Subsidized Contraception and Teen Fertility: The Effects of Medicaid Family Planning Program Eligibility Expansions on the Teen Birth Rate and 12th Grade Dropout Rate

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The Effects of Medicaid Family Planning Program Eligibility Expansions on the Teen Birth Rate and 12th Grade Dropout Rate

Katherine Qinyun Wang

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Abstract

Between 1997 and 2010, 12 states expanded Medicaid family planning program access by raising the income eligibility threshold for all women of childbearing age. I exploit the lagged implementation of these income-based expansions to explore the effects on the teen birth rate and 12th grade dropout rate. By subsidizing contraception among low-income teens, the expansions decreased the overall teen birth rate by 6.50%. Furthermore, the magnitude of this negative effect increased with the percent of females ages 15-19 living below 200% of the federal poverty line. Despite these robust results, I find no subsequent impact on the 12th grade dropout rate.

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Contents

1 Introduction

Since 1964, Medicaid family planning programs have provided reproductive health care services like contraception, sexually transmitted infection testing, and family planning counseling to low-income individuals across the country. Prior scholarship has found that these programs significantly reduce fertility rates (Mellor, 1998, Bailey, 2012, Lopoo and Raissian, 2012, Bailey Malkova, and Norling, 2014). In 1993, the federal government began allowing states to apply for Medicaid Section 1115 demonstration waivers to expand eligibility for family planning programs. Before these expansions, individuals qualified for Medicaid family planning programs if their family income fell below the general Medicaid eligibility threshold.[1] By the time Congress ratified the Affordable Care Act in 2010, 12 states had raised the family planning program income eligibility threshold for all women of childbearing age. These income-based waivers granted new access to those whose incomes lay above the threshold for general Medicaid eligibility but below the expanded family planning threshold. Approximately 27.2% of teenage females – females ages 15-19 – living in treated states gained newfound eligibility to family planning programs.[2]

I investigate the effects of Medicaid eligibility expansions for family planning programs on the teen birth rate and the female 12th grade dropout rate, exploiting variation in policy implementation across states. Kearney and Levine (2009, 2015) employ a difference-in-differences model and estimate that these expansions lowered the teen birth rate by more than 4%. Yang and Gaydos (2010) and Lindrooth and McCullough (2007) find similar negative effects of the expansions on the teen birth rate. I build on this literature using a triple difference-in-differences model that differentiates the effect of the income-based expansions

[1]States also granted eligibility for specific populations based on other characteristics. For example, in many states, low-income pregnant women qualified for Medicaid family planning programs. However, as the income-based waivers expanded eligibility solely on the basis of income for all women of childbearing age, I focus my analysis on income-based eligibility.

[2]Kearney and Levine (2009) estimate that 34.4% of females ages 20-24 and 22.5% of females ages 20-44 in treated states gained eligibility with the income-based expansions. They do not approximate a percentage newly eligible among females ages 15-19. However, my estimate seems reasonable given their calculations.

by the state-level percent of teenage females living below 200% of the federal poverty line (FPL). I define the percent of teenage females living below 200% of the FPL as the percent of females ages 15-19 with family incomes below 200% of the FPL. As most income-based expansions extended eligibility to women with family incomes below 200% of the FPL, this measure serves as a proxy for treatment intensity.

In addition, I estimate the effect of the income-based waivers on the school district-level 12th grade dropout rate with another triple difference-in-differences model. I interact the policy with an indicator for above average school district child poverty rate. The school district child poverty rate is the percent of children within the district with family income below 100% of the FPL. Note that I control for the child poverty rate instead of the percent of teenage females with family income below 200% of the FPL, unlike in the teen birth rate regressions described above. I use the child poverty rate because data for the percent of teenage females with family income below 200% of the FPL is not available at the school district-level. To assess the validity of using child poverty rate as a measure of treatment intensity, I repeat the teen birth rate triple difference-in-differences and replace the percent of teenage females living below 200% of the FPL with the state-level child poverty rate. The results of these regressions are very similar to the initial teen birth rate triple difference-in-differences. Thus, I argue that the school district-level child poverty rate is a close approximation of the percent of teenage females living below 200% of the FPL within the school district.

The income-based waivers likely only impact the 12th grade dropout rate through changes in the teen birth rate.[3] Thus, this analysis explores the connection between the teen birth rate and high school dropout rate. Although existing literature has found a strong correla-

[3]The income-based expansions may also have indirect effects on the teen birth rate. For example, the expansion may increase contraception use among mothers of low-income teenagers. By lowering the birth rate among mothers of low-income teenagers, the income-based expansions may decrease the high school dropout rate. Thus, it is possible that I overestimate the direct effect of the teen birth rate on the high school dropout rate. To fully isolate the impacts of income-based expansions that included teenagers, one could define the control group as states that passed income-based expansions that excluded teenagers and exclude states that never enacted an income-based expansion.

tion between teen motherhood and lower educational outcomes, the direction of causality is controversial.

With data from the 1995-2010 samples of the National Center for Health Statistics' Vital Statistics and the 1995 Annual Social and Economic Supplement of the Current Population Survey, I find that the income-based expansions lowered the overall teen birth rate by 6.50%. In addition, I estimate that a 1 percentage point increase in the percent of teenage females living below 200% of the FPL decreased the overall teen birth rate by 0.90%. Furthermore, the expansions only had a negative impact given an above average percent of teenage females living below 200% of the FPL. With the 1995-2010 samples of the National Center for Education Statistics' Common Core of Data and the U.S. Census Bureau's Small Area Income and Poverty Estimates, I find that the expansions had a negative effect on the difference in female and male 12th grade dropout rates in high poverty school districts. However, this effect is not significant when including state-specific linear time trends.

This thesis proceeds as follows. Section 2 details the eligibility expansions. Section 3 surveys the relevant literature and discusses the potential effects of the policies. Section 4 describes the data and its limitations. Section 5 illustrates the identification strategies. Section 6 reports the results of the analyses and robustness checks. Section 7 discusses the magnitude of the effects and their implications. Section 8 concludes.

2 The Scope of the Expansions

From 1993 to 2010, 26 states implemented Section 1115 demonstration waivers to expand Medicaid family planning eligibility. There were two types of expansions – income-based waivers and postpartum-based waivers. Income-based waivers increased the family income eligibility limit for family planning services, subsidizing contraception for low-income women.[4]

[4]Some states enacted income-based waivers that included men within the specified income eligibility limits. However, most family planning programs only provided men with contraceptive services in the form of vasectomies. Thus, the inclusion of men in an income-based expansion likely had little effect on the teen birth rate.

Postpartum-based waivers extended eligibility to recent mothers who lost Medicaid coverage after birth. Table 2 details all policy implementations by state.

To identify the effects on the teen birth rate and 12th grade dropout rate, I define a treated state as a state with an income-based waiver that included all women of childbearing age within the specified income limits. As postpartum-based waivers only reduced the cost of health care after birth, these policies likely had little impact on the teen birth rate. In addition, income-based waivers that excluded teenage females probably had no effect on teenage outcomes. I also restrict my analysis to states that enacted expansions prior to 2010 to avoid potentially confounding effects of the Affordable Care Act. In total, there are 12 treated states in my analysis.[5]

To evaluate the scope of the expansions, I estimate the percent of the teenage female population impacted by the income-based waivers within these 12 states. Income-based waivers granted eligibility to individuals with family incomes below the expanded threshold who were not otherwise eligible for Medicaid. With policy reports from the Henry J. Kaiser Family Foundation, I collect data on the general Medicaid family income threshold for children ages 6 to 19 in the year of policy implementation for each state (Martucci, 1997, Schneider et al., 1998, Ross and Cox, 2003, 2004, 2007, Ross and Horn, 2008). These thresholds serve as the lower limit of family income relative to the federal poverty line for newly eligible teens. For teens with family incomes below this lower limit, the expansions did not impact access to contraception as they qualified for Medicaid family planning services before and after policy implementation. Then, with data from the 1997 to 2009 samples of the Annual Social and Economic Supplement (ASEC) of the Current Population Survey, I calculate the state-level percent of teenage females with family incomes within the eligibility limits in the year of policy implementation.

Table 3 presents the initial income eligibility limits and percent of teenage females treated

[5]Technically, Wisconsin was the thirteenth state with an income-based expansion to include all women of childbearing age. However, Wisconsin's general Medicaid eligibility threshold for children was the same as its waiver expansion threshhold. Thus, no teens actually qualified for expanded eligibility. I consider Wisconsin untreated for the purposes of my analysis.

within each state. Figures 1, 2, 3, and 4 plot the state-level distributions of family income relative to the federal poverty line for teenage females in the year of policy implementation. Within these treated states, the percent of teenage females with qualifying family incomes ranges between 8.53% and 39.4%. The average percent impacted is 18.3%. To estimate the percent of teenage females impacted by the policies nationally, I use observations of state-level teenage female population from the U.S. Census Bureau and National Center for Health Statistics' Bridged-Race Population Estimates in 1995, the first year of my birth rate analysis. I use 1995 data to estimate pre-treatment population in all states as the income-based waivers began in 1997. As 35.7% of teenage females reside in the 12 treated states, the expansions intended to impact approximately 9.70% of all teenage females.[6]

3 Literature Review

A review of the relevant literature elucidates the potential impacts of the income-based expansions on the teen birth rate and high school dropout rate. Between 1991 and 2009, the teen birth rate declined by over 40% (Kearney and Levine, 2012). Over this time period, the teen abortion rate and the rate of teen sexual activity fell while the rate of teen contraceptive use rose (Kearney and Levine, 2012). Medicaid-funded family planning programs provide subsidized contraception but do not perform abortions. Thus, the primary method through which family planning programs may lower the teen birth rate is the provision of subsidized contraception.

Existing research has found that the creation of Medicaid family planning programs significantly reduced birth rates and improved economic outcomes for low-income women. Bailey (2012) employs an event-study design to estimate the impact of the initial roll out of Medicaid family planning programs from 1964 to 1973. She shows that this initial roll out lowered the birth rate among low-income women by 20-30% (Bailey, 2012). She later

[6] This is only an approximation. Note that the base year of implementation varies across states. I only use 1995 population estimates to get a consistent calculation. In addition, income eligibility limits change within treated states over time due to Medicaid reforms.

extends this analysis and finds that access to Medicaid subsidized contraception increased long-run labor market participation and wages (Bailey, 2013). Bailey, Malkova, and Norling (2014) further find that Medicaid family planning programs reduced child and adult poverty rates among those born after program establishment.

Further scholarship has shown that the income-based expansions significantly decrease the teen birth rate (Lindrooth and McCullough, 2007, Yang and Gaydos, 2010, Kearney and Levine 2009, 2015). However, existing literature has not examined the effects of the expansions on teen birth rates across income levels or poverty rates. There has also been little research on the impacts of income-based waivers on educational attainment or labor market outcomes.

A robust economics literature has shown that access to contraception increases educational and economic outcomes. The Food and Drug Administration (FDA) approved the first oral contraceptive, Enovid, in 1960, but state laws prevented many young, unmarried women from accessing the birth control pill until the early 1970's. Goldin and Katz (2002) analyze the diffusion of oral contraceptive availability and find that access to the pill increased professional education and the age at first marriage. Bailey (2006) uses a similar approach and concludes that access to the birth control pill increased women's labor force participation. By lowering the cost of delaying pregnancy, access to contraception increases the expected return to human capital investments, leading to higher economic and educational outcomes (Goldin and Katz, 2002).

In this vein, lowering the teen birth rate may decrease female high school dropout rates. Existing research has shown a significant positive correlation between teen motherhood and lower high school completion and income (Kearney and Levine, 2012). However, the evidence for a causal relationship between the teen birth rate and poor economic outcomes is inconclusive. Levine and Painter (2003) use within-school propensity-score-matching to predict that teen birth significantly reduces educational attainment for unmarried women. Kane et al. (2013) build on this approach and find similar negative effects. Fletcher and

Wolfe (2009) compare teens who gave birth with teens who suffered miscarriages and control for community-level fixed effects likely related to the probability of miscarriage. Even with these controls, they find that teen motherhood reduces the probability of high school graduation by 5 to 10 percentage points (Fletcher and Wolfe, 2009).

In contrast, Lovenheim et al. (2016) analyze the effects of school-based health center expansions on teen fertility and high school dropout rates. They find significant negative effects on the teen fertility rate but little effect on the high school dropout rate. Geronimus and Korenman (1992) compare sisters who gave birth to their first child at different ages and conclude that family background characteristics explain a significant amount of the variation in socioeconomic outcomes. Kearney and Levine (2016) further argue that teen birth does not increase the probability of a teenage female dropping out of high school. Instead, they argue that the positive correlation between high school dropout rate and teen birth reflects a selection effect as teen motherhood is indicative of characteristics that lead to lower educational attainment (Kearney and Levine, 2016).

Thus, the expected impact of the income-based expansions to family planning programs on the high school dropout rate is unclear. The income-based expansions primarily affect high school dropout rates through changes in the teen birth rate. Therefore, the effects of the income-based expansions on the high school dropout rate may elucidate the relationship between teen parenthood and lower educational attainment.

4 Data

4.1 Teen Birth Rates

First, I analyze the impact of the income-based expansions on the teen birth rate. With data from the National Center for Health Statistics' (NCHS) Vital Statistics and the U.S. Census Bureau and NCHS' Bridged-Race Population Estimates from 1995 to 2010, I estimate the state-level teen birth rate. The state-level teen birth rate is the number of births to mothers ages 15 to 19 divided by the total population of females ages 15 to 19 within a given state and year. Thus, the teen birth rate represents the number of births per 1,000 females ages 15-19. Teen birth rate data is available for all 50 states and the District of Columbia in each year, so this data set contains 816 observations across 16 years.

From 1995 to 2010, the national teen birth rate decreased by over 30%. Figure 5 depicts the national average teen birth rate from 1995 to 2010 while Figure 6 shows the mean weighted by the population of teenage females in each state. I include estimates weighted by population because state-level teenage female populations range between 12,100 and 1.36 million. Weighting the mean state-level birth rate by population estimates the overall number of births per 1,000 females ages 15-19 nationally. Figures 5 and 6 show similar downward trends. Between 1995 and 2010, the mean teen birth rate fell by 37.2% from 56.5 births per 1,000 females ages 15-19 to 35.5. The weighted mean fell by 32.3% from 53.4 to 36.1. The overall mean and weighted mean teen birth rate in the data are 44.3 and 44.9 births per 1,000 females ages 15-19, respectively.

4.2 12th Grade Dropout Rates

To investigate the impact of the income-based expansions on 12th grade dropout rates, I rely on the National Center for Education Statistics' Common Core of Data (CCD) restricted-use samples from 1995 to 2010. The CCD surveys public school districts across the country for data on demographics, financing, and dropout and completer rates. The data set consists of 112,167 observations from 9,900 school districts nationwide.

The CCD defines a dropout as a student who was enrolled in a given school year but not enrolled by October 1 of the next school year. For example, if a student was enrolled in September 2000 but left school by October 1, 2001, the CCD would count this student as a dropout for the 2000-2001 school year. However, if the student left school because they completed their high school education or an equivalent, transferred to another public or private school district, left temporarily due to illness, or died, the CCD would not classify the student as a dropout. Since the CCD adheres to the school year calendar from October 1 through September 30, I count dropouts in a school year as a dropout in the latter calendar year. For example, I compute the 2001 dropout rate based on the dropout count in the 2000-2001 school year.

The restricted-use samples of the CCD provide dropout counts by grade and gender for all samples years. I limit my analysis to the 12th grade dropout rate. As state-level compulsory education laws require students remain in school until at least age 16, dropout counts for 11th grade and below are more prone to reporting errors (Bush, 2010).[7] I calculate the dropout rate as the number of 12th grade dropouts divided by the total 12th grade enrollment, multiplied by 100. For dropout rates by gender, I divide the number of male or female 12th grade dropouts by the total male or female 12th grade enrollment, respectively. However, for observations from 1995 through 2002 and 2010, 12th grade enrollment is not available by gender. For these observations, I estimate male and female 12th grade enrollments to each be half of the total 12th grade enrollment.[8]

Figure 7 shows the trends in female and male 12th grade dropout rates from 1995 to 2010 across all states. I plot the means weighted by school district 12th grade enrollment to estimate the national dropout rates as enrollment varies greatly across school districts. The male dropout rate is consistently higher than the female dropout rate, although this difference decreases over time. The overall weighted mean dropout rates are 4.24% for females and 4.99% for males. The weighted mean of the difference is -0.75%.

The school district data suffers from several limitations. For example, the data is an unbalanced panel because a number of states did not report dropout counts in the initial years. The first sample with all states and the District of Columbia is from 2004. Each school district appears in the data set at minimum six times. The median school district is observed

[7]There is some variation in state-level compulsory education laws. All states require students stay in school until at least the age of 16. In addition, 31 states mandate schooling up to age 17 or 18 (Bush, 2010).

[8]Note that the CCD only includes observations for public school districts, so there should be no single-sex 12th grade enrollments. In expectation, the male and female 12th grade enrollments would each be half of the total 12th grade enrollment, so it is reasonable to estimate the male and female 12th grade enrollments in this way.

across eleven years. A balanced panel would require either limiting the years of analysis to the 2004-2010 samples or dropping a number of states. In either case, a balanced panel would not capture effects in several treated states. Even the unbalanced data set does not capture the pre-treatment 12th grade dropout rates for Florida, Maryland, South Carolina, and Washington as these states first appear in the data after policy implementation. Thus, I exclude these states from the analysis. Then, there are only eight treated states in the dropout rate regressions, so the treated population is smaller and the expected observed effect has lower power. In addition, there are many more missing observations from 1995 to 2000, especially after disaggregating the dropout rate by gender. Table 1 shows a breakdown of the number of school district observations by year.

4.3 State-Level Percent of Females Ages 15-19 Below 200% of the FPL

With data from the 1995 sample of the Annual Social and Economic Supplement (ASEC) of the CPS, I estimate the base year state-level percent of females ages 15-19 living below 200% of the federal poverty line (FPL). The ASEC Poverty sample provides the family income and FPL threshold for each individual in the data set. The FPL threshold varies by family size and age of head of household. I restrict the data set to females between the ages of 15 and 19. Then, I calculate family income relative to the FPL by dividing the individual's family income by their FPL threshold and multiplying by 100. Next, I compute the percent of teenage females within each state whose family income falls below 200% of the FPL. I weight each individual by the ASEC person-level weights, which specify how many people each observation represents. The percent of teenage females below 200% of the FPL nationally is 38.4%.[9]

I rely on observations from 1995, the first year of my birth rate data, for two reasons. First, as states began the expansions in 1997, using base year estimates avoids controlling

[9] I compute this as the mean across states weighted by state-level population of teenage females.

for any post-treatment variables. The percent of teenage females below 200% FPL is likely endogenous to the presence of an income-based expansion. If the income-based expansion lowered the teen birth rate, it decreased the number of teenage females with dependents. As income relative to the FPL is calculated based on family size, there would be a lower percent of teenage females below 200% FPL holding income constant. Second, the base year estimates are a uniform measure across treated and untreated states. Alternatively, I could control for observations in the year of policy implementation. However, with this method, there would be no unbiased way to select observations for untreated states as the expansions were lagged and the percent of teenage females living below 200% FPL decreases across states over time.

4.4 State and School District-Level Child Poverty Rates

I obtain estimates for the child poverty rate from the 1995-2010 samples of the U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program. The SAIPE provides child poverty rate estimates at the state, county, and school district-level. The child poverty rate is computed as the estimated population of children ages 5-17 with family income below 100% of the FPL divided by the estimated total population of children ages 5-17 in a given region and year, multiplied by 100. The SAIPE calculates poverty rate based on data from the Internal Revenue Service (IRS), Supplemental Nutrition Assistance Program (SNAP), and the Census Bureau's Poverty Universe. At the school district-level, the SAIPE only has observations for 1995, 1997, and 1999-2010. To avoid dropping observations, I impute the child poverty rate for 1996 and 1998 with the next available year of data.

For the school district 12th grade dropout rate regressions, I interact the presence of a state-level income-based waiver with an indicator for high school district-level child poverty rate. As the percent of teenage females living below 200% of the FPL is unavailable at the school district-level, I use child poverty rate as a proxy. To test the validity of this proxy, I rerun the state-level teen birth rate triple difference-in-differences, interacting the income-based waiver with base year state-level child poverty rate. I again define the base year as the first year of the data, 1995. The mean state child poverty rate in 1995, weighted by the state-level population of teenage females, is 20.7%. The unweighted mean state child poverty rate in 1995 is 19.4%. As the results of these regressions are consistent with the initial triple difference-in-differences regressions, I continue with the school district regressions.

I code high school district child poverty rate based on child poverty rates in the base year. Since the school district data set is an unbalanced panel, there is no common base year. For each school district, I thus define its base year as the first year that it appears in the data set.[10] A school district is high poverty if its base year child poverty rate is greater than the mean child poverty rate weighted by school district 12th grade enrollment in its base year. Otherwise, the school district is considered low poverty. For example, if a school district first appears in the data set in 1997, I compare its 1997 child poverty rate to the weighted mean child poverty rate across all school districts in 1997. If the district's child poverty rate is above the mean, I code the school district as high poverty in each subsequent year. I use the weighted mean to avoid over-weighting school districts with very few students. The weighted mean school district child poverty rate across all years is 14.8%.[11] I discretize school district-level child poverty rate to magnify the expected observed effect.

Note that the mean and weighted mean school district-level child poverty rate are much lower than the mean and weighted mean state-level child poverty rate. The data is likely missing observations from higher poverty school districts, which may bias results. As CCD dropout counts are self-reported by school administrators, it is feasible that higher poverty school districts have fewer resources and are less likely to report estimates.

[10] As mentioned earlier, I cut states without pre-treatment dropout rate data. Thus, the base year for each school district is pre-treatment.

[11] The unweighted mean base school district child poverty rate is 15.6%

4.5 Policy Changes and Demographics

For data on the Medicaid eligibility expansions, I hand coded details on policy implementation from state policy reports published by the Guttmacher Institute and the Henry J. Kaiser Family Foundation. I cross-referenced these reports with state waiver documentation prepared by the Center for Medicare and Medicaid Services that is available at Medicaid.gov.

In addition to income-based waivers, I control for relevant, contemporaneous policy changes. First, I include an indicator variable for postpartum-based waiver implementation. Some states bundled postpartum-based waiver and income-based waiver legislation. For example, Florida and New York implemented postpartum-based waivers and income-based waivers at the same time. Postpartum-based waiver implementation is thus likely correlated with income-based waiver implementation. Although Kearney and Levine (2009, 2015) find no significant effect of postpartum-based waivers on the teen birth rate, I control for these policies to minimize bias.

I also include an indicator for state-level Children's Health Insurance Program (CHIP) implementation. The federal government initiated CHIP with the Balanced Budget Act of 1997. CHIP provides states with funding to expand the Medicaid family income eligibility threshold for children. All states implemented CHIP between 1998 and 2001 (Kearney and Levine, 2015). I control for CHIP because in most states, children eligible for Medicaid under CHIP are no longer eligible for Medicaid family planning programs. In addition, CHIP provides adolescents with family planning services. However, Gold and Sonfield (2001) find that adolescents had low take up of CHIP family planning services. Similarly, Kearney and Levine (2015) find that CHIP has no significant impact on the teen birth rate. Still, I control for CHIP implementation to protect against omitted variable bias. For state-level CHIP changes, I rely on a data set compiled by Kearney and Levine (2015).

Next, I add controls for economic and racial demographics. With data from the Bureau of Labor Statistics, I incorporate state-level unemployment rates. As prior literature has found that teen birth rates and high school dropout rates may be higher for low socioeconomic status teens in states with higher inequality, I also provide estimates for lower tail income inequality (Kearney and Levine, 2012, 2016). With data from the Current Population Survey, I compute the ratio between the fiftieth percentile of household income and the tenth percentile. Finally, I include state-level racial demographics from the 1995-2010 samples of the Census Bureau's Intercensal Population Estimates.[12]

5 Methodology

5.1 State-Level Teen Birth Rate Difference-in-Differences

To analyze the effect of the income-based waivers on the teen birth rate, I first employ a difference-in-differences approach, following Kearney and Levine (2009). Figure 8 shows the mean teen birth rate in treated states by years since policy implementation. Years since policy implementation is calculated as the current year minus the first year of the income-based expansion. For example, Arkansas enacted its income-based waiver in 1997. Thus, in 1996 years since policy implementation is -1. The trend in mean teen birth rate is relatively flat, though there is a slight downward trend (Figure 8). The mean teen birth rate weighted by state-level population of teenage females years old shows greater variation and a stronger downward trend (Figure 9).

The regression of interest is as follows.

$$TeenBR_{s,t} = \beta_0 + \beta_1 IncomeWaiver_{s,t-1} + \beta_2 PostpartumWaiver_{s,t-1} +$$

$$\phi CHIP_{s,t-1} + \mathbf{X}_{s,t-1} + \delta_s + \gamma_t + \mu_s \tag{1}$$

I regress the teen birth rate in state s and year t on $IncomeWaiver_{s,t-1}$, an indicator

[12]Kearney and Levine (2015) provided me with this racial demographic data, in addition to data on CHIP changes.

variable for an income-based waiver in state s and year $t-1$. The indicator is 1 if state s had implemented the waiver in year $t-1$ and 0 otherwise. If the state implemented the waiver midway through year $t-1$, the indicator is equal to the fraction of the year it was active. Thus, β_1 is the coefficient of interest. I lag the income-based waiver variable by one year because of the minimum nine month lag in observed treatment effect. In this vein, I lag all explanatory variables by one year in the teen birth rate regressions.

$PostpartumWaiver_{s,t-1}$ is an indicator that is 1 if state s had implemented a postpartum-based waiver in year $t-1$ and 0 otherwise. $CHIP_{s,t-1}$ is an indicator that is 1 if state s had implemented CHIP in year $t-1$ and 0 otherwise. $\mathbf{X}_{s,t-1}$ represents a vector of state-level controls in year $t-1$. These include the unemployment rate, the 50/10 household income percentile ratio, and racial demographics. I do not control for the lagged percent of teenage females below 200% of the FPL because I am interested in effects of the income-based waiver, without holding this proportion constant. δ_s and γ_t represent state and year fixed effects, respectively. I cluster standard errors at the state-level. I also run a variant of this regression with state-specific linear time trends, following Kearney and Levine (2009).

I estimate each regression both with and without population weights. I weight by the state-level population of females ages 15-19 divided by the national population of females ages 15-19 in each year. I include population weights because the teenage female population varies greatly across states. Thus, weighting the regressions by population produces estimates that are representative of changes in the national birth rate. [13]

5.2 State-Level Teen Birth Rate Triple Difference-in-Differences

I extend the difference-in-differences approach by interacting the indicator for an income-based waiver with the base year percent of females ages 15-19 with family incomes below

[13] I also ran these regressions with inverse variance weights. I calculated the variance of the teen birth rate by modeling the teen birth rate as a Beta random variable where the teen birth count is the number of successes and the teenage girl population minus the teen birth count is the number of failures. However, as some women have multiple teen births, this estimate of variance is not entirely precise. These regressions produce similar results to the population weighted regressions. As the sample sizes for state-level teen birth rates are large, high variance within estimates is not a big issue.

200% of the FPL. The treatment group for the income-based expansions is individuals who are not eligible for general Medicaid coverage but whose family income falls below the expanded threshold. Returning to Table 3, we see that the eligible income range for most treated states lies between 100% and 200% of the FPL.[14] Thus, the percent of teenage females below 200% of the FPL serves as an estimate for treatment intensity within states. I include all teenage females living below 200% of the FPL to account for potential network effects. Baicker et al. (2014) find that a 2008 Medicaid expansion in Oregon increased overall take-up of services by spreading awareness of the program. By expanding the eligible population, the income-based waiver may increase public knowledge and thus take-up of Medicaid family planning services. Thus, the expansion could lower the teen birth rate among all eligible teens, rather than just those with newly granted access.

One might expect that the percent of teenage females within the income eligibility limits in the year of policy implementation would be a better estimate. However, this estimate measures initial intention to treat, rather than treatment intensity, because income eligibility limits changed within states over time. Some states increased their general Medicaid eligibility income threshold for children while other states did not. For example, in 1997, Maryland and Virginia both implemented income-based waivers. Initially, Maryland's income eligibility range for family planning programs was 33%-200% of the FPL while Virginia's was 133%-200% of the FPL. By 2004, Maryland had raised its threshold for children's Medicaid to 200%, so children were no longer eligible for the family planning waiver. In contrast, Virginia's limits stayed constant. As trends in eligibility limits varied greatly across states, the percent of females within the initial income eligibility limits may have low correlation with the size of the affected group post-treatment. Thus, the base percent of teenage females

[14] The one exception is Washington, where income eligibility ranges from 200%-250% FPL. I also run variants of these regressions, replacing percent of teenage females living below 200% of the FPL with percent of teenage females living below 150% of the FPL and percent of teenage females living below 250% of the FPL. The results are still negative and significant. I also test a variant with percent of teenage females living between 100% and 200% of the FPL. The results are negative but insignificant. This may be because of network effects or because there is less variation in this estimate across states.

below 200% of the FPL is a more stable estimate of the treatment intensity.[15]

Figure 10 shows the trends in mean teen birth rates for above and below average treated states by base percent of teenage females below 200% of the FPL. A state is considered above average if its base percent of teenage females below 200% of the FPL is above the national base percent of teenage females below 200% of the FPL. Otherwise, the state is considered below average. Figure 11 plots trends in weighted mean teen birth rates by base percent of teenage females below 200% of the FPL. The estimates are weighted by the state population of teenage females. In both figures, the teen birth rate in above average states falls relative to below average states after treatment. However, there are also slight negative pre-trends in the above average states.

The triple difference-in-differences model is estimated as follows.

$$TeenBR_{s,t} = \beta_0 + \beta_1 IncomeWaiver_{s,t-1} * PercentBelow200FPL_s + \alpha IncomeWaiver_{s,t-1} +$$

$$\gamma_t * PercentBelow200FPL_s + \beta_2 PostpartumWaiver_{s,t-1} +$$

$$\phi CHIP_{s,t-1} + \mathbf{X}_{s,t-1} + \delta_s + \gamma_t + \mu_s \tag{2}$$

As in the difference-in-differences model, $IncomeWaiver_{s,t-1}$ is an indicator for an active income-based expansion state s and year $t-1$. $PercentBelow200FPL_s$ is the base year percent of teenage females with incomes below 200% of the FPL in state s. Thus, the coefficient of interest is β_1.

This regression includes the three double interaction terms needed to accurately estimate the triple interaction coefficient. The indicator variable for income-based waiver is constructed as an interaction between treated state and post-treatment. Thus, the triple

[15] Still, I estimate the triple difference-in-differences model with the interaction between income-based waiver and percent of teenage females within the income eligibility limits in the year of policy implementation. These results are presented in Table 12, unweighted, and Table 13, weighted by female population ages 15-19, of the appendix. In all of these regressions, there are no significant effects on the triple difference-in-differences or on the indicator for income-based waiver. These results may be driven by lower variance in the percent of teenage females within the income eligibility limits in the year of policy implementation as this variable is 0 for all untreated states.

interaction is between treated state, post-treatment, and the percent of teenage females below 200% of the FPL. First, $IncomeWaiver_{s,t-1}$ is the interaction between treated state and post-treatment. Second, $\gamma_t * PercentBelow200FPL_s$ is the interaction between year fixed effects and the percent of teenage females below 200% of the FPL, which captures the interaction between post-treatment and the percent of teenage females below 200% of the FPL. Third, state fixed effects estimate the interaction between treated state and the percent of teenage females below 200% of the FPL as these controls are both time invariant. Similarly, state fixed effects encompass the effect of the percent of teenage females below 200% of the FPL, so the coefficient does not appear in the regression tables.

As in the difference-in-differences model, I include lagged controls for postpartum-based waiver, CHIP implementation, and the vector of economic and racial factors. I again cluster standard errors at the state-level, run a variant of this model with state-specific linear time trends, and estimate each regression with and without population weights.

5.3 School District-Level 12th Grade Dropout Rate Analysis

For the 12th grade dropout rate regressions, I modify the teen birth rate triple difference-in-differences approach by changing the triple interaction term. Instead of interacting the income-based waiver with the percent of females ages 15-19 living below 200% of the FPL, I interact the income-based waiver with an indicator for high school district-level child poverty rate. I alter the triple interaction term because data for the percent of teenage females living below 200% of the FPL is unavailable at the school district-level. To test the validity of this model, I repeat the teen birth rate regressions, replacing $PercentBelow200FPL_s$ with the base year state-level child poverty rate. The results are similar to those of the initial triple difference-in-differences.[16] In addition, the covariance between the two estimates is 41.6, and the correlation is 0.72. These statistics suggest that the child poverty rate is a

[16]See Table 13 of the Appendix for the population weighted regressions. The unweighted regressions have similar findings. Note that the interpretation of these results differs slightly from the initial teen birth rate triple difference-in-differences as the state-level child poverty is a measure of the percent of all children below the federal poverty line, not just teenage females.

valid approximation of the percent of teenage females living below 200% of the FPL. Thus, I proceed with the dropout rate triple difference-in-differences, interacting the policy with the school district-level child poverty rate.

I regress the model on the female 12th grade dropout rate, male 12th grade dropout rate, and the difference in female and male 12th grade dropout rates. I construct the difference as the female 12th grade dropout rate minus the male 12th grade dropout rate in a given school district and year. The regression on the difference in dropout rates thus constitutes a quadruple difference-in-differences. However, the male 12th grade dropout rate may not be a suitable control because the income-based expansions could have spillover effects affecting teenage males. Fletcher and Wolfe (2010) and Nock (1998) find that teen fatherhood lowers the likelihood of high school completion. Thus, the regressions on the difference in female and male dropout rates may underestimate the true effect of income-based waivers on females. Still, the literature on the effect of teen fatherhood on educational outcomes is relatively scarce and thus inconclusive.

Figure 12 show the trends in mean dropout rates by gender in treated states, weighted by school district 12th grade enrollment. Figure 13 shows the weighted mean difference between female and male dropout rates. These figures suggest the difference in dropout rates may have increased after treatment although the trends are unclear. The comparison between mean dropout rates in high and low poverty treated school districts shows clearer trends. Figure 14 depicts the weighted means of female and male dropout rate for high and low poverty school districts by years since policy implementation. The high and low poverty school districts do not exhibit parallel pre-trends. However, the difference in female and male dropout rates show parallel pre-trends in Figure 15. After treatment, the weighted mean difference in female and male dropout rates falls for high poverty school districts relative to low poverty school districts. The negative trend may be driven by a relative increase in the male dropout rate or a relative decrease in the female dropout rate in high poverty school districts.

The model is estimated as follows.

$$DRPF_{s,d,t} = \beta_0 + \beta_1 IncomeWaiver_{s,t-1} * HighPov_d + \alpha IncomeWaiver_{s,t-1} +$$

$$\gamma_t * HighPov_d + \beta_2 PostpartumWaiv_{s,t-1} + \phi CHIP_{s,t-1} + \mathbf{Y}_{s,t} + \delta_d + \gamma_t + \mu_s \quad (3)$$

$DRPF_{s,d,t}$ is the female 12th grade dropout rate in district d, state s, and year t. $HighPov_d$ is an indicator that is 1 if district d is a high poverty school district and 0 otherwise. $IncomeWaiver_{s,t-1}$ is still an indicator for the income-based waiver in state s and year $t-1$. Thus, the coefficient of interest is β_1.

I also control for lagged policy controls and a vector of contemporaneous economic controls, $\mathbf{Y}_{s,t}$. I lag the indicators for postpartum-based waiver and CHIP implementation because these policies hypothetically only impact the dropout rate through their effect on the teen birth rate. $\mathbf{Y}_{s,t}$ represents the state-level unemployment rate and 50/10 income percentile ratio. I do not lag these controls as economic factors in year t likely affect the dropout rate in year t.[17] I drop state-level racial demographics as they are likely uncorrelated with school district-level dropout rates. The data includes limited observations of school district-level racial demographics. However, I omit school district-level racial demographics to maintain a large sample size. Instead, I control for school district and year fixed effects, which likely capture school district-level racial demographic effects. I also add state-specific linear time trends in an iteration of this regression. As the state is the unit of variation, I cluster standard errors at the state-level.

I weight all school district dropout rate regressions by the inverse variance of the dropout rate.[18] It is necessary to weight by inverse variance because many school districts have small

[17] I ran these regressions with both the lagged and contemporaneous economic controls and found similar results, so I drop the lagged economic controls for simplicity of interpretation.

[18] In addition, I include school district rate regressions with 12th grade enrollment weights in Tables 14, 15, and 16 of the Appendix. The 12th grade enrollment weight is calculated as the school district 12th grade enrollment divided by the sum of all school district 12th grade enrollments in all years. I divide by the sum of enrollments in all years because samples from 1995 to 1998 have many fewer observations than later samples. Thus, dividing by the sum of enrollments in a given year would overweight school districts in earlier samples. These enrollment weights do not capture the variation in variance across observations. The

sample size. The 25th percentile of 12th grade enrollment is 42 students. As dropout rates are sample means within school districts, school districts with small 12th grade enrollment may suffer from noisy estimates.

To calculate dropout rate variance, I model the school district dropout count as a Binomial random variable.

$$DRPCount_{d,s,t} \sim Bin(n, r) \tag{4}$$

The number of dropouts is sampled from a Binomial distribution where n is the 12th grade enrollment and r is the dropout rate – the probability of a student dropping out. The dropout rate can thus be modeled as a Beta random variable.

$$r \sim Beta(d, x) \tag{5}$$

The parameters d and x represent the number of dropouts and non-dropouts, respectively, within a given school district and year. Non-dropouts are calculated as the 12th grade enrollment base minus the dropout count. Thus, $n = d + x$. The variance of a Beta random variable, $X \sim Beta(\alpha, \beta)$, is

$$Var(X) = \frac{\alpha\beta}{(\alpha+\beta)^2(\alpha+\beta+1)} \tag{6}$$

The variance of r, the dropout rate, can therefore be calculated as

$$Var(r) = \frac{dx}{(d+x)^2(d+x+1)} \tag{7}$$

However, 30.1% of school districts have a 0.00% dropout rate. These observations likely represent a mix of valid and biased estimates. The mean 12th grade enrollment among schools with 0.00% dropout rate is 48 students while the overall mean 12th grade enrollment is 230

coefficients of interest in these regressions are all insignificant. I also include robustness checks in Figures 20, 21, and 22 of the Appendix. Again, there seems to be no clear effect.

students. Although smaller or wealthier school districts may have a true 12th grade dropout rate of 0.00%, it is possible that some school districts reported 0 dropouts erroneously. For school districts with 0.00% dropout rate, $d = 0$, so the dropout rate variance is 0 and the inverse variance is infinite. Therefore, the raw inverse variances are not valid weights.

To circumvent this issue, I calculate the posterior inverse variances. The posterior of the dropout rate is proportional to the product of the prior and the likelihood. I model the prior as the Jeffreys prior because it is non-informative and invariant under monotonic transformation. For a Bernoulli distribution, the Jeffreys prior has the distribution of a $Beta(\frac{1}{2}, \frac{1}{2})$ random variable. The probability density function of a Beta random variable, $X \sim Beta(\alpha, \beta)$, is

$$P(x) = x^{\alpha-1}(1-x)^{\beta-1} * \frac{\Gamma(\alpha+\beta)}{\Gamma(\alpha)\Gamma(\beta)} \tag{8}$$

The prior probability of the dropout rate, r, is then

$$P(r) = r^{-\frac{1}{2}}(1-r)^{-\frac{1}{2}} * \frac{\Gamma(1)}{\Gamma(\frac{1}{2})\Gamma(\frac{1}{2})} \tag{9}$$

The likelihood of the dropout rate can be computed as the conditional density of observing the number of dropouts, d, and non-dropouts, x, in the data given a fixed dropout rate r. Thus, the likelihood can be modeled as the conditional probability density function of a Binomial random variable with d successes in $d + x$ trials given the rate of success, r.

$$L(d, x|r) = \binom{d+x}{d} r^d (1-r)^x \tag{10}$$

Next, I compute the posterior as proportional to the product of the prior and the likelihood.

$$P(r|d, x) \propto r^{-\frac{1}{2}}(1-r)^{-\frac{1}{2}} * \frac{\Gamma(1)}{\Gamma(\frac{1}{2})\Gamma(\frac{1}{2})} * \binom{d+x}{x} r^d (1-r)^x \tag{11}$$

After dropping all multiplicative constants that do not depend on r, I have

$$P(r|d,x) \propto r^{d-\frac{1}{2}}(1-r)^{x-\frac{1}{2}} \tag{12}$$

Finally, I can introduce a new multiplicative constant to show that the posterior is proportional to a Beta distribution.

$$P(r|d,x) \propto r^{d-\frac{1}{2}}(1-r)^{x-\frac{1}{2}} * \frac{\Gamma(d+x+1)}{\Gamma(d+\frac{1}{2})\Gamma(x+\frac{1}{2})} \tag{13}$$

I have shown that the posterior can be modeled as $Beta(d+\frac{1}{2}, x+\frac{1}{2})$, where d is the observed number of dropouts and x is the observed number of non-dropouts. Following the equation for variance of a Beta random variable (2.6), the posterior variance of the dropout rate is

$$Var(r|d,x) = \frac{(d+\frac{1}{2})(x+\frac{1}{2})}{(d+x+1)^2(d+x+2)} \tag{14}$$

I calculate the inverse variance weight as $\frac{1}{Var(r|d,x)}$ for each school district dropout rate observation in the data set. I calculate inverse variance separately for male and female dropout rates with only male or female dropout and non-dropout counts, respectively. For the difference between female and male dropout rates, I compute the inverse variance for the total 12th dropout rate.[19]

6 Results

6.1 Teen Birth Rate Analysis

The difference-in-differences analysis shows that the income-based expansions had significant negative impacts on the teen birth rate. Table 4 presents the difference-in-differences regres-

[19] I also run the regressions on male and female 12th grade dropout rates with inverse variance for the total 12th grade dropout rate. These regressions produce very similar results.

sions without weights. The effect of the income-based waiver is negative in all regressions. However, after including state and year fixed effects in Columns (3) and (4), the coefficient is no longer significant. Column (3) shows that the postpartum-based waiver lowered the teen birth rate by 7.90% at the 99% confidence level, but this effect is no longer significant after controlling for state-specific linear time trends.

Table 5 presents the difference-in-differences regressions weighted by the teenage female population in each state and year. With weights, the effect of the income-based waiver is significant and negative in all four regressions. Column (3) shows the results with state and year fixed effects. The income-based waiver is estimated to have lowered the teen birth rate by 8.90% at the 95% significance level. This estimate becomes smaller with the inclusion of state-specific linear time trends in Column (4), but it remains significant. Column (4) shows that the income-based waiver decreased the teen birth rate by 6.5% at the 95% confidence level. As the mean teen birth rate is 44.3, this equates to a decline of 2.88 births per 1,000 females ages 15-19. The magnitude of this effect is comparable to the results of Kearney and Levine (2009, 2015). Kearney and Levine (2009) employ a similar difference-in-differences approach and find that income-based waivers lowered the teen birth rate by more than 4%. Kearney and Levine (2015) add controls for health care and welfare reforms and estimate that income-based waivers reduced the teen birth rate by more than 5%.

Given this finding, I expect the triple difference-in-differences analysis to show similar negative impacts on the teen birth rate. Table 6 shows the unweighted results of the triple difference-in-differences model. The results are consistent across all four regressions. In a hypothetical state with 0% teenage females living below 200% of the FPL, the income-based waiver had a positive effect on the teen birth rate. As the percent of teenage females living below 200% of the FPL increases, the effect of the income-based waiver on the teen birth rate decreases. Column (4) includes state fixed effects, year fixed effects, and state-specific linear time trends. Column (4) shows that with 0% teenage females living below 200% of the FPL, the income-based waiver increased the teen birth rate by 31.1% relative to untreated states at the 99% significance level. For a 1 percentage point increase in the percent of teenage females living below 200% of the FPL, the income-based waiver decreased the teen birth rate by 0.90% at the 99% significance level. The national percent of teenage females living below 200% of the FPL is 38.4%. Thus, at the national average, the overall expected impact of the policy would be $0.311 - 0.009 * 38.4 = -0.0346$, a decrease in the teen birth rate of 3.46% relative to untreated states. Given the mean teen birth rate of 44.3, this would be a decrease of 1.53 births per 1,000 females ages 15-19.

The results are very similar after including population weights. Table 7 shows the triple difference-in-differences regressions weighted by state population of teenage females. Column (4) again includes state fixed effects, year fixed effects, and state-specific linear time trends. With 0% teenage females living below 200% of the FPL, the income-based waiver increased the teen birth rate by 32.3% relative to untreated states at the 99% significance level. For a 1 percentage point increase in the percent of teenage females living below 200% of the FPL, the income-based waiver decreased the teen birth rate by 0.90% at the 99% significance level. At the national average, the overall expected impact of the policy would be $0.323 - 0.009 * 38.4 = -0.0226$, a decrease in the teen birth rate of 2.26% relative to untreated states. Thus, the effect on the birth rate is smaller than but still consistent with the effect found in the difference-in-differences.

6.2 12th Grade Dropout Rate Analysis

The effects on the 12th grade dropout rates are less robust. Table 8 presents the results of the triple difference-in-differences regressions on the female 12th grade dropout rate. The income-based waiver has a significant impact with school district and year fixed effects in Columns (3) and (4). These regressions estimate the expansion increased the female 12th grade dropout rates in high poverty districts relative to low poverty at the 90% significance level. The income-based waiver had no significant effect in low poverty school districts. Although these results are counter-intuitive, it is still possible the policy decreased the female

12th grade dropout rate in high poverty, treated school districts relative to the male 12th grade dropout rate.

Table 9 presents the triple difference-in-differences regressions on the male 12th grade dropout rate. The income-based waiver only has a significant effect with school district and year fixed effects in Column (3). Column (3) estimates the expansion increased the male 12th grade dropout rate in high poverty school districts by 0.70% at the 95% confidence level and had no impact on low poverty school districts. Column (4) shows a similarly positive effect, but this effect is not significant. Note that the regression on female 12th grade dropout rate with school district and year fixed effects, shown in Column (3) of Table 8, estimated a smaller, positive triple interaction coefficient. These results suggest that the income-based waiver may have lowered the female dropout rate relative to the male dropout rate in high poverty, treated school districts, controlling for changes in the dropout rate difference in low poverty, treated school districts and untreated school districts.

To further investigate this relationship, I regress the triple difference-in-differences on the difference between the female and male 12th grade dropout rate. Table 10 shows the results of this quadruple difference-in-differences. With school district and year fixed effects, Column (3) shows that the income-based waiver lowered the difference in dropout rates in high poverty school districts relative to low poverty school districts. Additionally, the income-based waiver had no significant impact on low poverty school districts. Since decreases in the teen birth rate may have spillover effects for male educational outcomes, this result could underestimate the impact of the income-based expansions on the female dropout rate. However, Column (4) shows that the effect is no longer significant after including state-specific linear time trends. Thus, it may not be robust. Column (4) also predicts that the postpartum-based waivers reduced the difference in 12th grade dropout rates. As the postpartum-based waivers had no significant effect on the teen birth rate, this finding is likely spurious.

6.3 Robustness Checks

As a robustness check, I produce non-parametric estimates for each of the triple difference-in-differences regressions.

For the teen birth rate triple difference-in-differences, I regress teen birth rate on the interactions between the base year percent of teenage females living below 200% of the FPL and indicators for years since policy implementation. I include state and year fixed effects. Then, I plot the coefficients by years since policy implementation. The variable for years since policy implementation is missing for untreated states, so the regression excludes states that did not implement an income-based waiver by 2010. Thus, the coefficients capture the effect of treatment in high poverty states relative to low poverty states.

Figure 16 shows the unweighted results. The effect of the percent of teenage females living below 200% of the FPL is flat and insignificant prior to treatment. However, this effect becomes increasingly negative in the years after policy implementation. Figure 17 shows the results with regressions weighted by state population of teenage females. The coefficients in the weighted regressions shows a similar flat pre-trend and a strong downward post-trend. These graphs bolster the significant negative effects I find in my teen birth rate regressions.

For the 12th grade dropout rate triple difference-in-differences, I perform a similar check. I regress dropout rate on the interactions between high poverty school district and indicators for years since policy implementation with school district and year fixed effects. I weight each regression by the inverse variance of the dropout rate. Thus, the plots show the effect of treatment in high poverty school districts relative to low poverty school districts.

Figure 18 shows the estimated coefficients with female and male 12th grade dropout rate as the dependent variables. The effect of high school district child poverty rate on the female 12th grade dropout rate is flat before and after policy implementation. The effect on the male 12th grade dropout rate shows a slight upward post-trend. Figure 19 plots the results of the regression on the difference in dropout rates. The effect on the difference in dropout rates is flat before and after treatment, and the coefficients are not statistically significant at the 95% level. These estimates suggest that the income-based waivers do not have significant impacts on the 12th grade dropout rates in my data.

7 Discussion

7.1 Estimating the Magnitudes of the Effects

To interpret the results, I calculate the magnitude of the effects among treated populations. Earlier in this paper, I estimated that 35.7% of females ages 15-19 live in the 12 treated states with 1995 NCHS and U.S. Census Bureau population data.[20] The effect of the income-based waivers in treated states is the overall effect divided by the proportion of teenage females living in treated states. Thus, the estimated impact in treated states is $-6.5/0.357 = -18.2\%$. The income-based waivers lowered the teen birth rate in treated states by 18.2%. Given the mean teen birth rate of 44.3, this signifies a decrease of 8.06 births per 1,000 females ages 15-19 in treated states.

The triple difference-in-differences regressions predict that a 1 percentage point increase in teenage females living below 200% of the FPL decreased the teen birth rate by 0.90%. To interpret the magnitude, I first assume that the change in the overall birth rate was solely driven by a decrease in the birth rate among the marginal 1% of teenage females living below 200% of the FPL. Then, the treatment effect would be $-0.90/0.01 = -0.90$, a 90% decrease in the mean teen birth rate. Given the mean teen birth rate of 44.3, the income-based waiver lowered the teen birth rate among the marginal 1% of teenage females living below 200% of the FPL by 39.9 births per 1,000 teenage females.

Next, I calculate the estimated effect of the income-based waiver on the overall teen birth rate based on the triple difference-in-differences regression. The overall effect is the sum of

[20] I rely on population data from 1995 because the estimates are pre-treatment across all states.

the effect of the income-based waiver multiplied by the percent of teenage females living in treated states and the treatment effect multiplied by the percent of teenage females living below 200% of the FPL in treated states. Recall that the coefficient on the income-based waiver alone is 0.323. As 35.7% of teenage females lived in treated states, the effect of the income-based waiver with 0% teenage females living below 200% of the FPL is $0.323*0.357 = 0.115$, an 11.5% increase in the overall teen birth rate. Based on the 1995 population data, I estimate that 15.8% of teenage females lived in treated states and had incomes below 200% of the FPL. Thus, the impact of the treatment effect on the overall teen birth rate is $.158 * -0.90 = -0.142$. The overall effect of the income-based waiver is thus $0.115 - 0.142 = -0.027$, a 2.7% decrease in the national teen birth rate. Although the treatment effect is very large, the impact on the overall teen birth rate is smaller than the effect predicted by the difference-in-differences regression.

To further assess the validity of the treatment effect, I estimate the pre-treatment teen birth rate among females living below 200% of the FPL. Since the Vital Statistics data does not have information on mother's family income, I rely on data from The Opportunity Atlas project (Chetty, et al., 2018). The Opportunity Atlas reports outcomes for a nationally representative sample of individuals born from 1978 to 1983 by parent's income percentile. People in this data set were between the ages of 15 and 19 from 1993 to 2002, which overlaps with the early years of my data.[21] Overall, 19.8% of women in the sample had a teen birth. Dividing this proportion by four and multiplying by ten gives a rough estimate for the teen birth rate among females ages 15-19.[22] Thus, the national teen birth rate in this sample

[21] I cannot split the Opportunity Atlas data into smaller birth cohorts. Thus, this teen birth rate estimate includes post-treatment estimates in some states.

[22] The probability of a female ages 15-19 having a child is equal to the teen birth rate divided by ten. For example, the mean teen birth rate of 44.3 implies that teenage females have a 4.43% chance of giving birth in each year. The overall fraction of adult women who had a teen birth between the ages of 15-19 is approximately four times the average probability of a teen birth in each year. Thus, dividing the fraction of adult women who had a teen birth by four and multiplying by ten approximates the teen birth rate among females ages 15-19. However, the Opportunity Atlas defines a teen birth as a birth to a female between the ages of 13 and 19. Still, as the proportion of teen births among younger teens is relatively low, I can use the percent of women with a teen birth between the ages of 13 and 19 to roughly estimate the teen birth rate among 15-19 year old females (Kearney and Levine, 2009).

is 49.5 births per 1,000 females ages 15-19. For comparison, the national teen birth rate between 1995 and 2002 in the Vital Statistics data is also 49.5 births per 1,000 females ages 15-19. As the national percent of females ages 15-19 living below 200% of the FPL is 38.4% in my data set, I approximate the birth rate in this population with data for women below the 39th percentile of parental income in the Opportunity Atlas. 31.6% of women in this group had a teen birth.[23] Thus, the 1995-2002 teen birth rate among teenage females living below 200% of the FPL is approximately 78.9, 59.4% higher than the national teen birth rate.

Based on these statistics, I estimate the 1995-2010 teen birth rate among teenage females living below 200% of the FPL to be 59.4% higher than the national average of 44.3. Then, the teen birth rate in the treated group is approximately 70.6 births per 1,000 females ages 15-19. Thus, a decrease of 39.9 births per 1,000 females equates to a decrease of 56.5% of the teen birth rate among the marginal percentage teenage females living below 200% of the FPL. Although the magnitude of the effect is still large, it seems plausible.

Furthermore, the decrease in the teen birth rate may also be dispersed among all teenage females living below 200% of the FPL, rather than concentrated in the marginal 1 percentage point. As described earlier, there could be network effects in the expansion of Medicaid family planning programs. As the percent of teenage females living below 200% of the FPL increases, take-up of Medicaid family planning services may increase, lowering the teen birth rate among all eligible women.

7.2 The Relationship Between Teen Birth Rate and 12th Grade Dropout Rate

Although the income-based waivers had significant negative impacts on the teen birth rate, I find little effect on the female 12th grade dropout rate. However, it is possible that the

[23] This estimate is not very sensitive to the choice of parental income percentile. The percentages of women who had a teen birth with parental income below the 35th percentile and the 45th percentile are both within 1 percentage point of the estimate among women with parental income below the 39th percentile.

true effect of the income-based waivers on the female 12th grade dropout rate is negative. The limitations of my school district-level data set may hinder my ability to identify such effects. For example, the teen birth rate regressions estimate the effect on females ages 15-19 while I only consider 12th grade dropouts, who are likely between the ages of 17 and 19. Thus, my results overestimate the impact of the income-based waivers on the teen birth rate among 12th graders. As not all teen mothers drop out of high school, the expected impact on the female high school dropout rate would be even smaller. In addition, recall that the school district data only includes observations from eight of the 12 treated states, reducing the power of my estimates. Thus, it is plausible that the income-based waivers reduced the female 12th grade dropout rate, but the true effect is too small for my data set to identify. It may be especially difficult to estimate the true effect with my school district data because the observations seem skewed towards lower child poverty.[24]

However, it is also possible that the income-based waivers lowered teen birth rates with no impact on the high school dropout rate. As Kearney and Levine (2016) argue, teen birth may not cause lower economic outcomes. Instead, teen birth may simply be indicative of characteristics that lead to lower economic and educational outcomes (Kearney and Levine, 2016). In this case, my estimates may reflect the true lack of significant effects.

8 Conclusion

With a difference-in-differences model, I find that income-based Medicaid family planning eligibility expansions lowered the overall teen birth rate by 6.5%, aligning with prior literature (Kearney and Levine, 2009, 2015). Then, I interact the presence of an income-based eligibility expansion with the base year percent of females ages 15-19 living below 200% of the federal poverty line. This triple difference-in-differences model further isolates the treated group. I show that the income-based waivers only decreased the teen birth rate in areas with an

[24]In the data set, 18.1% of 12th graders live in states with income-based waivers. Furthermore, only 6.26% of 12th graders in the data live in high poverty school districts in treated states.

above average percent of teenage females living below 200% of the FPL. Furthermore, a 1 percentage point increase in teenage females living below 200% of the FPL decreased the teen birth rate by 0.90%.

Despite these robust results, I estimate that the expansions had little effect on the high school dropout rate. These findings may be driven by biases in the dropout rate data. The data is skewed towards lower poverty school districts and and only captures the treatment effect in eight of the 12 states. Although I do not find effects on the 12th grade dropout rate, existing literature shows that teen birth has robust, negative impacts on the economic and educational outcomes of the children of teen mothers (Kearney and Levine, 2012). Thus, the income-based expansions may still improve economic outcomes for future generations by lowering the teen birth rate among low-income women.

Future research should continue to explore the impacts of the income-based waivers on the educational and economic outcomes of teen mothers. With individual-level data on family income and teen birth, one could more precisely identify the treatment effect by directly evaluating eligibility. As income-based waivers may only affect high school completion through changes to the teen birth rate, these expansions could serve as a valuable instrument to evaluate the causality between teen motherhood and lower economic outcomes.

Tables and Figures

Table 1. School District Observations by Year

Year	School District Observations
1995	1,472
1996	1,732
1997	2,858
1998	5,071
1999	6,633
2000	6,448
2001	7,699
2002	8,617
2003	8,236
2004	8,320
2005	9,191
2006	8,977
2007	8,789
2008	9,540
2009	9,421
2010	9,118

Source: 1995-2010 samples of the National Center for Education Statistics' Common Core of Data Restricted Use Sample.

Table 2. Policy Implementations by State

State	Income Waiver Implemented	Postpartum Waiver Implemented	Ages	Genders
Alabama	October 1, 2000	October 1, 2000	19-55	F
Arizona		August 1, 1995		
Arkansas	September 1, 1997		All	F
California	January 1, 1997		All	All
Delaware		January 1, 1996		
Florida	October 1, 1998	October 1, 1998	All	F
Illinois	April 1, 2004	April 1, 2004	19-44	F
Iowa	February 1, 2006		All	All
Louisiana	October 1, 2006		19-44	F
Maryland	July 1, 1997	February 1, 1995	All	All
Michigan	April 1, 2006		19-44	F
Minnesota	July 1, 2006		15-50	All
Mississippi	October 1, 2003		All	F
Missouri	November 6, 2008	May 1, 1998	18-44	F
New Mexico	July 1, 1998	December 1992	18-55	F
New York	October 1, 2002	October 1, 2002	All	All
North Carolina	October 1, 2005		18-55	All
Oklahoma	April 1, 2005		19-55	All
Oregon	January 1, 1999		All	All
Pennsylvania	June 1, 2007		18-44	F
Rhode Island		August 1, 1994	All	F
South Carolina	July 1, 1997	July 1, 1994	All	F
Texas	January 1, 2007		18-44	F
Virginia	January 1, 2007	October 1, 2002	All	F
Washington	July 1, 2001		All	All
Wisconsin	January 1, 2003		19+	All

Note: Virginia implemented an income-based waiver on October 1, 2002. However, the waiver was extended to women of all ages in 2007, as reflected in the table. Wisconsin's income-based waiver claimed to include all women of childbearing age. However, individuals under the age of 19 did not qualify for the expansion given Wisconsin's eligibility limits for children's Medicaid. Thus, I count Wisconsin as only treating those above the age of 19. For my analysis, I exclude policy changes that are not available to all ages. In addition, I exclude policy changes that occurred after 2010 as they do not overlap with my data set.

Sources: Kearney and Levine (2009, 2015) list policy implementations by state. I cross-referenced their policy data with waiver demonstration fact sheets available through Medicaid.gov and reports of state policy changes from the Guttmacher Institute and Henry J. Kaiser Foundation.

Table 3. Percent of Females Ages 15-19 Affected by Income-Based Waivers in Year of Policy Implementation

State	**Eligible Income Limits Relative to the Federal Poverty Line**		**Percent of Households With Females Ages 15-19 Within Limits**
	Lower	**Upper**	
Arkansas	100%	200%	33.6%
California	100%	200%	19.7%
Florida	100%	185%	18.9%
Iowa	133%	300%	37.8%
Maryland	33%	200%	14.7%
Minnesota	150%	200%	8.63%
Mississippi	100%	185%	20.5%
New York	133%	200%	14.8%
Oregon	100%	250%	39.4%
South Carolina	150%	185%	10.1%
Virginia	133%	200%	12.0%
Washington	200%	250%	8.15%

Note: This table is restricted to states with income-based waivers that included teenagers. Most states defined eligible individuals as those below a certain income threshold relative to the federal poverty line who were not otherwise eligible for Medicaid. Thus, eligible individuals had household incomes lower than the specified upper limit but higher than the general Medicaid eligibility threshold. However, in California and Arkansas eligibility only required that the individual be otherwise uninsured for family planning services. Thus, I still use the general Medicaid eligibility limit as the lower limit.

Sources: The waiver demonstration fact sheets from Medicaid.gov included information on upper income limits. For lower income limits, I relied on reports from the Henry J. Kaiser Family Foundation that detailed Medicaid family income thresholds for children ages 6-19 from 1997 through 2009, the years of policy implementation across states. To calculate the percent of households within limits, I gathered data on household income relative to the federal poverty line from the Poverty Sample of the Current Population Survey in each state's year of implementation.

Table 4. State-Level Teen Birth Rate Difference-in-Differences Unweighted

	Dependent variable:			
	Log Teen Birth Rate *mean teen birth rate = 44.3*			
	(1)	(2)	(3)	(4)
Income-Based Waiver	-0.143^{***}	-0.176^{***}	-0.055	-0.028
	(0.038)	(0.033)	(0.035)	(0.026)
Postpartum-Based Waiver	0.109^{***}	0.038	-0.079^{***}	-0.008
	(0.032)	(0.028)	(0.030)	(0.023)
50/10 Income Percentile Ratio		-0.162^{***}	0.004	0.007
		(0.024)	(0.012)	(0.012)
Unemployment Rate		0.019^{***}	-0.009	-0.004
		(0.006)	(0.007)	(0.006)
Percent White		-1.100^{***}	-0.253	-0.131
		(0.077)	(0.203)	(0.116)
Percent Hispanic		0.859^{***}	-0.772^{*}	0.031
		(0.106)	(0.415)	(0.387)
CHIP		-0.195^{***}	0.029^{**}	0.010
		(0.021)	(0.013)	(0.010)
Constant	3.736^{***}	5.225^{***}		
	(0.013)	(0.133)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.024	0.326	0.936	0.960
Adjusted R^2	0.021	0.321	0.929	0.952
F Statistic	9.817^{***}	55.943^{***}		

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Note: Note: All regressions have standard errors clustered at the state-level.

Table 5. State-Level Teen Birth Rate Difference-in-Differences with Population Weights

	Dependent variable:			
	Log Teen Birth Rate			
	mean teen birth rate = 44.3			
	(1)	(2)	(3)	(4)
Income-Based Waiver	−0.157***	−0.262***	−0.089**	−0.065**
	(0.027)	(0.024)	(0.040)	(0.028)
Postpartum-Based Waiver	−0.026	0.004	−0.062	−0.025
	(0.030)	(0.023)	(0.039)	(0.021)
50/10 Income Percentile Ratio		−0.325***	−0.011	−0.007
		(0.022)	(0.014)	(0.012)
Unemployment Rate		−0.006	0.004	0.005
		(0.005)	(0.008)	(0.005)
Percent White		−1.363***	−0.106	−0.174
		(0.098)	(0.227)	(0.130)
Percent Hispanic		1.047***	−0.420	−0.396
		(0.070)	(0.579)	(0.353)
CHIP		−0.221***	0.016	0.001
		(0.018)	(0.013)	(0.011)
Constant	3.805***	6.232***		
	(0.012)	(0.138)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.050	0.454	0.952	0.973
Adjusted R^2	0.048	0.449	0.948	0.968
F Statistic	21.562***	95.869***		

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by the state-level 15-19 year old female population in each year.

Table 6. State-Level Teen Birth Rate Triple Difference-in-Differences Unweighted: Income-Based Waiver x Base Percent Females 15-19 Below 200% FPL

	Dependent variable:			
	Log Teen Birth Rate *mean teen birth rate = 44.3*			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.368	0.647*	0.405***	0.311***
	(0.421)	(0.339)	(0.149)	(0.083)
Income-Based Waiver x Percent Females 15-19 $\leq$ 200% FPL	−0.011	−0.020**	−0.011***	−0.009***
	(0.010)	(0.008)	(0.004)	(0.002)
Postpartum-Based Waiver	0.082	0.009	−0.071***	−0.001
	(0.084)	(0.061)	(0.027)	(0.020)
50/10 Income Percentile Ratio		−0.212***	−0.001	0.002
		(0.052)	(0.012)	(0.011)
Unemployment Rate		0.041***	−0.010	−0.002
		(0.014)	(0.007)	(0.006)
Percent White		−0.998***	−0.323*	−0.155
		(0.233)	(0.194)	(0.122)
Percent Hispanic		0.667***	−0.737*	0.051
		(0.256)	(0.403)	(0.416)
CHIP		−0.093	0.027**	0.007
		(0.064)	(0.013)	(0.011)
Constant	3.054***	4.568***		
	(0.161)	(0.390)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.334	0.541	0.940	0.962
Adjusted R^2	0.317	0.527	0.932	0.954

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level.

Table 7. State-Level Teen Birth Rate Triple Difference-in-Differences with Population Weights: Income-Based Waiver x Base Percent Females 15-19 Below 200% FPL

	Dependent variable:			
	Log Teen Birth Rate *mean teen birth rate = 44.3*			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.590 (0.510)	0.764* (0.419)	0.545*** (0.150)	0.323*** (0.065)
Income-Based Waiver x Percent Females 15-19 $\leq$ 200% FPL	−0.018 (0.011)	−0.023** (0.009)	−0.015*** (0.004)	−0.009*** (0.002)
Postpartum-Based Waiver	−0.040 (0.076)	−0.040 (0.049)	−0.059 (0.037)	−0.017 (0.021)
50/10 Income Percentile Ratio		−0.304*** (0.069)	−0.016 (0.014)	−0.011 (0.012)
Unemployment Rate		0.017 (0.014)	0.001 (0.008)	0.003 (0.006)
Percent White		−0.739*** (0.241)	−0.058 (0.199)	−0.106 (0.109)
Percent Hispanic		0.563** (0.230)	−0.502 (0.483)	−0.547* (0.326)
CHIP		−0.086 (0.076)	0.009 (0.013)	−0.002 (0.011)
Constant	2.828*** (0.149)	4.717*** (0.409)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.334	0.541	0.940	0.962
Adjusted R^2	0.317	0.527	0.932	0.954

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by the state-level 15-19 year old female population in each year.

Table 8. Effect of Eligibility Expansions on School District-Level Female 12th Grade Dropout Rates

	Dependent variable:			
	Female 12th Grade Dropout Rate			
	mean = 3.18			
	(1)	(2)	(3)	(4)
Income-Based Waiver	−0.058 (0.301)	0.100 (0.288)	0.067 (0.290)	0.061 (0.195)
Income-Based Waiver x High Child Poverty District	0.208 (0.631)	0.225 (0.638)	0.489* (0.274)	0.403* (0.211)
Postpartum-Based Waiver	−0.137 (0.214)	−0.125 (0.208)	−0.108 (0.184)	−0.225 (0.183)
State 50/10 Income Percentile Ratio		−0.482** (0.192)	0.074 (0.183)	−0.066 (0.110)
State Unemployment Rate		−0.012 (0.024)	−0.033 (0.082)	0.088 (0.064)
CHIP		−0.124 (0.188)	0.276 (0.289)	0.041 (0.131)
Constant	1.296*** (0.146)	3.432*** (0.830)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.061	0.066	0.506	0.521
Adjusted R^2	0.061	0.066	0.458	0.475

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by inverse variance. High poverty school districts are those with child poverty rates above the weighted mean in the first year they appear in the data set. The mean is calculated with the poverty rates of all school districts in that year, weighted by school district 12th grade enrollment.

Table 9. Effect of Eligibility Expansions on School District-Level Male 12th Grade Dropout Rates

	Dependent variable:			
	Male 12th Grade Dropout Rate			
	mean = 3.75			
	(1)	(2)	(3)	(4)
Income-Based Waiver	−0.082	0.162	0.116	0.177
	(0.375)	(0.356)	(0.374)	(0.168)
Income-Based Waiver x High Child Poverty District	0.419	0.392	0.697**	0.491
	(0.570)	(0.574)	(0.315)	(0.337)
Postpartum-Based Waiver	−0.097	−0.080	0.006	−0.063
	(0.233)	(0.228)	(0.251)	(0.252)
State 50/10 Income Percentile Ratio		−0.563**	0.195	−0.033
		(0.238)	(0.214)	(0.109)
State Unemployment Rate		−0.033	−0.033	0.092
		(0.028)	(0.086)	(0.077)
CHIP		−0.641**	0.317	0.142
		(0.315)	(0.500)	(0.278)
Constant	1.521***	4.574***		
	(0.174)	(1.041)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.058	0.068	0.532	0.549
Adjusted R^2	0.058	0.067	0.486	0.505

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by inverse variance. High poverty school districts are those with child poverty rates above the weighted mean in the first year they appear in the data set. The mean is calculated with the poverty rates of all school districts in that year, weighted by school district 12th grade enrollment.

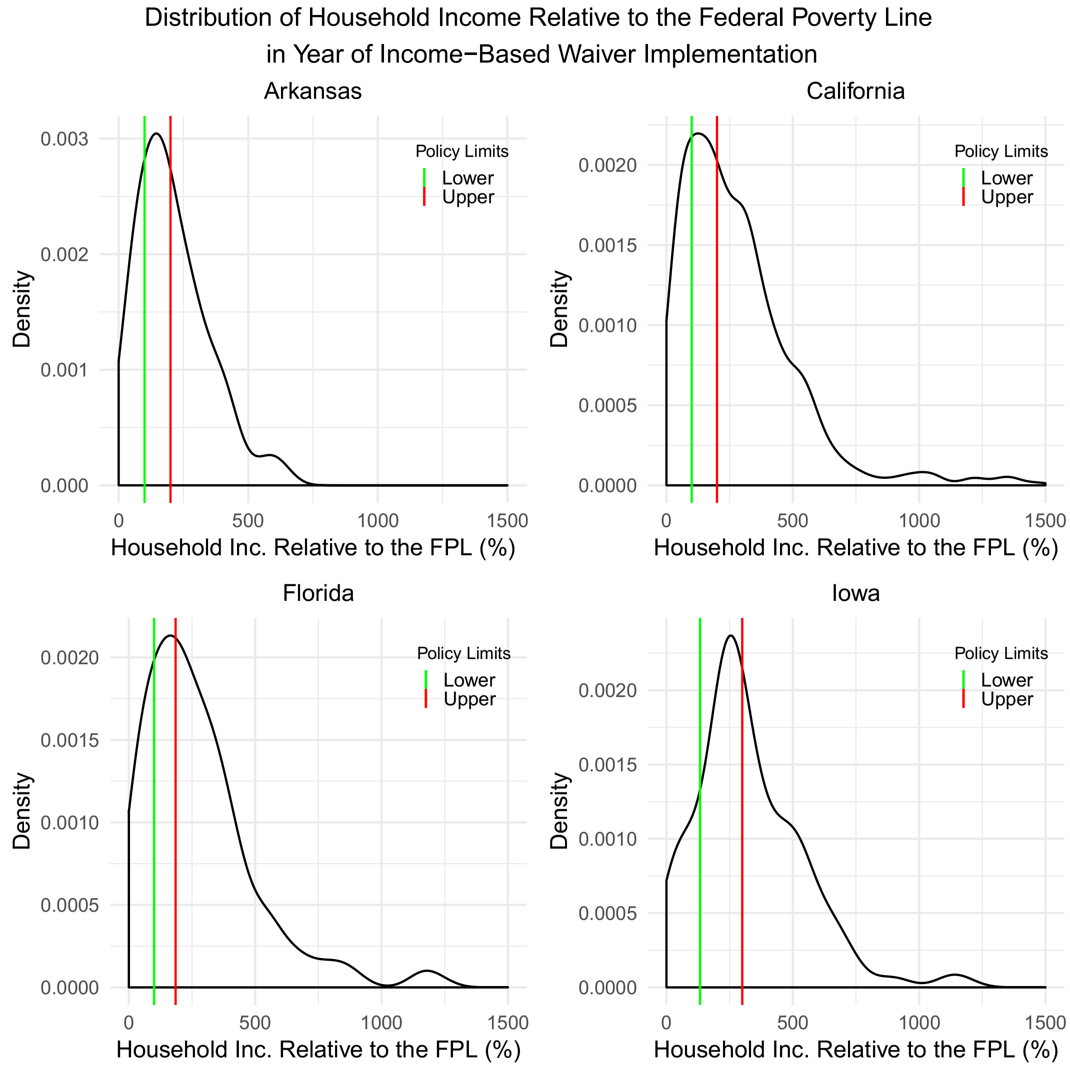
Table 10. Effect of Eligibility Expansions on the Difference in Female and Male School District 12th Grade Dropout Rates

	Dependent variable:			
	Female - Male 12th Grade Dropout Rate			
	mean = -0.57			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.100	0.016	−0.050	−0.059
	(0.098)	(0.099)	(0.120)	(0.047)
Income-Based Waiver x High Child Poverty District	−0.217**	−0.151	−0.210**	−0.097
	(0.091)	(0.094)	(0.086)	(0.142)
Postpartum-Based Waiver	−0.024	−0.029	−0.113	−0.196**
	(0.068)	(0.067)	(0.104)	(0.087)
State 50/10 Income Percentile Ratio		0.051	−0.137***	−0.052
		(0.060)	(0.044)	(0.035)
State Unemployment Rate		0.021**	−0.015	−0.019
		(0.008)	(0.026)	(0.022)
CHIP		0.499***	0.074	0.013
		(0.098)	(0.185)	(0.125)
Constant	−0.295***	−1.063***		
	(0.046)	(0.278)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.005	0.015	0.165	0.174
Adjusted R^2	0.005	0.015	0.084	0.093

*p<0.1; **p<0.05; ***p<0.01

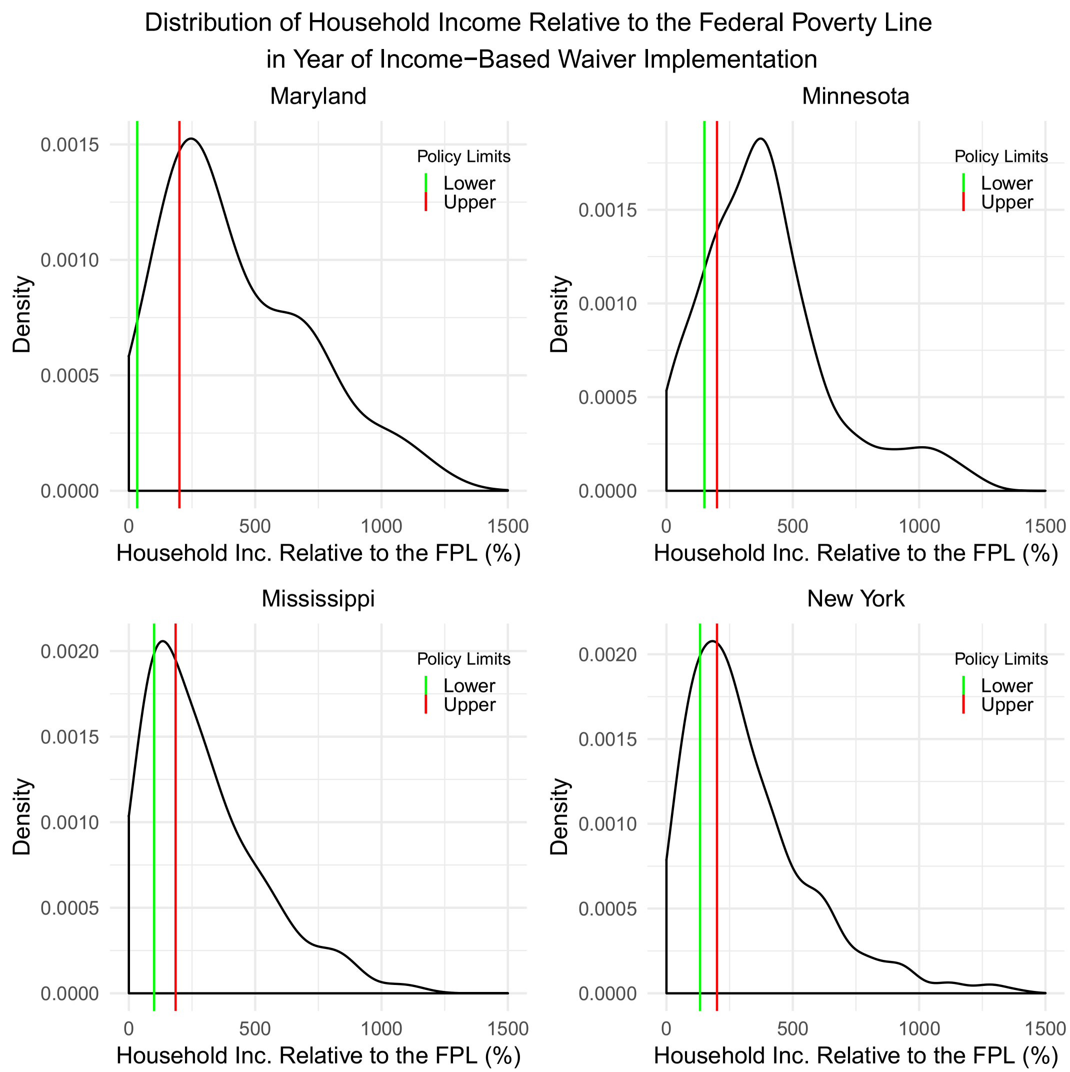
Note: All regressions have standard errors clustered at the state-level and are weighted by inverse variance. High poverty school districts are those with child poverty rates above the weighted mean in the first year they appear in the data set. The mean is calculated with the poverty rates of all school districts in that year, weighted by school district 12th grade enrollment.

Figure 1. Household Income Relative to Federal Poverty Line in Year of Policy Implementation Among Females 15-19 Years Old (Part 1)



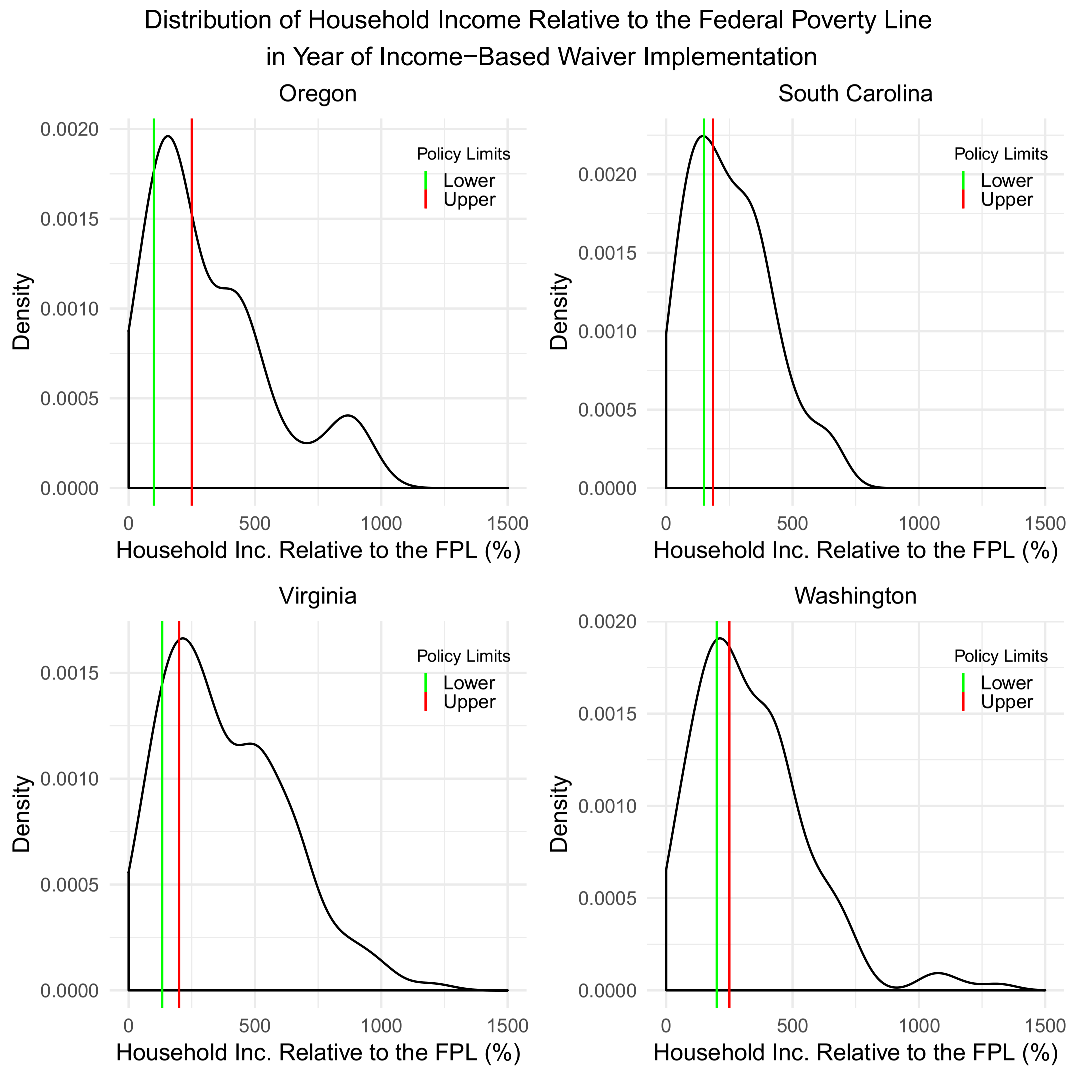
Note: Data from the 1997 through 2009 Annual Social and Economic Supplement of the Current Population Survey.

Figure 2. Household Income Relative to Federal Poverty Line in Year of Policy Implementation Among Females 15-19 Years Old (Part 2)



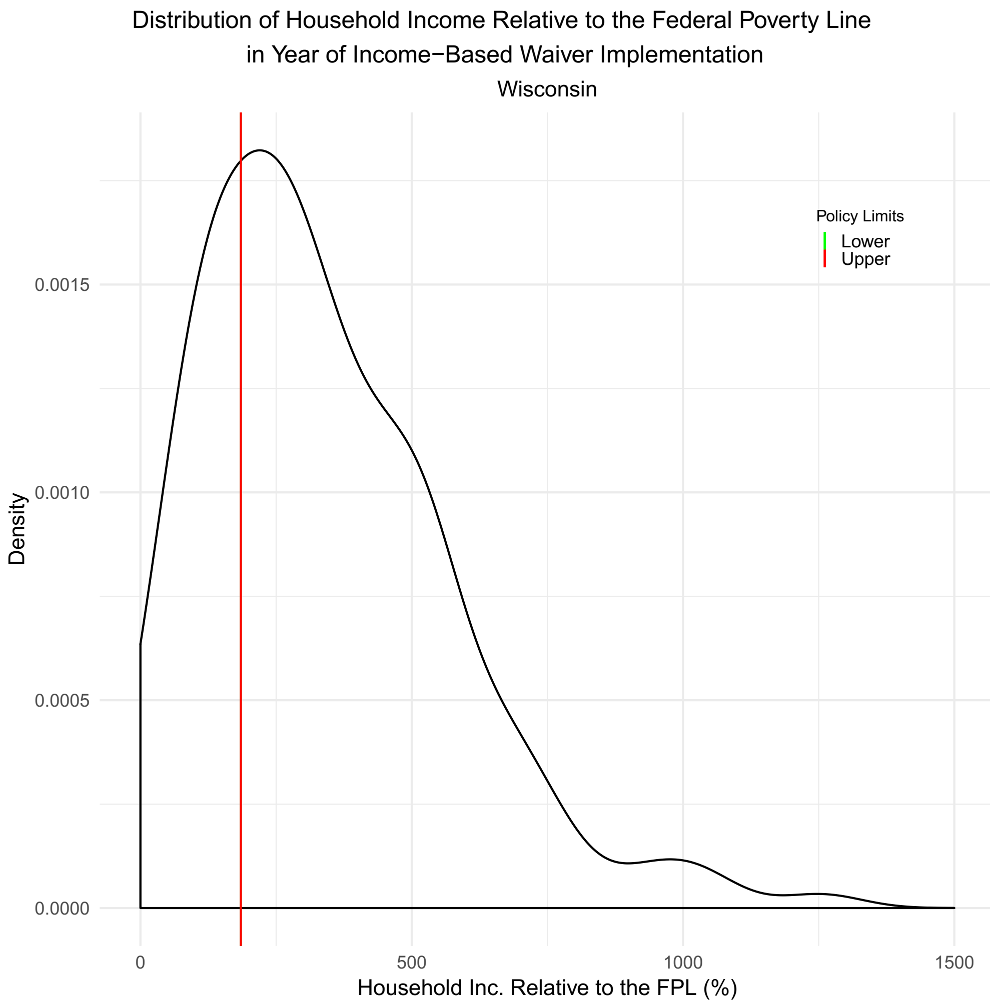
Note: Data from the 1997 through 2009 Annual Social and Economic Supplement of the Current Population Survey.

Figure 3. Household Income Relative to Federal Poverty Line in Year of Policy Implementation Among Females 15-19 Years Old (Part 3)



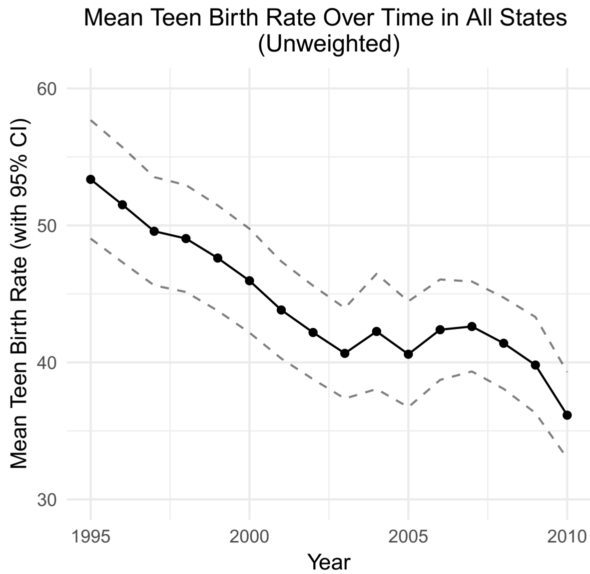
Note: Data from the 1997 through 2009 Annual Social and Economic Supplement of the Current Population Survey.

Figure 4. Household Income Relative to Federal Poverty Line in Year of Policy Implementation Among Females 15-19 Years Old (Part 4)



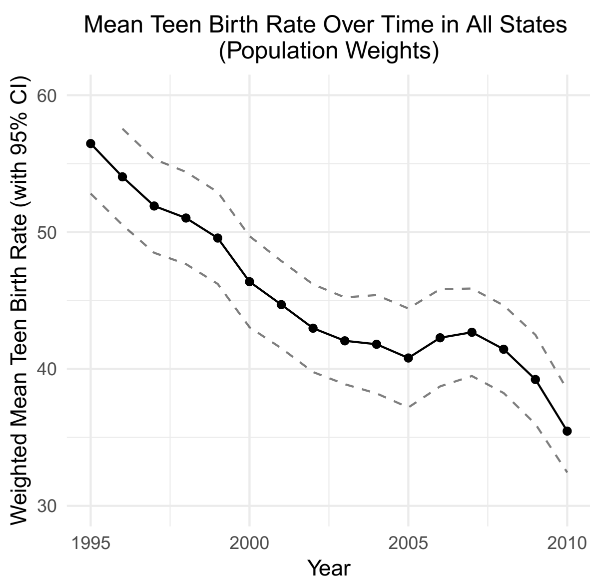
Note: Data from the 1997 through 2009 Annual Social and Economic Supplement of the Current Population Survey.

Figure 5. Mean Teen Birth Rate by Year in All States



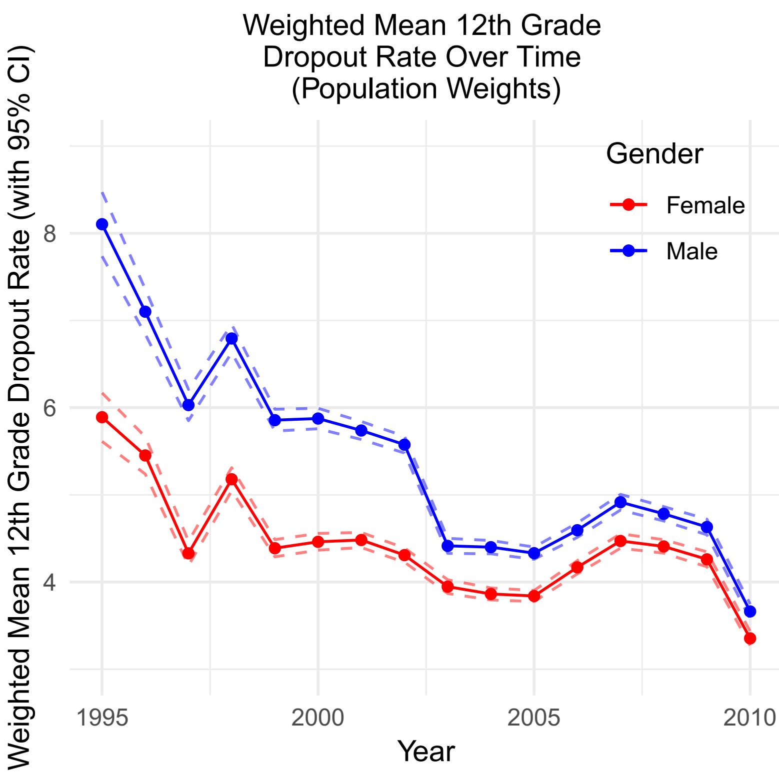
Note: Data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples.

Figure 6. Weighted Mean Time Birth Rate by Year in All States



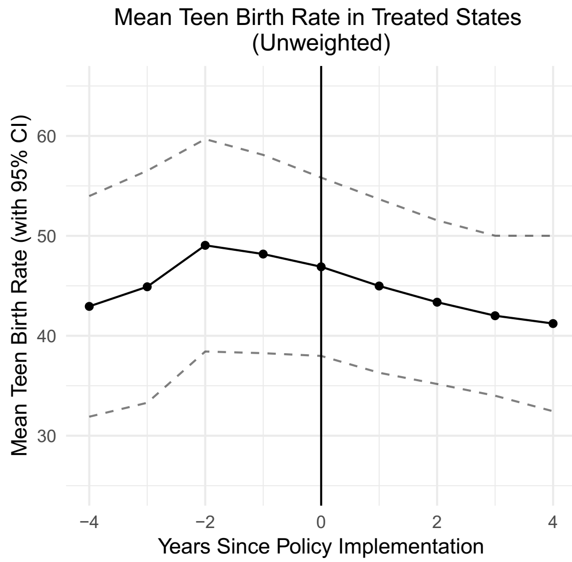
Note: Weighted by state 15-19 year old female population in each year. Data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples.

Figure 7. 12th Grade Dropout Rate by Year in All States



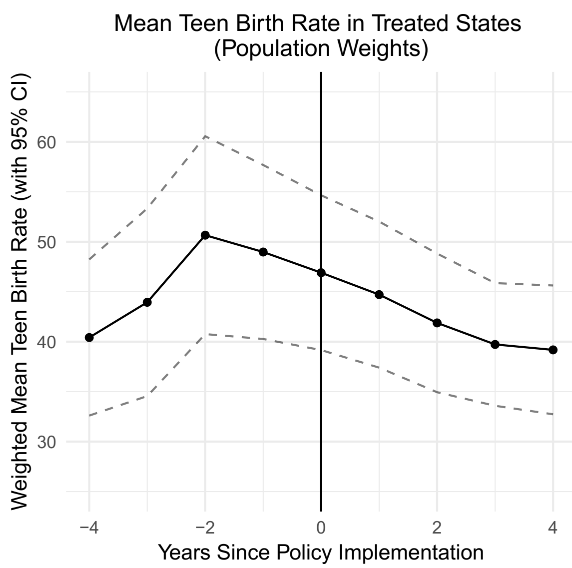
Note: Weighted by school district 12th grade enrollment in each year. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data.

Figure 8. Mean Teen Birth Rate by Years Since Policy Implementation in Treated States



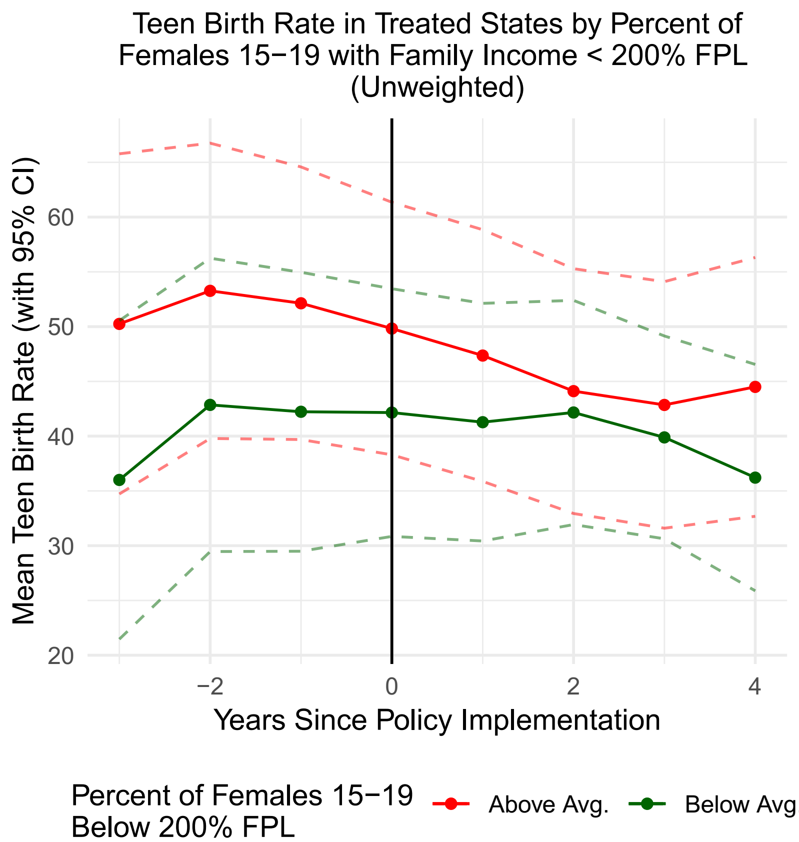
Note: Data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples.

Figure 9. Weighted Mean Teen Birth Rate by Years Since Policy Implementation in Treated States



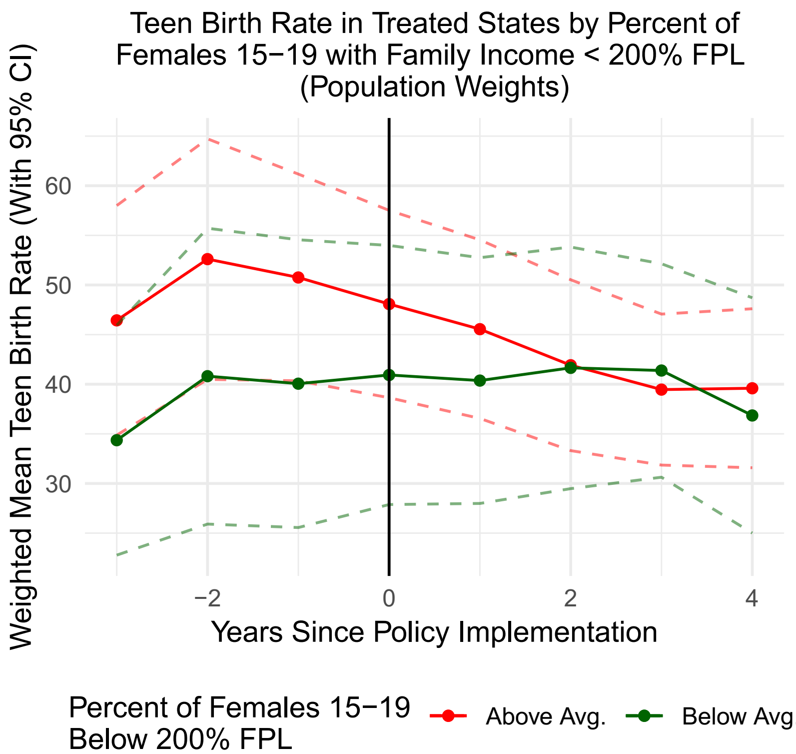
Note: Weighted by state 15-19 year old female population in each year. Data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples.

Figure 10. Mean Teen Birth Rate in Treated States with Above and Below Average State-Level Percent Females 15-19 Below 200% FPL



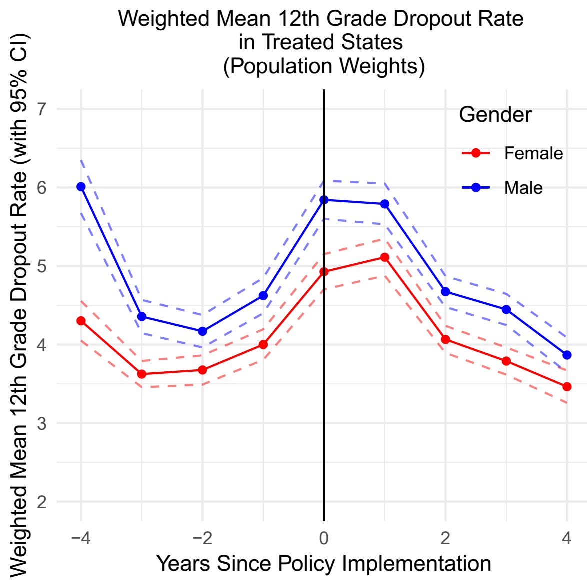
Note: Birth rate data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples. Percent females 15-19 below 200% FPL calculated from the Annual Social and Economic Supplement of the CPS. States with base year percent females 15-19 below 200% FPL above the national percent females 15-19 below 200% FPL are considered above average.

Figure 11. Weighted Mean Teen Birth Rate in Treated States with Above and Below Average State-Level Percent Females 15-19 Below 200% FPL



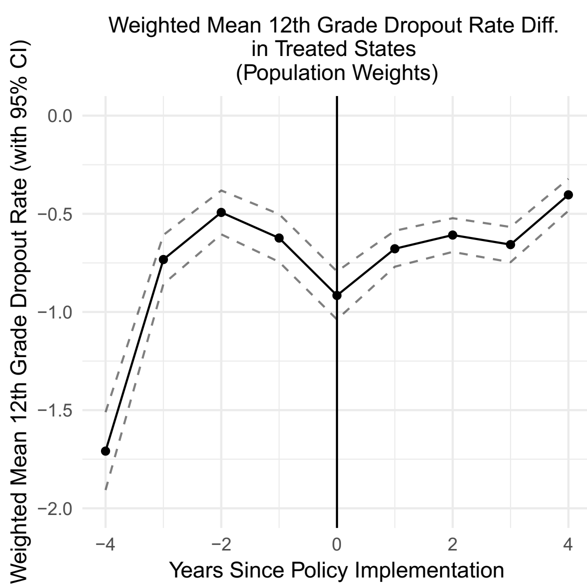
Note: Means are weighted by state-level females population ages 15-19 in each year. Birth rate data from the National Center for Health Statistics' 1995-2010 Vital Statistics samples. Percent females 15-19 below 200% FPL calculated from the Annual Social and Economic Supplement of the CPS. States with base year percent females 15-19 below 200% FPL above the national percent females 15-19 below 200% FPL are considered above average.

Figure 12. Weighted Mean 12th Grade Dropout Rate in Treated States by Gender



Note: Weighted by school district 12th grade enrollment in each year. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data.

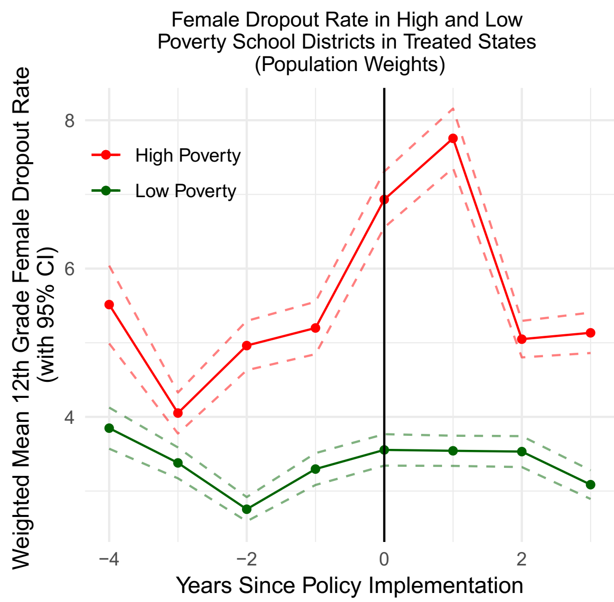
Figure 13. Weighted Mean Female - Male Dropout Rate in Treated States



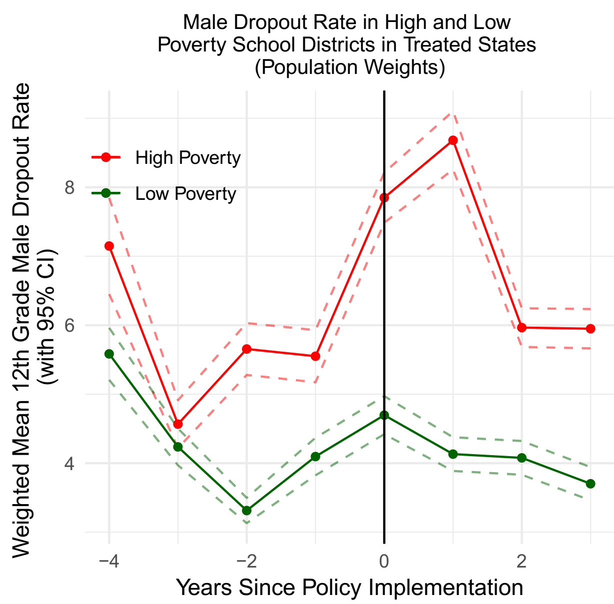
Note: Weighted by school district 12th grade enrollment in each year. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data.

Figure 14. Weighted Mean 12th Grade Dropout Rate by School District Child Poverty Rate and Gender in Treated States

(a) Female

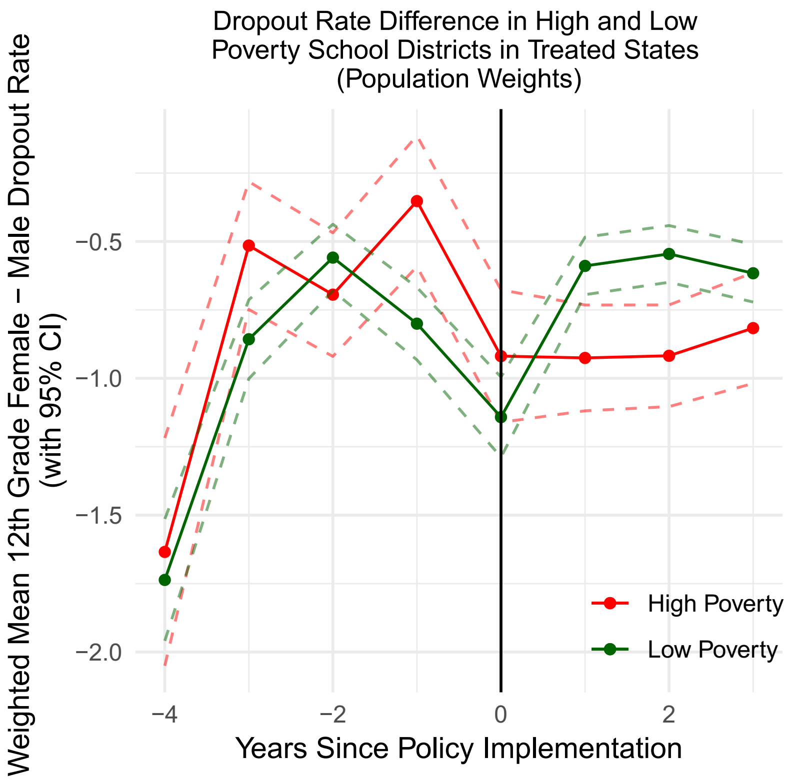


(b) Male



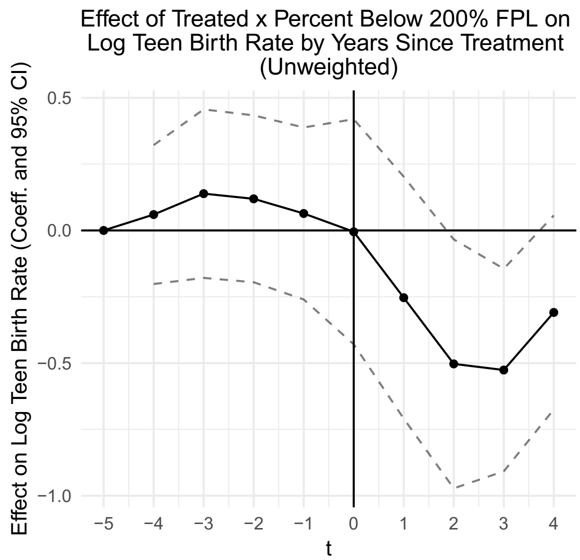
Note: Weighted by school district 12th grade enrollment. Data from the 1995-2010 Common Core of Data of the NCES and the 1995-2010 Small Area Income and Poverty Estimates. School districts with base child poverty rate above the weighted mean in their base year are considered high poverty.

Figure 15. Female - Male Dropout Rate by School District Child Poverty Rate in Treated States



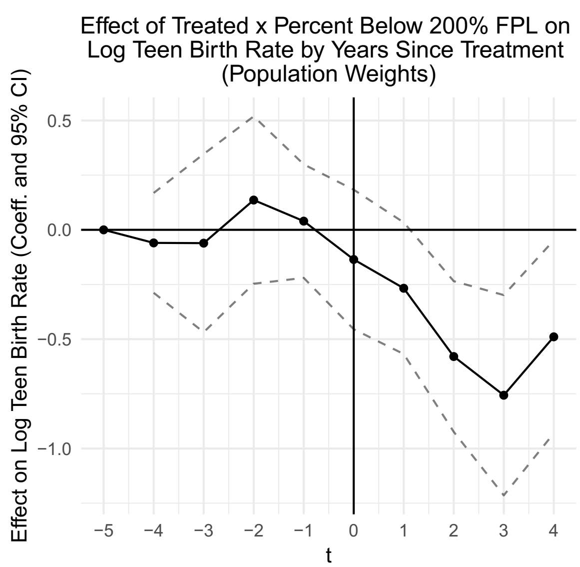
Note: Weighted by school district 12th grade enrollment in each year. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data and the 1995-2010 Small Area Income and Poverty Estimates. School districts with base child poverty rate above the weighted mean in their base year are considered high poverty. School districts below the mean are low poverty.

Figure 16. Effect of Treatment X Percent Females 15-19 Below 200% FPL on Log Teen Birth Rate



Note: The coefficient in the fifth year prior to treatment is subtracted from all coefficients as a base year. Data from the 1995-2010 Vital Statistics and the 1995-2010 ASEC of the CPS.

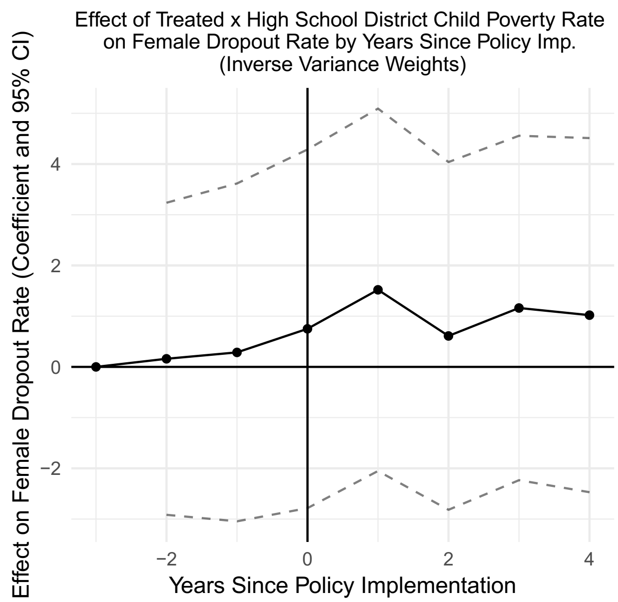
Figure 17. Effect of Treatment X Percent Females 15-19 Below 200% FPL on Log Teen Birth Rate with Population Weights



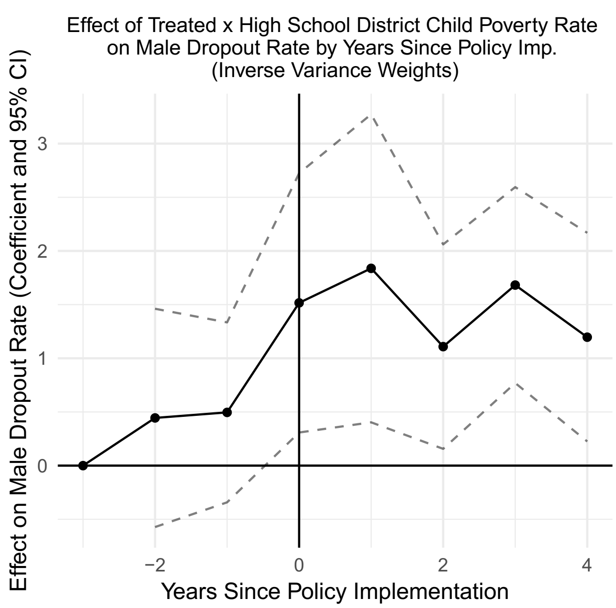
Note: The coefficient in the fifth year prior to treatment is subtracted from all coefficients as a base year. Weighted by state 15-19 year old female population in each year. Data from the 1995-2010 Vital Statistics and the 1995-2010 ASEC of the CPS.

Figure 18. Effect of Treatment in High Poverty Rate School Districts on School District 12th Grade Dropout Rate by Gender

(a) Female

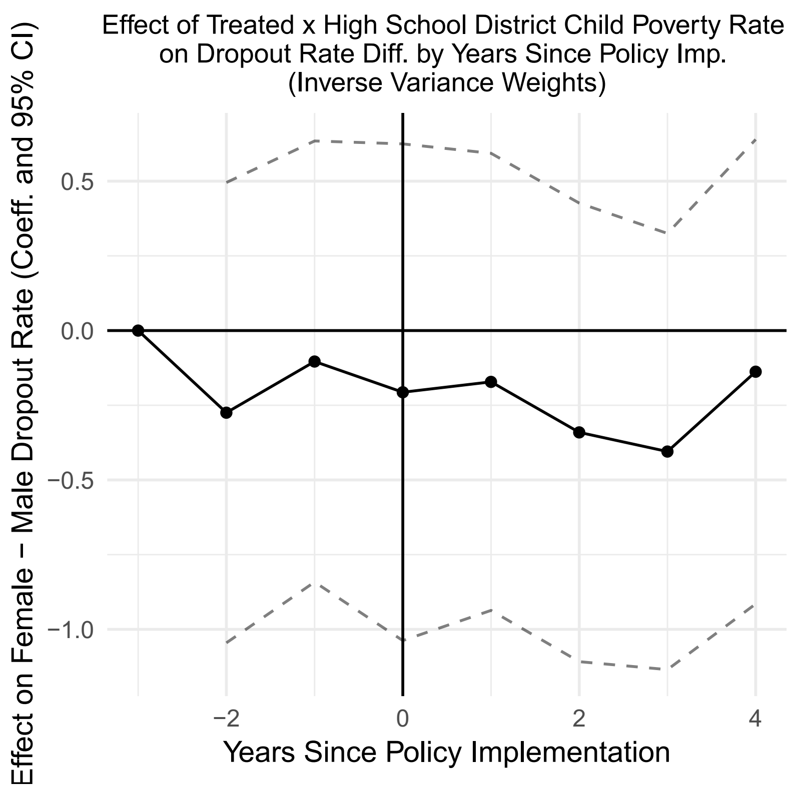


(b) Male



Note: The coefficient in the third year prior to policy implementation is subtracted from all coefficients as a base year. All regressions are weighted by inverse variance. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data and the 1995-2010 Small Area Income and Poverty Estimates.

Figure 19. Effect of Treatment Given High School District Poverty Rate on Female - Male 12th Grade Dropout Rate



Note: The coefficient in the third year prior to policy implementation is subtracted from all coefficients as a base year. All regressions are weighted by inverse variance. Data from the National Center for Education Statistics' 1995-2010 Common Core of Data and the 1995-2010 Small Area Income and Poverty Estimates.

Appendix

Table 11. State-Level Teen Birth Rate Triple Difference-in-Differences Unweighted: Income-Based Waiver x Percent Treated

	Dependent variable:			
	Log Teen Birth Rate *mean teen birth rate = 44.3*			
	(1)	(2)	(3)	(4)
Income-Based Waiver	−0.103	−0.162	−0.049	−0.025
	(0.165)	(0.151)	(0.076)	(0.057)
Income-Based Waiver x Percent Treated	0.114	0.014	0.061	−0.036
	(0.862)	(0.730)	(0.284)	(0.206)
Postpartum-Based Waiver	0.097	0.022	−0.081***	−0.007
	(0.098)	(0.078)	(0.031)	(0.023)
50/10 Income Percentile Ratio		−0.164**	0.002	0.006
		(0.075)	(0.012)	(0.012)
Unemployment Rate		0.025*	−0.009	−0.003
		(0.013)	(0.007)	(0.006)
Percent White		−1.064**	−0.247	−0.122
		(0.424)	(0.209)	(0.119)
Percent Hispanic		0.855***	−0.788*	−0.010
		(0.324)	(0.416)	(0.390)
CHIP		−0.197***	0.030**	0.011
		(0.022)	(0.014)	(0.010)
Constant	3.720***	5.165***		
	(0.052)	(0.578)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.435	0.645	0.958	0.976
Adjusted R^2	0.421	0.633	0.953	0.971

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level.

Table 12. State-Level Teen Birth Rate Triple Difference-in-Differences with Population Weights: Income-Based Waiver x Percent Treated

	Dependent variable:			
	mean teen birth rate = 44.3			
	(1)	(2)	(3)	(4)
Income-Based Waiver	-0.270	-0.284^{*}	-0.079	-0.058
	(0.193)	(0.147)	(0.081)	(0.064)
Income-Based Waiver x Percent Treated	1.561	0.686	0.128	-0.003
	(1.195)	(0.858)	(0.366)	(0.282)
Postpartum-Based Waiver	-0.003	-0.010	-0.066^{*}	-0.024
	(0.087)	(0.062)	(0.039)	(0.020)
50/10 Income Percentile Ratio		-0.326^{***}	-0.010	-0.007
		(0.072)	(0.013)	(0.012)
Unemployment Rate		0.001	0.003	0.003
		(0.011)	(0.008)	(0.006)
Percent White		-1.323^{***}	-0.069	-0.138
		(0.353)	(0.226)	(0.114)
Percent Hispanic		1.006^{***}	-0.363	-0.355
		(0.304)	(0.543)	(0.338)
CHIP		-0.233^{***}	0.017	0.003
		(0.025)	(0.013)	(0.011)
Constant	3.791^{***}	6.184^{***}		
	(0.073)	(0.453)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.124	0.464	0.955	0.974
Adjusted R^2	0.102	0.447	0.950	0.969

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and weighted by state 15-19 year old female population.

Table 13. State-Level Teen Birth Rate Triple Difference-in-Differences with Population Weights: Income-Based Waiver x Base Child Poverty Rate

	Dependent variable:			
	Log Teen Birth Rate *mean teen birth rate = 44.3*			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.294	0.283*	0.287**	0.147**
	(0.204)	(0.145)	(0.114)	(0.073)
Income-Based Waiver x Child Poverty Rate	−0.021**	−0.023***	−0.018***	−0.010***
	(0.010)	(0.007)	(0.005)	(0.004)
Postpartum-Based Waiver	−0.019	−0.030	−0.046	−0.010
	(0.064)	(0.035)	(0.038)	(0.024)
50/10 Income Percentile Ratio		−0.335***	−0.012	−0.010
		(0.076)	(0.014)	(0.012)
Unemployment Rate		−0.011	0.004	0.002
		(0.010)	(0.007)	(0.005)
Percent White		−0.652***	−0.003	−0.103
		(0.170)	(0.202)	(0.112)
Percent Hispanic		0.421**	−0.556	−0.515
		(0.168)	(0.457)	(0.318)
CHIP		−0.107*	0.005	−0.002
		(0.060)	(0.011)	(0.011)
Constant	3.003***	4.973***		
	(0.123)	(0.380)		
Observations	816	816	816	816
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.559	0.733	0.958	0.975
Adjusted R^2	0.548	0.725	0.952	0.970
Note:	*p<0.1; **p<0.05; ***p<0.01			

Note: All regressions have standard errors clustered at the state-level and are weighted by the state-level 15-19 year old female population. The teen birth rate is the number of births to 15-19 year old mothers divided by the population of 15-19 year old females in a given state year.

Table 14. Effect of Eligibility Expansions on School District-Level Female 12th Grade Dropout Rates with 12th Grade Enrollment Weights

	Dependent variable:			
	Female 12th Grade Dropout Rate			
	mean = 3.18			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.199	0.509	−0.143	−0.234
	(0.792)	(0.680)	(0.505)	(0.341)
Income-Based Waiver x High Child Poverty District	−0.086	−0.012	−0.076	−0.139
	(1.166)	(1.157)	(0.416)	(0.355)
Postpartum-Based Waiver	−0.459	−0.321	0.317	−0.461
	(0.515)	(0.480)	(0.408)	(0.494)
State 50/10 Income Percentile Ratio		−1.101***	0.080	−0.016
		(0.385)	(0.237)	(0.174)
State Unemployment Rate		0.003	−0.198	0.083
		(0.073)	(0.170)	(0.136)
CHIP		−0.749**	0.232	0.090
		(0.372)	(0.335)	(0.230)
Constant	3.431***	8.422***		
	(0.263)	(1.787)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.061	0.066	0.506	0.521
Adjusted R^2	0.061	0.066	0.458	0.475

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by school district 12th grade enrollment.

Table 15. Effect of Eligibility Expansions on School District-Level Male 12th Grade Dropout Rates with 12th Grade Enrollment Weights

	Dependent variable:			
	Male 12th Grade Dropout Rate			
	mean = 3.75			
	(1)	(2)	(3)	(4)
Income-Based Waiver	−0.116	0.418	−0.297	−0.220
	(1.065)	(0.938)	(0.640)	(0.354)
Income-Based Waiver x High Child Poverty Distrct	0.240	0.156	0.208	0.022
	(1.308)	(1.284)	(0.516)	(0.547)
Postpartum-Based Waiver	−0.313	−0.103	0.690	−0.144
	(0.609)	(0.570)	(0.450)	(0.503)
State 50/10 Income Percentile Ratio		−1.346***	0.232	0.057
		(0.444)	(0.238)	(0.156)
State Unemployment Rate		−0.028	−0.202	0.085
		(0.081)	(0.163)	(0.148)
CHIP		−1.908***	0.271	0.215
		(0.532)	(0.490)	(0.353)
Constant	4.179***	11.307***		
	(0.334)	(2.127)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.058	0.068	0.532	0.549
Adjusted R^2	0.058	0.067	0.486	0.505

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by school district 12th grade enrollment.

Table 16. Effect of Eligibility Expansions on the Difference in Female and Male School District 12th Grade Dropout Rates with 12th Grade Enrollment Rates

	Dependent variable:			
	Female - Male 12th Grade Dropout Rate			
	mean = -0.57			
	(1)	(2)	(3)	(4)
Income-Based Waiver	0.315	0.091	0.154	−0.014
	(0.287)	(0.278)	(0.190)	(0.149)
Income-Based Waiver x High Child Poverty District	−0.326	−0.167	−0.284	−0.160
	(0.266)	(0.271)	(0.202)	(0.274)
Postpartum-Based Waiver	−0.146	−0.219	-0.373^{**}	-0.316^{***}
	(0.157)	(0.141)	(0.146)	(0.118)
State 50/10 Income Percentile Ratio		0.246^{**}	-0.152^{**}	−0.072
		(0.119)	(0.069)	(0.071)
State Unemployment Rate		0.031	0.004	−0.002
		(0.019)	(0.050)	(0.035)
CHIP		1.160^{***}	−0.039	−0.126
		(0.195)	(0.230)	(0.174)
Constant	-0.748^{***}	-2.886^{***}		
	(0.107)	(0.569)		
Observations	112,167	112,167	112,167	112,167
School District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
State-Specific Linear Time Trends	No	No	No	Yes
R^2	0.005	0.015	0.165	0.174
Adjusted R^2	0.005	0.015	0.084	0.093

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions have standard errors clustered at the state-level and are weighted by 12th grade school district enrollment.

Figure 20. Effect of Treatment in High Poverty Rate School Districts on Female 12th Grade Dropout Rate with 12th Grade Enrollment Weights

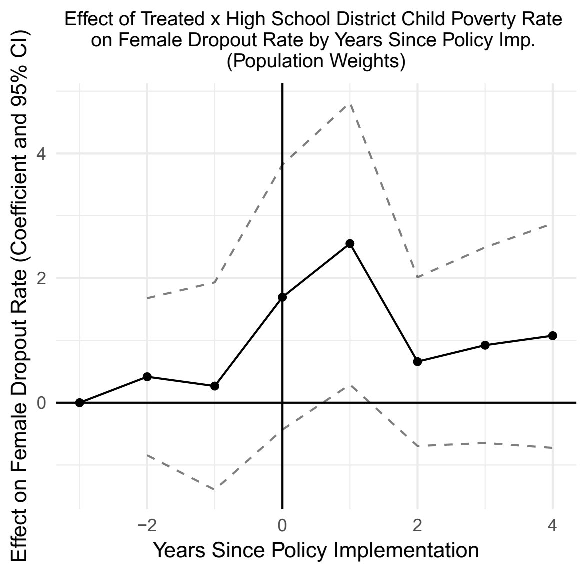
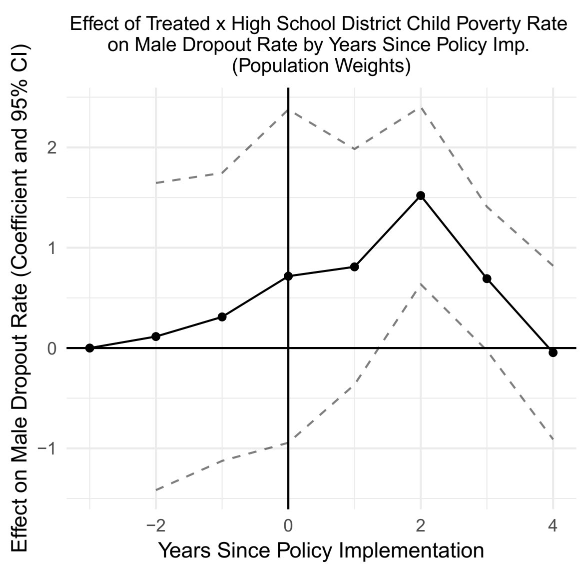
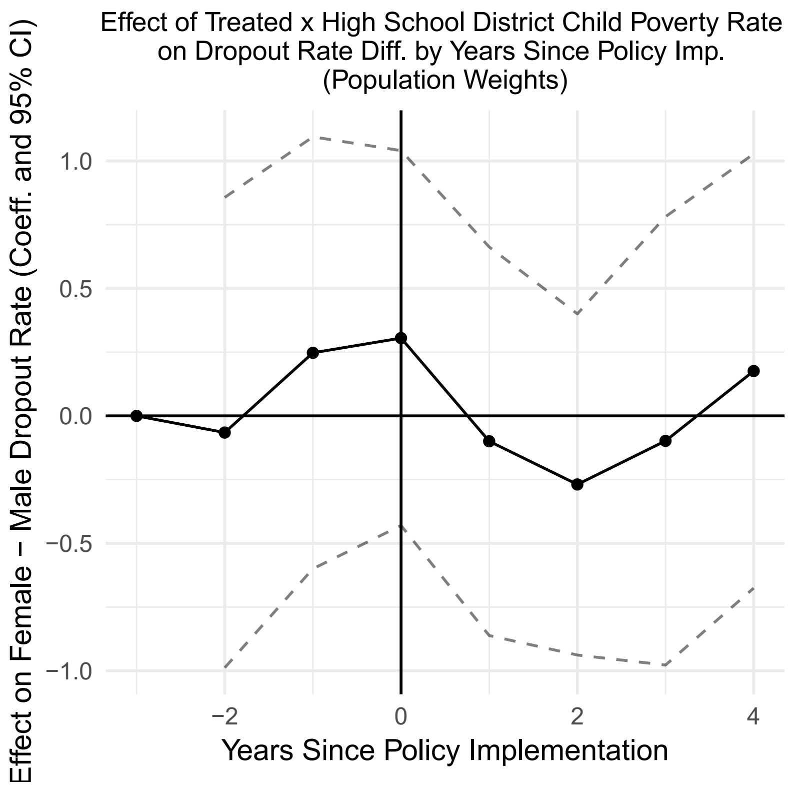


Figure 21. Effect of Treatment in High Poverty Rate School Districts on Male 12th Grade Dropout Rate



Note: Data from the 1995-2010 NCES' Common Core of Data and the 1995-2010 SAIPE. The coefficient in the third year prior to policy implementation is subtracted from all coefficients as a base year. Both figures are weighted by school district 12th grade enrollment in each year.

Figure 22. Effect of Treatment in High Poverty Rate School Districts on Female - Male 12th Grade Dropout Rate with 12th Grade Enrollment Weights



Note: The coefficient in the third year prior to policy implementation is subtracted from all coefficients as a base year. The regressions are weighted by school district 12th grade enrollment in each year.

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