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Lauren Hoehn-Velasco
Nikita Dhingra
Mayra Pineda-Torres

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ABSTRACT

Do restrictions on public funding create unintended reliance on social assistance? In this paper, we study the 1976 Hyde Amendment, which barred federal funding for abortion. Using county-level data and an event-study design, we show that reduced federal funding for abortion increased fertility among young women by 2%. These effects were concentrated among non-white women, who subsequently experienced greater welfare participation in states with larger abortion funding declines. The consequences extend into the next generation: non-white girls born after Hyde were more likely to rely on public assistance in adulthood. Abortion funding restrictions reinforce long-run economic inequality across generations.

Lauren Hoehn-Velasco
Georgia State University
Andrew Young School of Policy Studies
Department of Economics
lvelasco@gsu.edu

Nikita Dhingra
Georgia State University
ndhingra1@gsu.edu

Mayra Pineda-Torres
Georgia Institute of Technology
and NBER
mpinedat@gatech.edu

A Online Appendix is available at http://www.nber.org/data-appendix/w34548

1 Introduction

Public assistance programs are deeply interconnected, and eligibility criteria or structural changes to one program often influence participation in other programs (Remler and Glied, 2003; Currie and Grogger, 2002; Thompson and Garrett-Peltier, 2012; Kline and Walters, 2016). As a result, funding cuts or cost-containment measures in one area can lead to increased participation in other parts of the social safety net. Effects of such funding cuts are rarely distributed evenly; reductions in public spending disproportionately affect low-income and minority individuals, exacerbating existing disparities and reinforcing economic inequality (Currie and Grogger, 2002; Toffolutti and Suhrcke, 2019; Jackson et al., 2021). Conversely, expanding access to public programs can yield substantial, long-lasting benefits that span generations, acting as crucial investments in human capital and economic mobility (Sommers and Oellerich, 2013; Chetty et al., 2016; Hoynes et al., 2016; Aizer et al., 2016; Goodman-Bacon, 2018, 2021; Hoehn-Velasco, 2021; East et al., 2023).

A prime example of a long-standing and influential public funding restriction is the Hyde Amendment. The Hyde Amendment bars the use of federal Medicaid funds for abortion except in cases of rape, incest, or life endangerment, and has been in effect for nearly 50 years (Trump, January 24, 2025). Prior to its passage, Medicaid financed roughly 300,000 abortions each year, about one quarter of all U.S. abortions (Cates, 1981). Over the 1977–2016 period, this proportion implies that 4.8–8.3 million abortions were directly affected by the withdrawal of public funding.[1]

The Hyde Amendment funding restrictions remain relevant today, since one in five reproductive-age women, a total of 16 million women, rely on Medicaid for health coverage (ACOG, 2024; KFF, 2022b,a, 2024a). Women who seek abortions are also disproportionately low-income and therefore more likely to be insured through Medicaid (KFF, 2024a). While some states use their tax dollars to fund abortion services for Medicaid enrollees, many do not, leaving substantial gaps in funding for abortion services (Gold, 1980). These restrictions continue to create major barriers to reproductive healthcare and shape the economic well-being of disadvantaged households. Although public debate over Hyde has centered on moral and ethical issues, the policy also has profound economic implications for low-income women receiving Medicaid.

In this paper, we analyze both the immediate and longer-term effects of the Hyde Amendment. We begin by investigating the initial impacts on fertility. We use an event-study design that exploits variation in county-level Medicaid eligibility as a

[1] See authors' calculations in background Section 2.5.

measure of the Hyde Amendment's treatment intensity.[2] Our analysis draws on county-level fertility data from the Natality Detailed File, covering the years 1971 to 1982.[3]

We find that counties with higher Medicaid eligibility experience a clear increase in fertility following the Hyde Amendment. Our event-study estimates show a 2% increase in fertility among women under age 30, concentrated among those in their late teens and early twenties.[4] The increase is especially pronounced among unmarried women with children, the group most likely to be Medicaid-eligible. Complementary evidence from Georgia's county-level administrative records and the National Survey of Family Growth (NSFG) demonstrates that the Hyde Amendment reduced abortions among Medicaid-eligible women. In Georgia, counties with higher Medicaid eligibility display clear declines in abortions. In the NSFG, Medicaid recipients show a shift from abortion to live births after Hyde. Together, these findings reinforce that restrictions on publicly funded abortion translate directly into higher fertility.

We then examine the downstream economic consequences of this fertility response, which has potential distributional effects. When unwanted births increase among low-income women, household resources are strained, and families may turn to public assistance. In this way, the fertility effects of abortion funding restrictions generate broader pressures on the social safety net. To test this hypothesis, we use the Current Population Survey to study economic outcomes in states that experienced larger declines in Medicaid-funded abortions before and after Hyde and a triple difference design where we compare these outcomes for non-white and white women.[5] We find that non-white women in states with larger declines in Medicaid-funded abortions experienced a notable rise in welfare participation following Hyde. This increase in public assistance use is accompanied by a higher likelihood of having children in the household and a larger household size. These results show that the fertility effects of the Hyde Amendment were not merely demographic shifts but intensified reliance on welfare programs. Because young non-white women were especially likely to rely on Medicaid for healthcare, they bore the brunt of these restrictions.[6]

We then track children born around the time of the Hyde Amendment in the American Community Survey (ACS) from 2000 to 2019. We focus on women since they are more likely than men to receive public assistance. We expect the Hyde Amendment

[2] We identify Medicaid eligibility based on Aid to Families with Dependent Children (AFDC) participation (i.e., cash welfare; ASPE, 2024), which was categorically linked to Medicaid in the 1970s (Goodman-Bacon, 2018, 2021).

[3] While this time period overlaps with *Roe v. Wade*, we incorporate state-by-year fixed effects to limit the influence of state policy.

[4] Young women account for most unintended pregnancies (KFF, 2021) and disproportionately rely on Medicaid to cover abortion costs (authors' calculations from NHDS; see Figure A.4).

[5] Medicaid funding of abortions is from newly digitized publications of the number of Medicaid-funded abortions in each year.

[6] See Figure F.2. The focus on non-white women is motivated by state-level evidence showing that the fertility increase following the Hyde Amendment is concentrated among non-white women. Prior research has also shown that non-white women are especially affected by abortion restrictions (Lincoln et al., 1977; Joyce et al., 2013; Myers, 2017a; Farin et al., 2024; Gardner, 2024; Jones and Pineda-Torres, 2024).

to have lasting effects as children born during this period grew up in larger households with greater reliance on public assistance. Our triple-difference event-study estimates indicate that, as adults, non-white girls in states with larger declines in Medicaid abortions exhibit significantly higher participation in public assistance.

These second-generation impacts underscore how the Hyde Amendment has deepened economic inequality across generations. By restricting abortion access, the funding ban increased fertility among women already facing structural disadvantages, reinforcing long-term dependence on public assistance. These immediate fertility effects then shape the economic opportunities and well-being of disadvantaged women and their children. In this way, fertility responses to abortion funding restrictions emerge not merely as demographic outcomes but as central drivers of economic inequality. As a result, women who rely on Medicaid may require additional public investment and support as a direct result of Hyde.

Our study makes four key contributions to the literature. First, we show that abortion funding restrictions produced lasting economic consequences by altering fertility among low-income women. The Hyde Amendment increased births among Medicaid-eligible populations, which strained household resources and expanded reliance on welfare programs. In doing so, the policy shifted recipients of public support from a one-time Medicaid expense to other parts of the social safety net, thereby reinforcing cycles of inequality. These findings complement a large body of literature showing that when families receive adequate public support, their long-term economic and health outcomes improve (Sommers and Oellerich, 2013; Hoynes et al., 2016; Goodman-Bacon, 2018, 2021; East et al., 2023; Baker et al., 2023). Our findings advance this knowledge base by showing the reverse: the long-term costs of restricting public support. Limiting one public program can increase dependence on others, with effects that persist across generations.

Second, we provide one of the first comprehensive analyses of the Hyde Amendment, linking the immediate fertility effects with economic outcomes. While prior research focuses on fertility, we show a broader link between fertility and public assistance participation. By documenting the full chain of effects, our study moves beyond demographic outcomes to highlight how a targeted funding restriction can reshape the economic trajectories of disadvantaged families. This broader perspective is essential given Hyde's enduring reach: nearly fifty years after enactment, it continues to affect 16 million reproductive-age women annually (KFF, 2025).

Third, we contribute to the literature on the economic consequences of restricted abortion access by leveraging plausibly exogenous variation from federal funding bans.

Prior research has largely focused on the first generation directly affected by abortion restrictions, documenting impacts on outcomes such as educational attainment (Angrist and Evans, 1996; Pop-Eleches, 2006; Ananat et al., 2007; Mølland, 2016; González et al., 2018; Jones and Pineda-Torres, 2024), labor market outcomes (Angrist and Evans, 1996; Kalist, 2004; Mølland, 2016; Lindo et al., 2020b; Jones et al., 2021), and financial instability (Miller et al., 2023).[7] By contrast, the present study examines both the women whose ability to terminate a pregnancy was constrained by funding restrictions through the Hyde Amendment and the children born as a result. We add to this literature by showing that limiting abortion funding perpetuates reliance on the social safety net and the intergenerational transmission of economic inequality.[8]

Fourth, our paper is among the first to analyze the fertility effects of the Hyde Amendment using within-state variation, providing a more granular perspective than state-level studies. With the exception of Kane and Staiger (1996),[9] prior research has relied on state-level responses to the federal funding restrictions and shown no effect or reductions in fertility (Garbacz, 1990; Haas-Wilson, 1993; Blank et al., 1996; Haas-Wilson, 1996; Levine et al., 1996; Haas-Wilson, 1997; Matthews et al., 1997; Cook et al., 1999). By contrast, our county-level approach leverages variation in Medicaid eligibility to identify local impacts that are masked in state-level aggregates. Incorporating state-by-year fixed effects further allows us to control for contemporaneous state policy responses. Our design reveals clear increases in fertility for young women.

2 Background

2.1 The Hyde Amendment: A Ban on Federal Funding for Abortion

In 1976, the U.S. Congress passed the Hyde Amendment, introduced by Illinois Representative Henry Hyde, as a rider to the Department of Labor, Health, Education,

[7] Another set of studies has shown that abortion access affects outcomes for children (Gruber et al., 1999; Bitler and Zavodny, 2001; Ananat et al., 2009; Mitrut and Wolff, 2011; Lahey and Wanamaker, 2025), as well as adolescents and adults (Ananat et al., 2009; Charles and Stephens, 2006; Pop-Eleches, 2006; Mølland, 2016; Myers, 2017a; Gutierrez, 2022). Studies have also examined the long-term effects of abortion access, including Lahey and Wanamaker (2025); Farin (2024, 2025).

[8] Our study also contributes to the broader literature on abortion restrictions (Haas-Wilson, 1996; Bitler and Zavodny, 2001; Pop-Eleches, 2006; Guldi, 2008; Jacobson and Royer, 2011; Lahey, 2014b,a; Miller et al., 2023), abortion legalization (Angrist and Evans, 1996; Levine et al., 1999; Donohue and Levitt, 2001; Ananat et al., 2004; Kalist, 2004; Levine and Staiger, 2004; Oreffice, 2007; Guldi, 2008; Foote and Goetz, 2008; Donohue and Levitt, 2008; Ananat et al., 2009; Mølland, 2016; Myers, 2017a; González et al., 2018; Clarke and Mühlrad, 2021; Hajdu and Hajdu, 2021; Jones et al., 2021; Abboud, 2025), parental consent and notification (Kane and Staiger, 1996; Joyce and Kaestner, 1996; Matthews et al., 1997; Bitler and Zavodny, 2001; Levine, 2003; Klick and Stratmann, 2008; Sabia and Rees, 2013; Sabia and Anderson, 2016; Joyce et al., 2020; Myers and Ladd, 2020), abortion provider restrictions (Quast et al., 2017; Fischer et al., 2018; Lindo et al., 2020a; Muratori, 2021; Jones and Pineda-Torres, 2024; Gardner, 2024), and mandatory waiting periods (Joyce et al., 1997; Joyce and Kaestner, 2000, 2001; Bitler and Zavodny, 2001; Lindo and Pineda-Torres, 2021; Myers, 2021; Altındağ and Joyce, 2022; Durrance et al., 2024). Prior research has also shown the fertility effects of the pill (Goldin and Katz, 2000, 2002; Bailey, 2006; Ananat et al., 2007; Hock et al., 2007; Bailey, 2010; Bailey et al., 2012; Zuppann, 2012; Steingrimsdottir, 2016; Myers, 2017a).

[9] Our findings diverge from the earlier county-level study, Kane and Staiger (1996), in two key ways. First, many states did not fully replace the federal dollars lost due to the Hyde Amendment. For example, Illinois and New Jersey continued Medicaid funding using state funds but reduced the scope of funded abortions. Second, we define Medicaid eligibility based on AFDC participation rather than poverty rates. While the two are correlated, AFDC receipt is statutorily linked to Medicaid (Goodman-Bacon, 2018, 2021).

and Welfare Appropriations Bill (Copelon and Law, 2010; CRS, 2023). The Amendment prohibited the use of federal funds for abortion, initially allowing exceptions only when the mother's life was endangered (Copelon and Law, 2010; CRS, 2023). Since the 1990s, the Hyde Amendment has consistently allowed federal funds in cases of rape, incest, or when the mother's life is endangered.[10]

Although enacted in September 1976, the Amendment's enforcement was quickly halted. In October, a federal judge issued an injunction in *McRae v. Mathews*, blocking the Hyde Amendment's implementation (Copelon and Law, 2010; U.S. District Court for the Eastern District of New York, 1976). The injunction remained in effect until August 4, 1977, when it was lifted, allowing the Hyde Amendment to take full effect (Lincoln et al., 1977; Cates, 1981; Torres et al., 1986). As a result, abortions continued to receive federal funding in early 1977, but by the end of the year, those funds were barred. By 1978, federal support for abortion had fallen by 99% (Gold, 1980). The restrictions have remained in place ever since, except for a seven-month suspension in 1980 while the Supreme Court considered the law's constitutionality (Cates, 1981).

Despite delays in implementing the Hyde Amendment, the effects began as early as 1976. In that year, the Amendment received widespread publicity across the United States, which may have deterred women from seeking abortions funded by public programs (Trussell et al., 1980; Cates, 1981). As Cates (1981) explains, "The widespread publicity about the restriction of federal funds may have obscured the recognition of the continuing availability of state funds for abortion. This confusion may have deterred some low-income women from requesting abortions and some abortion providers from soliciting state reimbursement" (Cates, 1981, pg 1111).

In our own review, we found substantial coverage of the Hyde Amendment across both national and local media. Between 1976 and 1977, at least 110 news outlets in all 50 states reported on the Amendment. This widespread attention likely shaped public perception: many individuals may have been aware of the Amendment's existence but not fully informed about the legal status or the availability of alternative funding.[11] Media accounts at the time reflected strong reactions. For example, the Dayton Daily News described the Hyde Amendment as "an anti-poor amendment" and called it "blatantly unconstitutional on equal protection and privacy laws" (The Dayton Daily, 1976). The Anchorage Times noted, "The Department of Health, Education and Welfare, in 1975, reports that the government financed about 300,000 abortions for poor

[10] See Foundation (2021); CRS (2023). Over time, Congress revised the Hyde Amendment multiple times: with exceptions for rape and incest intermittently included and removed (CRS, 2023)

[11] Low-income women may not have known about the availability of state funds for abortion. According to Cates (1981), "the same confusion over the availability of state funds for abortion that led some women to use other means to finance their pregnancy terminations may have led other women to continue their pregnancies to term" (Cates, 1981, pg 1111). Some providers may also have refrained from seeking reimbursement from available state funds (Cates, 1981, pg 1111).

women at a cost of 40−50 million, so it is claimed that the new ban on funding will save a large sum for the tax payers" (The Anchorage Times, 1976).

2.2 Who does the Hyde Amendment Affect?

The Hyde Amendment primarily affects reproductive-age women participating in the Medicaid program (Cates, 1981).[12] Medicaid eligibility at the time was tied to AFDC receipt and other categorical welfare programs financed under the Social Security Act. For example, Medicaid/Medicare Management Institute (1978, 1979) states that "Medicaid eligibility is linked to the Federally assisted welfare programs of Aid to Families with Dependent Children (AFDC) and Supplemental Security Income (SSI) for the aged, blind, and disabled" (Medicaid/Medicare Management Institute, 1979, pg 24). To make this point clear, we show a breakdown of eligibility in Figures I, A.1, A.2, A.3. The eligibility categories illustrate that, for able-bodied reproductive-age women, AFDC was the clearest pathway to Medicaid eligibility.

Because the targeted population relied on welfare payments for income support and Medicaid for health insurance, AFDC recipients had limited means to pay for an abortion after the Hyde Amendment. At the time, *"the average cost of an abortion in the United States- $280-is $42 higher than the average monthly welfare payment for an entire family for food, clothing, shelter and all other necessities"* (Lincoln et al., 1977, pg 210). In some states, the payment discrepancy was even higher. In Mississippi, for example, *"the average cost of an abortion is 10 times higher than the monthly family AFDC payment"* (Lincoln et al., 1977, pg 210). The state-level ratios of AFDC payments to the cost of an abortion are outlined in Table 1 (Lincoln et al., 1977). Table 1 shows that in most U.S. states, the cost of an abortion is higher than the average AFDC payment.

Women receiving Medicaid also actively used public funds for abortions prior to the Hyde Amendment. In 1976, *"the rate of abortions per 1,000 Medicaid-eligible women aged 15-44, was about three times higher in FY 1976 than among the nonpoor"* (Lincoln et al., 1977, pg 210). Table 1 ((7)-(9)) shows that in the majority of U.S. states, the Medicaid abortion rate prior to the Hyde Amendment was higher than for non-Medicaid recipients. In 1977, an estimated 295,000 abortions were paid for with federal funds (CDC, 1977), representing one-quarter of the 1.3 million national abortions (Cates, 1981). In 1976, for 44 reporting states, there were 260,800 Medicaid-funded abortions

[12] Today, some federally qualified health centers (FQHCs) offer publicly funded abortions, but this was not the case in the 1970s. At that time, FQHCs did not provide abortion services, so the Hyde Amendment primarily constrained abortion coverage for Medicaid enrollees. FQHC involvement in abortion care grew only after the creation of the Reproductive Health Access Project in 2005, which supports clinics seeking to integrate medication abortion (Reproductive Health Access Project, 2025). Even so, several federal restrictions continue to limit availability. First, due to Hyde, FQHCs may provide abortions only with non-federal funds. Second, Title X funds cannot be used in programs where abortion is a method of family planning (Library of Congress, 2024). Third, Section 330 grantees under the Public Health Service Act may provide abortions only as separate lines of business, outside the scope of federally supported project costs (Reproductive Health Access Project, 2023). Finally, many FQHCs are unclear about their legal authority to provide abortions or fear jeopardizing federal funding because separating federal from non-federal resources in practice is difficult (Person-Centered Reproductive Health Program, 2025).

and 962,314 total abortions, translating into 27% of all abortions (Lincoln et al., 1977).

Historical evidence from the time also suggests that Black women were more affected by the Hyde Amendment than their white counterparts. An analysis at the time of the Hyde Amendment argued that "*proportionally more Black than white women will be forced to bear unwanted children*" due to the reduction in Medicaid-funded abortions (Lincoln et al., 1977, pg. 213). This differential racial impact is due to the fact that *"an estimated 39 percent of black women rely on Medicaid for their health care-including abortion-compared to just seven percent of white women"* (Lincoln et al., 1977, pg 213). Further, *"the abortion-to-live-birth ratio is nearly two times higher among blacks than whites"* (Lincoln et al., 1977, pg 213). Thus, the Hyde Amendment should disproportionately affect poor black women as compared with their white counterparts.

2.3 State Funding for Abortion

The Hyde Amendment primarily changed how abortions were paid for through the Medicaid program, which uses both federal and state funds to pay for the healthcare of low-income women. While the Hyde Amendment barred the use of federal funding for abortion, the Amendment did not prohibit states from using their own funding to cover abortions (Gold, 1980). After the Amendment, states could fund abortions entirely with state funds or discontinue the provision of abortion services through their Medicaid programs. To meet the gaps in funding created by the Hyde Amendment, "*eighteen States and the District of Columbia continued to fund abortions under most circumstances; the remaining 32 States did not fund abortions except to save the life of the woman*" (Lang et al., 1983, pg 12).

From 1977 to 1978, federally funded abortions fell from 295,000 to around 2,000 (Trussell et al., 1980). Meeting this decline in federal funding, in 1978, states fully funded 194,000 abortions (Gold, 1980). Based on those numbers, the Hyde Amendment appears to have directly prevented the public funding of 100,000 abortions. The majority of subsidized abortions that still occurred were in states with funding available for medical necessity (Gold, 1980). However, the number of prevented abortions could be larger. Gold (1980) and Torres et al. (1986) estimate that in 1978, there were 234,000 women in need of abortion who could not obtain one due to public funding restrictions. After the Hyde Amendment, Cates (1981) estimates that of the low-income women in need of abortion, *65% used state funds, 29% used other funds, 5% carried their pregnancies to term, and only 1% resorted to an illegal abortion*.

We compile data on both Medicaid-funded abortions and total abortions across U.S. states. Figure II presents the aggregate estimates, where we group states by their decline in abortion funds. In Figure II, Medicaid-funded abortions are displayed on

the left and total abortions from the CDC Abortion Surveillance Program on the right. We also provide the detailed state-level data in Table 1 Columns (8)-(11) as well as state-level abortions plotted over time in the Appendix (Figure A.5 and Figure A.6). Figure A.8 also shows the state-level funding per abortion (a proxy for cost).

Panel A of Figure II displays abortions in states with limited state funding after the Hyde Amendment, a 90%+ drop in funds. In these states, Medicaid abortions drop off almost completely, falling from 40,000 to less than 10,000. Despite the drop in funding, women in these states still continued to obtain abortions. Possible paths to obtain an abortion included self-funded abortions, reduced fees by abortion providers for Medicaid-eligible women, public hospitals with low-cost abortions, private donations, and family planning agencies (Gold and Cates Jr, 1979; Cates, 1981). However, evidence suggests that women obtained abortions at later gestational ages (by two weeks later, Gold and Cates Jr, 1979). Furthermore, a higher proportion of women carried unwanted pregnancies to term. In select unfunded states, Cates (1981) estimates that 18-35% of Medicaid-eligible unwanted pregnancies were carried to term.

In Panel B, we show states with partial funding drops (1-89%). Panel B reveals that even in states with available public funds, abortions decline after the Hyde Amendment. Medicaid-funded abortions fell from over 80,000 to below 60,000 in 1978 (for the available states). For example, in Illinois, the state abortion funding policy changed four times throughout 1978, and *"as a result, Illinois financed fewer than one-fourth the number of Medicaid-funded abortions than it had in the year before the Hyde Amendment"* (Cates, 1981, pg 1111). The decline in Medicaid abortions is large enough that it appears to affect the total number of abortions on the right-hand side, Panel B.ii.

In a few states, abortion funding remained quite high. Figure II Panels C-D show states with continued support for Medicaid-funded abortions after the Hyde Amendment. These states, especially California, fund more abortions through Medicaid after Hyde. Because these states had such high support for Medicaid-funded abortions, we do not include them in the county-level analysis and only consider them in a placebo test and as controls in the state-level analysis. However, these states may still have had hurdles to obtaining an abortion, even if purely informational. Thus, our state-level results will be biased downward if the informational effects are large.

2.4 Abortion in the 1970s

The Hyde Amendment followed shortly after the sweeping legalization of abortion through the U.S. Supreme Court decision in *Roe v. Wade* (henceforth, *Roe*). On January 22, 1973, *Roe* legalized abortion nationwide and overturned state-level restrictive

abortion laws. Here, we describe the characteristics of abortion in the 1970s.[13] In the wake of *Roe*, the legal abortion rate steadily increases (top panel of Figure III). Following the Hyde Amendment, the abortion rate dips and then rises again. Much of that rise comes from states with some funding available through state Medicaid programs (Panel B of Figure II). Although the abortion rate is higher for non-white women as compared to white women (right-top panel of Figure III), the non-white abortion rate falls after the Hyde Amendment, while the white abortion rate continues rising. Furthermore, the highest share of abortions occurs in women 20-24, with a large share also occurring in women 15-19 and 25-29. Women in their 30s and older and under 15 only make up a small share of abortions (second panel of Figure III).

The third row of Figure III presents the proportion of abortions by each method. By 1974, abortions were primarily performed with suction curettage (vacuum aspiration), replacing earlier methods of using sharp curettage (Kleinman and Senanayake, 1993; Rubin, 1994). Over the period of study, more than 80% of abortions were performed with curettage, mainly suction. Finally, the bottom of Figure III shows that the majority of abortions occurred in the first trimester, with almost half occurring before 9 weeks of gestation. Over the period, abortions at higher than 16 weeks declined, with a noticeable uptick in abortions occurring at 13-15 weeks after 1977.

2.5 Aggregate Impacts of the Hyde Amendment Over Time

The Hyde Amendment's funding restrictions directly affected a large share of abortions over time. Recent estimates suggest that about 30% of abortions are paid for by Medicaid (Guttmacher Institute, 2025). Estimates from the 1970s reported similar figures of 25–30% (Cates, 1981). Applying these rates implies that between 100,000 and 220,000 abortions were impacted each year in states with abortion funding restrictions (see Figure A.9). These annual totals amount to 4.8 million abortions affected by funding cuts over the 1977–2016 period. If we include states that replaced federal dollars with state funding, the number of abortions affected rises to 8.3 million. These tallies understate the true impact of the Hyde Amendment, as they do not account for women deterred from seeking abortions altogether after public funding declines.

3 Data

We assemble a variety of data sources to examine the consequences of the Hyde Amendment. We primarily rely on the Natality Detail Files (or birth certificate records)

[13] While informative about abortion characteristics in the 1970s and early 1980s, these data have important limitations. First, CDC abortion surveillance is only available at the state level, preventing us from examining Hyde's effects in our county-level specification (though we do so for Georgia). Second, not all states reported abortion characteristics, and many did so only intermittently; Figure III therefore uses a balanced panel of reporting states. Third, CDC figures are known to be underreported: even in 2000, CDC totals lagged behind estimates from the Alan Guttmacher Institute, which surveys providers directly (Elam-Evans et al., 2002).

and the *Surveillance, Epidemiology, and End Results (SEER)* population data. For the economic effects, we use the *Current Population Survey (CPS, Flood et al., 2024)* and the *American Community Survey (ACS, Ruggles et al., 2024)*. Here, we describe the main data for the fertility results, describing other data sources where relevant.

3.1 Fertility: U.S. Birth Certificate Records

We rely on the Natality Detail Files (or birth certificate records) from the National Vital Statistics System (NVSS) and National Center for Health Statistics (NCHS) (NCHS, 1970-1990) for county and state-level births. The Natality Detail File includes information on the location of the delivery, including occurrence state, mother's residence state, and occurrence/residence county. For our analysis, we primarily rely on the mother's county of residence. When measuring the number of births, we base the timing on conception rather than birth. Since most abortions occur early in pregnancy, using the conception date provides a clearer picture of when abortions would have occurred. We calculate the conception year by subtracting the reported gestational age from the birth year. If gestation is missing from birth records, we assume a roughly average gestation of 38 weeks.

The Natality Detail File includes all U.S. births, though, in certain early years, states only reported a 50% sample. Throughout the analysis, we use the sample weight to determine whether the delivery represents a 50% or 100% record. We collapse the data to the county level and multiply the number of deliveries by the record weight to account for the 50% samples. This mainly applies to deliveries that occurred before 1972, but some states continued to report a 50% sample until 1985.[14]

In our county and state-level analysis, we focus primarily on births to young women, specifically those under the age of 30. Women under the age of 30 make up the majority of births and abortions (Figure III and KFF (2021)). This age group is also of particular interest because women in their teens and 20s are more likely to use public funds to pay for an abortion[15] and teenagers are the most likely to have unmarried births (Kane and Staiger, 1996).[16]

[14] Prior to 1972, data are based on a 50-percent sample of birth certificates from all states. Beginning in 1972, data are based on a 100-percent sample of birth certificates from some states and on a 50-percent sample from the remaining states. The number of states from which 100 percent of the records are used has increased from 6 in 1972 to all States and the District of Columbia in 1985" (NBER, 2024). These data also include characteristics of the mother and newborn, although health information beyond birth weight and gestation is relatively limited during the study period.

[15] Author's calculations based on NHDS, see Figure A.4.

[16] In Figure VIII, we show ages 18–29, 20–29, and 21–29, omitting minors. Subsetting to adult women removes those affected by Supreme Court decisions (*Planned Parenthood v. Danforth* and *Bellotti v. Baird*) that affected consent for minors. (Myers, 2022). Complementing these results, in Figure VII, we also show compositional shifts towards non-white women in their early 20s.

3.2 Population: SEER Population Data

We combine the county-level births with SEER population data to construct fertility rates and controls (SEER, 2024). These data include the population counts by single-year age, sex, and race. We use this data to compute the exposure for Poisson specifications and the denominator of the fertility rates. The fertility rate is defined as the number of births per 1,000 females in the relevant population. We also use the SEER population data to calculate controls. Our main controls include the share female, the share white, and the unemployment rate (Haines, 2010).[17]

3.3 Medicaid Eligibility: AFDC Receipt

To measure Medicaid eligibility, we use the share of the population receiving AFDC, commonly referred to as "welfare." Specifically, we proxy Medicaid eligibility using the county-level share of the population receiving AFDC in 1976, based on data from United States Bureau of the Census (2012). Though, the results are similar if we use earlier years of AFDC (1970, 1972, 1975, see Figure VII Panel C). This approach, using AFDC participation to approximate Medicaid eligibility, has been adopted in related studies (Goodman-Bacon, 2018, 2021). As Goodman-Bacon (2018) points out, there was a "federal requirement that states cover all cash welfare recipients" and a "statutory link between welfare receipt and Medicaid eligibility" (Goodman-Bacon, 2018, pg. 1). Based on these facts, AFDC participation provides a reliable indicator of Medicaid eligibility for reproductive-age women, as Medicaid eligibility was statutorily tied to receipt of AFDC (Provost and Hughes, 2000).[18]

4 County-level Event Study for Fertility

To investigate the impact of the Hyde Amendment on county-level fertility rates, we employ an event-study design. While the Hyde Amendment had national effects, its impact varied widely depending on the prevalence of Medicaid participation, especially among young women of reproductive age. We incorporate the differential effect of the Hyde Amendment using the share of county residents participating in the AFDC program (or cash welfare) in 1976. AFDC participation acts as a proxy for Medicaid, as AFDC receipt is categorically linked to Medicaid eligibility (see Figures I, A.1, A.2, A.3). In our empirical design, a county will receive a larger treatment if it has a higher level of AFDC participation, which represents local Medicaid eligibility. AFDC participation has been previously used to identify Medicaid recipients in similar studies (Goodman-Bacon, 2018, 2021).

[17] Haines (2010) is not available in all years, and is linearly interpolated between years.
[18] Major Medicaid eligibility expansions occurred later in the 1980s (Currie and Gruber, 1996; East et al., 2023).

Our central empirical strategy relies on the assumption that the Hyde Amendment differentially increased fertility in counties with higher Medicaid eligibility, measured by the proportion of AFDC recipients in 1976. Descriptive evidence in Figure V, following Callaway et al. (2024), supports this assumption. Figure V shows that fertility increases are largest in counties with the highest baseline levels of Medicaid eligibility. Formally, we consider an event-study design for county j in state s and conception year $t = 1971, ..., 1982$ as:

$$\ln(\text{Fertility})_{jst} = \alpha + \sum_{m=1971}^{1982} \beta_m \text{ Hyde Amendment}_m \times \text{Proportion AFDC}_j + \mathbf{X}'_{jt}\gamma + a_j + \eta_{s(j)t} + \psi_{u(j)t} + \epsilon_{jst} \quad (1)$$

where the outcome, $\ln(\text{Fertility})_{jst}$, is the natural log of the fertility rate, the number of conceptions to young women, 29 and under, per 1,000 young women 29 and under in the conception year.[19] In our main results, we show the overall fertility rate and the fertility rate to young women (<30). We focus on young women because they are most likely to be affected by funding restrictions. The majority of unwanted pregnancies occur among women in their 20s (KFF, 2021), and women in their teens and 20s are also the most reliant on public funding for abortion services (see Figure A.4).

The effect of restricted funding for abortion is captured by Hyde Amendment$_m$ $\times$ Proportion AFDC$_j$. Together, the interaction term will capture the effect of the Hyde Amendment based on the proportion of the Medicaid-eligible population in a county. Proportion AFDC$_j$ measures the county-level AFDC recipients in 1976 from the United States Bureau of the Census (2012) relative to the county population. AFDC participation ranges from zero to one, with a zero indicating no AFDC recipients. The full distribution across the U.S. is shown in Figure IV. The effect of Proportion AFDC$_j$ (not interacted with Hyde) is absorbed by the county fixed effects as it is not time varying.

The Hyde Amendment$_m$ represents a set of indicator variables that captures the effect of the Hyde Amendment, which passed in 1976. Though the Amendment passed in 1976, it was only effective briefly. A federal court placed an injunction on the Amendment until August 1977 (Lincoln et al., 1977; CDC, 1977; Cates, 1981; Torres et al., 1986). Hyde then remained in effect until 1980, when it was temporarily lifted while a Supreme Court decision was pending (Lincoln et al., 1977; CDC, 1977; Cates, 1981; Torres et al., 1986). Thus, we anticipate that the Hyde Amendment will only be partially effective in '76/'77 (U.S. District Court for the Eastern District of New York, 1976; Supreme Court of the United States, 1980). That said, there were also strong informational effects. Newspapers in every state across the U.S. publicized the Amendment

[19] The results are similar if we use a Poisson model rather than OLS with the log of the fertility rate (see Figure VIII).

in '76/'77. Due to the complicated timing of the Hyde Amendment, we consider the post-period effects relative to the true pre-Hyde Amendment period, 1975.

a_j represents county fixed effects. η_{st} represent state-by-year (of-conception) fixed effects. These state-by-year fixed effects capture state-level changes, including state-level policies that could differentially affect fertility by state. State-by-year fixed effects allow us to interpret the effect, absent time-varying state-level changes that may affect fertility. Since our primary empirical strategy incorporates state-by-year fixed effects, state-level policy changes are effectively controlled for and do not pose a threat to identification. Only policies that disproportionately affect AFDC recipients will be of concern and could introduce potential bias.[20]

We also include fixed effects for population size by conception year and urbanicity by conception year, denoted as $\psi_{u(j)t}$. These fixed effects are constructed using time-invariant national quartiles of county-level urban share and population interacted with year-of-conception dummies.[21] We include these controls to account for any differential evolution of fertility across counties of varying population size and urbanization (Henshaw et al., 2009). For instance, urban areas might experience more rapid diffusion of social norms or fertility-related technologies, leading to distinct fertility patterns compared to rural areas. Similarly, counties with larger populations may follow different demographic trajectories than those with smaller populations. In the end, our main results remain virtually unchanged when these fixed effects are included or excluded (shown in Figure VI).

$\mathbf{X}_{jt}$ represents the county-year controls. Our main set of controls includes the share white, the share female, and the unemployment rate. We also include additional controls in Figure VIII. Importantly, we add controls for programs such as the Title X clinics studied in Bailey (2012) and the average county-level distance to an abortion provider considered in Kane and Staiger (1996). In the main results, we avoid using controls like income, age shares, or birth characteristics, as they could be "bad controls" in this context (Angrist and Pischke, 2008), which we demonstrate in Sections 6 and 8. Throughout the results, the regressions are weighted by the denominator of the fertility rate. Robust standard errors are clustered at the county level. We also exclusively rely on the canonical two-way fixed effects (TWFE) estimator because there is no staggered treatment timing in our analysis.

[20] Most major policies that could affect fertility either predate Hyde or occurred after. Expansions in contraceptive access—including *Griswold v. Connecticut* (1965) and *Eisenstadt v. Baird* (1972)—took place before our study period (Bailey, 2006, 2010; Myers, 2017b, 2022). Title X clinics were also established between 1964 and 1973 (Bailey, 2012). Abortion access expanded gradually in the late 1960s and early 1970s, culminating in *Roe v. Wade* (1973) (Myers, 2022). Similarly, Great Society programs such as food stamps began in the 1960s (Hoynes et al., 2016). By contrast, policies like parental involvement laws and Medicaid expansions were not implemented until the 1980s, so our state-level analysis focuses on the late 1970s (East et al., 2023; Myers, 2022) (see Section 8).

[21] Similar controls have been used in related empirical work (Bailey and Goodman-Bacon, 2015; Hoehn-Velasco, 2021).

This identification strategy relies on the assumption that, absent the Hyde Amendment, fertility trends would have followed similar trajectories in counties with higher versus lower AFDC participation. Several features of the AFDC program, highlighted in Goodman-Bacon (2018), support the plausibility of this parallel trends assumption. First, AFDC participation rates were relatively stable over time during our period of analysis. Second, long-run variation in AFDC receipt is largely uncorrelated with state-level policies or demographic characteristics (Goodman-Bacon, 2018, 2021). Third, Medicaid eligibility rules remained unchanged during the 1970s, ensuring that our treatment effect is not conflated with contemporaneous eligibility expansions (Klemm, 2000). Finally, by incorporating state-by-year (of conception) fixed effects and leveraging within-state, county-level variation in AFDC participation, our design further mitigates the risk of bias from unobserved, time-varying state-level shocks. In Section 6.1, we confirm that the 1976 fertility break is robust across multiple measures of Medicaid eligibility (alternative AFDC baseline years, proxies based on unmarried mothers, and separate AFDC adult and child participation rates), indicating that the results are not driven by one individual specification or eligibility measure.

In our baseline county-level analysis, we focus on within-state variation and exclude states that saw an increase in Medicaid-funded abortions following the Hyde Amendment (CA, HI, MI, NC, PA, WV, and WA). We omit these states because of both the strong information effects of the Hyde Amendment and the volatility in funding from year to year, shown in Figure II. However, in additional results, we also consider fertility effects in these states, as well as across the average state-level funding declines. This alternative state-level strategy offers a more direct approach to measuring the effects of the Hyde Amendment and tests whether states with larger declines in Medicaid-funded abortions had larger increases in fertility. However, the state-level approach sacrifices variation within state by aggregating variation to the state level.

5 Main Results

5.1 Fertility Rises in Counties with Higher Medicaid Eligibility

Figure VI presents the county-level rise in fertility (based on AFDC participation) following the Hyde Amendment. Panel A plots the impact on births per reproductive-age woman (ages 15–44), while Panel B focuses on women under age 30, who are expected to be most directly affected by the abortion funding restrictions.

Panel A shows a modest overall increase in fertility following the Hyde Amendment, with births rising by 0.9%. The effect becomes more pronounced, beginning in 1978, the first year of full enforcement, and reaches a sustained 1–2% increase in fertil-

ity through 1982. The smaller effects in 1976–77 are consistent with partial enforcement during the federal injunction on the Hyde Amendment (Cates and Rochat, 1976; Cates, 1981). Figure VI Panel B highlights a clearer rise in fertility among younger women, with an average post-period increase of 1.9%. This more pronounced increase in fertility is not surprising, given that women under 30 account for the majority of both births and abortions (KFF, 2021). Younger women are also more likely to use Medicaid to pay for abortion services (see Figure A.4).

The magnitudes in Figure VI are consistent with evidence from the period. Cates (1981) estimated that roughly 5% of the 300,000 Medicaid-funded abortions performed annually prior to the Hyde Amendment were instead carried to term, implying about 15,000 additional births nationwide each year. A later study, Henshaw (1991), reported a larger effect, suggesting that as many as 20% of women denied Medicaid coverage carried pregnancies to term, or 60,000. Our estimates are between these two approximations. Counterfactual estimates based on Figure VI show that births under age 30 rose by 45–216 per county annually (Figure A.10 Panel A), corresponding to a national increase of 32,000–43,000 additional births (Figure A.10 Panel B).

Our findings show key differences from prior work on the Hyde Amendment. The primary distinction lies in the use of within-state comparisons rather than state-level aggregates. Earlier studies found little or no impact (Levine et al., 1996; Haas-Wilson, 1997), but their designs compared states that did and did not fund abortions, often treating states with reduced funding as controls. As shown in Table A.3, the four states with the largest absolute declines in abortions continued to fund abortions but at reduced levels. Our county-level design overcomes this limitation by exploiting differences in AFDC participation within-state, capturing variation in exposure that state-level analyses obscure. Combined with state-by-year fixed effects, our approach isolates the Hyde Amendment's impact from other contemporaneous state policies.[22]

Contextualizing our findings within a broader literature, recent studies offer relevant parallels. Kim et al. (2025) documents that the introduction of Medicaid abortion coverage in Illinois in 2018 led to an 18.2% increase in abortions and a 2.8% decline in births, magnitudes that mirror our historical estimates. Similarly, Dench et al. (2024) reports a 2.3% increase in fertility in states enacting abortion restrictions following the *Dobbs v. Jackson Women's Health Organization* decision. These contemporary estimates reinforce the central insight of our study: reductions in abortion access, particularly for low-income populations reliant on Medicaid, produce measurable increases in fertility. Together, the evidence underscores the continued relevance of the restrictions

[22]Our results also differ from the county-level study by Kane and Staiger (1996), which found little impact. A key distinction lies in our exposure measure: we use AFDC participation, a direct indicator of Medicaid eligibility, whereas Kane and Staiger (1996) relied on poverty rates. While correlated, the share in poverty does not yield the same fertility patterns (see Figure C.1).

implemented through the Hyde Amendment fifty years later.

5.2 Births Shift Toward Unmarried Mothers with Children

We next examine whether the rise in fertility following the Hyde Amendment is concentrated among women most likely to be affected: unmarried mothers with existing children. Unmarried mothers were the primary recipients of AFDC and, therefore, disproportionately Medicaid-eligible. Figure VII Panel A analyzes changes in the share of births by marital status. Figure VII Panel A shows a rise in the share of births to unmarried women, and a decline in the share of married births. The rise in unmarried births is clearest for subsequent births or for women with existing children. These results validate that the effect occurs amongst the target population, as unmarried women with existing children are the most likely group to be Medicaid eligible.[23]

In Figure VII Panel B, there is also a clear compositional shift towards younger *non-white* births, particularly those ages 18–27. The compositional changes in births in Figure VII Panel B help address concerns over contemporaneous legal changes affecting minors' confidential access to abortion. The Supreme Court rulings, *Planned Parenthood v. Danforth (1976)* and *Bellotti v. Baird* (Myers, 2022), might confound the effects we attribute to the Hyde Amendment. But because the rulings applied mainly to minors, we would expect the fertility response to be concentrated among minors. Instead, the compositional changes in Figure B.3 Panel B show a shift towards non-white women in their early 20s. This pattern reinforces that the fertility increases we observe are attributable to funding restrictions under the Hyde Amendment rather than changes in minors' abortion access.

Together, these results indicate that the Hyde Amendment disproportionately increased births to younger, unmarried non-white women with existing children, a compositional change with important implications for family structure and welfare participation, which we explore in Section 8.

6 Robustness Checks and Other Possible Outcomes

6.1 Alternative Measures of Medicaid Eligibility

First, we test alternative measures of Medicaid eligibility in Figure VII Panel C. Panel C.1–C.3 show that the results remain robust when using earlier AFDC participation years (1970, 1972, and 1975), indicating that the observed 1976 fertility break is

[23] Table B.1 Panel B shows an increase in total birth order (Column (3)), along with a decline in the share of first births (Column (2)). Figure B.1 shows compositional changes for married first and subsequent births. Figure B.2 shows the compositional changes in births by marital status and birth order for births of all women of reproductive age (rather than under 30). Figure VII shows the effects by marital status and race. Figure B.4 drops states with large changes in births to married women due to changes in the definition over time on the birth certificate.

not sensitive to the specific AFDC year used. Panels (4)–(6) use an alternative proxy for Medicaid eligibility, the share of deliveries to unmarried mothers with existing children in 1975, 1972, and 1971, who represent the group most likely to receive AFDC and, therefore, Medicaid coverage. These results also show a sharp rise in fertility around 1976, reinforcing that the pattern is not unique to a single eligibility measure. In Appendix Figure C.1 Panels (7)–(8), we also separate 1976 AFDC participation into adults and children, and find that the fertility increase is concentrated among adults, consistent with Medicaid eligibility being determined by the mothers' AFDC status. Panel (9) confirms that poverty rates, though correlated with AFDC participation, do not produce the same fertility break, consistent with Kane and Staiger (1996).

The persistence of the rise in fertility across multiple independent proxies, and the absence of a rise in fertility for less directly related measures such as poverty (see Figure C.1), alleviates concern that AFDC participation merely captures high-fertility areas or general trends. The fact that the fertility change aligns with adult rather than child AFDC participation further supports the interpretation that the increase in fertility reflects Medicaid-linked eligibility rather than underlying demographic shifts.

6.2 Placebo Test with Less Affected States

A second concern is that other federal or nationwide policy changes in the mid-1970s, such as the introduction of the Earned Income Tax Credit or the 1976 minimum wage increase, may have independently changed fertility among low-income households, including AFDC recipients. If these contemporaneous policies were driving our results, we should observe similar fertility breaks in states that continued to fund abortions with state dollars after Hyde. These states were exposed to the same nationwide policy environment but were insulated from the Hyde Amendment's funding restrictions. Figure VII Panel D examines fertility trends for three groups: (1) Arizona, which did not have a state Medicaid program and therefore is completely unaffected, (2) California, which sharply *increased* Medicaid abortion coverage; and (3) all other states that increased state funding for Medicaid abortions after Hyde. Arizona is the only true placebo state, as other states experienced potential informational effects.

These results provide a powerful placebo test. There is no clear increase in fertility in counties with higher AFDC participation in states less affected by Hyde. If national policies targeting AFDC recipients were responsible for the fertility increases we attribute to Hyde, these states should display similar upward shifts in fertility, but there is no post-period rise in fertility.

6.3 Modified Treatments

We test whether our findings are sensitive to alternative treatment definitions, focusing on fertility for women under 30. Figure VIII Panels A.1–A.4 scale the baseline specification by state-level factors: the percent decline in state Medicaid abortions, the 1976 abortion cost-to-AFDC payment ratio, and the 1976 cost of an abortion. In all cases, the estimated post-Hyde increases in fertility closely mirror the baseline. Figure VIII Panel B explores alternative groupings of Medicaid eligibility, retaining the dose-response approximation, except for Panel B.4, which applies a binary treatment. There is a consistent rise in fertility across specifications using percentiles, deciles, or quartiles of AFDC participation, and when collapsed to a binary treatment.[24]

6.4 Additional Checks on the Baseline Specification

Next, Figure VIII Panel C.1 presents Poisson estimates, testing an alternative functional form. Panel C.1 models the fertility rate as the number of births per unit of exposure. The Poisson model yields a clear increase in fertility, with flatter pre-periods preceding the Hyde Amendment. Then, Figure VIII Panel C.2 considers only fertility for those ages 18-29, age 20-29, and ages 21-29, omitting minors. Subsetting to adult women removes those affected by the Supreme Court decision in 1979 that liberalized consent for minors.[25] After removing minors, the plotted points follow a similar pattern to the baseline

Panels C.3 and C.4 add additional controls. Importantly, Panel C.3 adds controls for programs that could directly affect county-level fertility, including the Title X clinics studied in Bailey (2012) and the average county-level distance to an abortion provider considered in Kane and Staiger (1996).[26] Then, Panel C.4 adds controls for age shares (18-19, 20-24, and 25-29), the share of nulliparous deliveries, the share with a high school degree, the share with a college degree, and the average birth order. These analyses show increases in fertility that are similar to our baseline results.

Panel D.1-D.2 shows that the results are robust to alternative methods of handling the standard errors. Panel D.1 clusters the standard errors at the state level to limit spatial correlation across counties. Panel D.2 adjusts the standard errors for spatial and serial correlation using the method described in Hsiang (2010). Then, in Figure VIII

[24] Figure C.2 shows that the majority of the effect is concentrated in the top quartile of AFDC receipt. Panels (1)-(3) highlight a notable increase in fertility for the top quartile relative to the 3rd, 2nd, and 1st quartiles. These results are consistent with the descriptive changes in fertility by quartile and decile shown in Figure V (following Callaway et al., 2024), where fertility changes are largest in counties with the highest eligibility.

[25] This Supreme Court ruling in *Bellotti v. Baird* determined that a Massachusetts law requiring parental consent was unconstitutional (Bellotti v. Baird, 443 U.S. 622, Bellotti v. Baird (1979); Myers (2022)). However, even if minors were included in our main sample, we would expect the effects of this Supreme Court decision to reduce fertility and increase abortions, and therefore weaken any increase in fertility.

[26] We gather this information from Bailey (2012) at the county level. Because the Title X clinic information from Bailey (2012) is not time-varying during our sample period, we interact the presence of a Title X program with year indicators.

Panel D.3, we also present pre-trends based on the methodology outlined by Roth (2022) and Caceres-Bravom (2024). While there is some evidence of pre-trends, they remain outside the confidence intervals of the main estimated treatment effect.

Another potential concern is migration across states. Though, we view migration as unlikely to bias our results. Historical evidence shows Medicaid recipients faced substantial financial and logistical barriers to traveling for abortion care (Cates, 1981; Henshaw, 1991). Residential moves are also rare among low-income households, particularly those tied to state-level benefits. If migration did occur, it would likely attenuate our estimates and reduce the observed fertility increases in high-eligibility counties. In Panel D.4, we consider only births occurring to mothers who still reside in their birth state, eliminating any state-level migration. The fertility response suggests a larger increase than the baseline, implying that any migration biases the effect downward. Next, Panel E restricts the sample to counties near states with different declines in abortion funding. Counties near other states should be similar in observable characteristics. Across counties in close proximity to states with different Medicaid abortion policies, high-AFDC counties show a post-Hyde fertility increase.

Finally, in Figures C.3-C.4, we drop each state individually from the sample to ensure the estimated effect is not driven by a single large state. Figures C.3-C.4 show a similar increase in fertility, even with each individual state removed.[27]

6.5 Permutation Placebo Test

We then conduct a permutation placebo test by randomizing AFDC participation across counties. To generate this placebo test, we randomly assign AFDC participation across counties 2,000 distinct times. Then, following Chetty et al. (2009), Buchmueller et al. (2011), Ohrn (2018) and Baron et al. (2020), we run the baseline regression from Equation 1 with a grouped post period. The plotted cumulative distribution function (CDF) from this exercise is shown in Figure C.5. The non-parametric p-value suggests that our baseline estimate is highly statistically significant.

6.6 Other Outcomes

Controls as Outcomes. Next, Figure C.6 examines several potential controls as outcomes, as the Hyde Amendment may have changed the characteristics of births. In the top panel, we focus on whether the decline in funding for abortions leads to the closure of abortion providers. Since Medicaid funds approximately 25% of abortions (Cates, 1981), a reduction in funding could lower demand, potentially exerting

[27] Virginia is an important state to drop as it experienced changes in geographic boundaries between independent cities and counties in 1980.

pressure on existing clinics. However, the first two panels of Figures C.6 show little change in the number of abortion providers. There is little evidence of an increase in closures or a reduction in the number of providers after the federal funding cuts for abortion services. Then, in the third panel of Figure C.6, we assess whether the Hyde Amendment affects the share of mothers who currently reside outside of their birth state, a proxy for migration. Although there is a linear trend upward, the results do not suggest that the Hyde Amendment itself increased migration.

We then turn to additional characteristics of births, including education, parity, marital status, and race. As indicated in Table B.1, we observe a shift away from births to married mothers and toward higher-order births. This shift aligns with expectations, as the increase in fertility is primarily driven by unmarried women with children, those most likely to be eligible for AFDC/Medicaid.

Mortality Outcomes. Figure D.1 examines mortality effects of the Hyde Amendment, plotting the log of mortality rates for infants, neonates, and children ages 1–4. Infant mortality rates show an increase, but this upward trend predates the Amendment. By contrast, there is little discernible change in neonatal or child mortality. We also investigate maternal outcomes; although the best measure of maternal health, maternal mortality, was relatively low during this period. More than 95% of county-level observations record zero deaths. Instead, aggregating maternal mortality to the state level yields more cases, but event-study estimates following Equation 2 show little change in maternal mortality (Figure D.2), consistent with the infant and child results.

For maternal morbidity, the best available source is the National Hospital Discharge Survey (NHDS). While NHDS provides discharge-level data, it lacks geographic identifiers for our study period. Restricted state-level information only appears in later waves. Using these data, we do find differences in abortion-related complications by marital status and race, but these individual-level comparisons are not geographically based and, therefore, should be interpreted as suggestive rather than definitive.[28]

6.7 State-level Changes in Fertility

Next, we shift the analysis to the state level and examine whether fertility increases more in states with larger declines in Medicaid-funded abortions. We consider the percent decline in Medicaid abortions, shown in Figure IV, which provides a more direct proxy for the effect of the Hyde Amendment than AFDC participation. The percent decline in Medicaid abortions captures the reduction in publicly financed abortions, and in the absence of substitution to other funding sources or better pre-

[28] These results are available on request.

vention of pregnancy, the decline in Medicaid-funded abortions will map one-to-one into higher fertility. Because many women were still able to obtain abortions despite the funding restrictions, the correspondence between the decline in Medicaid-funded abortions and births will not be one-for-one.

While ideally we would conduct this analysis at the county level, to our knowledge, no similar information on Medicaid abortion declines is available at that level of granularity. However, if states with continued funding for abortion experience informational effects from funding restrictions, our estimates will be downward-biased, and the county-level results will be more accurately specified. Formally, the event study for state s and conception year t appears as follows:

$$\ln(\text{Fertility})_{st} = \alpha + \sum_{m=1973}^{1982} \beta_m \, \text{Hyde}_m \times (\% \Downarrow \text{Medicaid Abortions}_s) + \mathbf{X}'_{st}\gamma + a_s + \eta_t + \epsilon_{st} \tag{2}$$

where the outcome variable $\ln(\text{Fertility})_{jst}$ reflects the natural log of the fertility rate. The effect of the Hyde Amendment is captured by Hyde_m. $\% \Downarrow \text{Medicaid Abortions}_s$ is the state-level average percent decline in Medicaid-funded abortions (Figure IV Panel B). The decline in Medicaid-funded abortions represents the decline in abortions paid for by Medicaid. Women still may have received an abortion through other means, including paying for an abortion directly. We compare states with higher percent declines in Medicaid-funded abortions to states with lower declines, based on the average decline from 1976 to 1978-1982. The treatment is measured continuously as the percent decline in Medicaid-funded abortions. States experiencing larger declines receive higher treatment intensity. States that exhibit no decline or that experience an increase in Medicaid-funded abortions effectively receive a zero treatment and therefore serve as control states. Arizona is also a control state because it did not have a Medicaid program until 1982 (Boudreaux et al., 2016).[29] State and year fixed effects, a_s and η_t, are included. Standard errors are clustered at the state level. We consider the years following *Roe v. Wade* in 1973, and before the majority of parental involvement laws were enacted in the 1980s.

6.7.1 Results: State-Level Fertility Increases Among Young Non-White Women

The state-level results using the observed decline in Medicaid abortions as the treatment provide further evidence that the Hyde Amendment led to increases in

[29] In addition to using the average decline, we also show the effect of the time-varying decline in Medicaid abortions in Figure X, the initial funding decline (1976-1978) in Figure E.2, a binary specification capturing the decline in funding in Figure E.3, and based on the share of reproductive-age women receiving AFDC for both non-white (Figure E.4) and white (Figure E.5). Further, Figures E.6 and X treat states with a rise in Medicaid-funded abortions with a negative effect, rather than considering these states as control states with zero treatment.

fertility. Fertility rises most clearly among populations most reliant on Medicaid for abortion access, younger non-white women. Figure IX shows that states experiencing larger reductions in Medicaid-funded abortions show significant increases in births among non-white women, especially those under age 25. Non-white births for those under 25 increase by 2-4%. By contrast, there is no consistent effect for white women.[30]

Figure X explores the time-varying relationship between changes in Medicaid-funded abortions and fertility. Because states display wide year-to-year variation in Medicaid abortions (see Table 1), we exploit the time-varying variation in funding in a difference-in-differences design that considers the effect of the percent change in Medicaid funding for each year relative to 1976.[31] Figure X shows an increase in fertility for those under age 25 using time-varying changes in Medicaid-funded abortions. The results with the time-varying declines mirror the findings using the average decline in Medicaid-funded abortions (Figure IX).

Taken together, the state-level results reinforce the core findings from the county-level analysis presented in Section 5.1. While the county-level analysis leverages variation in Medicaid eligibility within state, the state-level analysis relies on observed declines in Medicaid-funded abortions. In the state-level results, the rise in fertility is concentrated among young, non-white women, aligning with expectations, as younger non-white women are more likely to depend on Medicaid for abortion services (see Figures A.4 and F.2).

7 Mechanisms for the Rise in Fertility

7.1 Georgia's County-level Abortions Decline

To support the rise in fertility observed in high Medicaid-eligibility counties, we turn to county-level legal abortion data from the Vital Statistics of Georgia. Ideally, we would have county-level abortion data for the entire U.S. and produce a comprehensive "first stage." However, county-level abortion data is difficult to obtain, and (to our knowledge) national data is completely unavailable for the period of focus. Instead, we benefit from the fact that Georgia published detailed administrative data on abortions by county and race in its annual vital statistics publications (Georgia Department of Public Health, 1975-1981). This data is reported by residence based on the individual's usual county of residence (Georgia Department of Public Health, 1975, pg 7). We digitize the abortion data and display the results in Figure XI using our baseline

[30] There is also a clear relative rise in non-white fertility, compared to white fertility, after the Hyde Amendment (Figure E.1). The relative rise in fertility occurs most clearly for non-white women under the age of 25. This relative increase in fertility for non-white compared to white women helps to motivate our analysis in Section 8.

[31] If a state has higher Medicaid abortions than in 1976, the annual change will be marked as zero. We focus on the years up to 1980 because the main fertility effect appears before 1981.

county-level specification.

Figure XI reveals a sharp decline in abortions in Georgia in counties with higher AFDC participation. Using the assumption that the average abortion-to-live-birth ratio in 1977 was approximately 300 abortions per 1,000 births, a reduction in abortions by 9.6% would translate into approximately 26 additional births per 1,000 live births (Center for Disease Control, 1979). This would correspond to a 2.5% increase in fertility, very similar to the point estimate in Figure XI Panel B, and slightly higher than the 1-2% increase in fertility observed in our main results. Georgia's decline in abortions in counties with higher AFDC participation supports the hypothesis that the Hyde Amendment's funding restrictions contributed to an increase in births in counties with higher Medicaid eligibility. The decline in abortions in counties with higher Medicaid eligibility in Georgia adds credibility to the fertility results and suggests that similar mechanisms are at play nationwide.

7.2 Medicaid Births Increase and Abortions Decline in the NSFG

We also verify the correspondence between the increase in births and decline in abortions using alternative survey data, the 1982 National Survey of Family Growth (NSFG, Cycle III) (NCHS, 2010). The NSFG is a nationally representative survey of the U.S. population of women ages 15–44. The survey collects fertility histories, including detailed contraceptive use, fertility intentions, demographic characteristics, and retrospective pregnancy histories. The pregnancy histories allow us to estimate whether the share of abortion, live births, or miscarriage based on the pregnancy year, changes after the Hyde Amendment. Our identification strategy leverages variation across Medicaid coverage groups and time, comparing outcomes for women covered by Medicaid after the Hyde Amendment against pregnancy outcomes for women who were not covered by Medicaid. Formally, we estimate the following specification:

$$\text{Outcome}_{it} = \alpha + \beta\,(\text{Medicaid}_i \times \text{Post-Hyde}_t) + \mathbf{X}_i'\gamma + 1(\text{Medicaid})_i + \eta_t + \epsilon_{it}, \quad (3)$$

where Outcome_{ijt} denotes the pregnancy outcome for woman i in the year the pregnancy ended t. Pregnancy outcomes can include birth, miscarriage, stillbirth, or abortion. The variable $1(\text{Medicaid})_i$ is an indicator for whether woman i reported Medicaid coverage. Post Hyde_t equals one for pregnancies that ended after 1976. The coefficient of interest, β, captures the differential change in pregnancy outcomes for Medicaid recipients after the Hyde Amendment.[32] The effect is relative to non-Medicaid women and relative to the pre-Hyde period. $\mathbf{X}_i$ controls for individual characteristics, including age during pregnancy and the race of the mother (indicators for white

[32]Unfortunately, Medicaid receipt is only available at the time of the survey rather than at the time of the pregnancy.

and black). Specifications also include Medicaid indicators, 1(Medicaid)$_i$, to account for differences in baseline pregnancy outcomes between coverage groups, as well as pregnancy-year fixed effects, η_t. Regressions are weighted by the individual weight provided in the NFGS. Standard errors are clustered at the individual level.

7.3 Results: Live Births Increase and Abortions Decline

Table 2 presents the pregnancy outcomes before and after the Hyde Amendment for those receiving Medicaid. The estimates reveal a clear shift towards live births and away from abortions for those receiving Medicaid following the implementation of the Hyde Amendment, but no similar change in miscarriage or stillbirths after Hyde.

Beginning with abortions in Columns (1)-(4), the Hyde Amendment appears to reduce abortions as a pregnancy outcome for Medicaid recipients. The coefficients suggest a 7–8 percentage point reduction in abortions for Medicaid recipients after the Hyde Amendment. These declines are evident both when restricting the sample to women under age 30 and in the full sample. Consistent with the reduction in abortions, the estimates indicate a statistically significant increase in live births by 9–11 percentage points. This pattern suggests that the reduction in abortions among Medicaid-eligible women translates directly into an increase in live births.

By contrast, the estimates for miscarriage are small, negative, and statistically insignificant. This serves as a falsification check, supporting the interpretation that the Hyde Amendment's impact was concentrated on abortion and live birth outcomes rather than reflecting spurious changes, reporting differences, or broader shifts in pregnancy outcomes. Overall, the results indicate that the Hyde Amendment's funding restrictions meaningfully reduced abortions and produced a corresponding rise in live births for Medicaid recipients.

8 Welfare Receipt Rises for Young Non-white Women

Next, we consider whether the Hyde Amendment forces more women to rely on welfare support using the CPS (Flood et al., 2024). In this specification, we use an event study that exploits two sources of variation. First, we capture the observed decline in Medicaid funding for abortion at the state level, using the percent decline in Medicaid-funded abortions (Figure IV, Panel B). States with higher percent reductions in Medicaid-funded abortions have larger increases in fertility, particularly for non-white women (Figure IX). Descriptive evidence in Figure F.1 supports the underlying assumption: increases in AFDC participation are larger in states with larger declines in Medicaid abortions (for non-white women).

Second, because non-white fertility increases by more than white fertility at the state level, we also compare non-white women to white women within states with higher percent declines in Medicaid-funded abortions. Several factors motivate the comparison by race. Figure F.2 shows that non-white women are more likely to be eligible for Medicaid and more likely to pay for abortions through Medicaid as compared to white women. Almost 50% of abortions to single non-white women are paid for by Medicaid, even after the Hyde Amendment. Further, half of non-white women receive welfare before the Hyde Amendment (Figure F.2), which is categorically linked to Medicaid during the study timeframe. Similar studies have relied on the fact that Black women are more affected by abortion restrictions than white women and performed a comparable triple difference design by race (Gardner, 2024). Finally, states with larger reductions in Medicaid abortions show the clearest increases in fertility for non-white women (Figure X). Formally, our revised event study for individual i in state s and year t appears as:

$$\begin{aligned}\text{Welfare}_{ist} = \alpha + \sum_{m=1974}^{1980} \beta_m \text{ Hyde}_m \times (\% \Downarrow \text{ Medicaid Abortions}_s) \times 1(\text{Non-white})_i + \delta_1 1(\text{Non-white})_i \\ + \delta_2 (\% \Downarrow \text{ Medicaid Abortions}_s) \times 1(\text{Post})_t + \delta_3 \text{ Non-white}_i \times 1(\text{Post})_t \\ + \delta_4 (\% \Downarrow \text{ Medicaid Abortions}_s) \times 1(\text{Non-white})_i + \mathbf{X}_{it}'\gamma + a_s + \phi_t + \epsilon_{ist}\end{aligned} \tag{4}$$

where Welfare_{ist} represents whether or not an individual is receiving any welfare payment.[33] We focus primarily on welfare participation as our main outcome. In additional analyses, we also examine household income, employment, and the number of children in the household, helping to validate our fertility results from the birth certificate data. The effect of the Hyde Amendment is captured by $\text{Hyde Amendment}_m \times (\% \Downarrow \text{ Medicaid Abortions}_s) \times 1(\text{Non-white})_i$. To match the conception results, where we consider conceptions beginning in 1976, we consider the effect on economic outcomes beginning in 1977, to correspond with the timing of the birth. In the present specification, 1976 represents the omitted period. $\%$ Decline Medicaid Abortions_s is the state-level average percent decline in Medicaid abortions, used in previous state-level analyses (Figure IV). $1(\text{Non-white})_i$ captures whether the individual is non-white.[34] Though we show the time-varying declines by race (separately) in Figure X.

$\mathbf{X}_{it}'$ includes controls, which include the age of the woman. a_s is the state fixed

[33] Welfare is based on a binary variable that captures any welfare receipt based on the CPS definition: "INCWELFR indicates how much pre-tax income (if any) the respondent received during the previous calendar year from various public assistance programs commonly referred to as 'welfare.'" See https://cps.ipums.org/cps-action/variables/INCWELFR#description_section. For comparability across years, welfare is combined with SSI receipt because the two variables were split in the CPS in 1976. Few individuals in the sample receive SSI. Table F.1 shows descriptive statistics on welfare assistance, family and household structure, and employment in the CPS.

[34] We also include the other terms necessary for a difference-in-difference-in-differences. $1(\text{Non-white})_i \times 1(\text{Post})_t$ captures the effect of being non-white in the post period (1977 and onward). $\%$ Decline Medicaid $\text{Abortions}_s \times 1(\text{Post})_t$ captures the effect of declines in Medicaid abortions in the post period (1977 and onward). $\%$ Decline Medicaid $\text{Abortions}_s \times 1(\text{Non-white})_i$ captures the effect of declines in Medicaid abortions while being non-white.

effects and ϕ_t is the time fixed effect.[35] ϵ_{ist} is the regression error term. Robust standard errors are clustered at the state level. As in the fertility results, we focus on women under 30 for the main results. We weight the regressions by the household weights provided in the CPS.

8.1 Results: More Non-white Women Receive Welfare

Figure XII.A shows the effect of the Hyde Amendment on welfare receipt. Figure XII.A shows that welfare participation rises among non-white women in states with larger declines in Medicaid-funded abortions. In states with a 100% decline in Medicaid abortions, welfare participation increases by 3.0 percentage points for non-white relative to white women.[36] Then, Panel B illustrates that restricting the sample to non-white households under age 30 yields similar increases in welfare participation to non-white women in Panel A. Whether focusing on women or households, the pattern is consistent: non-white welfare receipt rises after the Hyde Amendment in states that experienced larger cuts to Medicaid abortions.

The results are similar if we consider difference-in-differences point estimates based on the time-varying changes in Medicaid abortions in Figure X. The point estimates suggest that welfare claims increase the most for non-white women aged 18–24. These percentage-point effects translate into substantial changes in the number of women receiving AFDC, an increase in welfare participation by 38,000 women under 30 (see Figure F.6).

The increase in welfare receipt is unlikely to reflect large numbers of new families suddenly entering AFDC, since eligibility already required dependent children, and participation was concentrated among low-income households. A more plausible interpretation is that restrictions on abortion access raised fertility within families already close to or participating in AFDC. Additional births could extend the length of welfare spells, increase benefit amounts through more dependents, and delay program exit. Consistent with this interpretation, Figure XIII shows that welfare income among recipients rose significantly after Hyde, accompanied by modest declines in employment. Importantly, other government transfers remain flat, serving as a falsification

[35] A limitation of the CPS data is that, at the beginning of the sample, only large states are available. During the study timeframe, smaller states are grouped together. While the groupings also change over time, we begin the analysis in 1974 to avoid changes in the grouping. For grouped states, we take the average decline in Medicaid abortions, and we regroup states consistently together over time. We also drop any grouped states as a robustness check. Similar to the state-level fertility results, we conclude the analysis in 1980. This avoids the beginning of a series of major welfare reforms in 1981 (Ferejohn, 1991), changes in parental involvement laws, and major Medicaid expansions (East et al., 2023).

[36] Figure F.3 shows a series of robustness checks. These include excluding grouped states, dropping minors, focusing on those under 25, restricting to single non-white women (the group most likely to qualify for AFDC), and removing weights. We also replicate the results with binary treatment indicators, exclude states one at a time, and examine annual variation in Medicaid abortion funding. The rise in welfare participation is clearest for single non-white women aged 18–24, aligning with the fertility effects.

test reinforcing the specific rise in AFDC participation.[37]

Panel B reveals parallel changes in fertility: a higher probability of having children in the household, an increase in the overall number of children, and a shift toward three children. These changes align with AFDC's eligibility rules, since additional dependents increase payments and extend program duration. While we cannot directly observe spell length in the CPS, the combination of higher welfare payments, reduced employment, and larger family size is consistent with deeper reliance among families already connected to AFDC.

Taken together, these findings indicate that the Hyde Amendment exacerbated economic inequality, particularly among younger non-white women and households. Restrictions on abortion funding raised fertility rates and increased reliance on AFDC. Our results mirror evidence from the Turnaway Study, which shows that women denied abortions were less likely to be employed and more likely to rely on welfare (Foster et al., 2022). Miller et al. (2020, 2023) also document increased financial hardship following abortion denial.

These findings also suggest that the Hyde Amendment reversed some of the economic gains achieved through abortion legalization. Whereas Gruber et al. (1999) found that abortion legalization improved economic outcomes and reduced welfare participation, the opposite trend emerges just four years later.

9 Second Generation Effects: More Non-white Women Receive Public Assistance

Finally, we conclude by examining the long-term second-generation effects of the Hyde Amendment. In this specification, we focus on women born during the period 1974-1980 in the American Community Survey (ACS, Ruggles et al., 2024) in 2000-2019. We focus on girls born around the Hyde Amendment as adults because women are more likely to be eligible for public assistance through programs like welfare and Medicaid. Though we also show the effects for men in the appendix (see Figure G.4).[38]

The ACS has advantages over the CPS, including more observations and the fact that all states are documented separately. The ACS also includes information on Medicaid and public insurance participation (from 2008 onward). One disadvantage of the

[37] The CPS includes non-welfare/social security government assistance; see https://cps.ipums.org/cps-action/variables/INCGOV#description_section for more information.

[38] An important outcome for men is incarceration, which we proxy using the IPUMS "group quarters" variable and captures individuals living in institutional settings (correctional facilities and mental institutions). We find no significant change in group quarters residency, our best available proxy for incarceration, suggesting that the Hyde Amendment did not have a clear effect on criminal involvement later in life (Donohue and Levitt, 2001). Across other outcomes, men show minimal changes, with the exception of a modest decline in college degree attainment.

ACS is that we are unable to isolate children of young women or Medicaid recipients around the Hyde Amendment. So these results are more general than the CPS results.

To examine outcomes in the ACS, we follow a similar empirical strategy to Equation 4, except that we account for both the birth state and year, in addition to the contemporary state and year. Formally, our revised event study for individual i in year t and state s who was born in year y and state of birth b appears as follows:

$$\begin{aligned}\text{Welfare}_{itsyb} = \alpha + \sum_{m=1974}^{1980} \beta_m \text{Hyde}_m \times (\% \Downarrow \text{Medicaid Abortions}_b) \times 1(\text{Non-white})_i \\ + \delta_1\, 1(\text{Non-white})_i + \delta_2\, (\% \Downarrow \text{Medicaid Abortions}_b) \times 1(\text{Post})_y \\ + \delta_3\, (\% \Downarrow \text{Medicaid Abortions}_b) \times 1(\text{Non-white})_i \\ + \delta_4 1(\text{Non-white})_i \times 1(\text{Post})_y + \mathbf{X}'_{ist}\gamma + a_s + \eta_{st} + a_y + \pi_b + \epsilon_{itsyb}\end{aligned} \tag{5}$$

where the majority of Equation 5 reflects Equation 4, except the following modifications. First, the Hyde Amendment timing is based on the birth year y and birth state b rather than the current time and place. We thus include current state-by-year fixed effects, η_{st}, to absorb recent competing state-level policies that may affect public assistance. We also include fixed effects for the birth year a_y and birth state π_b. Robust standard errors are clustered at the birth state level. We also collapse the data to the current state, birth state, birth year, current year, and mother's birth state and birth year level for computational ease. But the individual-level point estimates are shown in Figure X, when we consider time-varying changes in Medicaid abortions. We consider birth years 1974 to 1980 and women under age 45.[39]

9.1 Second-Generation Effects: Public Assistance Rises

Figure XIV demonstrates that, beyond its immediate impact on fertility and welfare receipt, the Hyde Amendment also generates second-generation consequences. Figure XIV illustrates the long-run second-generation effects by examining the differential impact of the Hyde Amendment on public assistance among the children of women affected by Hyde. These girls are observed as adult women in the ACS from 2000 to 2019. The event studies reveal a notable increase in public assistance participation, particularly in the areas of public insurance and Medicaid enrollment. Public assistance participation grows by 2.0 percentage points, while public insurance participation grows by 1.8 percentage points.[40,41]

We also find consistent evidence of increased public program participation when

[39] In the ACS, "welfare" is defined slightly differently than in the CPS. The variable INCWELFR measures pre-tax income received in the past year from public assistance programs. See https://usa.ipums.org/usa-action/variables/incwelfr#description_section

[40] Insurance variables in the ACS are only available after 2007, losing observations in the bottom two panels of Figure XIV.

[41] Table G.1 shows descriptive statistics for the ACS sample in the treated states.

focusing on non-white women alone, rather than comparing them to white women (Figures G.1 and G.2). The DiD event studies indicate a 1.5 percentage point rise in public insurance enrollment, driven largely by Medicaid, and some evidence of increased welfare participation among non-white women in states with larger declines in Medicaid-funded abortions. These figures also show a higher likelihood of non-white women being unmarried with children (Figure G.2).[42]

Finally, Figure X reports public assistance outcomes and DiD estimates using the time-varying decline in Medicaid-funded abortions. Panel C indicates that non-white women experienced significant increases in welfare receipt and public insurance coverage. The annual estimates for non-white women imply that roughly 35,000 additional second-generation women rely on public assistance. 21,000 additional women rely on public insurance (Panel B of Figure F.6).

These second-generation effects establish that the Hyde Amendment perpetuates economic disadvantage across generations. By restricting access to abortion, the Amendment increases the likelihood that children would be born into lower-income households already reliant on public assistance, conditions that frequently persist into adulthood. These long-term impacts highlight that many of the children born in the years surrounding the Hyde Amendment restrictions now face the same economic vulnerabilities as adults. The effects of the Hyde Amendment extend beyond the original generation directly targeted.

The second-generation impacts add to a broad literature showing how public policy shapes children's long-run outcomes (Sommers and Oellerich, 2013; Chetty et al., 2016; Hoynes et al., 2016; Aizer et al., 2016; Goodman-Bacon, 2018, 2021; Hoehn-Velasco, 2021; East et al., 2023; Lahey and Wanamaker, 2025). Research on abortion legalization has shown that access to abortion improves the long-term well-being of children (Gruber et al., 1999; Ananat et al., 2009; Farin, 2024), while expansions of public insurance in childhood have been linked to gains in health and educational attainment (Brown et al., 2015; Goodman-Bacon, 2018, 2021; East et al., 2023). Our results reveal the reverse dynamic: restrictions on abortion funding not only constrain the lives of the first generation but also alter the economic trajectories of affected children as adults. Taken together, these results indicate that the policy-induced changes in fertility among non-white Medicaid-eligible women created additional demands for public assistance, underscoring how the abortion funding restrictions shaped subsequent economic need.

[42] Figure G.1 provides robustness checks across the public assistance outcomes, including considering the initial drop in funding, a binary indicator for any drop in funding, dropping movers, and alternative ways of collapsing the data. Figure G.3 shows event studies for other outcomes of the second generation, including food stamps receipt, family structure, educational attainment, and employment. Overall, we do not see evidence of any changes in these outcomes for the second generation as a result of Hyde.

10 Conclusion

In this paper, we demonstrate that the Hyde Amendment raised fertility rates in counties with higher Medicaid eligibility. Our county-level analysis reveals that, on average, fertility increases by 2% for young adult women and 1% overall, leading to 40,000 additional births nationally. Complementary evidence from survey data and administrative records confirms a corresponding decline in abortions, underscoring the direct link between reduced public funding for abortion and higher fertility.

These fertility responses are not merely demographic shifts; they carry economic consequences. We show that the additional births induced by Hyde translate into greater reliance on welfare programs, particularly among non-white women, who are more likely to depend on Medicaid. The rise in public assistance use is accompanied by larger household size and greater post-Hyde AFDC participation, illustrating how targeted funding cuts to one program can increase demand for others. Following affected cohorts into adulthood, we further demonstrate that daughters born as a result of Hyde's restrictions are more likely to rely on public assistance themselves (especially public insurance), revealing an intergenerational transmission of disadvantage.

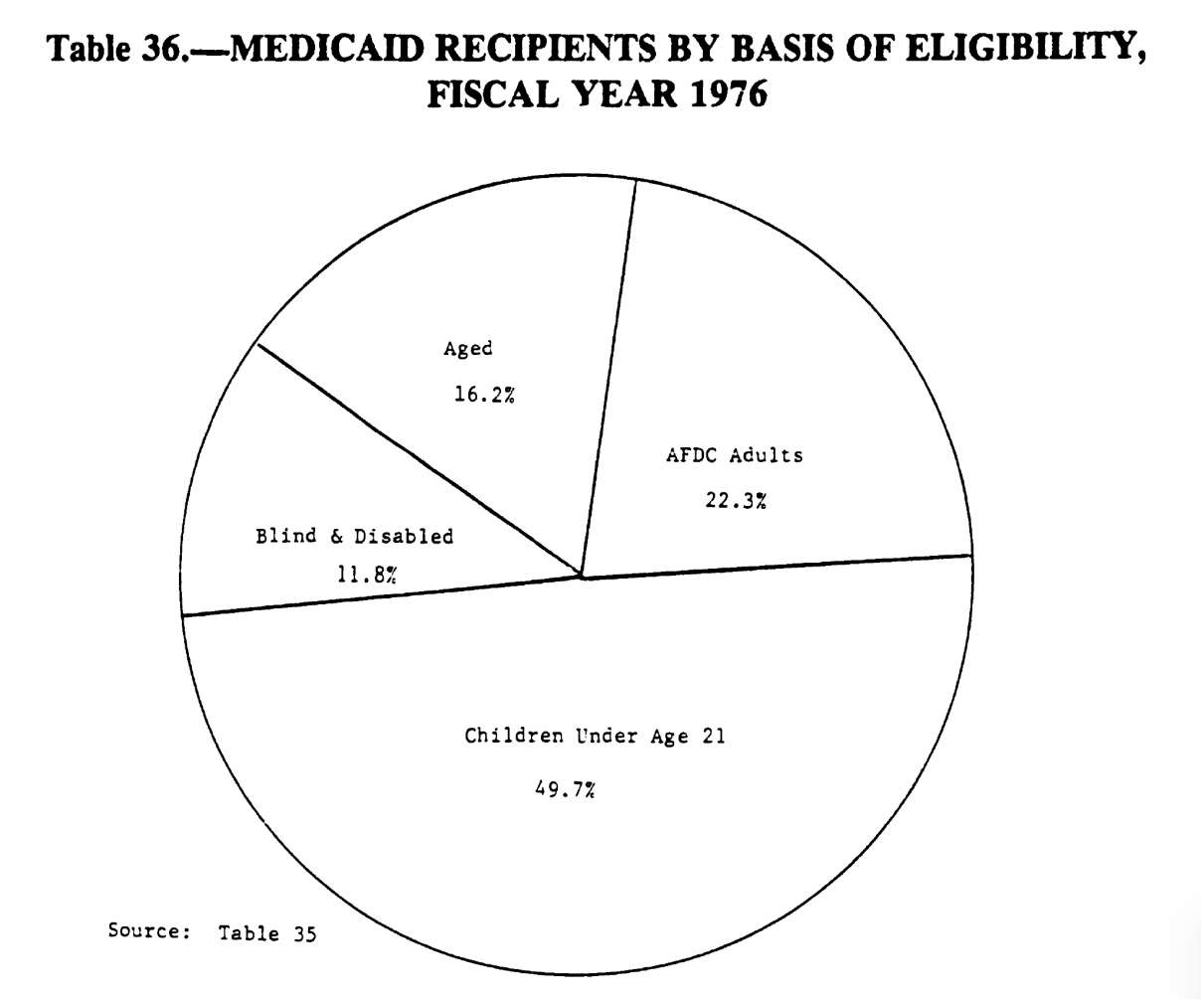
These findings reveal the significant and unintended economic consequences of abortion funding restrictions. By limiting funding for abortion and, subsequently, its access to low-income women, the Hyde Amendment increases household size and reliance on public assistance programs. This shift redirects costs from a one-time healthcare service to long-term welfare support, showing how targeted funding cuts can reshape the structure of public spending. Quantifying the additional public assistance payments, our estimates suggest that families would require an additional $112 million in AFDC payments ($595 million in 2025).[43] The second generation increases in public insurance correspond to additional assistance ranging from $126 to $168 million per year.[44] The scale of the effects shows the considerable additional need for public support created by the Hyde Amendment. The trade-off between restricting abortion and higher demand for public assistance highlights the potentially overlooked economic implications of such policies. The seemingly narrow funding restriction has ripple effects across the safety net and reinforces inequality in ways policymakers may fail to anticipate.

[43] Assuming that the *average monthly AFDC payment per family* in 1977 was $246, or about $2,955 annually per family (1977 dollars; Social Security Administration 2005).

[44] Approximately 21,000 additional adult women rely on public insurance, primarily Medicaid. These estimates are calculated assuming average annual per-enrollee expenditures of $6,000–$8,000 (KFF, 2024b; MACPAC, 2022).

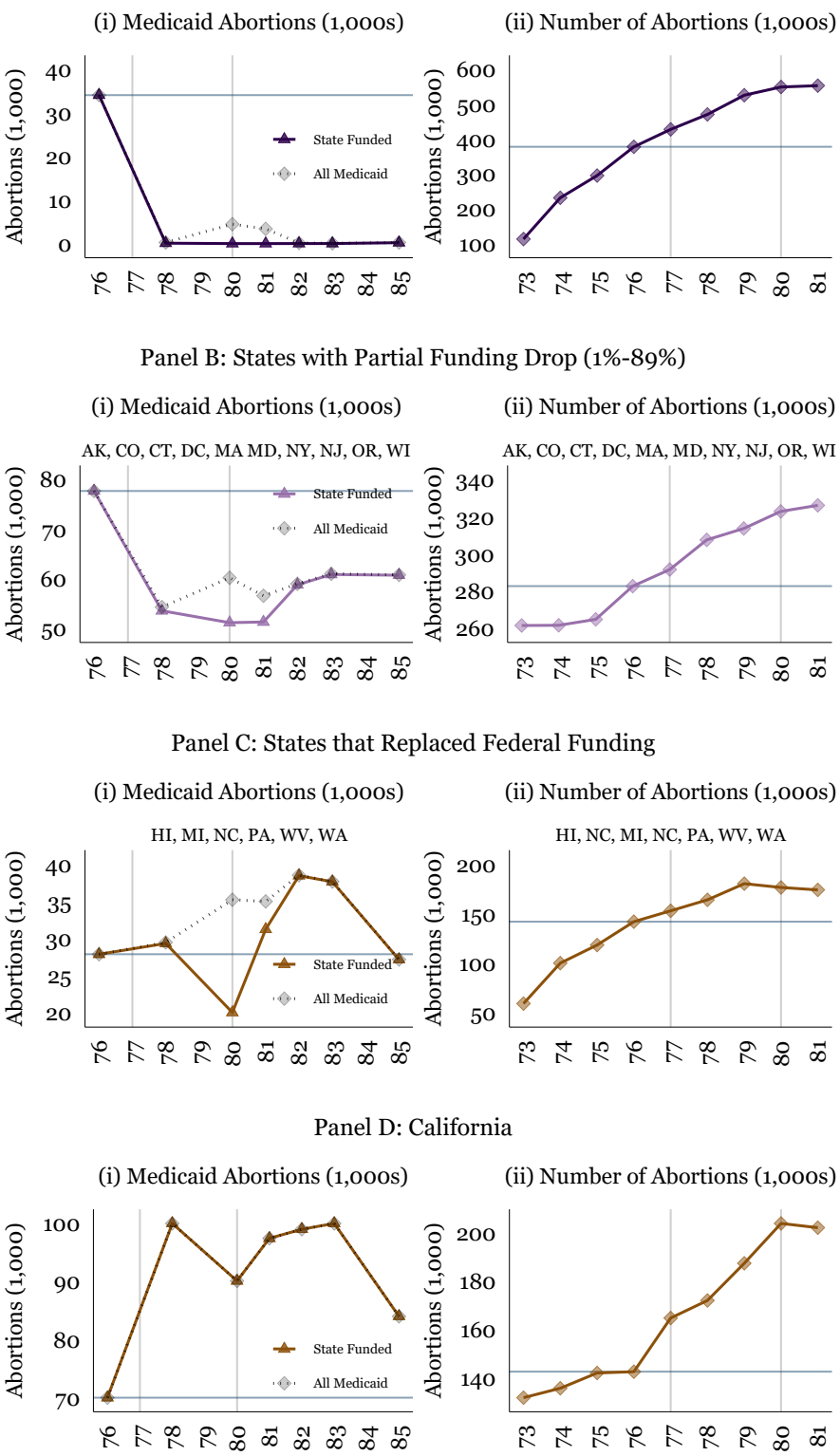
11 Figures

Figure I: Medicaid Eligibility



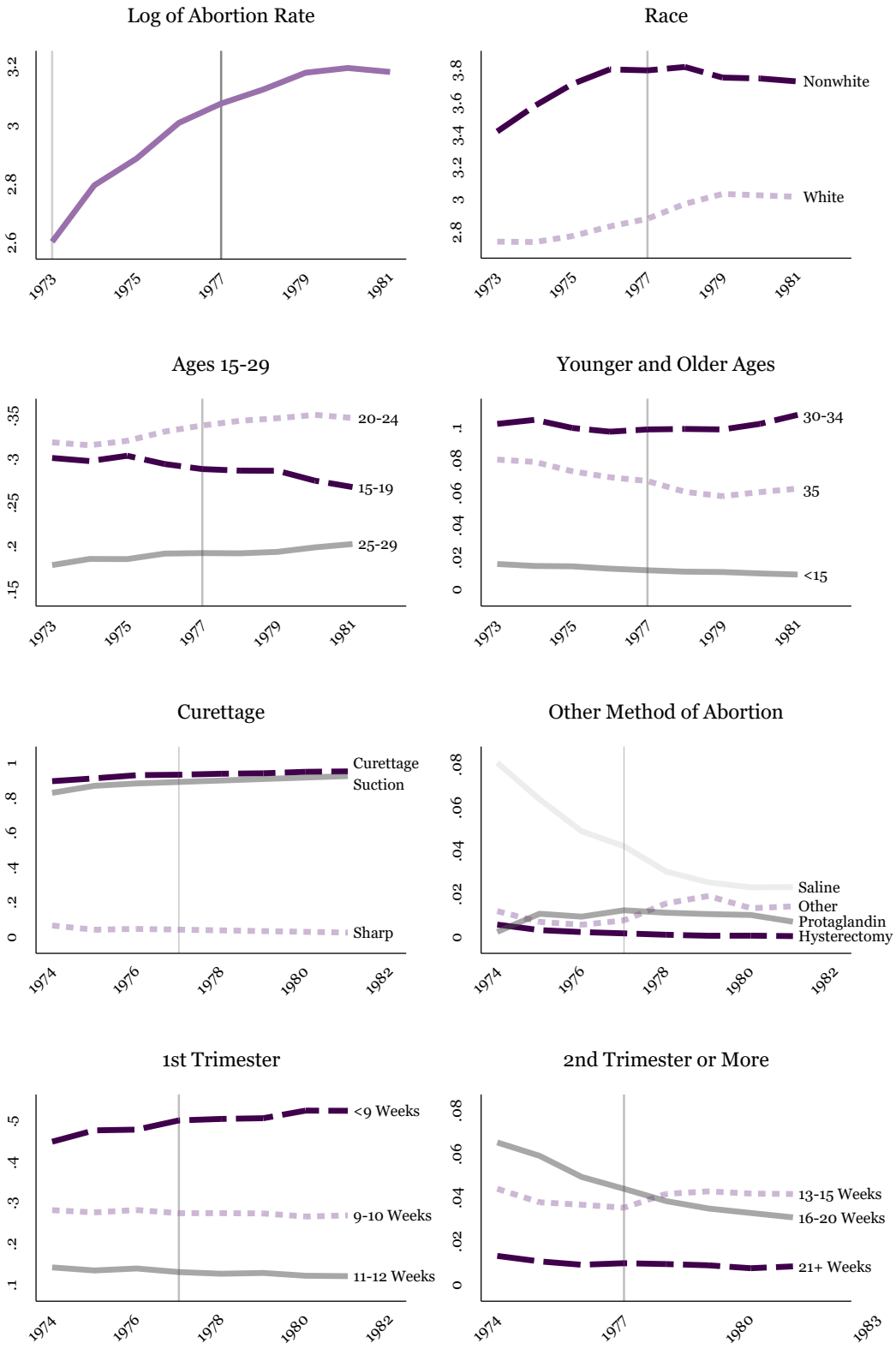
SOURCE: Medicaid/Medicare Management Institute (1979) Page 65.

Figure II: State Medicaid Abortions



SOURCES: Lincoln et al. (1977), Gold (1980), Gold (1982), Nestor and Gold (1984), Gold and Nestor (1985), Gold and Macias (1986). Abortion data from the CDC Abortion Surveillance Program (CDC, 1977).
NOTES: The left side of the graph shows the number of Medicaid abortions in 1,000s, both the total Medicaid abortions (federal + state-funded abortions, gray diamonds) and state-funded abortions (colored triangle/solid line). Only states with a balanced panel are reported. Thus, the sum of the panels of Medicaid abortions will not necessarily reflect the total number of *national* Medicaid abortions due to missing state information. The right graph, Panel (ii), shows all abortions in 1,000s. Panel A shows states with very limited funding for Medicaid abortions, most states, or those not included in Panels B-D. Note that the states reported in (i) of Panel A are not the complete set of states reported in (ii). Only certain states have complete Medicaid funding available, as shown in the state-by-state results (Figure A.5). California is shown separately because it is an outlier for the number of Medicaid-funded abortions performed after the Hyde Amendment. The horizontal blue line shows the starting number of abortions in 6. See Figure A.5, Figure A.6, and Figure A.8 for the individual state-level graphs.

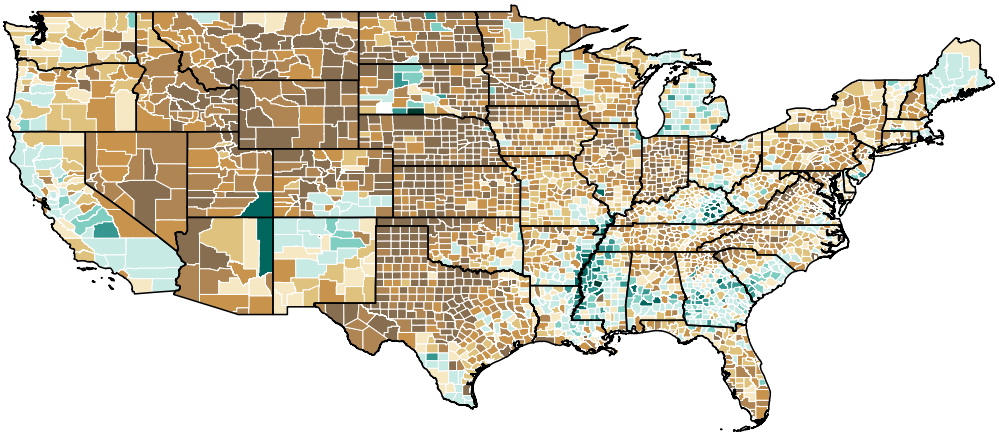
Figure III: Abortion Rate and Share of Abortions by Characteristic



SOURCES: Abortion data from the CDC Abortion Surveillance Program (CDC, 1977).
NOTES: The top two graphs show the abortion rate. For the remaining panels, the graphs show the share of abortions by each characteristic for the balanced panel of states. Data for suction and sharp curettage is missing between 1977 and 1980; however, this data is available prior to 1977 and in 1981.

Figure IV: Map of Main Treatment Variables

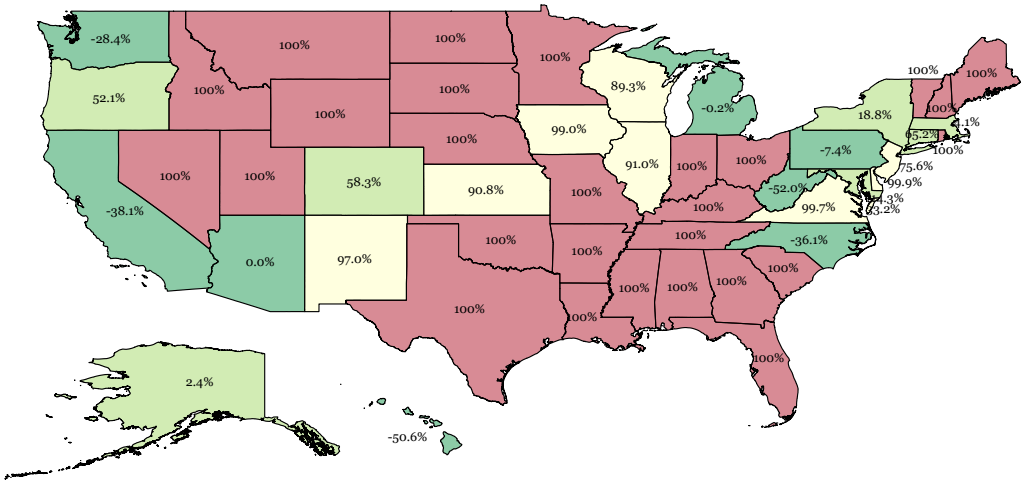
Panel A: County-level Medicaid Eligibility: Percent AFDC



Percent AFDC Participants, 1976

(20,30] (15,20] (10,15] (8,10] (5,8] (4,5] (3,4] (2,3] (1,2] [0,1] No data

Panel B: State-Level Percent Decline in State Medicaid Abortions



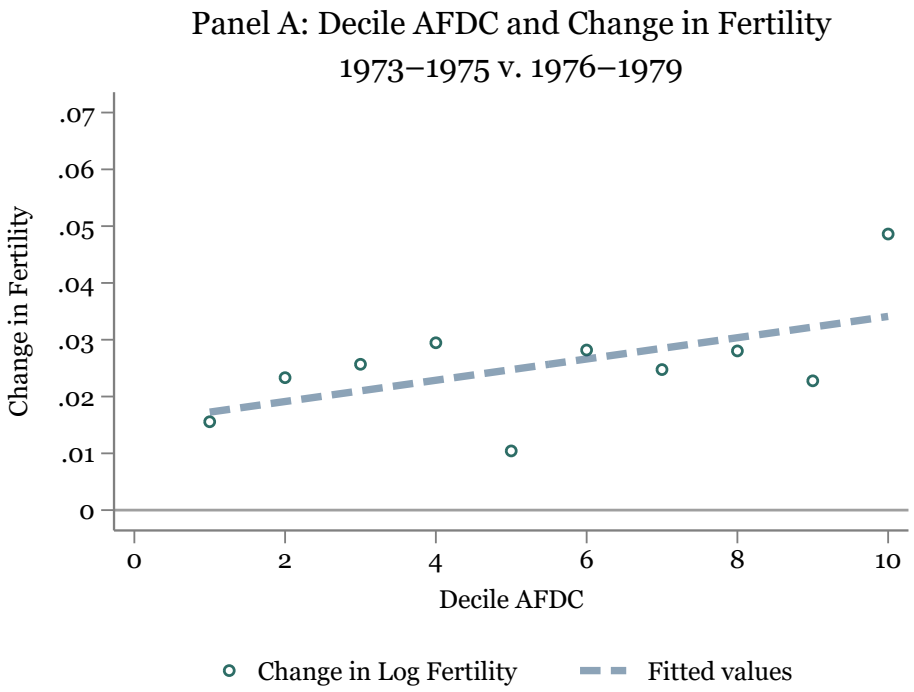
% Decline in Medicaid Abortions

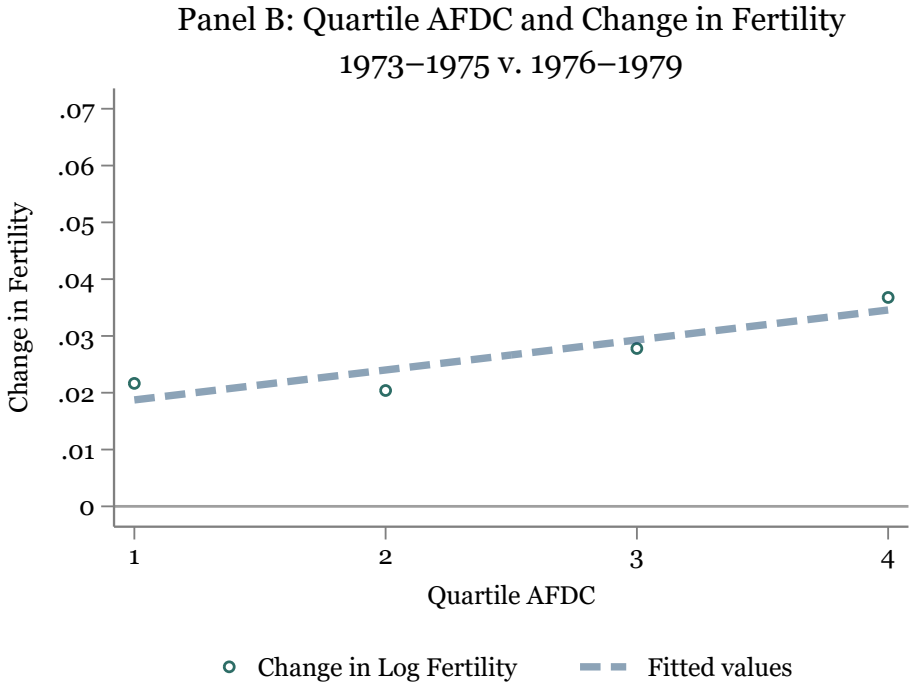
(100,100] (75,100] (0,75] [-100,0]

SOURCES: Lincoln et al. (1977), Gold (1980), Gold (1982), Nestor and Gold (1984), Gold and Nestor (1985), Gold and Macias (1986), Haines (2010), and United States Bureau of the Census (2012).

NOTES: The top map shows the percent of residents receiving AFDC at the county level, our main source of variation in the fertility results. In the event study, these percentages (0 to 100) are described as proportions (0 to 1). The bottom map shows the percentage decline in state Medicaid abortions, or the average decline from 1976 over 1978-1982. Panel B is our main source of variation in state-level specifications.

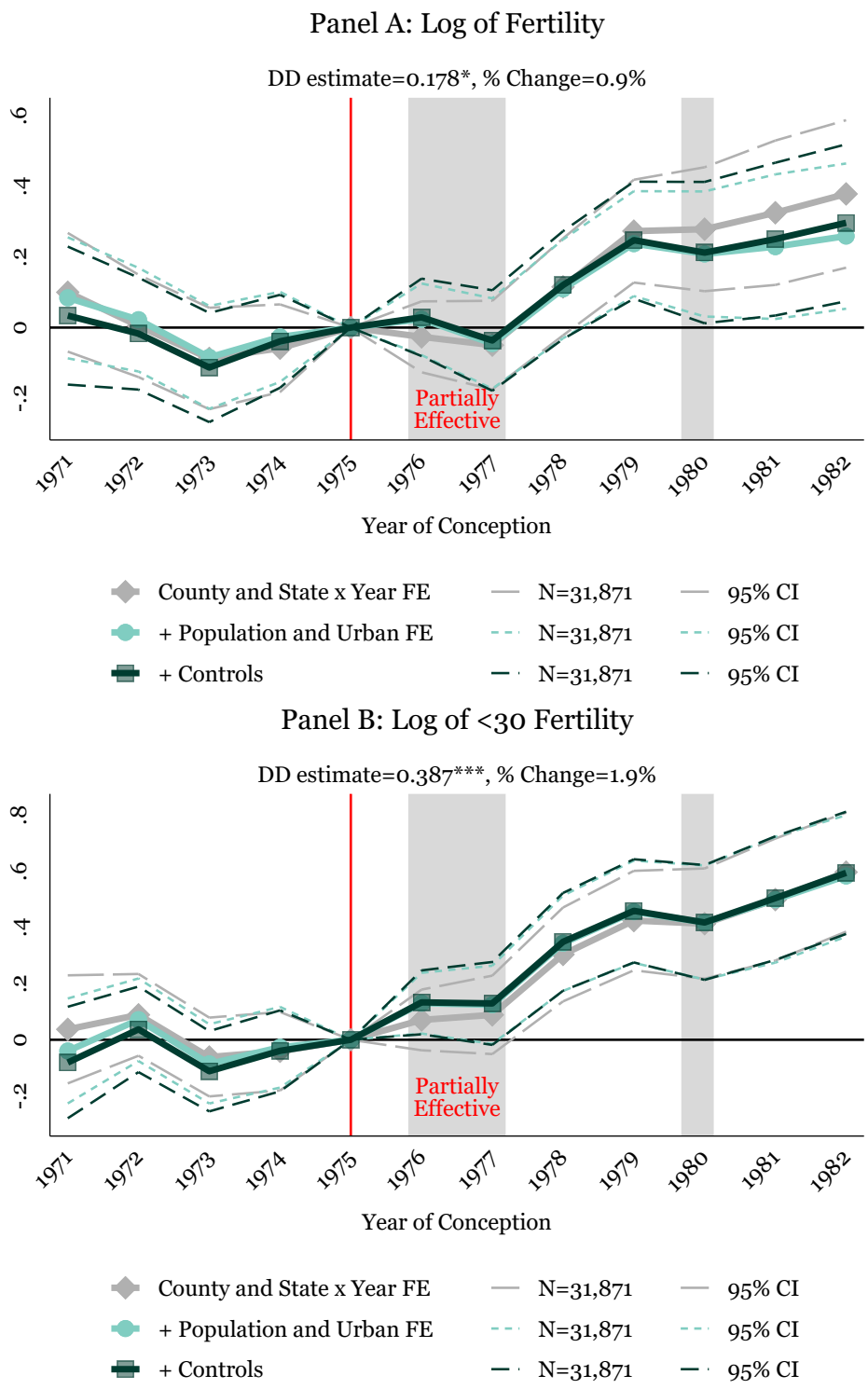
Figure V: Change in Fertility for Women <30 by AFDC Receipt





NOTES: Shows the descriptive change in fertility from 1973-75 to 1976-79 by two measures of Medicaid eligibility–the decile of AFDC receipt and the quartile of AFDC eligibility. Attempted depiction of Figure IV from Callaway et al. (2024).

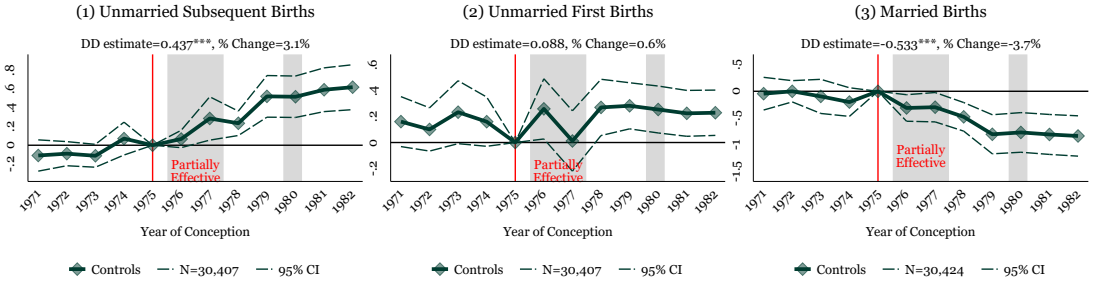
Figure VI: The Hyde Amendment and County-level Fertility
Hyde Amendment x Time x Proportion Medicaid Eligibility



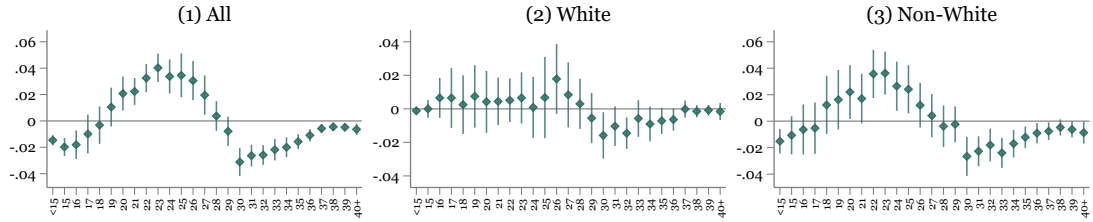
NOTES AND SOURCES: Vital Statistics Natality Birth Data (Birth Certificate Records) from the National Vital Statistics System of the National Center for Health Statistics. SEER Population data and Haines (2010) used for the controls and the population counts. Medicaid eligibility determined by the share of the population in the county receiving AFDC in 1976 (United States Bureau of the Census, 2012). Displays OLS coefficients. The outcome is the log of the fertility rate. In Panel A, the fertility rate is the number of births per 1,000 reproductive-age women 15-44. In Panel B, the fertility rate is the number of births to women under 30 per 1,000 women under 30. Regressions are weighted by the denominator of the rate. Baseline fixed effects include county fixed effects and state-by-year fixed effects (gray diamonds). Green (circles/squares) also include the quartile of the share urban-by-year fixed effects, and quartile of the population-size-by-year fixed effects. Plotted coefficients are dummy variables on each year before and after the Hyde amendment went into effect, interacted with the share AFDC in a county (see Equation 1). The period just before the legal change is the excluded period–indicated by the vertical line. States that **increased** their funding for Medicaid abortions excluded from the county-level specification (CA, HI, MI, NC, PA, WV, WA). Arizona, which did not have a Medicaid program until 1982, is also excluded. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the county level. The grouped post period, or DD estimate, represents the effect of funding restrictions on abortion from the Hyde Amendment interacted with the share of AFDC recipients in a county. Percentage effect calculated using the point estimate at the average county-level AFDC participation. Our main set of controls includes the share white, the share female, and the unemployment rate.

Figure VII: The Hyde Amendment and Fertility, Additional Results

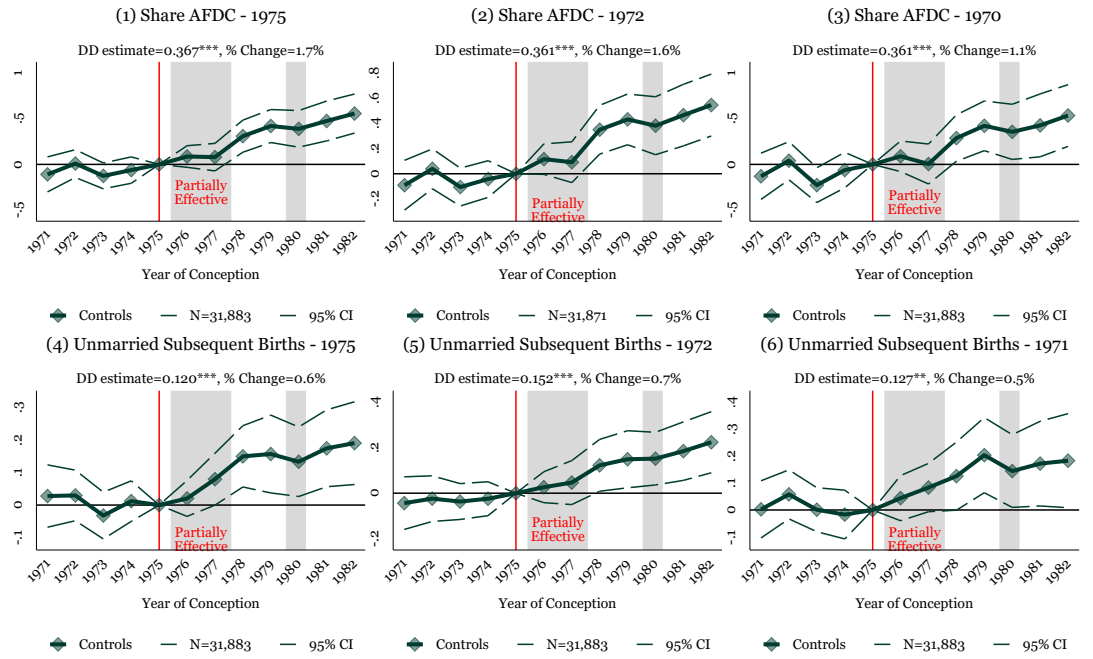
Panel A: Composition Changes in Married and Unmarried Births



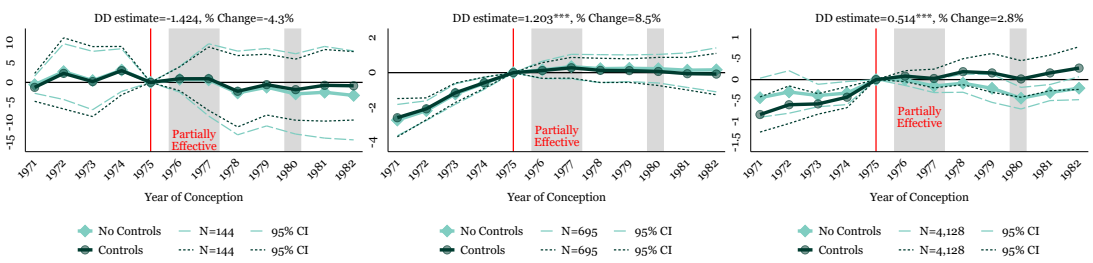
Panel B: Compositional Changes in Births by Age and Race



Panel C: Main Results, Alternative Measures of Medicaid Eligibility

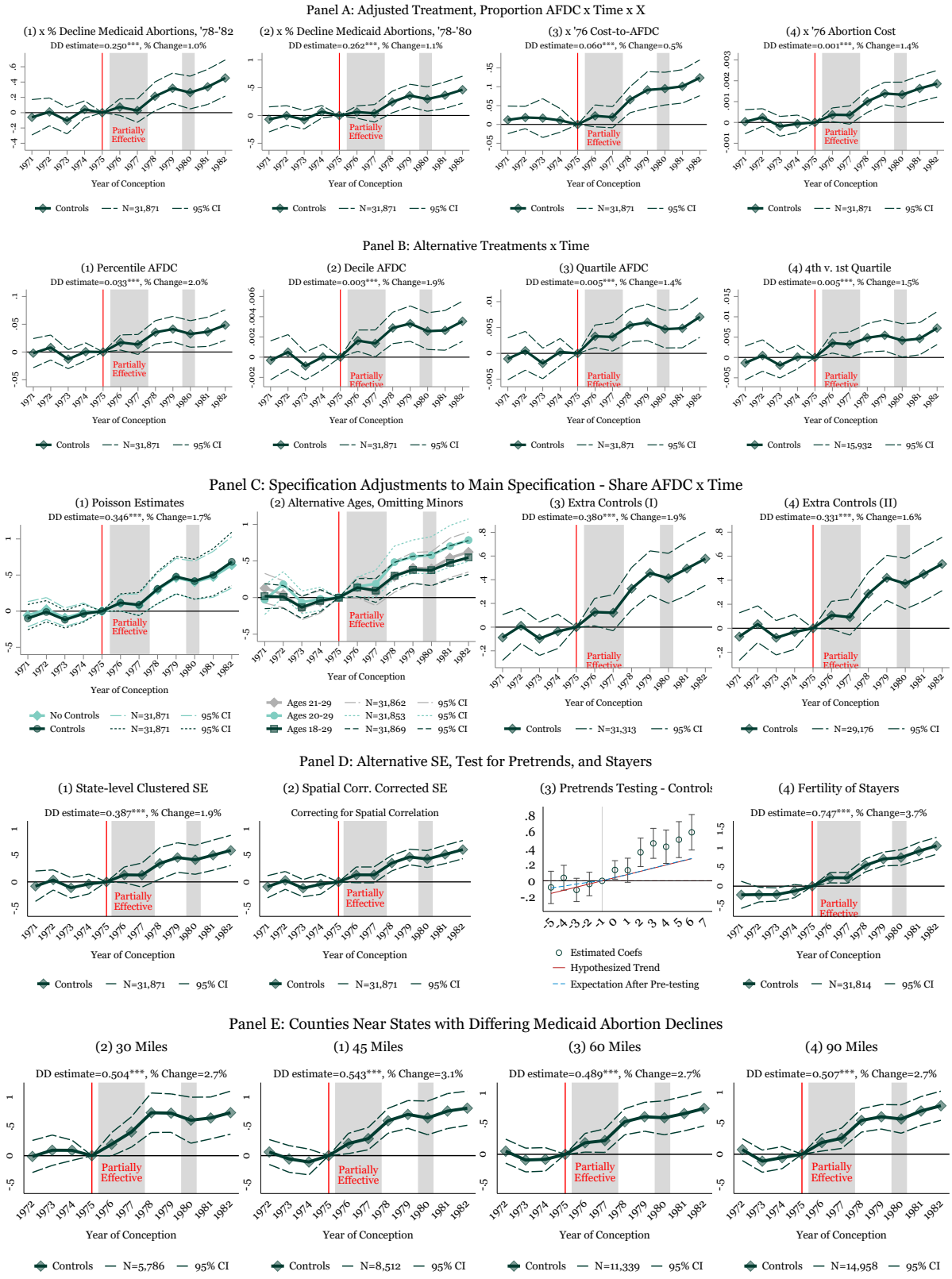


Panel D: Placebo Test Based on "Untreated" or Less Affected States



NOTES: Panel A shows the specification in Panel B of Figure VI except considering the share of births of each type–unmarried/subsequent, unmarried first, and married. The regressions are weighted by the number of births. Sample from births to women less than 30, see Figure B.1 for married subsequent and first births, see Figure B.2 for all births (not just under 30) and Figure B.3 for births by race. Panel B reflects the grouped post-period specification in Figure VI except breaking out the effect by share of each age in the left panel, and the share of age by race in the right two panels. Regressions weighted by the number of births, all, white, and non-white. Panel C shows the specification in Figure VI, except considering alternative measures of Medicaid eligibility. Certain AFDC measures from Cunningham and Goodman-Bacon (2025). Panel D shows Figure VI Panel B estimated placebo states where there is no Medicaid program (AZ) or an average increase in Medicaid funding.

Figure VIII: Robustness Checks on Figure VI

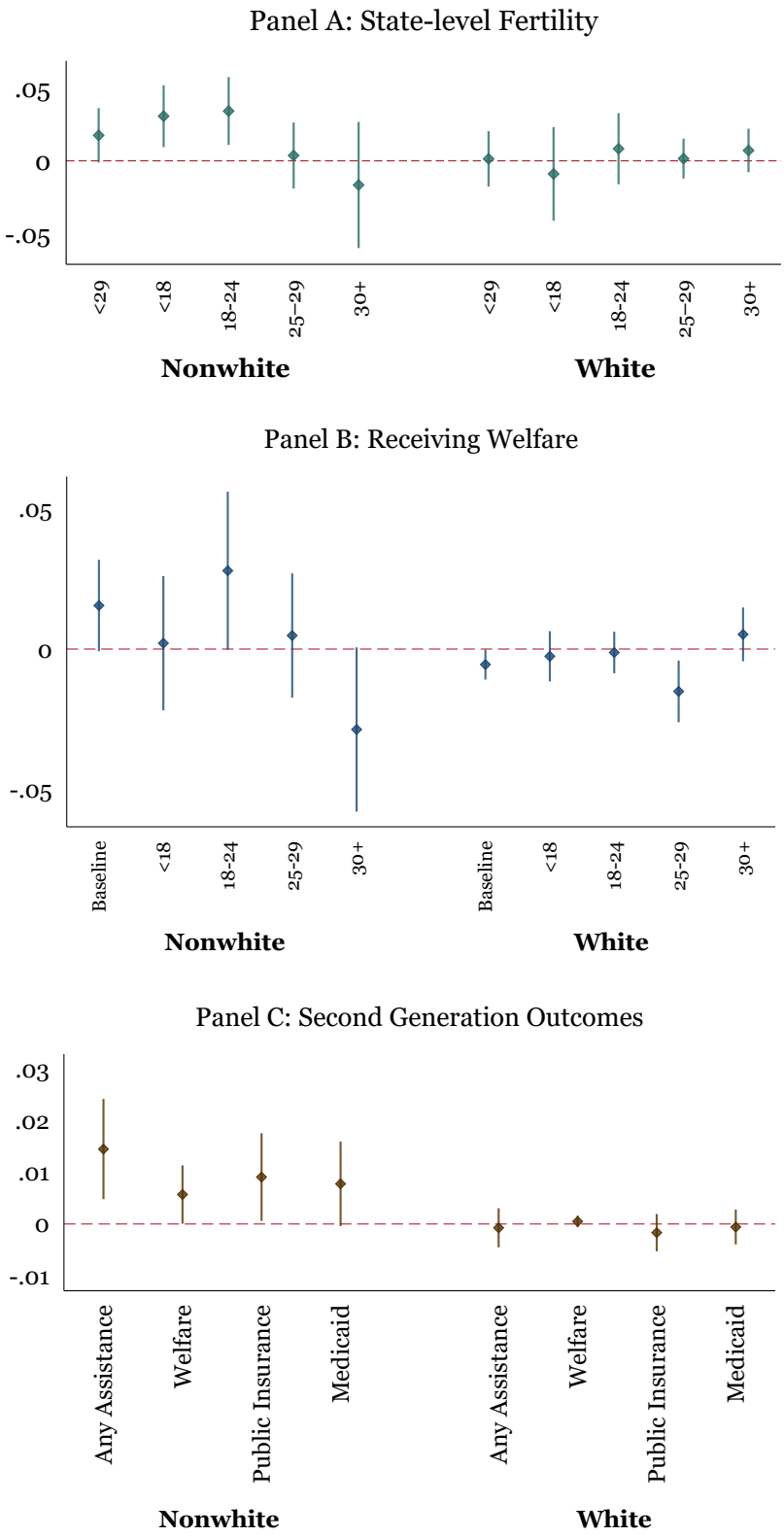


NOTES: Represents county-level specification from Figure VI, except with the following modifications. Panels A.1-A.4 show the treatment (Hyde x share AFDC) also interacted with factors that may affect the strength of the fertility change. Panels A.1-A.4 show the share of AFDC with the % decline in Medicaid Abortions in 1978-1982 (A.1) and 1978-1980 (A.2), the cost-to-AFDC ratio in 1976 (A.3), and the 1976 abortion cost (A.4). Panel B uses alternative treatments rather than the share of AFDC recipients in a county. Panel B.1 uses the percentile of AFDC receipt, Panel B.2 shows the decile of AFDC receipt, and Panel B.3 interacts with the quartile of AFDC receipt. Panel B.4 shows the 4th versus the 1st quartile, a binary treatment rather than a dose-response treatment. Panel C.1 shows the Poisson estimates, where the outcome is the number of births to women less than 30, and the exposure is the number of young adult women less than 30 in each county. Panel C.2 considers young adults, 18-29, 20-29, and 21-29. Panel C.3 controls for the inverse hyperbolic sine of the average distance to an abortion provider (based on county centroids) and controls for fertility programs through Title X (Bailey, 2012) at the county level. Because the Title X clinic information is not time-varying during our sample period, we interact the presence of a Title X program with year indicators. Then, Panel C.4 also adds controls for the share 18-19, the share 20-24, the share 25-29; the share of nulliparous deliveries, the share with a high school degree, the share with a college degree, and the average birth order. Panel D.1 clusters the standard errors at the state level. In Panel D.2, the standard errors are corrected for spatial correlation as described in Hsiang (2010). Panel D.3 tests for pretrends from Roth (2022); Caceres-Bravom (2024). Panel D.4 considers only mothers who still reside in their birth state. In other words, those who have not migrated out of their birth state. Panel E restricts the sample to counties within x (30, 45, 60, 90) miles of states with differing abortion declines.

Figure IX: State-level % Decline in Medicaid Abortions x Hyde Amendment

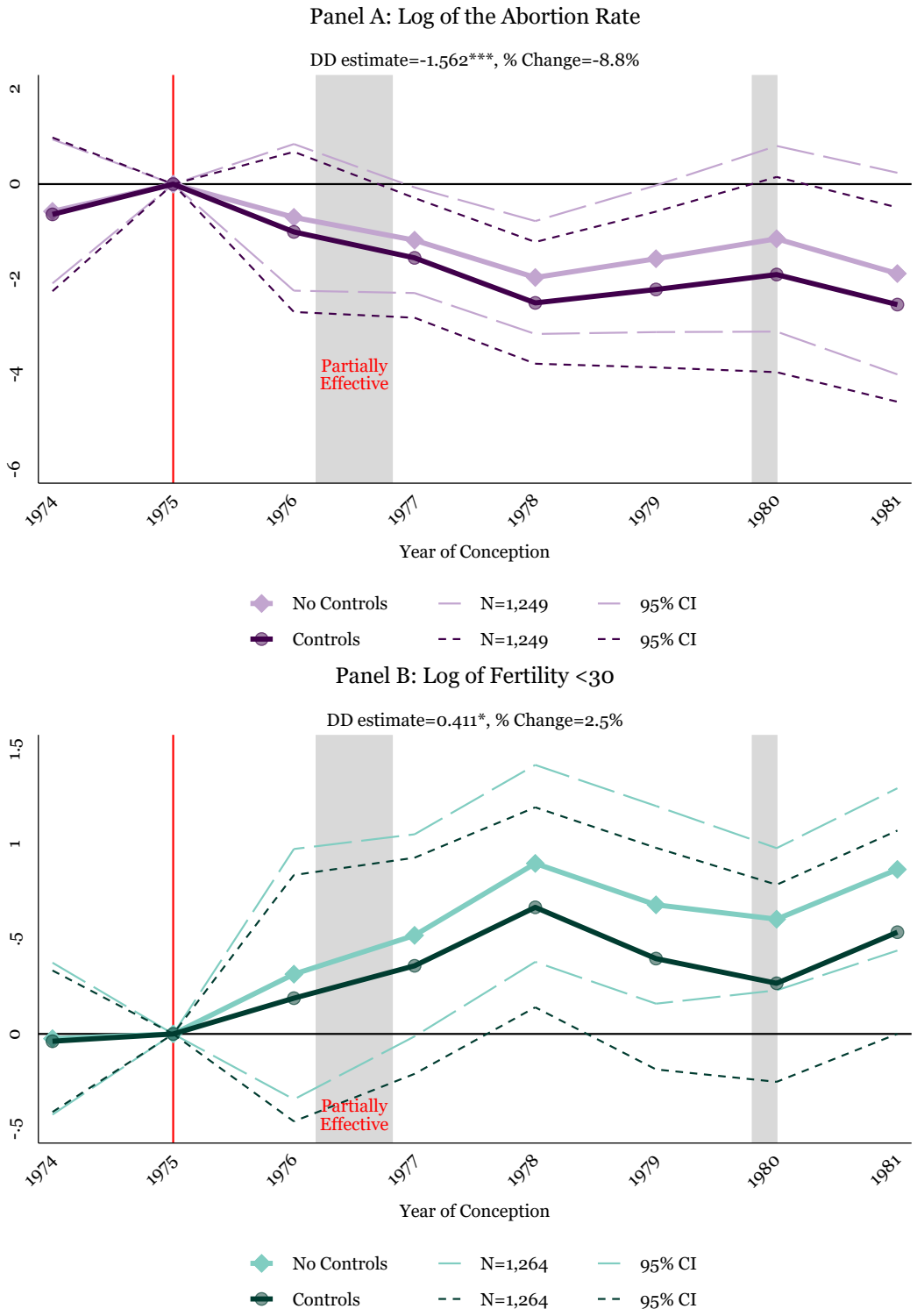
NOTES: Represents state-level event study specification based on Equation 2. Similar to the county-level specification in Figure VI, except here the treatment is the average % decline in Medicaid abortions. States with increases in Medicaid funding for abortion are included as control states (CA, HI, MI, NC, PA, WV, WA). Arizona is the only true zero because it did not have a state Medicaid program. State-fixed effects and year-fixed effects are included. Our main set of controls includes the share white, the share female, and the unemployment rate. Robust standard errors clustered at the state level. We also show the effect of the time-varying decline in Medicaid abortions in Figure X, the initial funding decline (1976-1978) in Figure E.2, a binary specification capturing the decline in funding in Figure E.3, and based on the share of reproductive-age women receiving AFDC for both non-white (Figure E.4) and white (Figure E.5). Further, Figure E.6 shows a specification where states with a rise in funding for Medicaid abortions are treated with a negative effect, rather than considering these states as control states with zero treatment.

Figure X: State-level Time-Varying % Change in Medicaid Abortions



NOTES: Difference-in-difference results, considering the percent decline in Medicaid abortions relative to 1976. Represents the state-level version of the county-level specification in Figure VI, except here the treatment is the annual % decline in Medicaid abortions from 1976 to each year 1977-1980. Unfortunately, data for 1977 and 1979 are unavailable, and we replace these missing years with 1978 and 1980, respectively. States with increases in Medicaid abortions will receive a negative treatment. We focus on the years up to 1980 because the main fertility decline appears before 1981, or 1977-1980, before the temporary pause on the Hyde Amendment and the major changes to AFDC/welfare and Medicaid expansions (East et al., 2023). The effects above are absolute effects on each group noted by the Medicaid abortion declines. Panel C is for children born from 1974-1980, around the time of the Hyde Amendment.

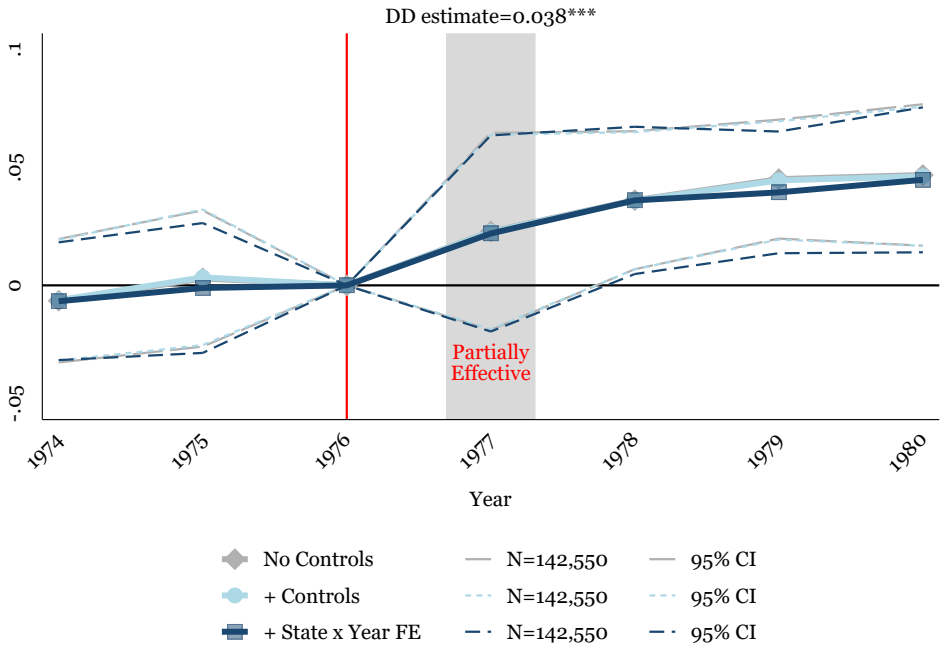
Figure XI: The Hyde Amendment and Abortions in Georgia
Hyde Amendment x Time x Proportion Medicaid Eligibility



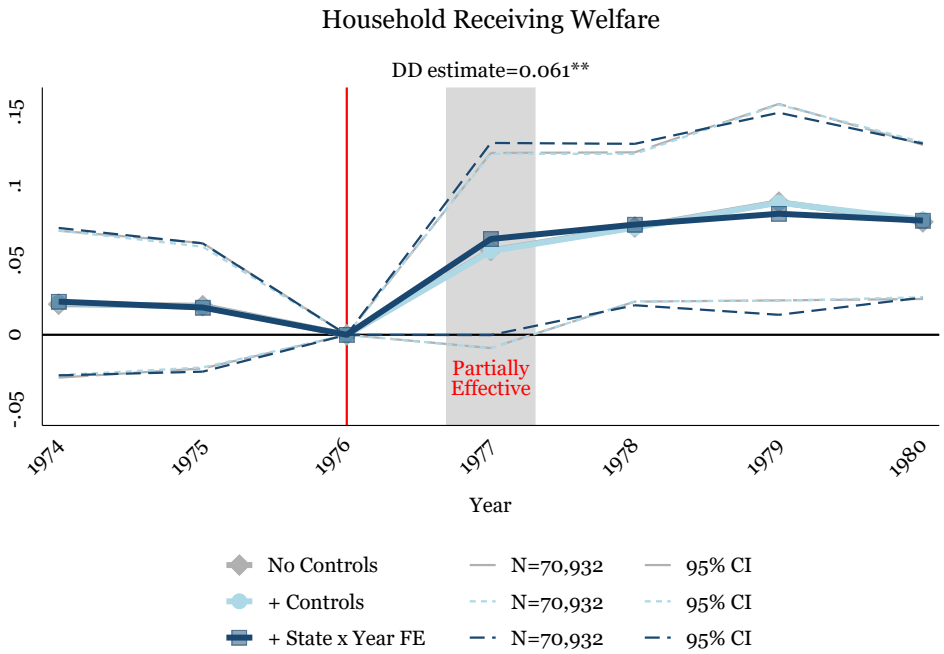
SOURCES: Georgia abortion data from annual vital statistics publications in Georgia Department of Public Health (1975-1981)
NOTES: Displays OLS coefficients. The outcome is the log of abortions per 1,000 reproductive-age women, with weights applied by the denominator of the rate. Results are only shown for Georgia, so they represent the declines in abortions by AFDC participation in a single state. Baseline fixed effects include the county and year (because we only have a single state). Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the county level. The grouped post period, or DD estimate, represents the effect of funding restrictions on abortion from the Hyde Amendment interacted with the share of AFDC recipients in a county. Percentage effect calculated using the point estimate at the average county-level AFDC participation. Our main set of controls includes the share white, the share female, and the unemployment rate. Percentage effect calculated using the point estimate at the average county-level AFDC participation.

Figure XII: CPS Data–% Decline in Medicaid Abortions x Non-white x Hyde

Panel A: Main Effect: Welfare Participation All Women 15-29

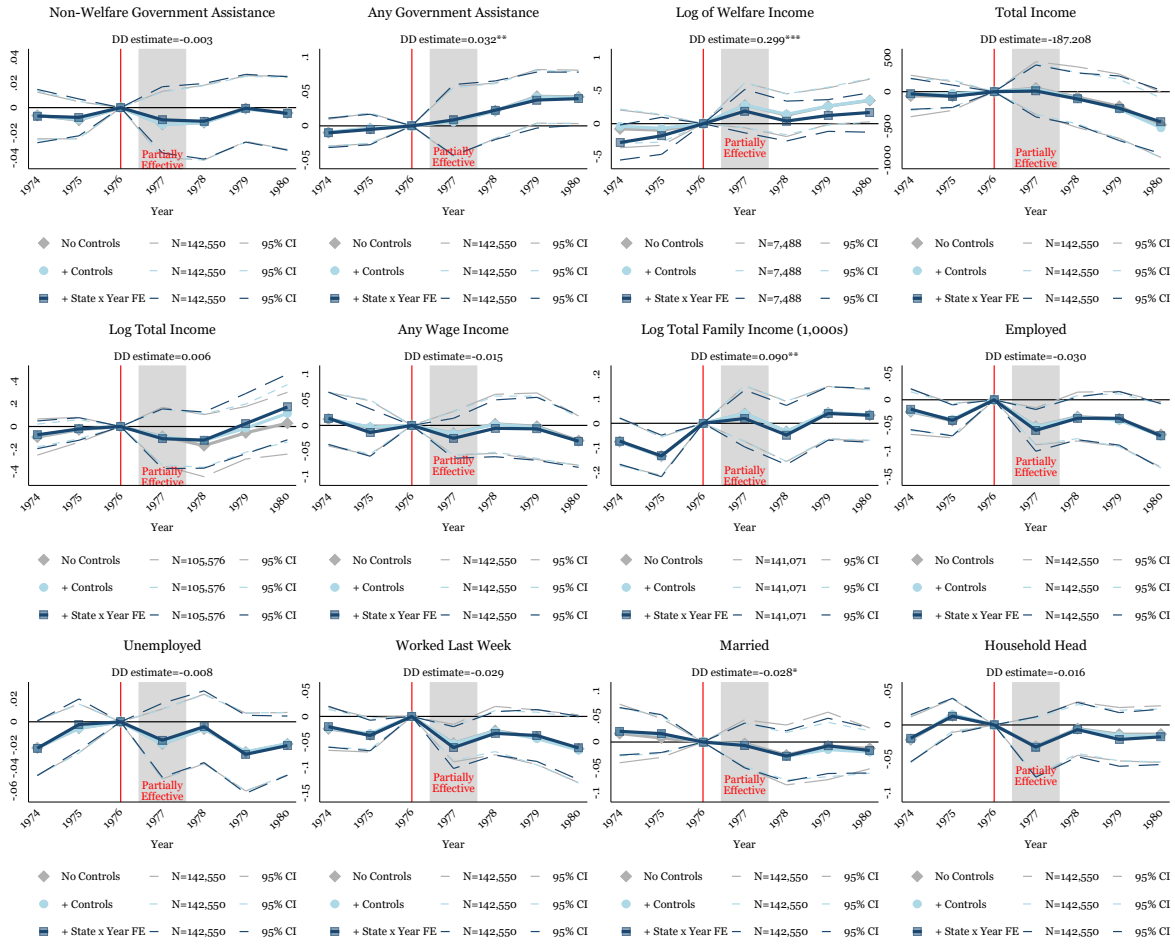


Panel B: Household Welfare Participation, Household Head Under 30

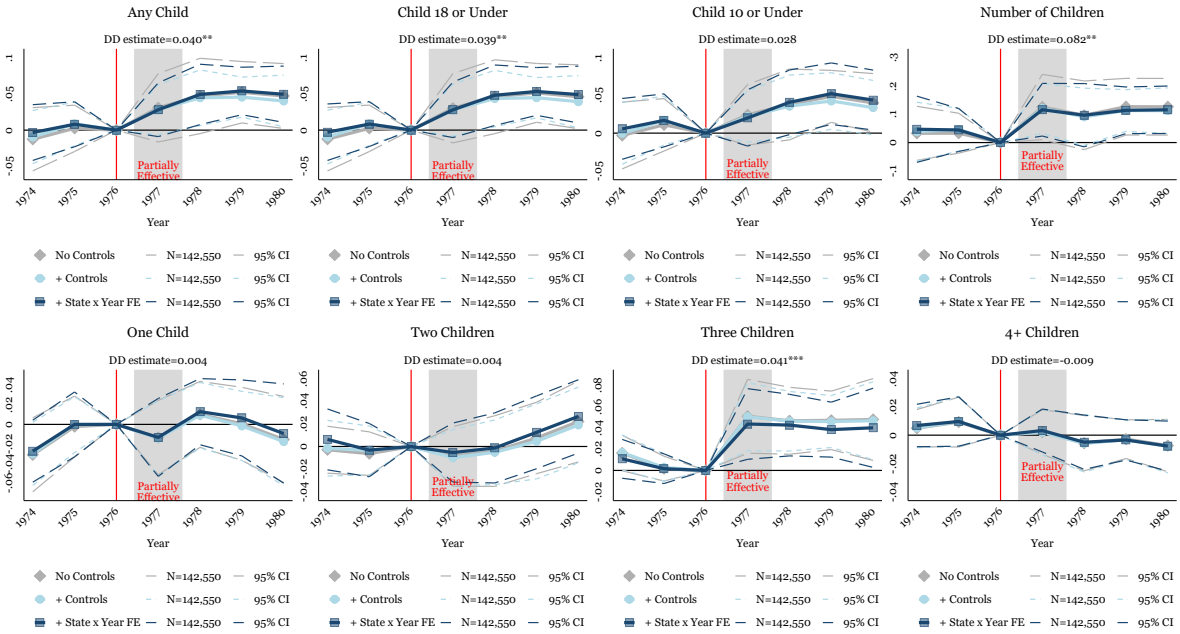


SOURCE: CPS data (Flood et al., 2024).
NOTES: Panel A shows only individual-level welfare receipt for women under 30. Panel B shows household-level welfare receipt for household heads under 30 of both sexes. OLS coefficients reported. Considers the impact of the Hyde Amendment, using the % decline in state-level Medicaid abortions interacted with year indicators and an indicator for non-white. Fixed effects include state, year, non-white, non-white-x-percent decline, non-white-x-post, and percent-decline-x-post fixed effects. The CPS data reports state groups at the beginning of the sample, so we take the average % decline in Medicaid abortions for grouped states (weighted by the reproductive age women). States also grouped similarly over time for consistency. Controls include age; most other controls, like education, would be "bad controls." Robust standard errors clustered at the state level. Only those 15-29 are included in the sample. See Figure F.3 for robustness checks.

Figure XIII: CPS Data–Mechanisms for the Rise in Welfare Payments
Panel A: Employment and Income

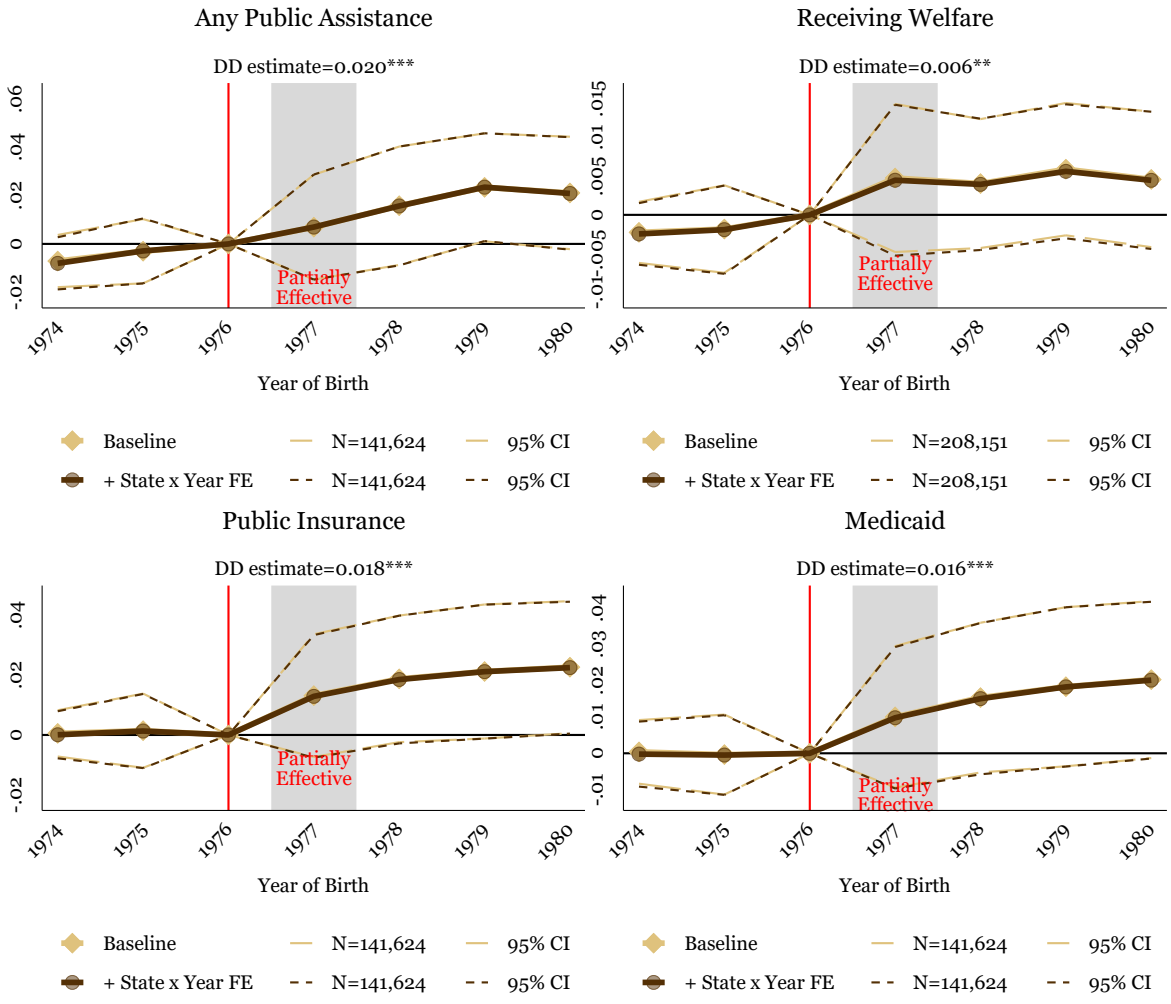


Panel B: Fertility-related Outcomes



NOTES: Reflects Panel A of Figure XII except considering other outcomes. Figure F.5 shows these for the household level, analogous to Panel B of Figure XII.

Figure XIV: ACS Data–Public Assistance of the Second Generation Women



SOURCE: ACS Data (Ruggles et al., 2024).
NOTES: ACS data collapsed to the race (non-white v white), state, state of birth, year, and birth year level. Considers the impact of the Hyde Amendment, using the % decline in Medicaid abortions interacted with year indicators and an indicator for non-white. Only includes adult women under 45 born between 1974 and 1980. Fixed effects include the current year of birth, birth state, non-white-x-percent decline, non-white-x-post, and percent-decline-x-post fixed effects. Current state-by-year fixed effects are included where noted, and in regressions without state-by-year FE, current state and current year fixed effects are included. Controls include the average age. Regressions are weighted by the sum of the weights in the ACS. Robust standard errors clustered at the state of birth level.

12 Tables

Table 1: State Medicaid Abortion Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
State	Medicaid Abortions 1976	State Funded Abortions 1978	State Funded Abortions 1980	State Funded Abortions 1981	State Abortions 1982	Average % Decline Medicaid Abortions	Medicaid Abortion Rate 1976	Non-Medicaid Abortion Rate 1976	Ratio Medicaid to Non Medicaid Abortions 1976	Average Abortion Cost 1976	Average AFDC Payment 1976	Cost-to-AFDC payment 1976
CA	70000	100000	90108	97437	99000	-38.1	100	24	4.2	307	135	2.3
NY	50000	41326	37500	37500	46000	18.8	85	35	2.5	289	140	2.1
IL	18300	4400			0	91.0	60	19	3.1	400	256	1.6
MI	14800	13700	6750	19400	19470	-0.2	52	16	3.1	218	300	0.7
TX	13300	0	0	0	0	100.0	91	16	5.6	196	104	1.9
PA	10000	14535	7596	6125	14697	-7.4	39	17	2.2	415	260	1.6
NJ	10000	0	421	5399	3952	75.6	52	17	3.0	321	217	1.5
OH	10000	0			0	100.0	46	16	2.8	160	256	0.6
DC	7400	5782	4921	4753	4313	33.2	190	171	1.1	460	207	2.2
VA	5200	0	21	19	30	99.7	65	15	4.2	181	255	0.7
MA	5100		0	6934	6695	4.1	21	28	0.7	280	175	1.6
MD	5000	6009	5179	2766	3193	14.3	56	21	2.6	274	193	1.4
KS	3700	904			0	90.8	82	24	3.4	310	247	1.3
GA	3700	0	0	0	6	100.0	30	21	1.5	211	139	1.5
NC	3100	1123	5730	5730	4295	-36.1	29	15	2.0	309	363	0.9
CO	2800	404	2125	823	1319	58.3	77	20	3.8	379	297	1.3
WA	2700		3840	3326	3213	-28.4	37	27	1.4	253	191	1.3
WI	2500	29	997	45	3	89.3	34	11	3.2	460	196	2.3
MO	2400	0	0	0	0	100.0	25	10	2.5	460	48	9.6
OR	2000	1191			657	52.1	43	25	1.7	169	203	0.8
CT	2000	0			951	65.2	37	17	2.2	301	205	1.5
FL	1600	0	0	0	0	100.0	16	28	0.6	239	242	1.0
SC	1400	0	0	0	0	100.0	25	8	3.1	448	263	1.7
AL	1300	0	0	1	0	100.0	20	7	3.0	280	238	1.2
MN	1300	0				100.0	22	14	1.6	232	296	0.8
HI	1200	1406	2100	2100	1623	-50.6	56	27	2.1	214	97	2.2
TN	1200	0	0	0	0	100.0	15	22	0.7	160	205	0.8
RI	1000	0	0	0		100.0	30	16	1.9	298	283	1.1
IA	1000				8	99.0	25	10	2.7	244	168	1.5
NM	900	109	0	0	0	97.0	41	17	2.4	280	263	1.1
KY	900	0	0	0	0	100.0	7	12	0.6	379	229	1.7
MT	700	0	0	0	0	100.0	46	5	8.3	167	137	1.2
OK	700	0			0	100.0	14	12	1.2	202	195	1.0
AR	500	0	0	0	0	100.0	11	6	1.8	207	141	1.5
ME	500	0	0	0	0	100.0	17	7	2.3	160	119	1.3
DE	400	2	0	0	0	99.9	30	17	1.8	409	266	1.5
NV	400	0	0	0		100.0	60	25	2.4	202	210	1.0
WV	300	459	325		547	-52.0	13	1	15.9	223	252	0.9
AK	300	312				2.4	74	16	4.6	310	113	2.7
UT	300	0	0	0	0	100.0	18	8	2.4	208	104	2.0
VT	300	0	0	0	0	100.0	32	21	1.5	184	246	0.7
NE	300	0			0	100.0	16	14	1.2	383	173	2.2
NH	200	0	0	0	0	100.0	18	12	1.4	250	157	1.6
WY	100	0	0	0	0	100.0	28	5	5.4	241	296	0.8
ID		0	0	0	0	100.0		7		430	363	1.2
IN		0	0	0		100.0		7		225	269	0.8
LA		0	0	0	0	100.0		9		199	171	1.2
MS		0	0	0	0	100.0		1		278	266	1.0
ND		0	0	0	0	100.0		10		331	154	2.1
SD		0	0	0	0	100.0		11		226	85	2.6
AZ						0.0		16		293	297	0.8

SOURCE: Lincoln et al. (1977), Gold (1980), Gold (1982), Nestor and Gold (1984), Gold and Nestor (1985), Gold and Macias (1986).

Table 2: Effects of Hyde × Medicaid on Abortion, Live Birth, Stillbirth, and Miscarriage from the NSFG

	Abortion				Live Birth				Miscarriage				Stillbirth			
	(1) <30	(2) <30	(3) All	(4) All	(5) <30	(6) <30	(7) All	(8) All	(9) <30	(10) <30	(11) All	(12) All	(13) <30	(14) <30	(15) All	(16) All
1(Hyde)=1 × 1(Medicaid)=1	-0.0748*	-0.0848**	-0.0672*	-0.0710*	0.0933*	0.1063**	0.1049**	0.0999**	-0.0261	-0.0275	-0.0461	-0.0354	0.0076	0.0061	0.0084	0.0065
	(0.0403)	(0.0406)	(0.0377)	(0.0375)	(0.0521)	(0.0508)	(0.0508)	(0.0490)	(0.0413)	(0.0403)	(0.0465)	(0.0417)	(0.0086)	(0.0083)	(0.0079)	(0.0078)
N	3,608	3,608	4,253	4,251	3,608	3,608	4,253	4,251	3,608	3,608	4,253	4,251	3,608	3,608	4,253	4,251
Adjusted R-squared	0.001	0.031	0.001	0.036	0.000	0.042	0.001	0.044	0.002	0.016	0.001	0.021	0.001	0.009	0.000	0.006
Medicaid FE and Pregnancy Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Controls		X		X		X		X		X		X		X		X

NOTES: Reports linear probability model difference-in-differences capturing the interaction of the Hyde Amendment (post-1976) with Medicaid receipt. Columns show the pregnancy outcomes, with the possible outcomes including abortion, live birth, stillbirth, and miscarriage. Columns (1)–(4) present estimates for the probability of abortion. Columns (5)–(8) show the probability of a live birth. Columns (9)–(12) display the probability of miscarriage. Columns (13)–(16) present the probability of stillbirth. The first two rows in each outcome are restricted to women younger than 30, and subsequent rows show the full sample. All regressions include Medicaid fixed effects and pregnancy-year fixed effects. Specifications with controls include race indicators (black and white) and controls for maternal age. Standard errors are clustered at the individual level and regressions weighted by the survey weight. Data from the 1982 version of NCHS (2010). We keep only the years of pregnancy between 1974 and 1979. We do not include 1980 because of the pending Supreme Court Decision that paused the Hyde Amendment in 1980, and the fact that we only consider a grouped post period in the present analysis.

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