) | (.902) | (1.28) | (2.86) |
| *6 Years After Medicare* | -1.35 | -1.08 | -1.16** | -.366 | 1.77 | -4.71 |
| | (.878) | (1.02) | (.561) | (.914) | (1.44) | (3.73) |
| N | 1,022 | 1,022 | 1,200 | 1,200 | 1,200 | 1,200 |
| $R^2$ | .04 | .24 | .04 | .23 | .17 | .24 |
| County fixed effects | No | Yes | No | Yes | Yes | Yes |
| County-level covariates | No | Yes | No | Yes | Yes | Yes |
| County-specific linear trend | No | Yes | No | Yes | No | Yes |
| Year fixed effects | No | No | No | No | Yes | Yes |

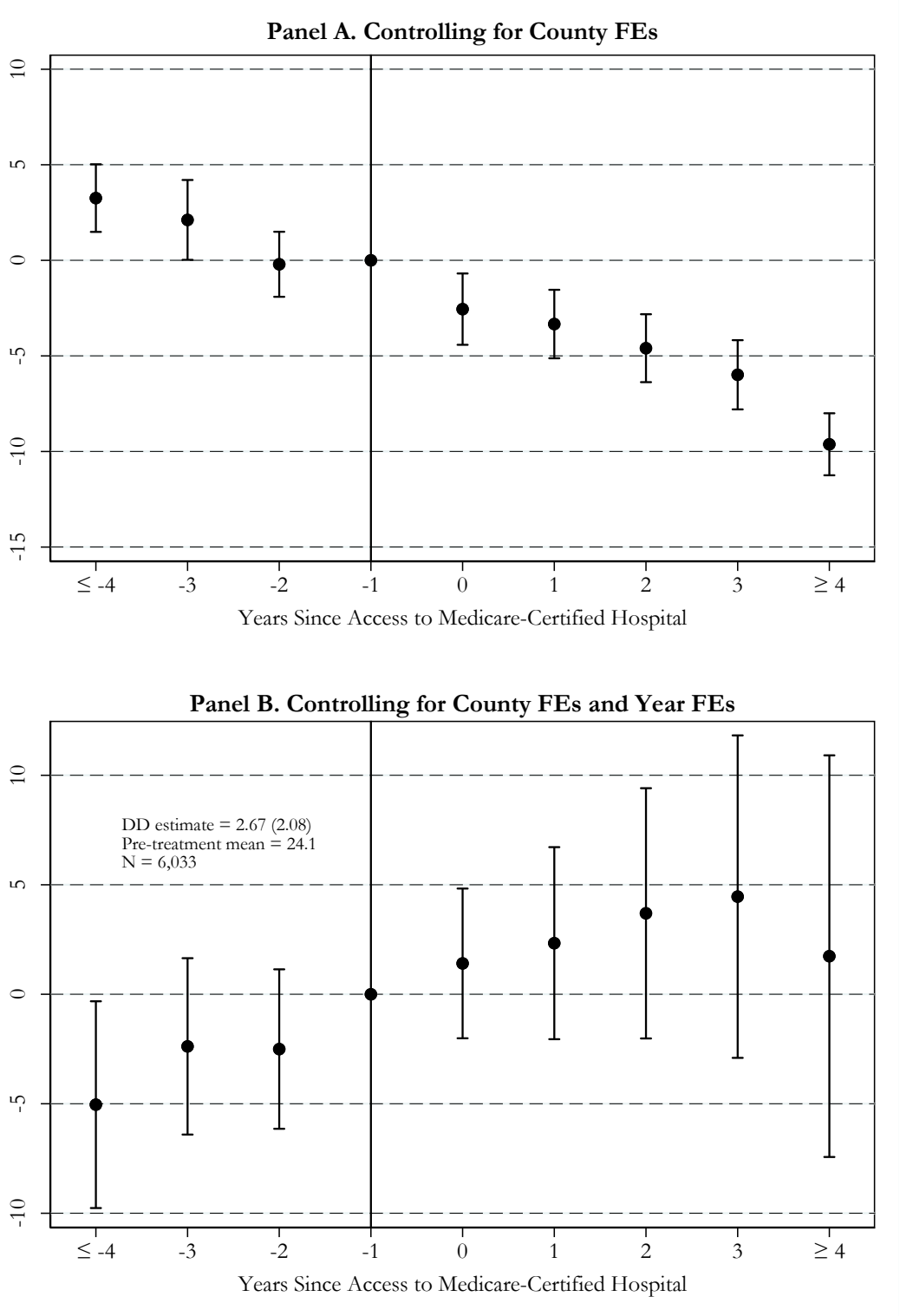
*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

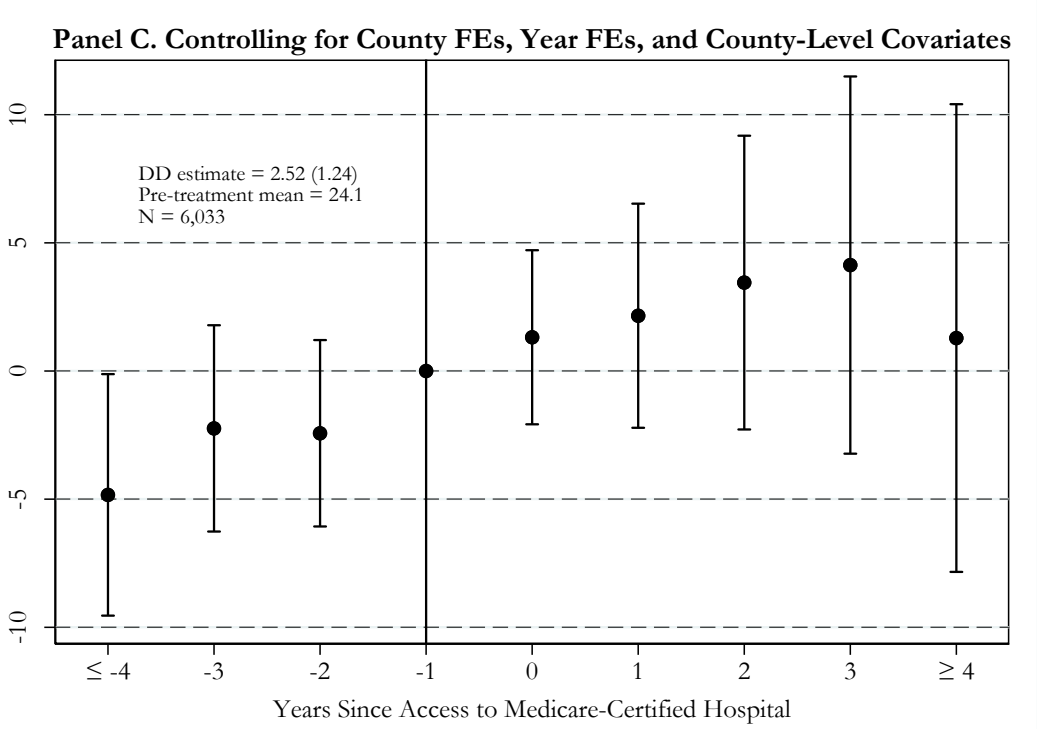
Notes: Each column within each panel represents results from a separate OLS regression. The dependent variable is equal to the number of postneonatal deaths per 1,000 race-specific live births in county *c* and year *t*. Medicare certification dates come from Almond et al. (2006). The models in columns (1) and (3) also include a pre-Medicare certification linear trend. In column (2), the county-level covariates used by Almond et al. (2006) include measures of maternal characteristics, per capita income and government transfer payments. The county-level covariates used in columns (4)-(6) are listed in Appendix Table A3. Almond et al. (2006) restricted their sample to no more than 7 years before and 6 years after Medicare certification. In columns (3)-(6), the sample is based on all county-year combinations for the period 1959-1973. Standard errors, corrected for clustering at the county level, are in parentheses.

## Appendix A

For Online Publication

**Appendix Figure A1. The Effect of the Hospital Desegregation Campaign on the Black-White Difference in Infant Mortality, 1959-1973**

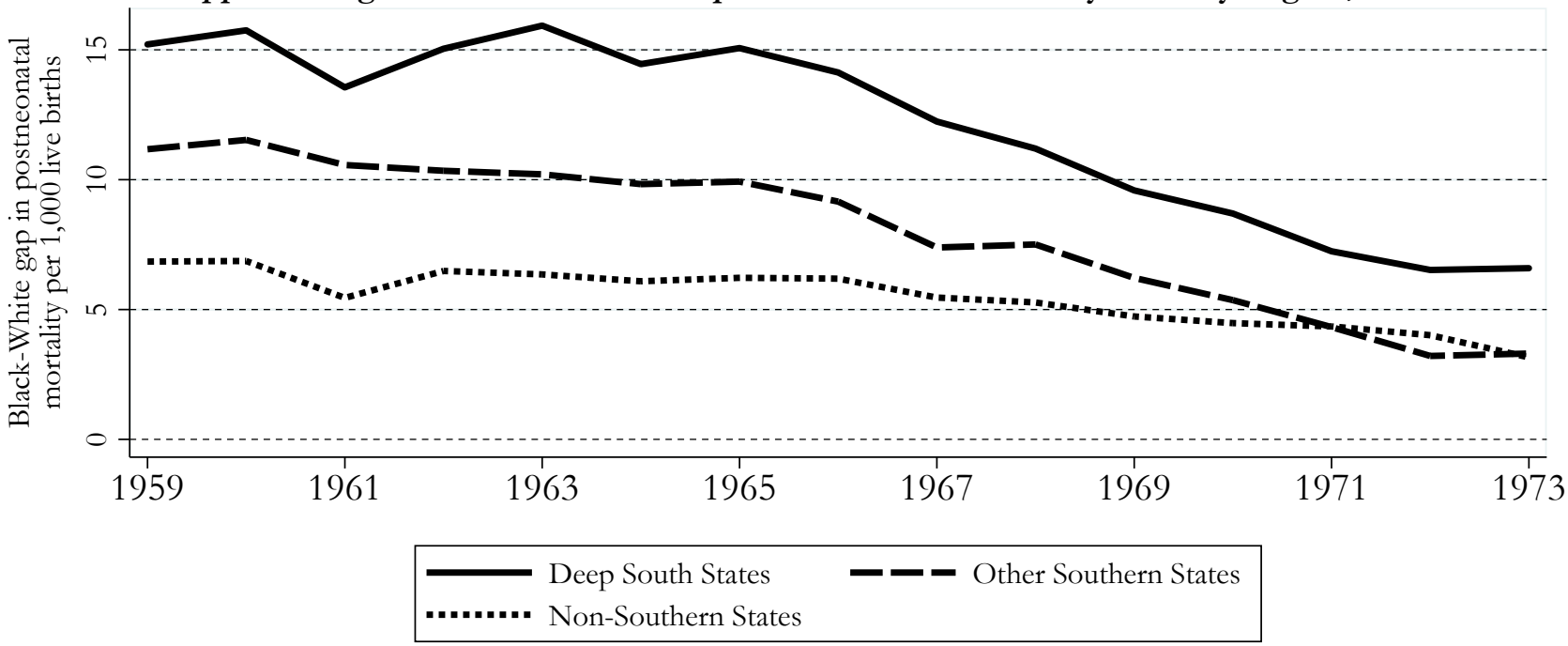





*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

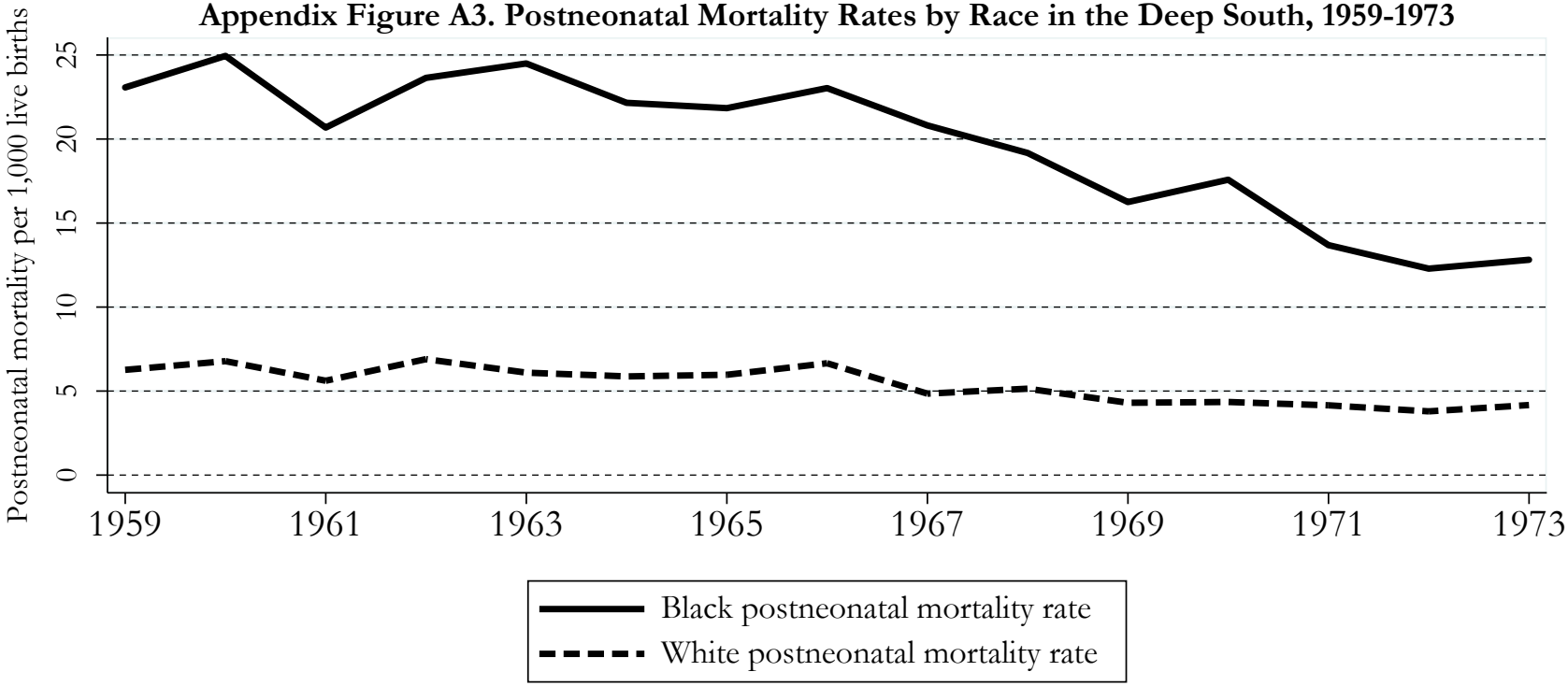
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the Black-White difference in infant mortality rates in county *c* and year *t*. All models control for county fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Appendix Figure A2. Black-White Gap in Postneonatal Mortality Rates by Region, 1959-1973**




Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

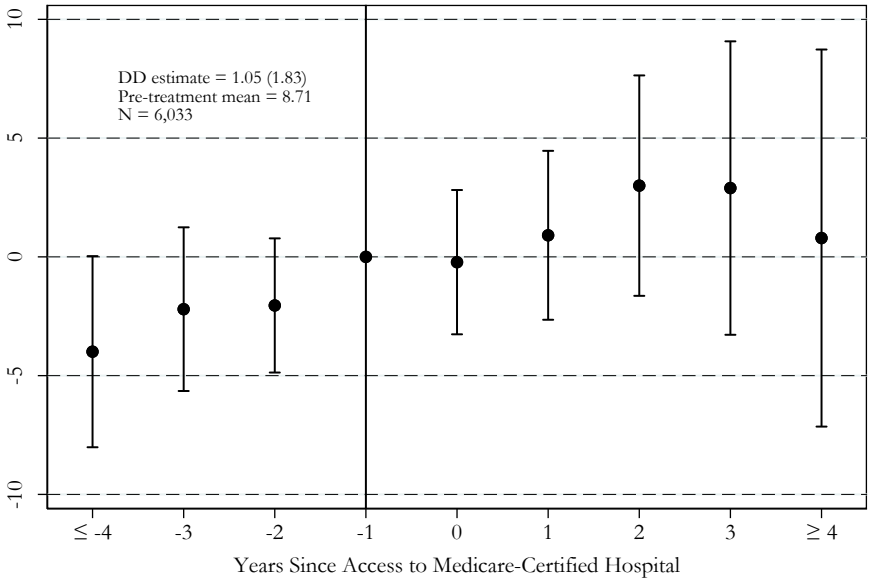
**Appendix Figure A3. Postneonatal Mortality Rates by Race in the Deep South, 1959-1973**



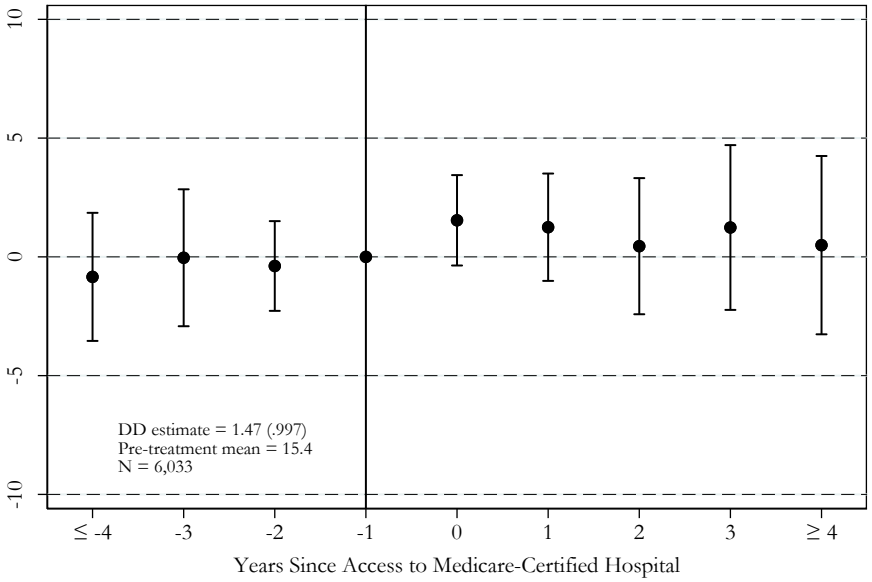

Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

## Appendix Figure A4. The Effect of the Hospital Desegregation Campaign on the Black-White Difference in Neonatal and Postneonatal Mortality, 1959-1973

**Panel A.** *Black-White Difference in Neonatal Mortality*



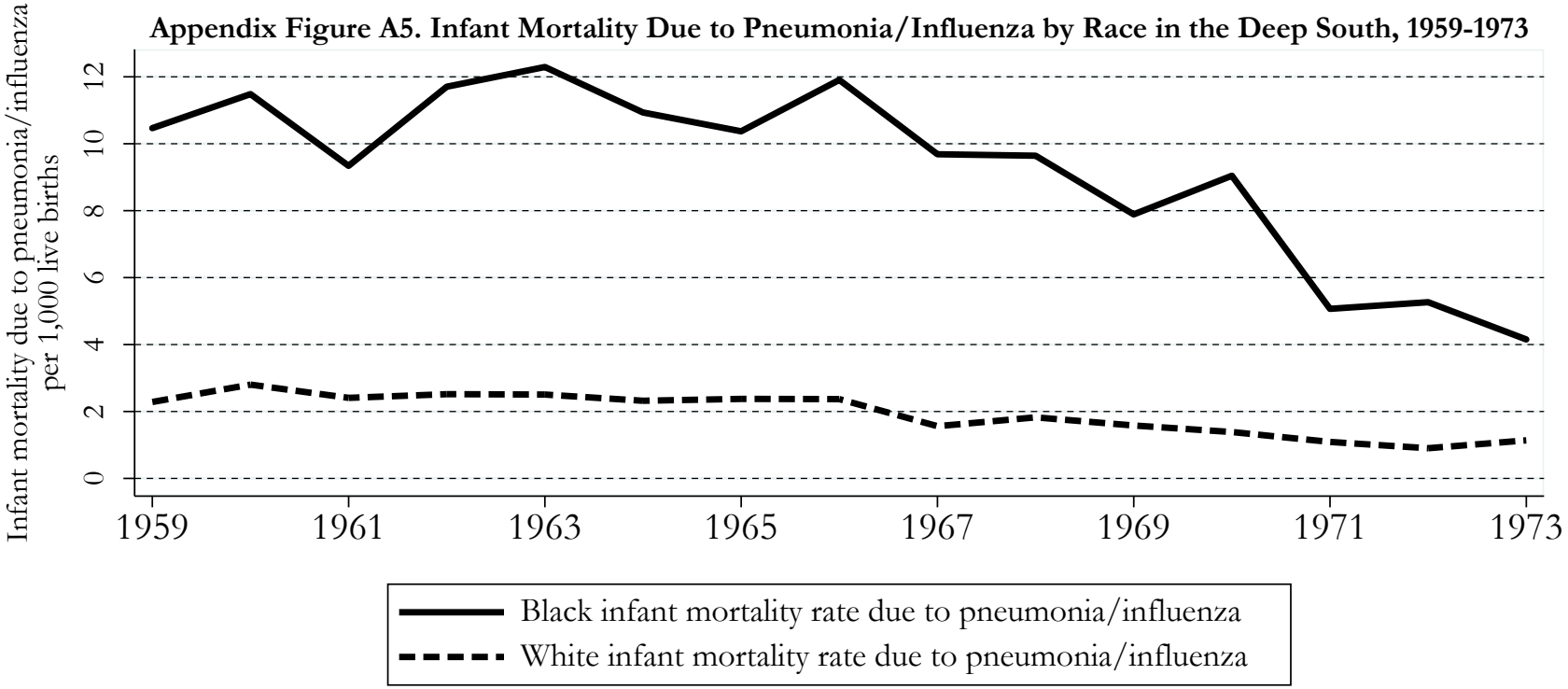

**Panel B.** *Black-White Difference in Postneonatal Mortality*




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

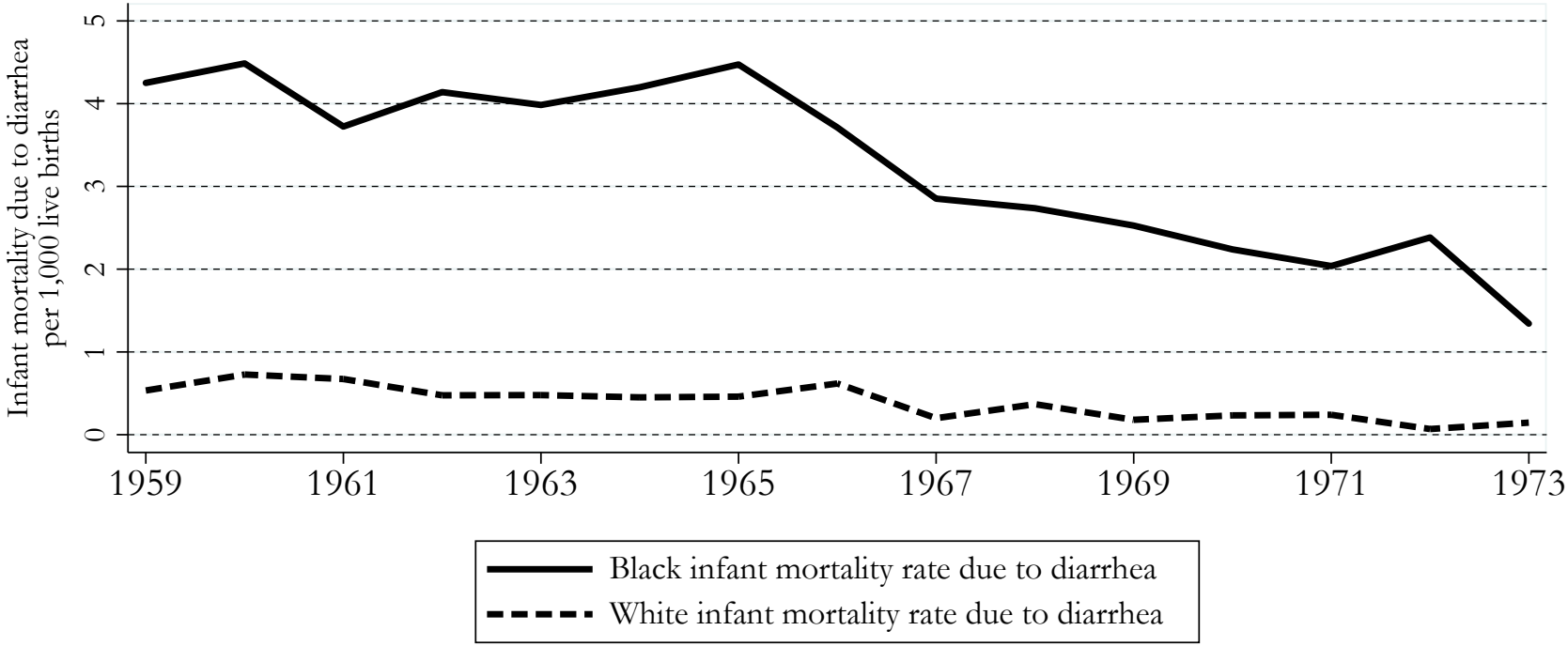
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the Black-White difference in the specified mortality rate in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Appendix Figure A5. Infant Mortality Due to Pneumonia/Influenza by Race in the Deep South, 1959-1973**




Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

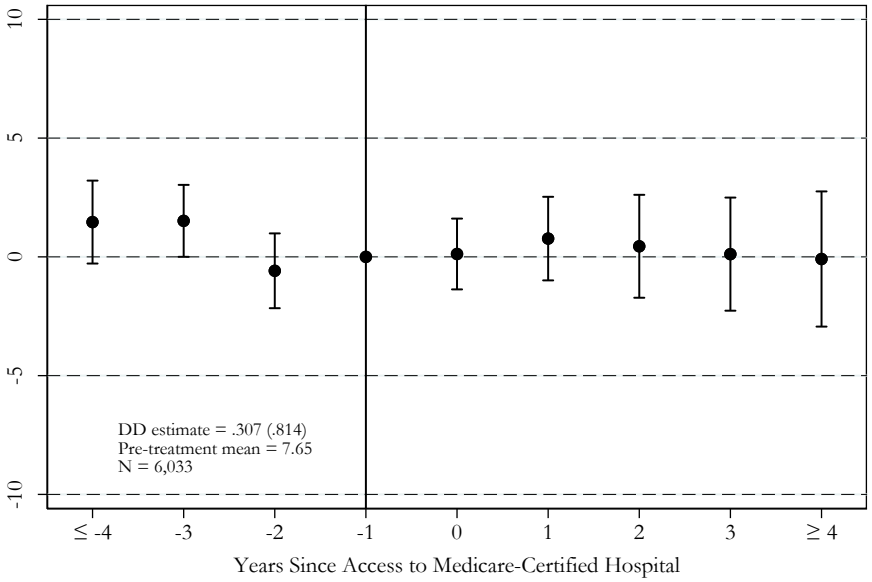
**Appendix Figure A6. Infant Mortality Due to Diarrhea by Race in the Deep South, 1959-1973**



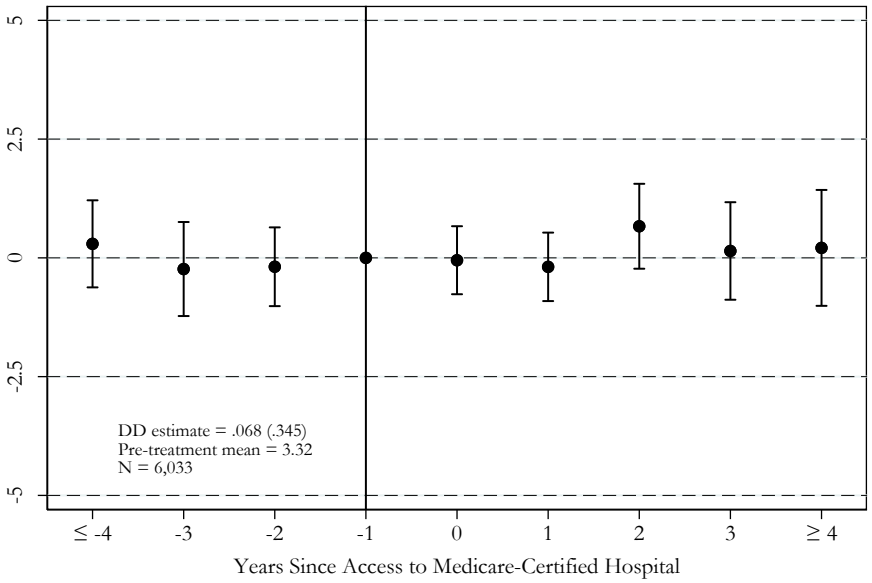

Notes: Based on annual data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System.

## Appendix Figure A7. The Effect of the Hospital Desegregation Campaign on the Black-White Difference in Infant Mortality by Cause, 1959-1973

**Panel A.** *Black-White Difference in Infant Mortality Due to Pneumonia/Influenza*




**Panel B.** *Black-White Difference in Infant Mortality Due to Diarrhea*

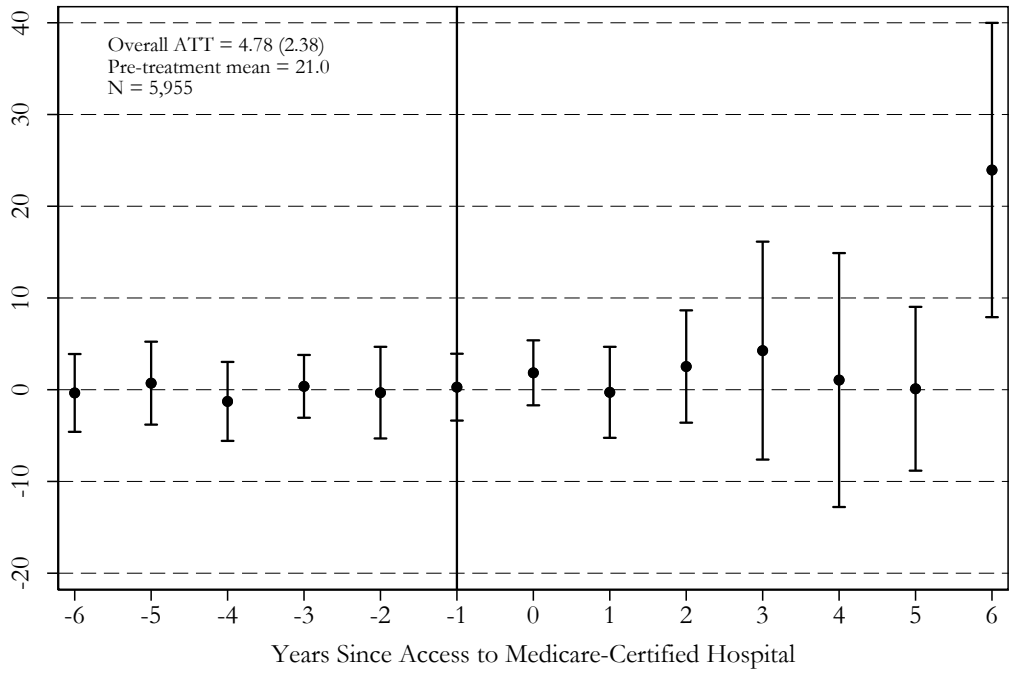



*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

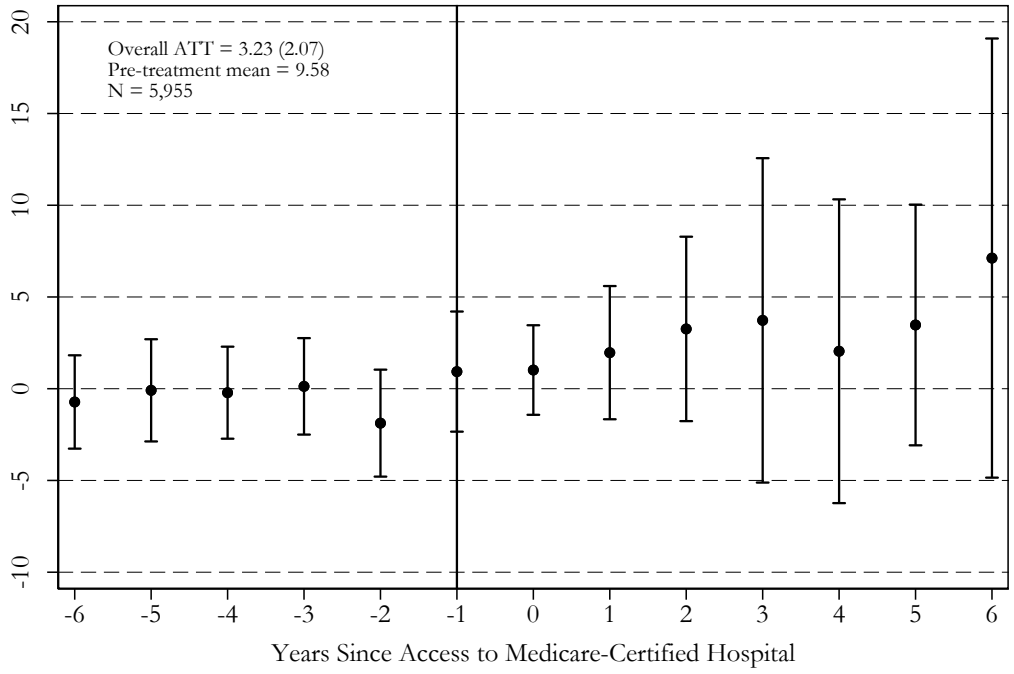
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the Black-White difference in the specified mortality rate in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Appendix Figure A8. Callaway and Sant'Anna (2021): The Effect of the Hospital Desegregation Campaign on Black Postneonatal Mortality and Black Infant Mortality by Cause, 1959-1973**

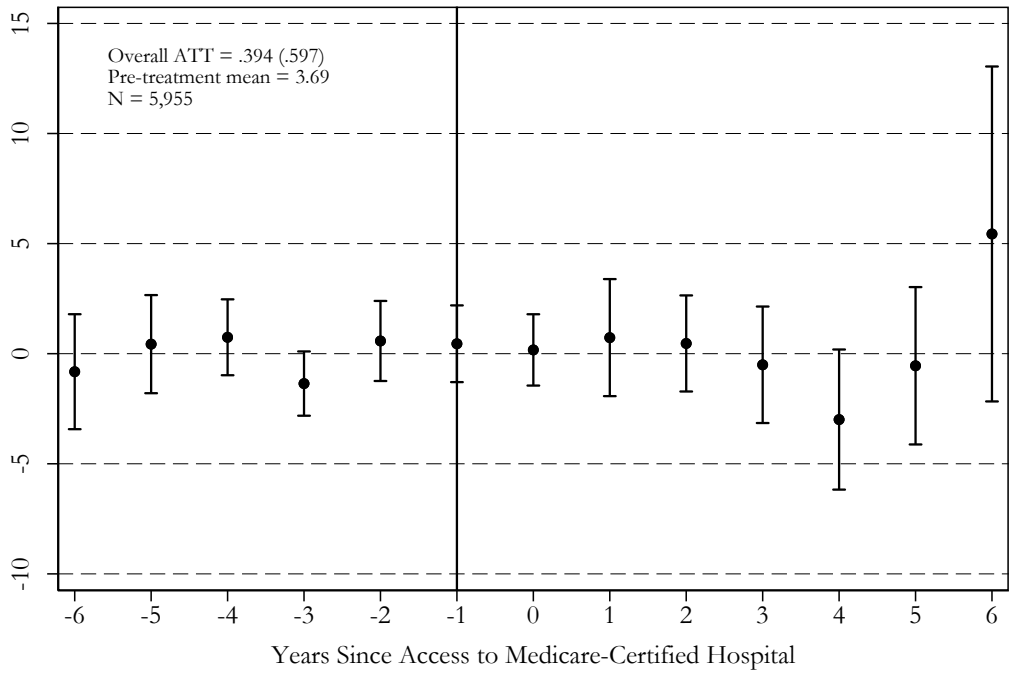
**Panel A.** *Black Postneonatal Mortality*




**Panel B.** *Black Infant Mortality Due to Pneumonia/Influenza*



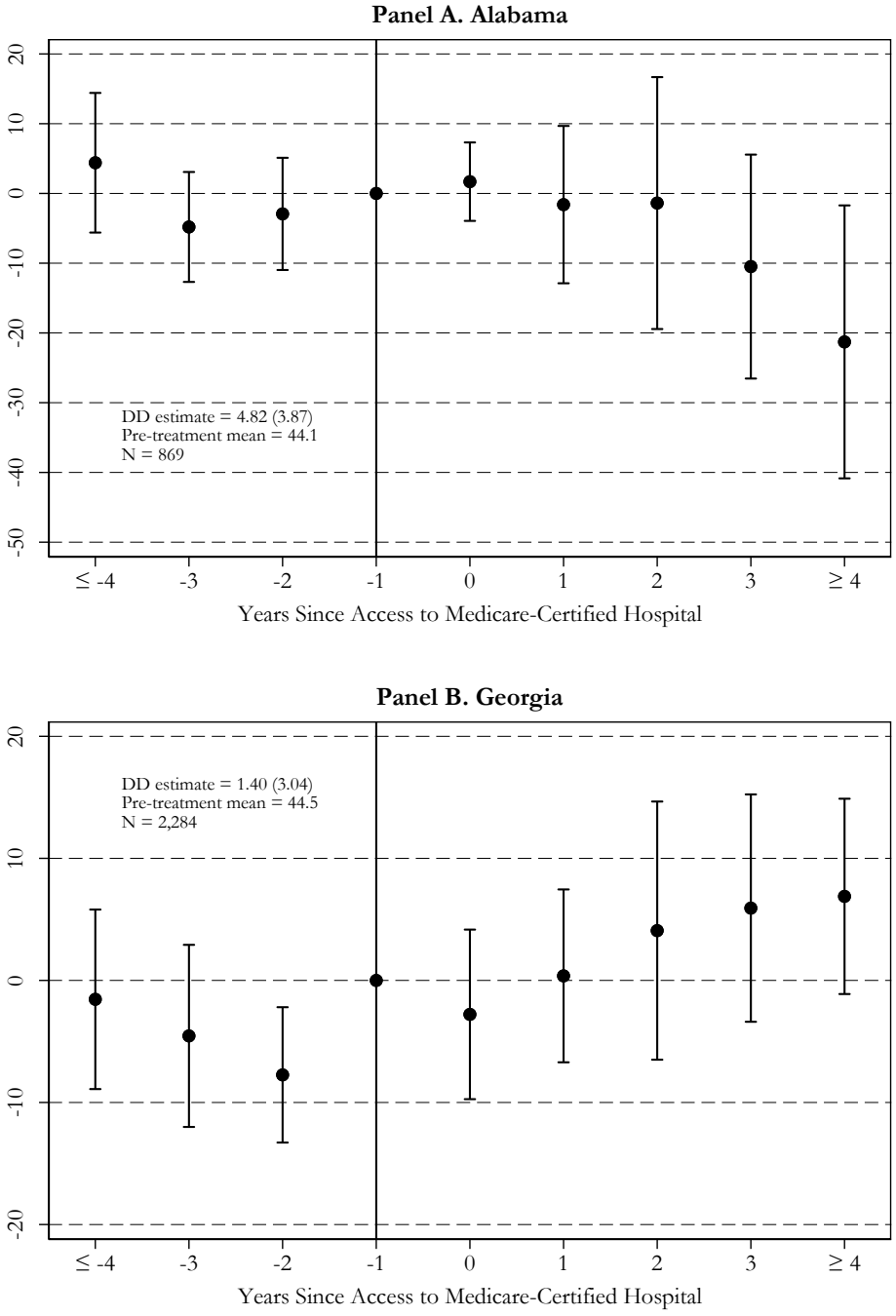
**Panel C.** *Black Infant Mortality Due to Diarrhea*



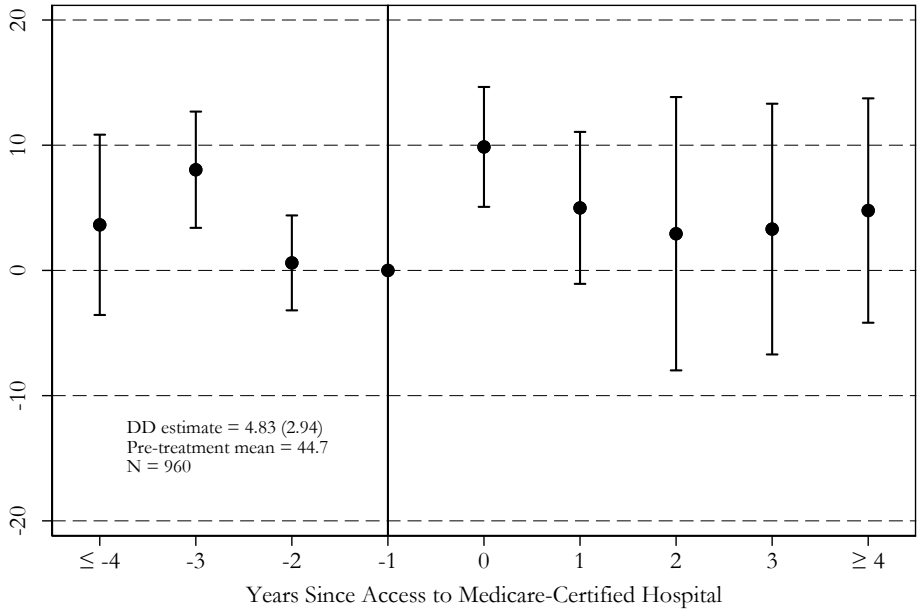

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Estimates of group-time average treatment effects on the treated (ATTs) and their 90% confidence intervals are reported. ATT estimates are from equation (3.4) in Callaway and Sant'Anna (2021). The dependent variable is equal to the number of specified Black deaths per 1,000 live Black births in county *c* and year *t*. Estimated ATTs are weighted by live Black births and standard errors are corrected for clustering at the county level. The overall ATTs are the average of the estimated ATTs in the post-treatment period and are from equation (3.12) in Callaway and Sant'Anna (2021).

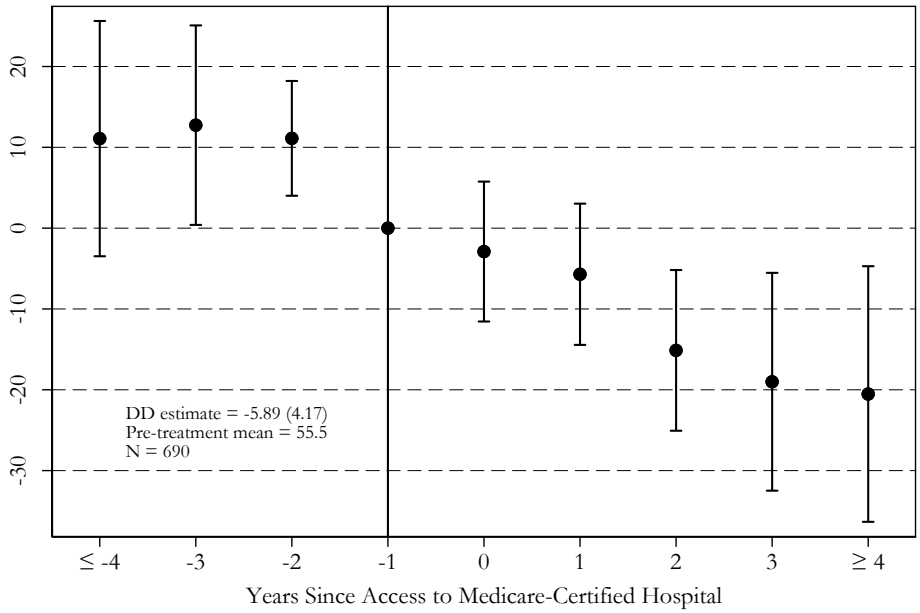
**Appendix Figure A9. The Effect of the Hospital Desegregation Campaign on Black Infant Mortality by State, 1959-1973**



**Panel C. Louisiana**



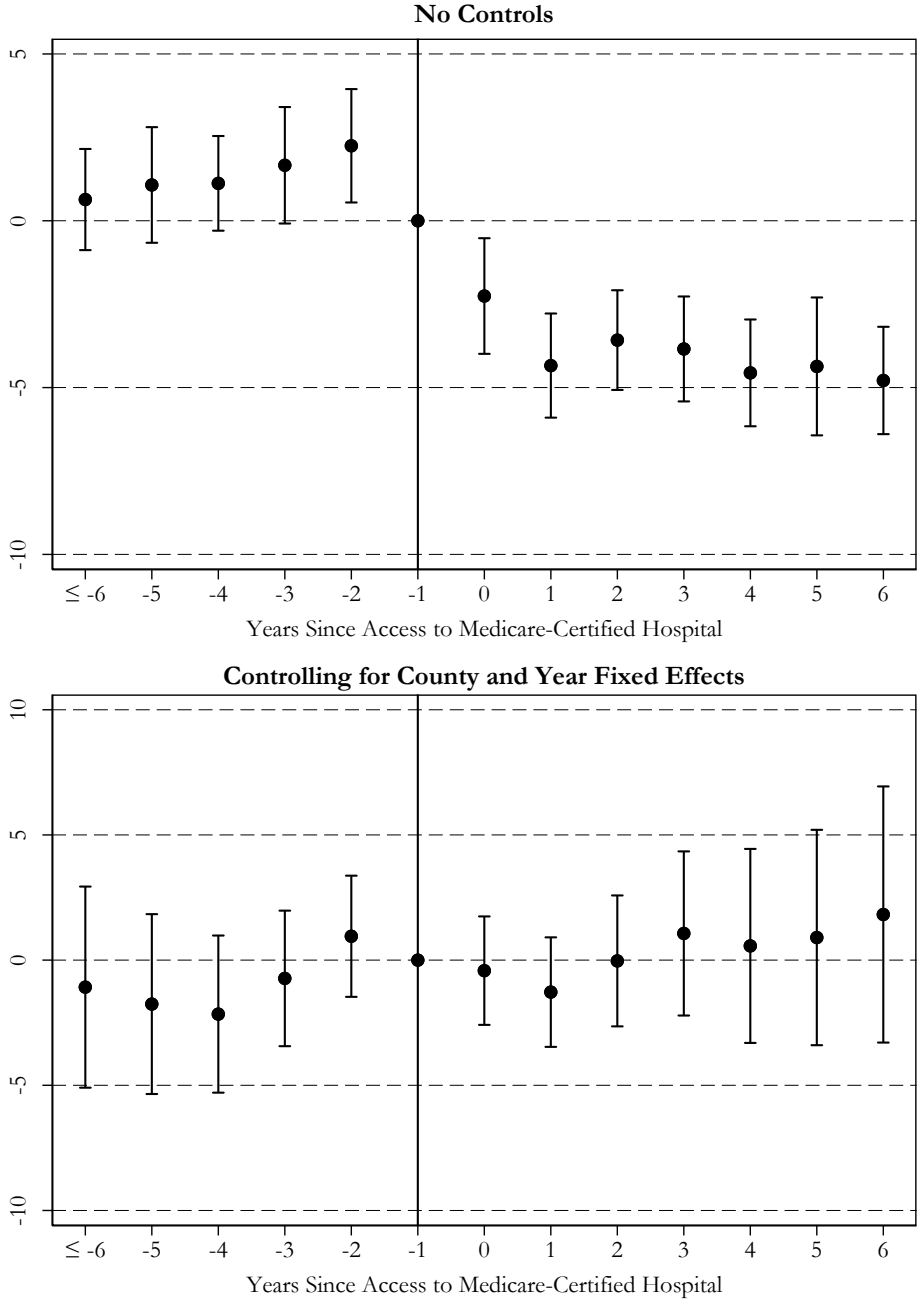

**Panel D. South Carolina**




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

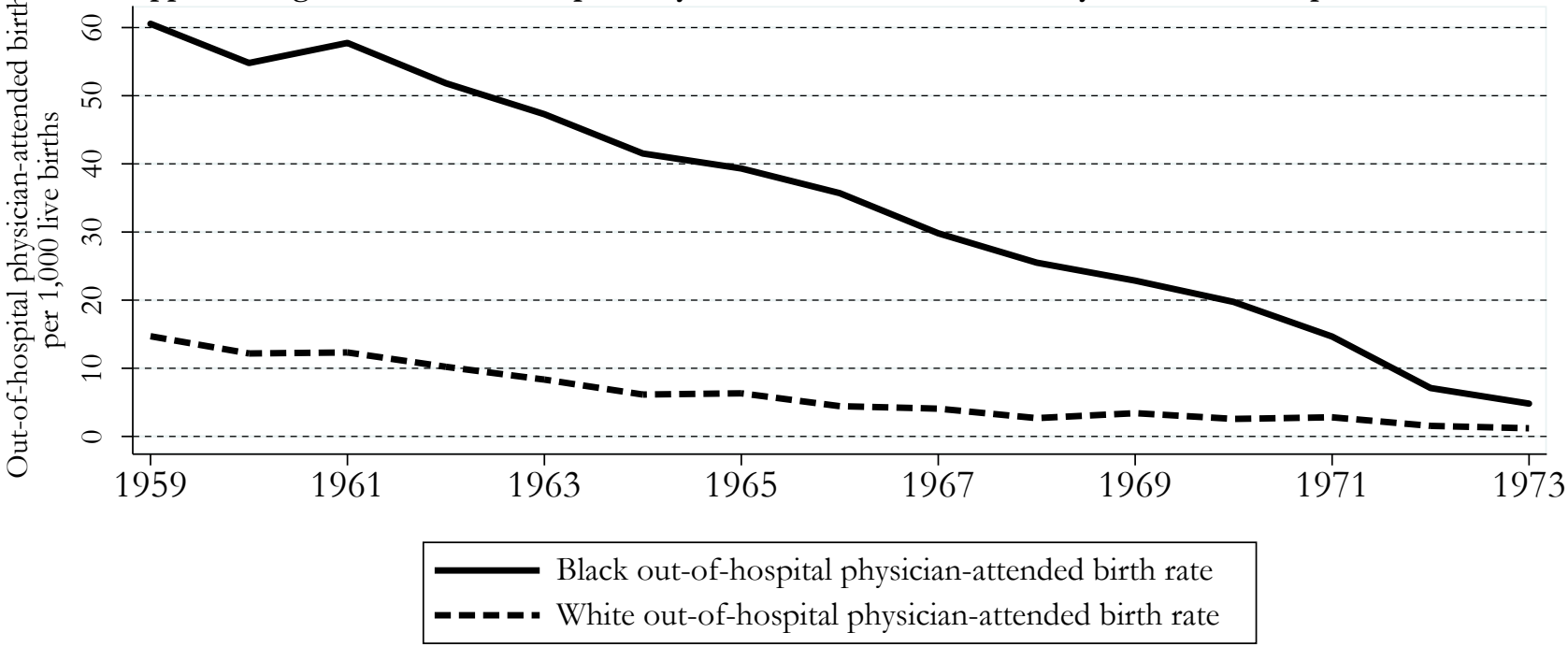
Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Appendix Figure A10. Replicating and Extending Event-Study Estimates from Almond et al. (2006) on Black-White Difference in Postneonatal Mortality due to Pneumonia, Influenza, and Diarrhea in Mississippi**



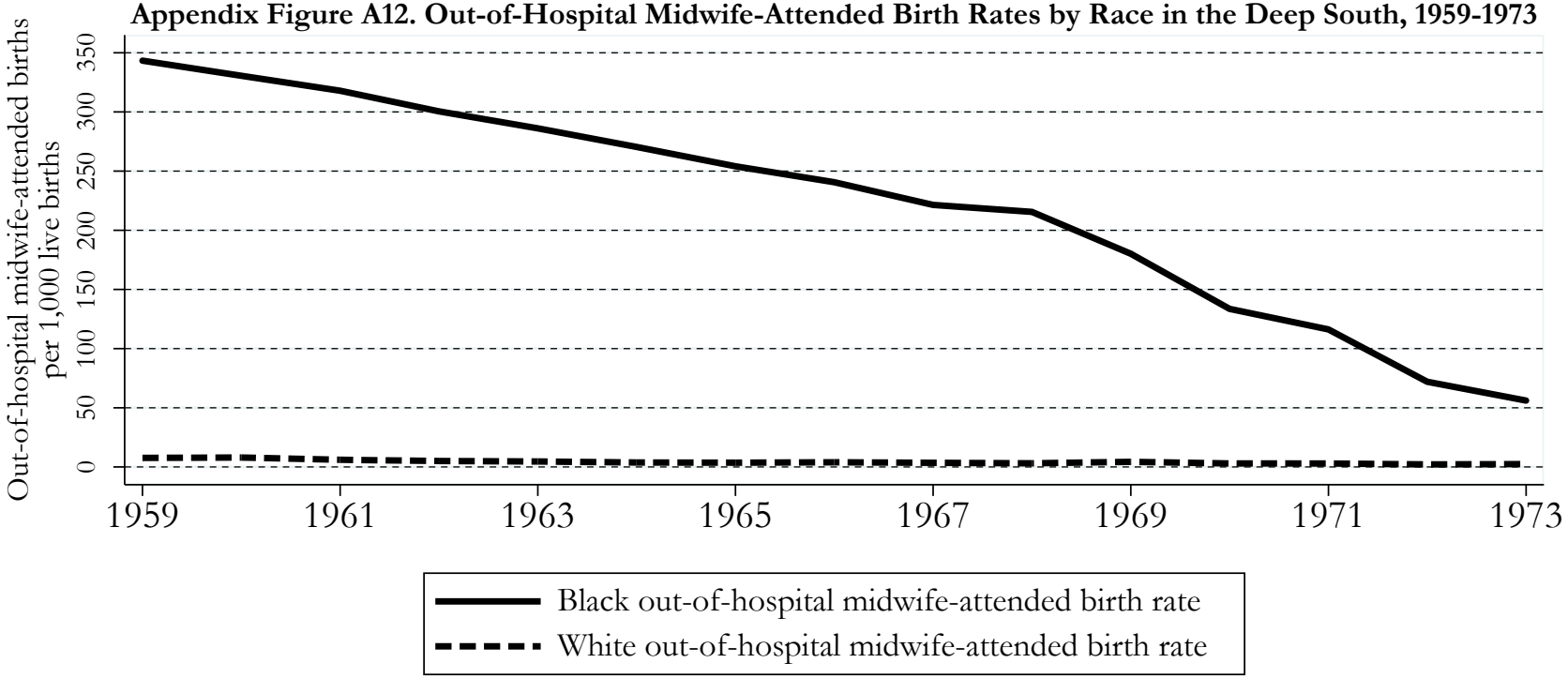

Notes: OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the Black-White difference in the postneonatal mortality rate due to pneumonia, influenza, and diarrhea in county *c* and year *t*. Regressions are weighted by live Black births. Standard errors are corrected for clustering at the county level.

**Appendix Figure A11. Out-of-Hospital Physician-Attended Birth Rates by Race in the Deep South, 1959-1973**



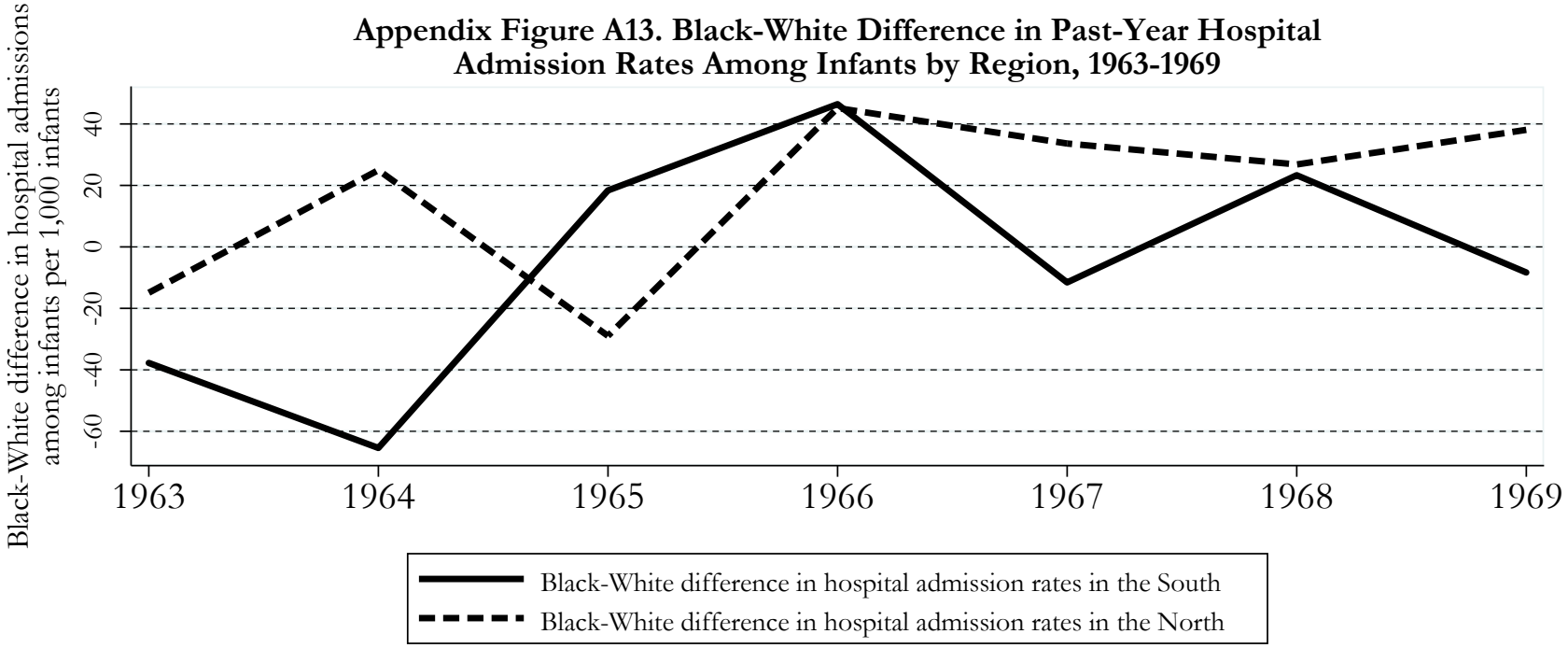

Notes: Based on annual data from the Natality Files, published by the National Vital Statistics System.

**Appendix Figure A12. Out-of-Hospital Midwife-Attended Birth Rates by Race in the Deep South, 1959-1973**




Notes: Based on annual data from the Natality Files, published by the National Vital Statistics System.

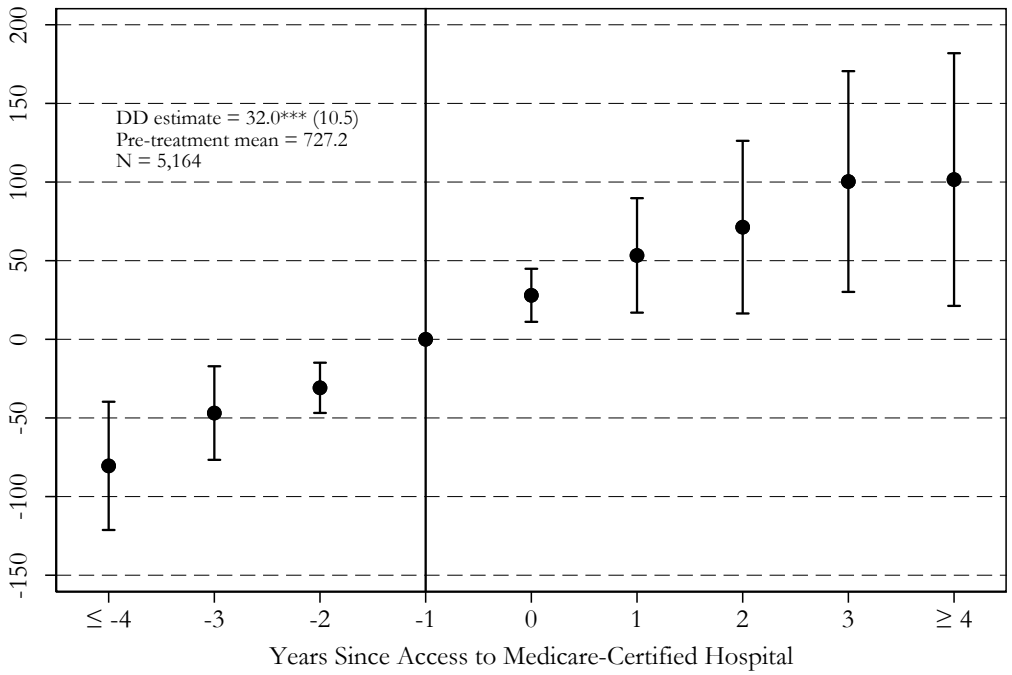
**Appendix Figure A13. Black-White Difference in Past-Year Hospital Admission Rates Among Infants by Region, 1963-1969**



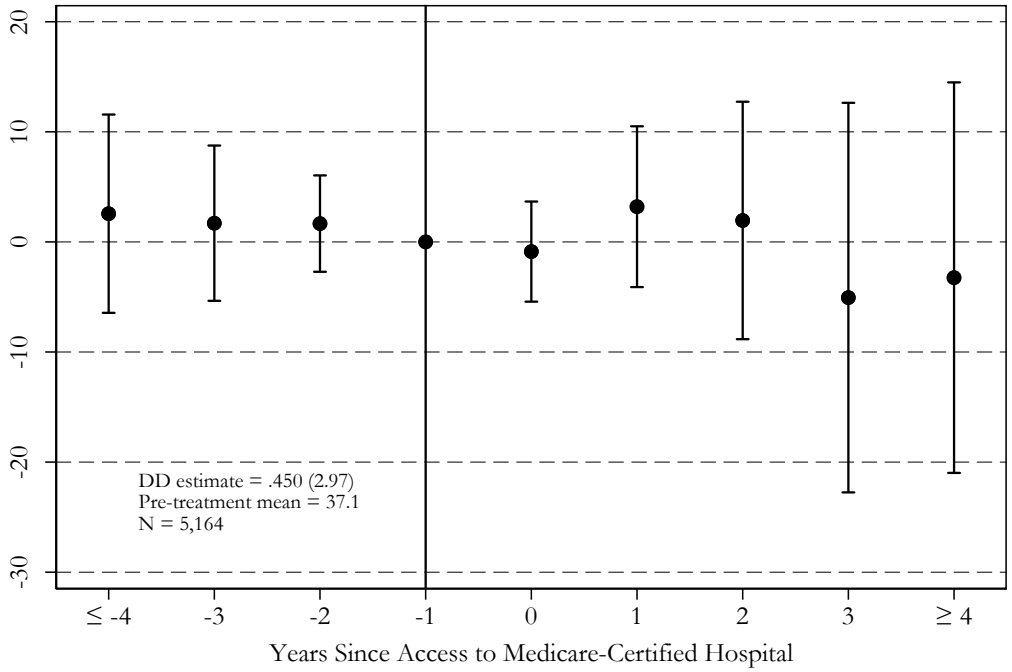

Notes: Based on annual data from the National Health Interview Survey. Southern states include those in the West South Central, East South Central, and South Atlantic census divisions. Northern states include those in the West North Central, East North Central, and Middle Atlantic census divisions.

**Appendix Figure A14. Black Births by Location/Attendant, 1959-1973 (Controlling for County Time Trends)**

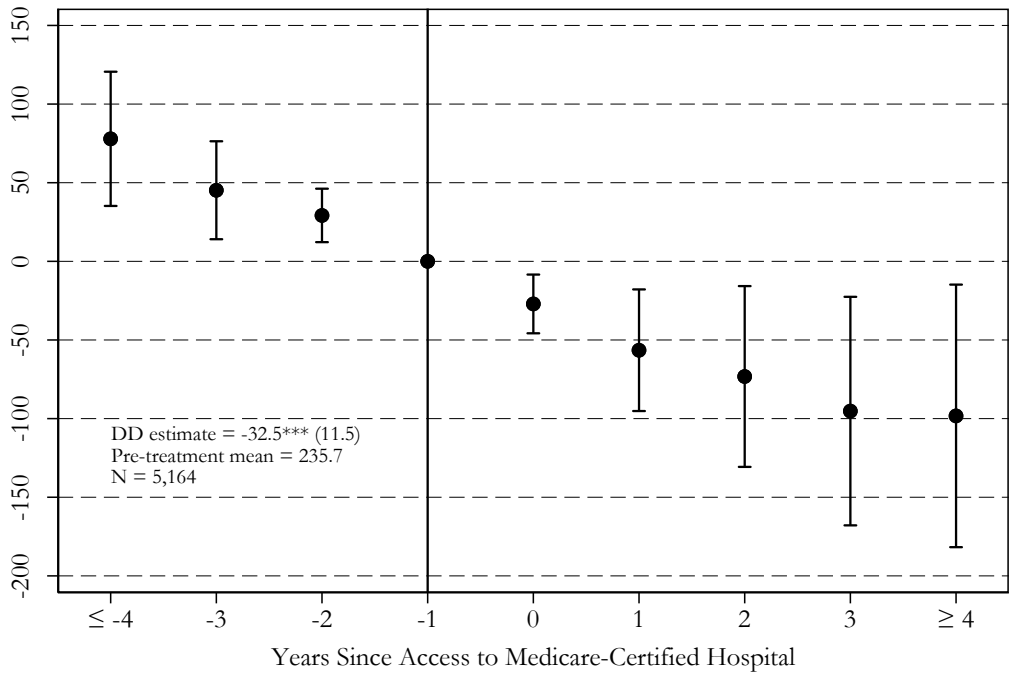
**Panel A.** *In-Hospital Black Births*




**Panel B.** *Out-of-Hospital Black Births by Physicians*



**Panel C.** *Out-of-Hospital Black Births by Midwives*




*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from individual state vital statistics reports and the Natality Files, published by the National Vital Statistics System. OLS coefficient estimates (and their 90% confidence intervals) are reported, where the omitted category is one year before treatment. The dependent variable is equal to the number of live Black births by location and attendant per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, year fixed effects, and county-specific linear time trends. Regressions are weighted by live Black births and standard errors are corrected for clustering at the county level. Reported DD estimates (and their standard errors) come from regressions where the event-study indicators are replaced by the variable *Medicare* (equal to one if Black mothers in county *c* and year *t* had access to a Medicare-eligible hospital, and equal to zero otherwise).

**Appendix Table A1. Number of Counties with Access to a Medicare Certified Hospital by State and Year**

| State | 1967 | 1968 | 1969 | 1970 or later |
|---|---|---|---|---|
| Alabama | 51 | 56 | 56 | 58 |
| Total counties = 58[a] | [.931] | [.990] | [.990] | [1.00] |
| Georgia | 133 | 146 | 149 | 153 |
| Total counties = 153[b] | [.911] | [.968] | [.986] | [1.00] |
| Louisiana | 54 | 58 | 58 | 64 |
| Total counties = 64 | [.912] | [.956] | [.956] | [1.00] |
| Mississippi | 57 | 74 | 77 | 82 |
| Total counties = 82 | [.635] | [.884] | [.953] | [1.00] |
| South Carolina | 40 | 44 | 45 | 46 |
| Total counties = 46 | [.846] | [.952] | [.992] | [1.00] |

[a] Nine Alabama counties are excluded from the analysis due to missing live black birth data.

[b] Six Georgia counties are excluded from the analysis because no live black births were recorded during the sample period.

Notes: Numbers in brackets represent the fraction of live black births that occurred in counties with access to a Medicare certified hospital.

**Appendix Table A2. The Effect of the Hospital Desegregation Campaign on Infant Mortality by Race, 1959-1973: Event-Study Estimates**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Black Infant Mortality* | *Black Infant Mortality* | *Black Infant Mortality* | *White Infant Mortality* | *White Infant Mortality* | *White Infant Mortality* |
| *4+ Years Before Medicare* | 6.08*** | 1.41 | 1.50 | 3.14*** | 3.36* | 3.15* |
| | (1.06) | (2.11) | (2.10) | (.503) | (1.71) | (1.74) |
| *3 Years Before Medicare* | 3.66*** | 1.63 | 1.69 | 2.11*** | 2.30 | 2.16 |
| | (1.20) | (1.84) | (1.84) | (.617) | (1.66) | (1.68) |
| *2 Years Before Medicare* | .911 | -1.32 | -1.29 | 1.36*** | .785 | .716 |
| | (.865) | (1.39) | (1.38) | (.434) | (1.10) | (1.10) |
| *1 Year Before Medicare* | … | … | … | … | … | … |
| *Year of Medicare Certification* | -3.21*** | 1.52 | 1.48 | -.730 | -1.36 | -1.30 |
| | (.824) | (1.36) | (1.35) | (.491) | (1.30) | (1.28) |
| *1 Year After Medicare* | -4.34*** | 1.38 | 1.31 | -1.10** | -2.70 | -2.57 |
| | (.876) | (1.64) | (1.64) | (.495) | (2.01) | (1.97) |
| *2 Years After Medicare* | -7.02*** | 1.17 | 1.05 | -2.19*** | -3.37 | -3.21 |
| | (.888) | (2.29) | (2.30) | (.511) | (2.26) | (2.18) |
| *3 Years After Medicare* | -9.11*** | .845 | .677 | -2.77*** | -3.98 | -3.80 |
| | (.914) | (2.63) | (2.63) | (.510) | (2.95) | (2.86) |
| *4+ Years After Medicare* | -13.3*** | -.662 | -.927 | -3.66*** | -1.92 | -1.72 |
| | (.805) | (3.09) | (3.07) | (.462) | (3.71) | (3.59) |
| Pre-treatment mean | 47.8 | 47.8 | 47.8 | 23.5 | 23.5 | 23.5 |
| N | 6,033 | 6,033 | 6,033 | 6,033 | 6,033 | 6,033 |
| $R^2$ | .238 | .246 | .247 | .118 | .123 | .124 |
| Year fixed effects | No | Yes | Yes | No | Yes | Yes |
| County-level covariates | No | No | Yes | No | No | Yes |

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Each column represents results from a separate OLS regression, where the omitted category is 1 year before treatment. The dependent variable is equal to the number of infant deaths per 1,000 race-specific live births in county *c* and year *t*. All models control for county fixed effects. Regressions are weighted by race-specific live births. Standard errors, corrected for clustering at the county level, are in parentheses.

**Appendix Table A3. Descriptive Statistics**

| | Mean (SD) | Description | Source |
|---|---|---|---|
| *Black Infant Mortality* | 42.2 (16.7) | Number of black infant deaths per 1,000 live black births in county *c* and year *t* | Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Available at: https://data.nber.org/data/vital-statistics-mortality-data-multiple-cause-of-death.html. |
| *White Infant Mortality* | 21.9 (15.5) | Number of white infant deaths per 1,000 live white births in county *c* and year *t* | |
| *High School Degree* | 35.2 (10.8) | Percent of county population that was 25 years of age or older with a high school diploma | County and City Data Book Consolidated File: County Data, 1947-1977 (ICPSR 7736). Available at: https://www.icpsr.umich.edu/web/ICPSR/studies/7736. Missing values were calculated via linear interpolation. |
| *Health Spending* | 15.8 (16.8) | County direct health and hospital expenditures per capita (1960 dollars) | Data Base on Historical Finances of Local Governments: Fiscal Years 1957-2002. Available at: https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html. Missing values were calculated via linear interpolation and extrapolation. |
| *Employment to Population* | .030 (.007) | County employment to population ratio | Data Base on Historical Employment of Local Governments: 1957-2007. Available at: https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html. Missing values were calculated via linear interpolation and extrapolation. |

Notes: Means are weighted by live births and standard deviations are in parentheses. N = 6,033.

**Appendix Table A4. Extensions and Robustness Checks: The Effect of the Hospital Desegregation Campaign on Black Infant Mortality, 1959-1973**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Alternative treatment thresholds | | | Early vs. late adopters | Drop no-hospital counties | Medicare-certified counties on border | Unweighted | Interaction between *Medicare* and *Medicaid* | Interaction between *Medicare* and *Black Hospital* |
| *Medicare – 25% of Beds* | 2.05* | … | … | … | … | … | … | … | … |
| | (1.23) | | | | | | | | |
| *Medicare – 50% of Beds* | … | .481 | … | … | … | … | … | … | … |
| | | (1.12) | | | | | | | |
| *Medicare – 75% of Beds* | … | … | 2.50 | … | … | … | … | … | … |
| | | | (1.92) | | | | | | |
| *Medicare – 1967* | … | … | … | 1.44 | … | … | … | … | … |
| | | | | (1.44) | | | | | |
| *Medicare – 1968 or later* | … | … | … | 1.97 | … | … | … | … | … |
| | | | | (1.56) | | | | | |
| *Medicare* | … | … | … | … | 1.31 | 1.74 | 2.85 | 2.12 | 2.14 |
| | | | | | (1.26) | (1.23) | (2.58) | (1.32) | (1.24) |
| *Number of Medicare-Certified Counties on Border* | … | … | … | … | … | -.354 | … | … | … |
| | | | | | | (.488) | | | |
| *Medicare*Medicaid* | … | … | … | … | … | … | … | -1.04 | … |
| | | | | | | | | (.872) | |
| *Medicare*Black Hospital* | … | … | … | … | … | … | … | … | -2.06 |
| | | | | | | | | | (2.12) |
| Pre-treatment mean | 47.8 | 47.8 | 47.8 | 47.8 | 47.7 | 47.8 | 48.2 | 47.8 | 47.8 |
| N | 6,033 | 6,033 | 6,033 | 6,033 | 4,913 | 6,033 | 6,033 | 6,033 | 6,033 |
| $R^2$ | .247 | .247 | .247 | .247 | .272 | .247 | .071 | .247 | .248 |

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Each column represents results from a separate OLS regression. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. All models control for the county-level covariates listed in Appendix Table A3, county fixed effects, and year fixed effects. Unless specified otherwise, regressions are weighted by live Black births. Standard errors, corrected for clustering at the county level, are in parentheses.

**Appendix Table A5. Proportion of Counties Treated Based on Alternative Thresholds**

| State | Proportion of counties treated by year (any-hospital threshold) | | | | Proportion of counties treated by year (bed threshold = 25%) | | | | Proportion of counties treated by year (bed threshold = 50%) | | | | Proportion of counties treated by year (bed threshold = 75%) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1967 | 1968 | 1969 | 1970 | 1967 | 1968 | 1969 | 1970 | 1967 | 1968 | 1969 | 1970 | 1967 | 1968 | 1969 | 1970 |
| Alabama | .879 | .966 | .966 | .983 | .862 | .948 | .966 | .983 | .845 | .931 | .948 | .983 | .810 | .897 | .914 | .966 |
| Georgia | .869 | .954 | .974 | .980 | .863 | .954 | .974 | .980 | .850 | .948 | .967 | .974 | .850 | .948 | .967 | .974 |
| Louisiana | .844 | .906 | .906 | .938 | .828 | .906 | .906 | .938 | .734 | .875 | .891 | .906 | .656 | .813 | .859 | .875 |
| Mississippi | .695 | .902 | .939 | .976 | .683 | .890 | .939 | .976 | .622 | .841 | .927 | .963 | .598 | .756 | .817 | .890 |
| South Carolina | .870 | .957 | .978 | .978 | .848 | .935 | .978 | .978 | .848 | .935 | .978 | .978 | .783 | .891 | .935 | .935 |

Notes: Treatment thresholds based on percent of beds in Medicare-certified hospitals.

**Appendix Table A6. The Effect of the Hospital Desegregation Campaign on Black Infant Mortality by State, 1959-1973**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel I: Alabama** | | |
| *Medicare* | -8.92*** | 4.29 | 4.82 |
| | (.842) | (3.97) | (3.87) |
| Pre-treatment mean | 44.1 | 44.1 | 44.1 |
| N | 869 | 869 | 869 |
| $R^2$ | .139 | .174 | .177 |
| | **Panel II: Georgia** | | |
| *Medicare* | -11.7*** | 1.48 | 1.40 |
| | (.953) | (3.08) | (3.04) |
| Pre-treatment mean | 44.5 | 44.5 | 44.5 |
| N | 2,284 | 2,284 | 2,284 |
| $R^2$ | .120 | .173 | .173 |
| | **Panel III: Louisiana** | | |
| *Medicare* | -13.0*** | 4.69* | 4.83 |
| | (.666) | (2.75) | (2.94) |
| Pre-treatment mean | 44.7 | 44.7 | 44.7 |
| N | 960 | 960 | 960 |
| $R^2$ | .268 | .355 | .361 |
| | **Panel IV: Mississippi** | | |
| *Medicare* | -12.8*** | 3.80* | 4.05** |
| | (1.18) | (1.98) | (1.96) |
| Pre-treatment mean | 52.7 | 52.7 | 52.7 |
| N | 1,230 | 1,230 | 1,230 |
| $R^2$ | .171 | .257 | .258 |
| | **Panel V: South Carolina** | | |
| *Medicare* | -23.0*** | -5.85 | -5.89 |
| | (2.68) | (4.04) | (4.17) |
| Pre-treatment mean | 55.5 | 55.5 | 55.5 |
| N | 690 | 690 | 690 |
| $R^2$ | .363 | .474 | .492 |
| Year fixed effects | No | Yes | Yes |
| County-level covariates | No | No | Yes |

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Based on annual county-level data from the Multiple Cause-of-Death Mortality Files, published by the National Vital Statistics System. Each column within each panel represents results from a separate OLS regression. The dependent variable is equal to the number of Black infant deaths per 1,000 live Black births in county *c* and year *t*. All models control for county fixed effects. Regressions are weighted by live Black births. Standard errors, corrected for clustering at the county level, are in parentheses.

**Appendix Table A7. Replicating and Extending Estimates from Almond et al. (2006) on the Effect of the Hospital Desegregation Campaign on Postneonatal Mortality due to Pneumonia, Influenza, and Diarrhea in Mississippi by Race**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Estimates reported in Almond et al. (2006) | | Replicating and extending estimates reported in Almond et al. (2006) | | | |
| **Panel I. Black postneonatal mortality due to pneumonia, influenza, and diarrhea** | | | | | | |
| *1 Year After Medicare* | -2.91*** | -2.60*** | -4.14*** | -3.98*** | -.992 | -1.76 |
| | (.984) | (.962) | (.618) | (.620) | (.789) | (1.00) |
| *2 Years After Medicare* | -4.75*** | -4.40*** | -4.06*** | -3.45*** | -.011 | -1.28 |
| | (1.00) | (1.05) | (.661) | (.906) | (1.00) | (1.37) |
| *3 Years After Medicare* | -4.21*** | -3.91*** | -4.79*** | -4.07*** | .261 | -1.57 |
| | (1.17) | (1.31) | (.571) | (.912) | (1.06) | (2.05) |
| *4 Years After Medicare* | -4.99*** | -4.64*** | -5.70*** | -5.37*** | -.480 | -3.16 |
| | (1.44) | (1.58) | (.786) | (1.02) | (1.48) | (2.89) |
| *5 Years After Medicare* | -5.36*** | -4.96*** | -5.47*** | -5.15*** | .285 | -2.89 |
| | (1.59) | (1.61) | (.936) | (1.26) | (2.05) | (3.84) |
| *6 Years After Medicare* | -5.78*** | -5.20*** | -5.57*** | -5.10*** | .898 | -3.04 |
| | (1.83) | (1.96) | (.745) | (1.33) | (2.24) | (4.62) |
| N | 1,022 | 1,022 | 1,200 | 1,200 | 1,200 | 1,200 |
| $R^2$ | .16 | .43 | .14 | .41 | .38 | .44 |
| **Panel II. White postneonatal mortality due to pneumonia, influenza, and diarrhea** | | | | | | |
| *1 Year After Medicare* | .341 | .331 | -.312 | .185 | .415 | -.086 |
| | (.242) | (.262) | (.250) | (.292) | (.361) | (.420) |
| *2 Years After Medicare* | .409 | .395 | -.475** | .076 | .168 | -.653 |
| | (.287) | (.333) | (.194) | (.242) | (.531) | (.556) |
| *3 Years After Medicare* | .222 | .225 | -.749*** | -.145 | -.225 | -1.46** |
| | (.259) | (.300) | (.201) | (.314) | (.618) | (.726) |
| *4 Years After Medicare* | .170 | .131 | -.936*** | -.389 | .091 | -1.62* |
| | (.327) | (.370) | (.176) | (.292) | (.612) | (.861) |
| *5 Years After Medicare* | .074 | .001 | -.988*** | -.402 | .371 | -1.81 |
| | (.293) | (.331) | (.218) | (.316) | (.692) | (1.10) |
| *6 Years After Medicare* | .067 | -.033 | -.555* | .074 | .321 | -2.60* |
| | (.355) | (.411) | (.285) | (.476) | (.799) | (1.56) |
| N | 1,022 | 1,022 | 1,200 | 1,200 | 1,200 | 1,200 |
| $R^2$ | .03 | .25 | .02 | .20 | .15 | .21 |
| County fixed effects | No | Yes | No | Yes | Yes | Yes |
| County-level covariates | No | Yes | No | Yes | Yes | Yes |
| County-specific linear trend | No | Yes | No | Yes | No | Yes |
| Year fixed effects | No | No | No | No | Yes | Yes |

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column within each panel represents results from a separate OLS regression. The dependent variable is equal to the number of postneonatal deaths due to pneumonia, influenza, and diarrhea per 1,000 race-specific live births in county *c* and year *t*. Medicare certification dates come from Almond et al. (2006). The models in columns (1) and (3) also include a pre-Medicare certification linear trend. In column (2), the county-level covariates used by Almond et al. (2006) include measures of maternal characteristics, per capita income and government transfer payments. The county-level covariates used in columns (4)-(6) are listed in Appendix Table A3. Almond et al. (2006) restricted their sample to no more than 7 years before and 6 years after Medicare certification. In columns (3)-(6), the sample is based on all county-year combinations for the period 1959-1973. Standard errors, corrected for clustering at the county level, are in parentheses.

**Appendix Table A8. Using and Extending Almond et al.'s (2006) Specification to Estimate the Effect of the Hospital Desegregation Campaign on Black Postneonatal Mortality in the Five Deep South States**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel I. Black postneonatal mortality** | | | | |
| *1 Year After Medicare* | -4.83*** | -2.12*** | 1.02 | 1.10 |
| | (.543) | (.548) | (.991) | (1.16) |
| *2 Years After Medicare* | -6.90*** | -3.71*** | .068 | .237 |
| | (.551) | (.546) | (1.33) | (1.66) |
| *3 Years After Medicare* | -8.07*** | -4.33*** | -.148 | .266 |
| | (.579) | (.679) | (1.59) | (2.17) |
| *4 Years After Medicare* | -9.73*** | -5.59*** | -.944 | -.322 |
| | (.613) | (.778) | (1.81) | (2.44) |
| *5 Years After Medicare* | -10.2*** | -5.59*** | .551 | 1.29 |
| | (.619) | (.920) | (2.26) | (3.17) |
| *6 Years After Medicare* | -9.86*** | -4.57*** | 2.77 | 3.94 |
| | (.637) | (1.00) | (2.76) | (3.97) |
| N | 6,033 | 6,033 | 6,033 | 6,033 |
| $R^2$ | .14 | .50 | .43 | .51 |
| **Panel II. Black postneonatal mortality due to pneumonia, influenza, and diarrhea** | | | | |
| *1 Year After Medicare* | -2.19*** | -.584 | 1.68** | 2.23*** |
| | (.399) | (.483) | (.727) | (.807) |
| *2 Years After Medicare* | -3.42*** | -1.51*** | 1.54 | 2.50** |
| | (.442) | (.536) | (.995) | (1.14) |
| *3 Years After Medicare* | -4.52*** | -2.27*** | 1.56 | 3.14** |
| | (.426) | (.562) | (1.13) | (1.50) |
| *4 Years After Medicare* | -5.52*** | -3.04*** | 1.31 | 3.51* |
| | (.501) | (.667) | (1.43) | (1.97) |
| *5 Years After Medicare* | -5.78*** | -3.08*** | 2.25 | 5.21** |
| | (.471) | (.689) | (1.79) | (2.46) |
| *6 Years After Medicare* | -6.47** | -3.30*** | 3.08 | 7.30** |
| | (.475) | (.727) | (2.04) | (2.95) |
| N | 6,033 | 6,033 | 6,033 | 6,033 |
| $R^2$ | .09 | .48 | .40 | .48 |
| County fixed effects | No | Yes | Yes | Yes |
| County-level covariates | No | Yes | Yes | Yes |
| County-specific linear trend | No | Yes | No | Yes |
| Year fixed effects | No | No | Yes | Yes |

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column within each panel represents results from a separate OLS regression. The dependent variable is equal to the number of postneonatal deaths per 1,000 race-specific live births in county *c* and year *t*. The model in column (1) also includes a pre-Medicare certification linear trend. Standard errors, corrected for clustering at the county level, are in parentheses.

## Appendix B

For Online Publication

**Appendix Table B1. Data Sources for County-Level Live Births by Race and Attendant**

| State | Sources | Notes |
|---|---|---|
| Alabama | 1959-1967: Yearly volumes of *Vital Statistics of the United States*, made available through the NBER with support from NIA grant P30-AG012810. Available at: https://data.nber.org/births/1940-1968/.<br><br>1968-1973: Compiled by the authors using the National Center for Health Statistics' birth certificate data, made available through the NBER at: https://data.nber.org/data/vital-statistics-natality-data.html. | Information on county-level births by race is not available from Alabama vital statistics, which only report state-level aggregates.<br><br>Information on county-level births by race is unavailable from the *Vital Statistics of the United States* for the years 1959-1967 for 9 counties. These counties are excluded from all analyses.<br><br>Information on county-level births by out-of-hospital attendant (i.e., physician vs. midwife) is unavailable from the *Vital Statistics of the United States* for the years 1960-1967 for all counties. Consequently, Alabama is excluded from the birth location/attendant analysis in Section 4. |
| Georgia | 1959-1961: Yearly volumes of *Vital Statistics, Georgia*, made available through inter-library loan with Cornell University.<br><br>1962-1963: Yearly volumes of *Georgia Vital and Morbidity Statistics*, made available through inter-library loan with Georgia College and State University.<br><br>1964: *Georgia Vital and Morbidity Statistics*, made available through inter-library loan with Cornell University.<br><br>1965-1970: Yearly volumes of *Georgia Vital and Morbidity Statistics*, made available through inter-library loan with Georgia College and State University.<br><br>1971: *Georgia Vital and Morbidity Statistics*, made available through inter-library loan with Georgia Southern University.<br><br>1972-1973: Compiled by the authors using the National Center for Health Statistics' birth certificate data. Available at: https://data.nber.org/data/vital-statistics-natality-data.html. | Information on county-level births by out-of-hospital attendant (i.e., physician vs. midwife) is unavailable from the Georgia vital statistics for the years 1972-1973. We used the natality data made available through the NBER to compute county-level births by out-of-hospital attendant for these years.<br><br>For the period 1959-1973, there are 101 county-year observations in Georgia where there were no Black births. These observations are excluded from all analyses. |

| | |
|---|---|
| Louisiana | 1959-1966: Yearly volumes of *Statistical Report of the Division of Public Health Statistics*, made available through correspondence with the Vital Records Central Office, Louisiana Department of Health.<br><br>1967-1972: Yearly volumes of *Statistical Report of the Bureau of Vital Statistics*, made available through correspondence with the Vital Records Central Office, Louisiana Department of Health.<br><br>1973: *Vital Statistics of Louisiana*, made available through correspondence with the Vital Records Central Office, Louisiana Department of Health. |
| Mississippi | 1959-1961: Yearly volumes of *Public Health Statistics State of Mississippi*, made available through correspondence with the Office of Public Health Statistics, Mississippi State Department of Health.<br><br>1962-1973: Yearly volumes of *Vital Statistics Mississippi*, made available through correspondence with the Office of Public Health Statistics, Mississippi State Department of Health. |
| South Carolina | 1959-1973: Yearly volumes of *Annual Report of the State Board of Health of South Carolina*, made available through inter-library loan with South Carolina State University. |

**Appendix Table B2. International Classification of Disease (ICD) Codes Used to Generate Infant Mortality Rates by Cause of Death**

| Cause of death | ICD codes, 7th revision, 1959-1967 | ICD codes, 8th revision, 1968-1973 |
|---|---|---|
| Pneumonia | Lobar pneumonia (490)<br>Bronchopneumonia (491)<br>Primary atypical pneumonia (492)<br>Pneumonia, other and unspecified (493)<br>Pneumonia of newborns (763) | Viral pneumonia (480)<br>Pneumococcal pneumonia (481)<br>Other bacterial pneumonia (482)<br>Pneumonia due to other specified organism (483)<br>Acute interstitial pneumonia (484)<br>Bronchopneumonia, unspecified (485)<br>Pneumonia, unspecified (486) |
| Influenza | Influenza with pneumonia (480)<br>Influenza with other respiratory manifestations, and influenza unqualified (481)<br>Influenza with digestive manifestations, but without respiratory symptoms (482)<br>Influenza with nervous manifestations, but without digestive or respiratory symptoms (483) | Influenza unqualified (470)<br>Influenza with pneumonia (471)<br>Influenza with other respiratory manifestations (472)<br>Influenza with digestive manifestations (473)<br>Influenza with nervous manifestations (474) |
| Diarrhea (including dysentery) | Bacillary dysentery (045)<br>Amoebiasis (046)<br>Other protozoal dysentery (047)<br>Unspecified form of dysentery (048)<br>Gastritis and duodenitis (543)<br>Gastro-enteritis and colitis, except ulcerative, age 4 weeks and older (571)<br>Chronic enteritis and ulcerative colitis (572)<br>Diarrhea of newborns (764) | Bacillary dysentery (004)<br>Amoebiasis (006)<br>Enteritis due to other specified organism (008)<br>Diarrhoeal disease (009)<br>Gastritis and duodenitis (535)<br>Gastro-enteritis and colitis, except ulcerative, of noninfectious origin (561)<br>Chronic enteritis and ulcerative colitis (563) |

Notes: Three-digit ICD codes are in parentheses.