# ESSAYS IN LABOR AND HEALTH

by

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A DISSERTATION

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DISSERTATION ABSTRACT

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This dissertation consists of three essays:

"The Effect of Women's Access to Free Health Care on Breastfeeding Practices: Evidence from Armenia" examines how women's decisions regarding breastfeeding respond to increased maternal access to medical care. Specifically, I examine the effects of the Armenian "For You, Women" program which grants women access to free health care services for a period of one month each year. Employing a differences-in-differences specification, I explore how the prevalence and duration of breastfeeding are affected by the program. I find that access to free women's health care leads to a decline in the likelihood of breastfeeding.

In "Impacts of the Relocation Program on Native American Migration and Fertility," I estimate the historical migratory and fertility effects of the US Relocation Program. Between 1952 and 1973, the US federal government attempted to move Native Americans off reservations and to urban areas under the promises of financial assistance and job training. Using the variation in which cities were targeted by the program, I employ a difference-in-differences strategy and estimate that the Relocation Program significantly increased the Native

American population in target cities. I also find evidence that second-generation Native American women living in cities have a substantially lower fertility rate than Native American women living on tribal land. Jointly, these findings indicate that this federal program substantially shifted the spatial distribution of the Native American population in the US throughout the $20^{th}$ century.

Finally, "Right to Carry Laws and Intimate Partner Homicide," examines to what extend right to carry laws impact intimate partner homicide rates. I employ generalized difference-in-differences, event study, and synthetic control methods specifications to examine the impact of right to carry laws on annual, state-level intimate partner homicide rates. I find no evidence of an impact of right to carry laws on female victim intimate partner homicide rates. While there are some specifications that suggest a decrease in male victim firearm intimate partner homicide rates, these results are limited and fluctuating across model specifications. Overall, the estimates are noisy but suggest little evidence of an impact, highlighting the importance of more formalized sensitivity analysis for testing the robustness of results.

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# CHAPTER I

# INTRODUCTION

In "The Effect of Women's Access to Free Health Care on Breastfeeding Practices: Evidence from Armenia," I examine the impacts of women's increased access to health care on breastfeeding outcomes. More specifically, I analyze how the "For You, Women" program in Armenia, which extended free health care to women for one month each year, affected two main breastfeeding outcomes, namely: the duration of breasting and the likelihood of ever breastfeeding. Using the timing of the program's implementation and the geographic variation in where services were located, I employ a difference-in-differences specification to identify a causal impact of the program on breastfeeding.

I find that there is a roughly 5% decrease in the likelihood of ever breastfeeding associated with the increased access to health care. I find no impact of the program on the duration of breastfeeding for those who choose to breastfeed. These findings are robust to a placebo check and are unlikely to be driven by changes in female employment. Even with Armenia's strong son-preference, I find no strong evidence of a differential impact for male versus female children though this may be due to a lack of precision among the disaggregated estimates. One mechanism that is likely contributing to this decrease is the documented lack of supportive practices for breastfeeding in the region's medical centers, which I verify using data from the Demographic and Health Surveys.

The second essay, "Impacts of the Relocation Program on Native American Migration and Fertility," analyzes the migratory and fertility impacts of the decades-long US federal Relocation Program, which attempted to relocate Native

Americans off of rural reservations into urban centers by offering minimal financial assistance to facilitate the move and promising employment upon arrival in the new city. I employ a difference-in-differences methodology, comparing cities that were targeted by the program to cities that were not, to examine the impact of the Relocation Program on Native American migration and provide descriptive evidence of the impact on second-generation fertility.

I find that the program significantly increased the Native American population in cities targeted by the program relative to other US cities. Specially, I find that the program caused more than 30% of the baseline targeted population to relocate to target cities, constituting a major shift in the spatial distribution of the Native American population. These findings are robust to the use of different subsets of the control group and to a doubly robust difference-in-differences specification based on propensity score matching techniques. Additionally, I find descriptive evidence that the fertility rate for second-generation Native American women living in cities is about 50% lower than that of Native American women living on tribal lands.

Finally, the third essay, "Right to Carry and Intimate Partner Homicide," estimates the effect of right to carry laws on rates of intimate partner homicide. I use three methods to analyze this relationship—difference-in-differences, event study, and synthetic control methods. In my main specifications, I analyze the intimate partner homicide rates for male and female victims separately and focus only on homicides committed using a firearm. I consider rates of non-firearm intimate partner homicides by victim sex as an additional check.

I find no evidence for an impact of right to carry laws on female victim intimate partner homicide rates and only weak evidence of an effect for male

victims. The results are highly sensitive to defensible researcher choices about model specification related to which sample is used, which methodology is employed, and which fixed effects and controls are included. Overall, these results suggest little to no impact of right to carry laws on intimate partner homicides. Further, these findings highlight the need for more formal sensitivity analysis as standard practice in social science research and particularly within the right to carry literature where my findings illustrate the severity of the problem.

# CHAPTER II

# THE EFFECT OF WOMEN'S ACCESS TO FREE HEALTH CARE ON BREASTFEEDING PRACTICES: EVIDENCE FROM ARMENIA

## Introduction

Breastfeeding provides health benefits for infants and mothers, particularly in areas where food and water may be contaminated or scarce. Infants who are breastfed benefit from decreased mortality as well as reduced morbidity from diarrhea and respiratory infections. Additionally, breastfeeding has been shown to protect mothers against breast cancer and, some evidence suggests, ovarian cancer. The benefits of breastfeeding are particularly pronounced in low- and middle-income countries with benefits increasing in the duration and exclusivity of breastfeeding (Victora et al., 2016). Despite the substantial gains documented from breastfeeding, typical breastfeeding practices across most countries remain well below the World Health Organization (WHO) recommendations. There are still gaps in our understanding of the costs, barriers, and challenges that are contributing to this disparity between recommended and existing levels of breastfeeding given the known benefits.

This paper explores the extent to which women's access to free medical care can influence women's decisions regarding breastfeeding practices. Specifically, I examine the extent to which breastfeeding duration and prevalence are affected by a program offering free clinical visits to women in Armenia for one month each year beginning on March $8^{th}$, International Women's Day, and running through April $7^{th}$, the Armenia holiday of motherhood and beauty. In addition to examining

the overall effect of maternal access to free health care on women's breastfeeding decisions, I also examine how this program may affect gender asymmetries in breastfeeding practices given the country's strong sex-preferences and previous works suggesting that strong son-preferences can lead to breastfeeding disparities.

The "For You, Women" program is based in the capital city of Yerevan. It first began in 2009 at a single medical clinic but expanded to several clinics in the region in response to the program's success. This study exploits the regional variation in the program within Armenia to causally identify the effects of women's access to free medical care on breastfeeding outcomes. I employ a differences-in-differences specification using data from the Integrated Living Conditions Survey on region of residence and year of birth to determine a child's exposure to the "For You, Women" campaign. Questions about whether or not the child was breastfed and for how many months are used as the main measures of breastfeeding prevalence and duration.

The results of the analysis show a decrease in the likelihood of ever breastfeeding by roughly 5% in response to the program, while the average length of time spent breastfeeding, given that the child is breastfed, remains unaffected. Interestingly, there is no significant difference in these effects when the sample is broken down by the gender of the child. As a robustness check, I also examine how women's employment changes during the time of the study, finding that increased maternal employment is likely not driving the results.

While these results may seem counterintuitive at first, there are a number of reasons why increased exposure to health care could be associated with a decreased likelihood of breastfeeding. The most compelling possible explanation is that the medical clinics that participate in the "For You, Women" program may not adhere

to practices that support breastfeeding. Therefore, women's increased exposure to these health care facilities could negatively impact breastfeeding rates. Previous literature examining low levels of exclusive breastfeeding in Armenia and analyzing infant formula promotion in the country have shown that Armenian medical clinics, particularly those in Yerevan, fall short of best practices for promoting breastfeeding among new mothers. Using data from the Demographic and Health Survey in Armenia, I verify the findings from the previous literature and provide additional evidence for a lack of breastfeeding support in the Armenian medical community as a likely driver of my main results.

## Literature Review

### *Benefits of Breastfeeding*

Victora et al. (2016) provide a review of the current literature on the benefits of breastfeeding. They find consistent evidence that breastfeeding protects against the risk of infant mortality due to infectious diseases along with strong evidence of decreased incidences of diarrhea, respiratory infections, and malocclusion. Further, they find increased intelligence associated with breastfeeding as well as suggestive evidence that breastfeeding may decrease incidences of obesity and diabetes. Additionally, nursing mothers are shown to gain protection against breast cancer with some suggestive evidence of protection against ovarian cancer as well. Victora et al. also estimate that substantial increases in breastfeeding rates to near universal levels could prevent 832,000 annual deaths of children under the age of 5 and 20,000 annual deaths of women from breast cancer.

Beyond these overarching benefits, Brandtzaeg (2010) suggests that breast milk is specifically tailored to protect against infectious agents and antigens present

in the mother's environment, which allows the infant's immune system to develop in a way that specially protects him or her from diseases and infections common to that particular environment. Le Doare et al. (2018) suggest that breast milk also contains important microbiota, the symbiotic microbial cells contained in all peoples' bodies (Ursell et al., 2012). They review the literature on breast milk microbiota which suggests that breast milk also contributes to the development of the child's immune system through the pass-down of microbiota, which aids in the healthy development of the intestinal microbiome, providing long-term benefits to breastfed infants.

### *Determinants of Breastfeeding*

The many benefits of breastfeeding outlined above highlight the importance of understanding the determinants of breastfeeding choices. Rollins et al. (2016) provide an up-to-date review of the literature analyzing what determines breastfeeding practices. They find that hospital practices can significantly affect breastfeeding rates with decreased breastfeeding associated with early mother-infant separation (Righard & Alade, 1990), prelacteal supplementation, and the distribution of free infant formula samples (Wright et al., 1996). Further, Rollins et al. suggest that health system interventions aimed at promoting breastfeeding may have a significant positive impact on initiation and duration of breastfeeding.

Pérez-Escamilla et al. (2016) provide a review of one of the most popular interventions aimed at promoting breastfeeding, the Baby-friendly Hospital Initiative (BFHI). The BFHI was initially launched in 1991-1992 as part of the WHO/UNICEF Global Strategy for Infant and Young Child Feeding. There are currently 156 countries participating in the initiative with an estimate of

over 20,000 maternity hospitals and birthing centers ever designated as "Baby-friendly" worldwide[1] (World Health Organization, 2009). The BFHI consists of ten-steps that health care facilities can implement to encourage breastfeeding such as training medical staff in supportive breastfeeding practices and organizing community support groups to encourage mothers in continued breastfeeding after they leave the hospital. Pérez-Escamilla et al. find that implementing the ten steps increases breastfeeding in both the short-term and the long-term. Additionally, they find a dose-effect in the response of breastfeeding outcomes to the number of steps a hospital has instituted.

Rollins and co-authors also suggest that individual characteristics play an important role in breastfeeding decisions. Women who receive poor advice or limited support that undermines their self-efficacy or increases the anticipation of difficulties breastfeeding have lower rates of breastfeeding (Avery et al., 2009; Brown et al., 2014). Further, maternal health factors including smoking (Leung et al., 2002; Liu et al., 2006), overweight/obesity (Turcksin et al., 2014), and depression (Dennis & McQueen, 2009) are associated with reduced breastfeeding.

Included in the extensive literature examining breastfeeding determinants are a set of papers examining the effects of prenatal care. The wider literature on prenatal care focuses mainly on birth outcomes, such as birth weight, with mixed results (Conway & Deb, 2005; Reichman et al., 2009); however, two papers have analyzed the effects on maternal postpartum health behaviors including breastfeeding. Reichman et al. (2010) examine how the timing of prenatal care affects maternal health behaviors postpartum finding that first trimester prenatal care is associated with a decreased likelihood of postpartum smoking and an

[1]Estimates as of 2009

increased number of well-baby visits compared to prenatal care initiated later in the pregnancy. They also find some suggestive evidence that early prenatal care may improve breastfeeding outcomes, but their findings are sensitive to model specification. Yan (2017) extends this analysis by examining the effects of prenatal care utilization on both maternal health and maternal health behaviors. Yan finds an increase in no breastfeeding associated with a low frequency of prenatal visits and the late initiation of prenatal care, in the second or third trimesters. This research expands on these studies by examining how a broad increase in access to health care for all women affects breastfeeding outcomes with antithetical findings possibly due to differences in health care facility practices.

### *Breastfeeding and Gender*

In addition to the literature on the overall determinants of breastfeeding, there have also been some studies that explore the determinants of heterogeneous breastfeeding decisions along gender lines. Jayachandran and Kuziemko (2011) provide a model whereby son preference can lead to decreased breastfeeding duration for female children not because of inequitable valuing of health between male and female children but because of the negative relationship between breastfeeding and fertility. Specifically, they base their model on two important facts regarding breastfeeding. First, women who are breastfeeding experience reduced fertility. Second, women who become pregnant often stop breastfeeding current children because of the physical demands of pregnancy. Their model predicts that this negative relationship between breastfeeding and fertility coupled with a "try until you have a son" fertility pattern may contribute to asymmetric breastfeeding practices between male and female children. They test their model

empirically with data from India where there exists notably strong son preferences. They find evidence supporting their theory that the decision to breastfeed female children less is likely in part a "passive" outcome of wanting to have a son and therefore choosing to reduce the period of decreased fecundity during breastfeeding or ceasing breastfeeding current children if the mother becomes pregnant as a result of trying for a son. Chakravarty (2015) and Hafeez and Quintana-Domeque (2018) confirm Jayachandran and Kuziemko's findings in Egypt and Pakistan, respectively.

## Background

FIGURE 1. Map of Armenia


*Source:* CIA World Factbook Armenia

Armenia is located in the South Caucasus region of Asia neighboring Georgia, Azerbaijan, Turkey, and Iran as seen in Figure 1. The population of Armenia is

2,972,700. Roughly one third of the population lives in the capital city of Yerevan[2] (Statisitcal Committee RA, 2018). The official language of Armenia is Armenian and a majority of the population is ethnically Armenian. A large majority of the population is Christian with more than 92% of the population belonging to the Armenian Apostolic Church as of 2011. Armenia is approximately 29,743 square km and is divided into 11 regions (marz), one of which is the capital city. GDP per capita (PPP) is $9,500 with 18.9% unemployment according to 2017 estimates.

The Armenia Demographic and Health Surveys (ADHS) conducted in 2000, 2005, 2010, and 2015-16 provide insight into fertility and family planning practices in Armenia. According to these surveys, the average age at first marriage for women in Armenia is roughly 21 with the first birth occurring on average about one year later. Throughout the period considered in this study, the total fertility rate in Armenia is 1.7. The percent of women who received some prenatal care increased over the sample period from 92% in 2000 to more than 99% in 2015. During the Soviet period, abortion was used as the primary means of controlling fertility (National Statistical Service [Armenia], 2001). Abortion is still legal in Armenia though the percentage of pregnancies ending in induced abortion decreased drastically during the time period studied from over 50% in 2000 to 23% in 2015. As noted in the introduction, the sex ratio at birth is currently among the highest in the world with 113 males born for every 100 females. This figure has been decreasing steadily from a high of 117 males for every 100 females born in 2002 (The World Bank, 2017).

[2]Estimates as of 2018

*Breastfeeding in Armenia*

Culturally, there is no stigma associated with breastfeeding in public and family members have great respect for mothers who choose to breastfeed. Prior to the onset of Soviet rule in Armenia, breastfeeding practices such as early initiation and exclusive breastfeeding were common. During Soviet times, these practices were abandoned and instead it was common to initiate breastfeeding later and to provide prelacteal supplementation. In 1988, a devastating earthquake hit Armenia, which in conjunction with the transition to independence in 1991, caused many families to suffer economic difficulties and to further decrease their likelihood of breastfeeding. Over this time period, rates of fully breastfeeding infants at 4 months of age dropped from 64% in 1988 to 23% in 1993 (Harutyunyan, 2004).

Contributing to this massive decline in breastfeeding was the widespread availability of free infant formula provided by humanitarian aid after the earthquake along with unfavorable health facility practices as well as a lack of knowledge among medical professionals and the general population regarding the benefits of breastfeeding. In response to the decline in breastfeeding, the Ministry of Health (MOH) and aid organizations stopped their distribution of free infant formula in 1995. Additionally, in 1993, the MOH implemented a national breastfeeding promotional program which later included encouraging hospitals and clinics to adopt "Baby-friendly" practices (Amirkhanyan, 2008). By 2001, the rate of fully breastfed 4-month-old children was back up to 69%. In 2008, however, the Baby-friendly Hospital Initiative and the National Breastfeeding Promotion Committee were discontinued.

The WHO recommends that breastfeeding be initiated early, within the first hour after delivery, and that children be exclusively breastfed until they

reach 6 months of age. More recent Demographic and Health Survey trends have shown steady increases in these commonly used breastfeeding indicators. Early initiation of breastfeeding increased from 24% in 2000 to 40% in 2015. Exclusive breastfeeding under 6 months also increased over this time period from 29% in 2000 to almost 45% in 2015. Rates of ever breastfed children are consistent across the 2005, 2010, and 2015 waves of the survey at 96-97%, increasing slightly from 90% in 2000.

In 2014, the Armenian National Assembly passed a law limiting the marketing of breast milk substitutes in line with international standards (Harutyunyan, 2015). A previous law adopted in 1999 had attempted to curb marketing of breast milk substitutes but lacked enforcement mechanisms. Thus, prior to 2014, Armenia had significant reports of serious violations of the World Health Organization's International Code of Marketing of Breast-milk Substitutes such as brand endorsements in health care facilities and infant formula sponsorships of medical conferences ("Confidence" Health NGO, 2011).

### *"For You, Women" Program*

The "For You, Women" campaign is a program based in Armenia's capital city of Yerevan which provides women with free health care services for one month every year. The campaign began in 2009 at a single clinic, "Sourb Astvatsamayr (St. Mary)" Medical Centre (SAMC). In the first year, 221 women applied for medical services as part of the program (Yerevan City Hall, 2010). By 2013, participation had increased twenty-fold, and the program expanded to include multiple clinics in the Yerevan region (Yerevan City Hall, 2013). The campaign consists of providing free medical services and if needed surgical interventions.

Some examples of the services provided include gynecological services, opthalmic services, ultrasounds of the thyroid glands, and CT scans.

The campaign begins each year on March $8^{th}$, International Women's Day, and runs through April $7^{th}$, the Armenian holiday dedicated to motherhood and beauty. If all the women who applied for consultations and treatment are unable to be seen within the thirty-one day period, the participating clinics are instructed to continue to provide services for free until everyone who applied can be seen. Though the program is based in Yerevan, women from outside the region are able to participate; however, participation rates for those outside the capital are low at around 25%. Thus, for the proceeding analysis women living in Yerevan are considered the treated group and women living in other regions of Armenia are considered the control group.

## Data

### *Integrated Living Conditions Survey*

The baseline analysis utilizes data from the Integrated Living Conditions Survey (ILCS) between the years of 2001 and 2016. The ILCS is an annual survey conducted by the National Statistical Service of the Republic of Armenia (NSS) with aid from the World Bank and USAID as well as additional assistance from the Millennium Challenge Account - Armenia between the years 2007 and 2011. Between 4,000 and 8,000 households are surveyed each year depending on the funding.

The survey consists of two main components, namely, a questionnaire with information about the household's standard of living and a diary of consumption and income for the past thirty days. Included in the questionnaire is a section

pertaining to health and health care utilization for children between the ages of 0 and 5 at the time of the survey. This section includes questions about whether or not the child was breastfed and if so for how many months, which are used as measures of the two main breastfeeding outcomes. Additionally, the survey includes questions on current region of residence and birth year, which are used to create the treatment dummy variables for the differences-in-differences analysis. The ILCS also has information on the education and employment of individual's over the age of 15 as well as the number of members in the household and the number of rooms in the household, all of which are used as controls.

For the main analysis, I limit the sample to households that include exactly one woman of child-bearing age in order to include controls for education and employment of the presumed mother, dropping roughly a quarter of the original sample. Analysis containing the full sample can be found in the appendix. The coefficients and significance levels for the full sample are consistent with the findings from the limited sample used in the main analysis. Further, similarly constricted sampling of the Demographic and Household Survey in Armenia,[3] which contains an identifying marker for mothers of children in the sample, suggests that in households with one child-bearing aged female, that female is the mother of all children under five in 99% of cases.

[3]This verification holds true across all years for which DHS data is available.

TABLE 1. Summary Statistics

| Statistic | Male | Female | Total |
|---|---|---|---|
| Ever Breastfed | 0.927 | 0.929 | 0.928 |
| | (0.261) | (0.257) | (0.259) |
| Months Breastfed | 8.981 | 8.854 | 8.923 |
| | (6.350) | (6.120) | (6.246) |
| Months Breastfed | 9.692* | 9.530* | 9.618 |
| If Ever Breastfed | (6.052) | (5.820) | (5.947) |
| Number of | 5.393 | 5.372 | 5.384 |
| HH Members | (1.283) | (1.303) | (1.293) |
| Number of | 3.151 | 3.137 | 3.145 |
| Rooms in HH | (1.192) | (1.191) | (1.192) |
| Mother's | 2.229 | 2.233 | 2.231 |
| Education | (0.429) | (0.429) | (0.429) |
| Mother's | 0.277 | 0.272 | 0.275 |
| Work Status | (0.448) | (0.445) | (0.447) |
| Age | 2.072 | 2.106 | 2.088 |
| | (1.420) | (1.424) | (1.422) |
| *Notes:* | *p<0.1; **p<0.05; ***p<0.01 | | |

Table 1 provides summary statistics for the variables of interest from the ILCS for the subsample of households with one female of child-bearing age. The table depicts averages for each variable of interest for males and females separately as well as the total sample means. The number of months breastfed for children who were ever breastfed is the only variable whose mean is statistically different between the male and female subsamples. Standard deviations are in parentheses below the mean. 54% of the children in the sample are male. This number is higher than the natural rate but fairly consistent with the population of Armenia where the sex ratio at birth is 53-54% over the sample period (The World Bank,

2017). Almost 93% of the children in the full sample are ever breastfed, and for those children who are breastfed, the average duration of breastfeeding is about 9.6 months. Households on average have between 5 and 6 members with three generations typically sharing a house, and about 28% of the presumed mothers are employed at the time of the survey.

FIGURE 2. Armenia Breastfeeding Trends By Treatment Status
2000-2014


*Notes:* Trend lines for the rate of children ever breastfed by treatment status calculated using the ILCS subsample of household with only one child-bearing age female. Includes only years with more than 500 observations. The vertical dashed line signifies the treatment year, 2009.
*Sources:* Author's calculations from Armenia ILCS

Trends in the rates of children ever breastfed are depicted in Figure 2. Separate trend lines are shown for Yerevan, the treatment group, and the other regions of Armenia, the control group. Prior to 2009, both trend lines show increasing rates of ever breastfeeding. After the intervention in 2009, however, the trend line for Yerevan takes a downward turn while the trend line for other

regions continues to increase steadily. Hence, Figure 2 provides suggestive evidence of parallel trends prior to treatment with trends diverging between the treatment and control groups following the introduction of the "For You, Women" program in 2009.

## Empirical Strategy

The baseline regression takes the following form. For child i, born in year t, living in region r, and surveyed in ILCS wave w:

$$\begin{aligned} BF_{itrw} &= \alpha + \beta_1 Post_t + \beta_2 Yerevan_r + \beta_3 Post_t \times Yerevan_r + \mathbf{X}_{itrw} \\ &\quad + \gamma_r + \delta_t + \omega_w + \varepsilon_{itrw} \end{aligned}$$

Here $BF_{itrw}$ refers to one of two main outcome variables–the number of months spent breastfeeding or a dummy variable for whether or not the child was ever breastfed. In the first context, $BF_{itrw}$ is a continuous variable running from 0 to 60 months for the baseline results and 0.1 to 60 months when considering only the subsample of children who were ever breastfed. In the second context, $BF_{itrw}$ is a dummy variable which takes on a value of 1 if the child was ever breastfed and zero otherwise.

Two different definitions of $Post_t$ are used based on the outcome variable. For the outcome variable of months breastfed among those children who were ever breastfed, $Post_t$ is equal to 1 if a child is born during or after June 2008 and 0 otherwise. For this outcome variable, it is possible for children born prior to the beginning of the intervention to be affected as the choice of when to stop breastfeeding a child, given breastfeeding was initiated, can be made essentially

any time after the child is born. June 2008 is specifically chosen because it corresponds to the average length of breastfeeding duration for the full sample. Therefore, June 2008 seems a natural point at which to consider individuals treated as this is the point when the average mother in the sample is choosing to discontinue breastfeeding. When the outcome variable is whether or not the child was breastfed, $Post_t$ is defined as any child born in or after March 2009 as this is the month in which the program began. This definition of $Post_t$ is natural for the outcome variable ever breastfed as breastfeeding initiation occurs only very near to the time of the birth. Therefore, the decision of whether or not to initiate breastfeeding children born in February 2009 or earlier should not have been affected by a program that started in March 2009 and, therefore, these children are considered untreated while children born after the program began are considered treated.

$Yerevan_r$ is a dummy variable for whether or not the child currently resides in the capital region where the program is based. $Yerevan_r$ is equal to 1 if the child lives in Yerevan and 0 otherwise. The variable $Yerevan_r$ is used as a proxy for whether or not the child was born in Yerevan because actual place of birth is not recorded in early versions of the survey. Specifications that use a direct measure of place of birth by excluding the early surveys show similar results.

$\mathbf{X}_{itrw}$ is a set of additional controls. $\mathbf{X}_{itrw}$ includes the number of rooms in the house and the number of members living in the household as well as the education level and employment status of the woman of child-bearing age. In the regression tables to follow the two latter controls are referred to as mother's education and mother's work status for ease, but as there is no direct variable linking the child to a mother, the relationship is assumed rather than established

in the data. $\gamma_r$, $\delta_t$, and $\omega_w$ are fixed effects for region of current residence, birth year, and wave of the survey, respectively. All standard errors are clustered using the wild cluster bootstrap method as suggested by Cameron and Miller (2015) for data with few clusters.

The coefficient of interest is $\beta_3$, which represents the effect of a child being born in Yerevan during the time of the program on the given breastfeeding outcome. The identifying assumption for this to be considered the effect of the "For You, Women" program on the breastfeeding outcome is that absent the "For You, Women" program, trends in breastfeeding practices in Yerevan would not differ from those in the other regions of Armenia. The effect captured by $\beta_3$ should be interpreted as an intent to treat because there is no direct measure of who is using the free medical care. In other words, $\beta_3$ captures the effect of the program which increases access to maternal health care rather than the effect of utilization of the free health care services.

## Results

The results for the baseline regression, which looks at the total number of months the child was breastfed, can be found in Table 2. The main specification captures both the extensive and intensive margins with changes in duration and prevalence estimated separately in subsequent sections. Columns (1) - (3) use the first definition of $Post_t$ with treatment beginning in June 2008, and columns (4) - (6) use the secondary definition of $Post_t$ with individuals only considered treated if they were born after the program began in March 2009. Columns (1) and (4) display the results for the simple OLS specification. Columns (2) and (5) include the full set of fixed effects for year of birth, region of residence, and survey wave.

Household and mother controls are added in columns (3) and (6). Columns (3) and (6) with the full set of fixed effects as well as the household and mother controls are the preferred specifications.

The results of the baseline specification, though statistically insignificant, provide the first evidence of a possible decrease in breastfeeding in response to the program. The baseline definition of breastfeeding, however, may be too broad to pick up significant changes in breastfeeding practices. Specifically, the baseline definition of breastfeeding, which simply measures the total duration in months a mother spent breastfeeding, does not distinguish between changes in initiation of breastfeeding versus changes in the duration of breastfeeding. Therefore, to better understand the true effect of the program on breastfeeding, it is necessary to examine each of these aspects individually by exploring the extensive and intensive margins separately.

*Extensive Versus Intensive Margin*

The extensive margin is measured by a dummy variable for whether or not the child was ever breastfed. The results, which can be found in columns (1) - (3) of Table 3, show how the prevalence of breastfeeding responds to the "For You, Women" campaign. Additional controls are again included across the columns from column (1) to column (3) with column (3) including the full set of fixed effects, household controls, and mother controls.

The results presented in the first three columns of Table 3 suggest that the program does have an effect on the prevalence of breastfeeding. For those children exposed to the "For You, Women" program, there is a 4.9 percentage point decrease in the likelihood of ever being breastfed. This estimate is robust across

TABLE 2. Effect of Access to Free Maternal Healthcare on the Number of Months Spent Breastfeeding

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Months Breastfed | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post June08 | −0.169 | −0.489* | −0.485* | | | |
| | (0.210) | (0.325) | (0.307) | | | |
| Post March09 | | | | −0.188 | −0.865 | −0.851 |
| | | | | (0.204) | (0.598) | (0.602) |
| Yerevan | −0.685 | | | −0.717 | | |
| | (1.438) | | | (1.412) | | |
| Number of HH Members | | | −0.017 | | | −0.017 |
| | | | (0.056) | | | (0.057) |
| Number of Rooms in HH | | | −0.065 | | | −0.065 |
| | | | (0.074) | | | (0.074) |
| Mother's Education | | | −0.292 | | | −0.295 |
| | | | (0.299) | | | (0.299) |
| Mother's Work Status | | | 0.021 | | | 0.025 |
| | | | (0.289) | | | (0.290) |
| Post June08 × Yerevan | −0.469 | −0.832 | −0.826 | | | |
| | (0.797) | (0.995) | (0.994) | | | |
| Post March09 × Yerevan | | | | −0.444 | −0.744 | −0.741 |
| | | | | (0.308) | (0.914) | (0.921) |
| Time FE | No | Yes | Yes | No | Yes | Yes |
| Region FE | No | Yes | Yes | No | Yes | Yes |
| Wave FE | No | Yes | Yes | No | Yes | Yes |
| Household Controls | No | No | Yes | No | No | Yes |
| Mother Controls | No | No | Yes | No | No | Yes |
| Observations | 17,358 | 17,358 | 17,349 | 17,358 | 17,358 | 17,349 |
| $R^2$ | 0.004 | 0.081 | 0.081 | 0.004 | 0.081 | 0.081 |
| Adjusted $R^2$ | 0.004 | 0.078 | 0.079 | 0.004 | 0.078 | 0.078 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

TABLE 3. Extensive and Intensive Margins

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Extensive Margin | | | Intensive Margin | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post March09 | 0.034*** | 0.002 | 0.002 | | | |
| | (0.006) | (0.016) | (0.016) | | | |
| Post June08 | | | | −0.526*** | −0.686** | −0.689** |
| | | | | (0.195) | (0.307) | (0.291) |
| Yerevan | 0.002 | | | −0.748 | | |
| | (0.031) | | | (1.271) | | |
| Number of HH Members | | | 0.004 | | | −0.058 |
| | | | (0.003) | | | (0.053) |
| Number of Rooms in HH | | | 0.002 | | | −0.095 |
| | | | (0.004) | | | (0.068) |
| Mother's Education | | | −0.009 | | | −0.243 |
| | | | (0.009) | | | (0.275) |
| Mother's Works Status | | | 0.010* | | | −0.084 |
| | | | (0.006) | | | (0.266) |
| Post March09 × Yerevan | −0.041** | −0.049** | −0.049** | | | |
| | (0.021) | (0.025) | (0.025) | | | |
| Post June08 × Yerevan | | | | −0.200 | −0.483 | −0.477 |
| | | | | (0.8) | (0.972) | (0.960) |
| Time FE | No | Yes | Yes | No | Yes | Yes |
| Region FE | No | Yes | Yes | No | Yes | Yes |
| Wave FE | No | Yes | Yes | No | Yes | Yes |
| Household Controls | No | No | Yes | No | No | Yes |
| Mother Controls | No | No | Yes | No | No | Yes |
| Observations | 17,358 | 17,358 | 17,349 | 16,104 | 16,104 | 16,095 |
| $R^2$ | 0.003 | 0.024 | 0.025 | 0.005 | 0.096 | 0.097 |
| Adjusted $R^2$ | 0.003 | 0.022 | 0.023 | 0.005 | 0.093 | 0.094 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

all specifications and significant at the 5% level, though the naive OLS estimate is slightly smaller. To better understand the magnitude of this change, the effects are a little over half the size of the decrease in the likelihood of breastfeeding associated with a mother returning to work within the first 12 weeks after delivery (Berger et al., 2005).

To examine the intensive margin, the sample is limited to only those children who were ever breastfed. The breastfeeding outcome measure is the number of months the child was breastfed. Thus, the outcome variable is a measure of breastfeeding duration for the subset of children who were ever breastfed. Columns (4) - (6) of Table 3 contain the intensive margin results with controls again added successively across columns. The results of the intensive margin analysis suggest that on average breastfeeding duration does not respond to the increased access to maternal health care provided by the "For You, Women" program.

Given these findings, we can conclude that changes in breastfeeding in response to the program are concentrated among decreased prevalence rather than decreased duration. In other words, exposure to the "For You, Women" campaign is associated with an increase in the likelihood that a woman will choose to never breastfeed but does not seem to affect the average length of time spent breastfeeding for women who still decide to breastfeed.

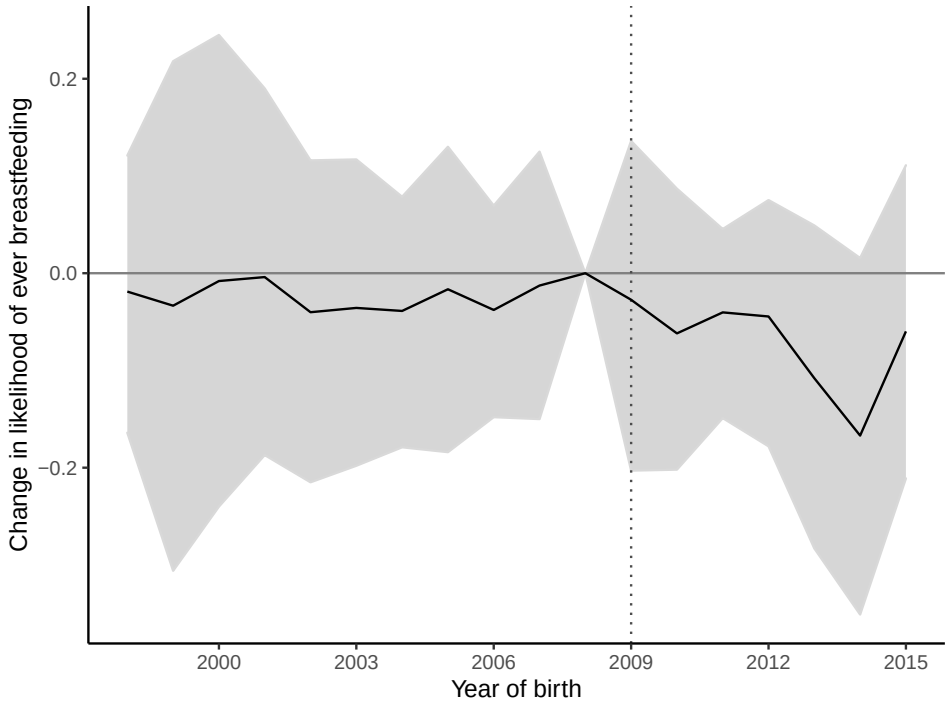
### *Additional Analysis of the Extensive Margin*

#### Event Study

As an additional test of the robustness of the extensive margin results, I perform an event study where the dummy variable for ever breastfed is regressed on a set of interaction terms between the indicator for living in the treatment region

of Yerevan and the indicators for the year of birth. The full set of fixed effects is included. Figure 3 shows the results of the event study with the coefficients on the interaction terms plotted across birth years for the years 1998-2015. Prior to the treatment in 2009, the coefficients are fairly stable and all close to zero. Following the intervention in 2009, however, there is evidence of a downward trend in the coefficient, consistent with the findings in Table 3.

FIGURE 3. Event Study: Likelihood Ever Breastfed



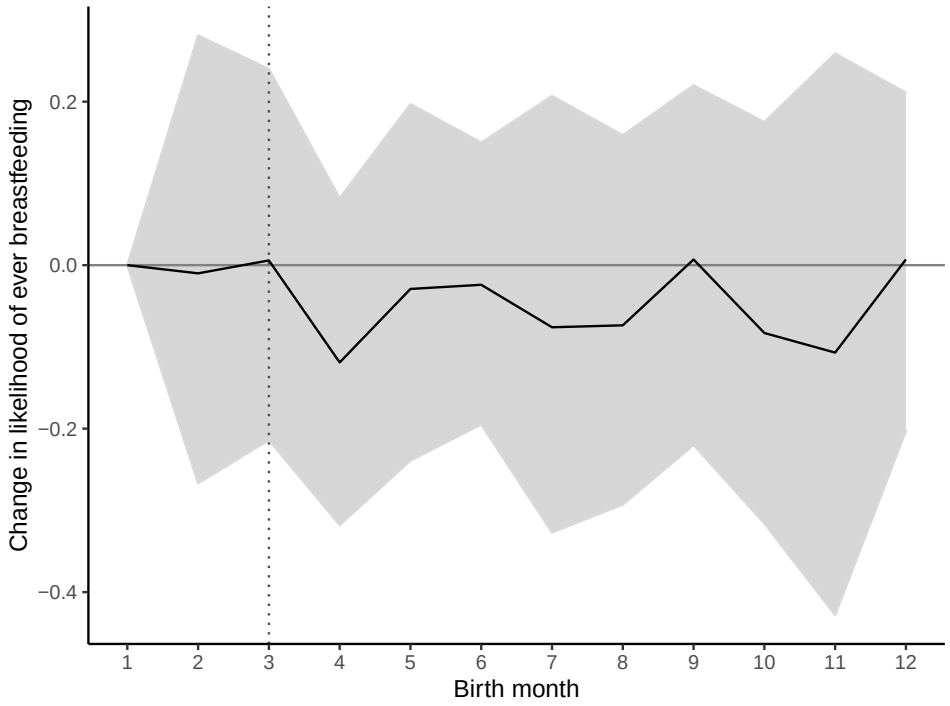

*Notes:* Estimates for the coefficients on the indicator for Yerevan as the region of residence interacted with the child's year of birth controlling for year, region, and wave fixed effects. The ribbon reflects the 95% confidence intervals, and the dashed vertical line represents the start of the program in 2009. Estimates are normalized to 2008, the year just prior to treatment.

Month of Birth Triple Difference

In addition to the event study, I also examine how the main effect differs by month of birth using a triple difference specification. We would expect that if the

decreased prevalence of breastfeeding is associated with the "For You, Women" program that the largest effects should be seen in March and April, the months in which the additional health care access is available. Figure 4 depicts the coefficient of the main variable of interest, $Post_t \times Yerevan_i$ interacted with each month of birth. The dashed vertical line indicates the starting month of the program, March. While none of the interaction coefficients are statistically significant, the largest negative coefficient is in April which is consistent with the main findings.

FIGURE 4. Triple Difference Estimates by Month




*Notes:* Estimates for the coefficients of the triple difference: the main variable of interest, $Post_t \times Yerevan_i$, interacted with each month of birth. Regression includes year, region, and wave fixed effects. The ribbon reflects the 95% confidence intervals, and the vertical dashed line represents the beginning month of the intervention each year. Estimates are normalized to January.

*Gender Heterogeneity*

As noted previously, Armenia has a strong preference for sons as evidenced by the high sex ratio at birth of 113 males for every 100 females (The World Bank, 2017). Thus, it is possible that breastfeeding practices differ across genders as is the case in other countries with strong son preferences such as India and Pakistan. To determine if there are any heterogeneous effects of the "For You, Women" program by gender, I examine separate regressions for the subset of male and female children. These results are found in Table 4 and Table 5. All specifications include the full set of controls including all fixed effects, household controls, and mother controls.

TABLE 4. Heterogeneity by Gender on Months Breastfed

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Months Breastfed | | | |
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| Post June08 | −0.388 | −1.293 | | |
| × Yerevan | (1.335) | (0.964) | | |
| Post March09 | | | −0.265 | −1.249 |
| × Yerevan | | | (1.138) | (0.910) |
| Time FE | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes |
| Wave FE | Yes | Yes | Yes | Yes |
| Household Controls | Yes | Yes | Yes | Yes |
| Mother Controls | Yes | Yes | Yes | Yes |
| Observations | 9,388 | 7,961 | 9,388 | 7,961 |
| $R^2$ | 0.078 | 0.092 | 0.079 | 0.092 |
| Adjusted $R^2$ | 0.073 | 0.086 | 0.073 | 0.086 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

Table 4 presents the results of the overall change in the number of months spent breastfeeding with no breastfeeding coded as zero for male and female

children. Here, we see that there is no statistically significant difference in breastfeeding in response to the program among female versus male children though the negative point estimate for females is larger than the point estimate for males. Therefore, it is again possible that true differences are being masked by the inclusion of both extensive and intensive margin effects in the main outcome variable in Table 4. Thus, Table 5 looks at the extensive and intensive margins separately for male and female children.

TABLE 5. Heterogeneity by Gender Extensive and Intensive Margins

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Extensive Margin | | Intensive Margin | |
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| Post June08 | | | 0.054 | −1.051 |
| × Yerevan | | | (1.329) | (0.867) |
| Post March09 | −0.053** | −0.045** | | |
| × Yerevan | (0.030) | (0.027) | | |
| Time FE | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes |
| Wave FE | Yes | Yes | Yes | Yes |
| Household Controls | Yes | Yes | Yes | Yes |
| Mother Controls | Yes | Yes | Yes | Yes |
| Observations | 9,388 | 7,961 | 8,699 | 7,396 |
| $R^2$ | 0.033 | 0.025 | 0.095 | 0.108 |
| Adjusted $R^2$ | 0.028 | 0.019 | 0.090 | 0.101 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

Columns (1) and (2) of Table 5 depict the extensive margin for the male and female subsamples respectively, while columns (3) and (4) examine the intensive margin. On the extensive margin, the coefficients of interest for the male and female subsamples are both negative, significant at the 5% level, and similar in magnitude to the results from the full sample. Additionally, though the coefficient

for the male sample found in column (1) is actually larger in magnitude than that of the female sample found in column (2), the two are not statistically different.[4] These results suggest that, on the extensive margin, there is no differential response to the program by gender. Exposure to increased maternal health care access is associated with a decrease in the likelihood of ever breastfeeding for both male and female children. On the intensive margin, found in columns (3) and (4), the coefficients on the interaction term $PostJune08 \times Yerevan$ are insignificant for both the female and male subsamples, and again the two are not significantly different.[5] This indicates that there is also not strong evidence suggesting differences in the duration of breastfeeding in response to the program based on the gender of the child.

These results are surprising given the significant son preference in Armenia and the findings from prior studies suggesting a connection between son preference and breastfeeding asymmetries between male and female children. There are, however, various possibilities for why these results might occur. First, it may just be that the estimates here are not precise enough. These are intent to treat estimates, and more precise measures of who exactly is taking up the program may find different results. It is also possible that while this program does seem to negatively impact breastfeeding, it may positively impact women's attitudes toward female children particularly given that the program is focused on women and women's health. Finally, the fertility rate in Armenia is low compared to the

[4]The t-statistic from the regression interacting a male dummy variable with the differences-in-differences estimator is -1.1899 with a p-value of 0.430.

[5]The t-statistic from the regression interacting a male dummy variable with the differences-in-differences estimator is 4.2258 with a p-value of 0.230.

countries previously studied in this context.[6] This may cause a relaxing of the biological time constraint for meeting the dual goals of ideal family size and son preferences proposed by Jayachandran and Kuziemko (2011). This could mean that women may feel less pressure to cease breastfeeding early to meet these goals.

## Robustness

### *Placebo*

I perform a set of placebo regressions to test the robustness of the main findings. I consider placebo treatments in 2003 and 2005.[7] The placebo treatment dummies replace the variable $Post_t$ in the main regression equation and the same differences-in-differences equation is estimated using the subset of pre-treatment data. The results of the placebo regressions are found in Table 6. The placebo regressions look only at the effect on the extensive margin with the outcome variable ever breastfed since the extensive margin is where the main results are focused. Hence, I use the second definition of $Post_t$, defining the placebo treatment group as children born during or after March of the placebo treatment year. The results of the placebo regressions are similar to the corresponding coefficients from the event study though smaller in magnitude. None of the interaction terms are significant, supporting the main findings.

[6]Over the period studied the average fertility rate in Armenia is 1.66. At the time of the other studies the fertility rates in India, Pakistan, and Egypt were all at least double this.

[7]2003 and 2005 were chosen to maximize the balance in the number of pre- and post-treatment observations given the size of the restricted dataset.

TABLE 6. Placebo Regressions

| | *Dependent variable:* | |
|---|---|---|
| | Ever Breastfed | |
| | (1) | (2) |
| Post March03 | −0.020 | |
| × Yerevan | (0.074) | |
| Post March05 | | −0.005 |
| × Yerevan | | (0.051) |
| Full Set FE | Yes | Yes |
| Household Controls | Yes | Yes |
| Mother Controls | Yes | Yes |
| Observations | 11,549 | 11,549 |
| $R^2$ | 0.029 | 0.028 |
| Adjusted $R^2$ | 0.025 | 0.025 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

*Female Employment*

Throughout the study period the overall female employment in Armenia was increasing. Because female employment has been shown to have a negative effect on breastfeeding rates, the increase in female employment could be driving the main results if female employment is growing faster in the capital city than the other regions of Armenia. Therefore, I estimate a differences-in-differences model to test the extent to which female employment changes differentially in the capital city at the time of the program. The regression takes the following form. For woman j, in survey wave w, living in region r:

$$EMP_{jrw} = \alpha + \beta_1 Post_w + \beta_2 Yerevan_r + \beta_3 Post_w \times Yerevan_r + \mathbf{X}_{jrw} + \gamma_r + \omega_w + \varepsilon_{jrw}$$

Here $EMP_{jrw}$ is an indicator variable for whether or not the woman is employed. The time subscript is dropped as the survey asks about current employment so the year of employment perfectly correlates with the year of the survey. $\mathbf{X}_{jrw}$ is a set of household and woman control variables that includes the number of members in the household, the number of rooms in the household, the woman's marital status, and the woman's education level. $\gamma_r$ and $\omega_w$ are fixed effects for region of current residence and wave of the survey, respectively. If there is a significant change in female employment in Yerevan coinciding with the implementation of the program, we would expect $\beta_3$ to be positive.

TABLE 7. Women's Employment

| | *Dependent variable:* | |
|---|---|---|
| | Work Status | |
| | All Women | Women in Subsample |
| | (1) | (2) |
| Post Jan09 | −0.005 | −0.016 |
| × Yerevan | (0.046) | (0.071) |
| Full Set FE | Yes | Yes |
| Household Controls | Yes | Yes |
| Individual Controls | Yes | Yes |
| Observations | 84,478 | 13,829 |
| $R^2$ | 0.093 | 0.071 |
| Adjusted $R^2$ | 0.093 | 0.069 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01 All standard errors are wild bootstrap standard errors.

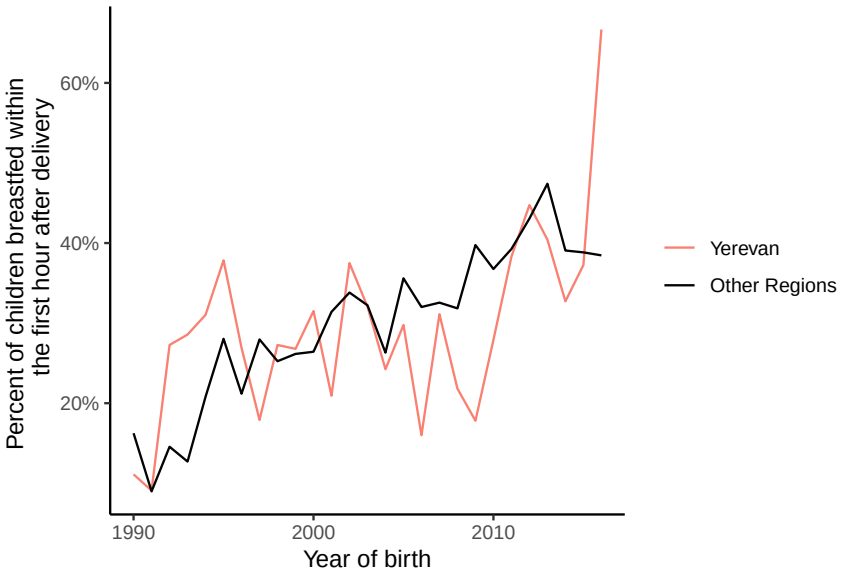
Table 7 presents the results of the above regression. The coefficient on the interaction term suggests that there is no statistically significant change in female employment status coinciding with the start of the "For You, Women" program. Thus, differential changes in female employment between the treatment and control regions over the course of the study is likely not driving the results found in the baseline specification.

## Mechanism

One possible mechanism that could explain these somewhat counterintuitive results is the documented lack of support for breastfeeding and the promotion of infant formula in Yerevan medical community. In a recent paper examining the contributors to low levels of exclusive breastfeeding in Armenia, Demirchyan and Melkomian (2020) find that a lack of counseling and lactation support, misconceptions surrounding infant nutrition, and low motivation by doctors to provide breastfeeding support all contribute to the low levels of exclusive breastfeeding. Demirchyan and Melkomian also note that in Yerevan specifically, there is evidence of "hospitals cooperating with pharmaceutical companies to promote infant formula" as well as an increased likelihood of pre-lacteal feedings. The infant formula promotion findings are further verified in work by Boban and Zakarija-Grkovic (2016) and Harutyunyan et al. (2011).

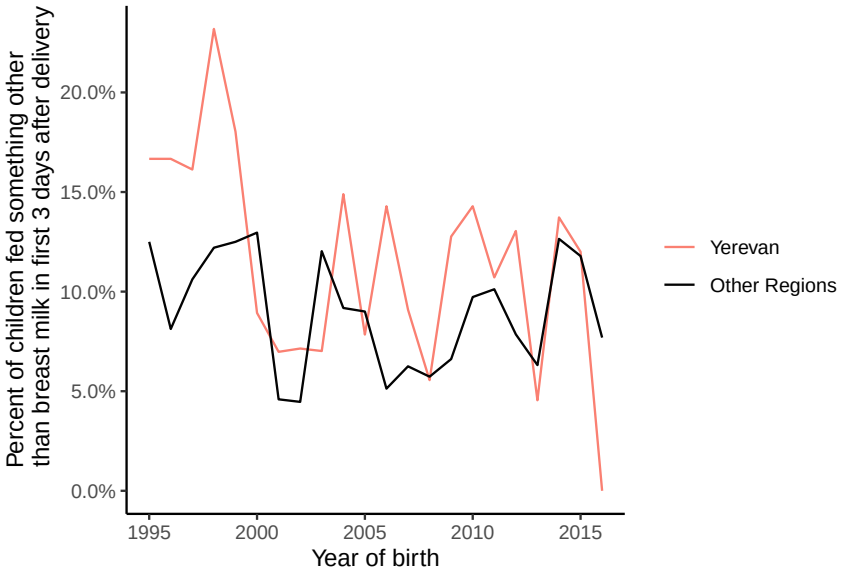
Using data from the Demographic and Health Surveys from 2000, 2005, 2010, and 2015, I examine hospital practices related to early breastfeeding and infant formula use among children under six months to substantiate previous findings and determine how these attitudes and practices could serve as contributors to the main results. Figures 5 and 6 show that, compared to the other regions of Armenia, in Yerevan a lower percentage of children were breastfed within the first hour after delivery and a higher percentage of children were given something other than breastmilk within the first three days. While neither of these gaps are especially large, they are both consistent with lower support for breastfeeding in Yerevan hospitals.

FIGURE 5. Percent of Children Breastfed within the First Hour



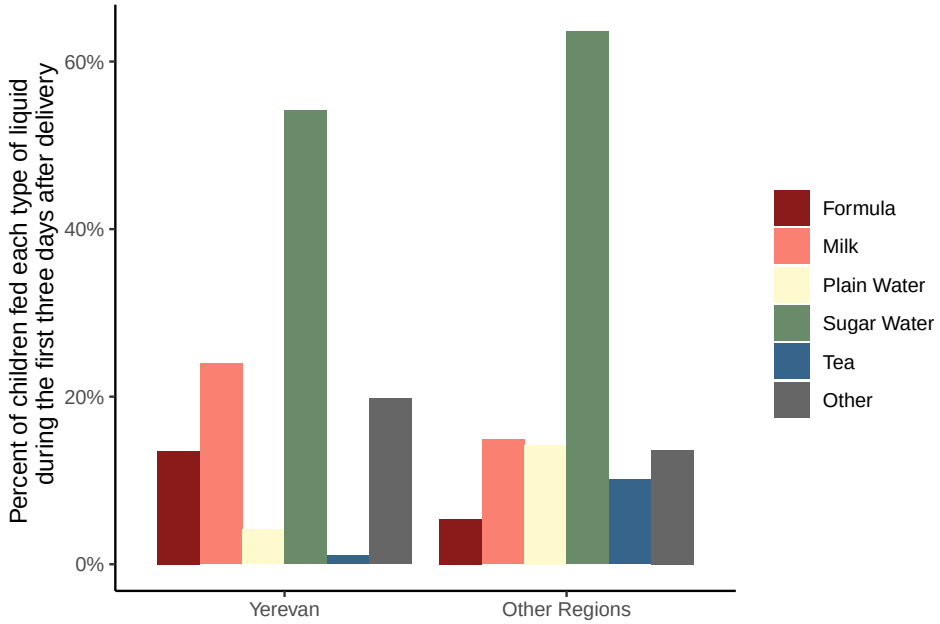

*Sources:* Author's calculations from the Armenia DHS 2000, 2005, 2010, & 2015

FIGURE 6. Percent of Children Fed Something Other Than Breastmilk in First 3 Days




*Sources:* Author's calculations from the Armenia DHS 2000, 2005, 2010, & 2015

FIGURE 7. Type of Food Given to Children in First 3 Days

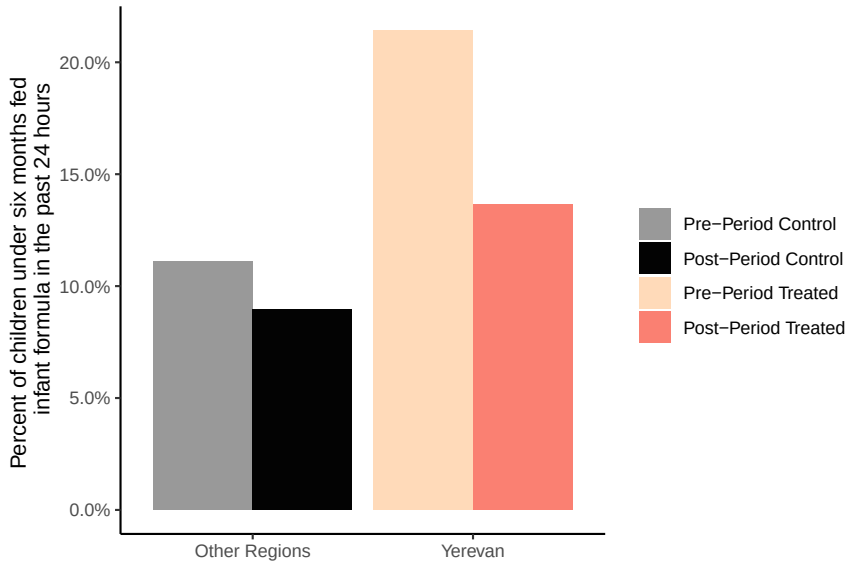



*Notes:* Only children who were fed something other than breastmilk in the first 3 days are included. The categories are non-exclusionary, so the same child may be included in more than one bar. 2015 data is excluded because the question was dropped from that wave of the survey.
*Sources:* Author's calculations from the Armenia DHS 2000, 2005, & 2010

Further, Figure 7 shows that while infant formula was not the type of food or drink most often given to children during the first three days after delivery, hospitals in Yerevan were more than twice as likely to feed children with infant formula in the first three days compared to other regions of Armenia. Finally, children under six months old living in Yerevan were slightly more likely than children in other regions to have been fed infant formula in the past 24 hours as seen in Figure 8. These data in Figures 7 and 8 are not only consistent with the previous literature's findings of greater infant formula promotion in Yerevan hospitals, but they also provide some evidence for greater infant formula usage more generally. Overall, this is consistent with a story in which increased contact with the medical community in Yerevan, which has a lack of support for

breastfeeding and a documented prevalence of infant formula promotion, reduced the likelihood of ever breastfeeding for women exposed to the "For You, Women" program.

FIGURE 8. Percent of Children Fed Infant Formula in Last 24 Hours




*Sources:* Author's calculations from the Armenia DHS 2000, 2005, 2010, & 2015

## Conclusion

Overall, the "For You, Women" program, which aimed to increase medical access for women in Armenia, is associated with a significant decline in the likelihood of breastfeeding by almost 5 percentage points. There is no significant effect on the number of months spent breastfeeding among mothers who chose to breastfeed. Taken together, this study finds that increased access to maternal health care has a negative affect on the prevalence of breastfeeding while having no significant impact on the duration.

Surprisingly, there is no strong evidence of a differential affect of the program on breastfeeding practices based on the gender of the child as the previous

literature may have suggested. One potential reason for this discrepancy may be the low fertility rate in Armenia. It is also possible, however, that my analysis using the ILCS sample is simply not precise enough to capture gender specific differences, and further research with more precise data would contribute to our understanding of heterogeneity in these results.

The decline in breastfeeding that is associated with the "For You, Women" program is likely driven by low levels of support for breastfeeding in medical clinics in Yerevan. Therefore, updating practices at clinics providing services for the women seeking treatment through the "For You, Women" program in line with WHO recommendations may serve to curb these inadvertent outcomes.

CHAPTER III

# IMPACTS OF THE RELOCATION PROGRAM ON NATIVE AMERICAN MIGRATION AND FERTILITY

## Introduction

Throughout US history, the United States government has regularly sought to relocate Native Americans[1] both through force and through policy. While some of these policies are infamous–the Indian Removal Act of 1830 led to between 4,000 and 8,000 Cherokee deaths[2]–there is relatively little known about the long-term economic and demographic consequences of more recent relocation policies. In addition, the economic literature has largely ignored these programs and their effects.

My paper examines the migratory impacts of one such program known simply as the Relocation Program.[3] The Relocation Program, which ran from 1952 to 1973, offered financial assistance and job counseling to Native Americans who left tribal land and moved to one of the program's target cities.[4] As a crucial first step in understanding the lasting impacts of these types of programs, this paper

[1]Because of the broad scope of the Relocation Program, I use the term Native America throughout this paper to refer to the original inhabitants of North America generally, but I acknowledge that the term is imprecise and disputed. In cases where the data or policy use different terminology, I use the term that is consistent with the data and policy language.

[2]The 4,000 to 8,000 death estimate refers solely to deaths on the Trail of Tears. Historical estimates place total population losses at greater than 10,000 (Thornton, 1984).

[3]The program was renamed the Employment Assistance Program when Congress expanded the program through the Indian Relocation Act of 1956, but for consistency will be referred to as the the Relocation Program or Relocation throughout this paper.

[4]More information about the history and details of the Relocation Program are provided in the Background section.

analyzes the implications of the Relocation Program on the migratory outcomes of the Native American population in the second half of the $20^{th}$ century.

As far as I am aware, this is the first paper to estimate the causal effect of the Relocation Program on Native American migration. Previous work in anthropology and sociology has examined factors associated with migration through the program (Ablon, 1965; Chadwick & White, 1973) and the demographic characteristics of participants as well as economic and assimilation outcomes for those who were part of the program (Ablon, 1964; Gundlach, Reid, & Roberts, 1977; Gundlach & Roberts, 1978; Price, 1968; Sorkin, 1969, 1971, 1978). This paper extends the literature by establishing a causal link between the program and Native American migratory outcomes, providing valuable insights into the effects of such relocation programs on migration and setting the stage for examining the causal impacts of the Relocation Program on other outcomes.

Additionally, this paper establishes a basis for analyzing the long-run effects of migration programs with the stated aim of expanding economic opportunities for migrants, similar to programs such as the Moving to Opportunity program in the US or the Transmigration Program in Indonesia. Thus, this paper adds to the growing literature on the impacts of these employment targeting migration programs (Chetty & Hendren, 2018a, 2018b; Chetty et al., 2016; Elmhirst, 2002; Katz et al., 2001; Kling et al., 2007; Pollack et al., 2019, and more). Of particular importance is the added value of the Relocation Program's timing. The Moving to Opportunity literature has specifically found that the main impacts of these types of programs are realized among the young children of movers. The Moving to Opportunity program, however, is still relatively recent and some of the longer term or second-generation outcomes are yet to be determined. Because the Relocation

Program began almost 70 years ago, the long-term outcomes for movers, as well as the movers' children, have been realized and are able to be studied. Therefore, establishing the effects of the program on migration provides a first step for further research on long-term outcomes that are unable to be examined in more recent programs. My analysis of the fertility rates for the second-generation of program participants provides some preliminary evidence of long-run outcomes of the program.

Finally, this paper also adds to the discussion of Indigenous population migration, particularly regarding migration off reservations. There have been a number of papers that have examined the unique factors that contribute specifically to the migration outcomes of Indigenous populations (Cebula & Belton, 1994; Davis et al., 2017; Gardner, 1994; Geib, 2001; Huskey et al., 2004). My research contributes to this literature by analyzing how this type of jobs-related relocation program affects the migration patterns of Native Americans.

I employ a generalized difference-in-differences approach to answer the question of how the Relocation Program affected Native American migration patterns. For my main specification, I use decennial census data to compare the American Indian and Alaska Native population in the relocation target cities to populations in all other metropolitan statistical areas (MSAs). I find that there was a large and statistically significant increase in the Native American population in the program's target cities relative to the other MSAs. The baseline estimates suggest that there was an increase in the Native American population of almost 12,000 individuals on average in the target MSAs relative to the non-target MSAs between 1960 and 1990. The magnitude of this estimate is particularly stark given that the median MSA had a Native American population of less than 100 (with a

mean population of about 1,000) in 1950, just prior to the start of the program. The effect of the program is consistent in magnitude and significance across various subsets of the main control group. Further, the results remain significant when I apply a doubly robust difference-in-differences specification using propensity-score methods though the magnitude is somewhat attenuated. To provide some insight into the magnitude of this impact on tribal land areas, I also calculate counterfactual population estimates for tribal land areas absent the program.

In addition to the main migration results, I also examine the impacts of the Relocation Program on second-generation fertility. I analyze fertility for a number of reasons. Firstly, fertility is a fundamentally important question in this setting where the government has historically played a role in reducing the Native American population and where there is a concurrent program of forcible sterilization at government run clinics on reservations.[5] Further, second-generation fertility provides key insights into the long-run impacts of the program on the primary goal of relocating the Native American population. Differential fertility rates across different locations can alter the spatial distribution of populations over time and is therefore vital to understanding the lasting impacts of relocation. Lastly, fertility can also provide some noisy indication about the long-run impacts of relocation on other outcomes, such as economic well-being and assimilation, both of which have known associations with changes in fertility in certain cases.

I find that there is a gap in the general fertility rate among Native American women living in urban areas in the 1990s relative to Native American women who live in tribal land areas during this period. Specifically, the data show that the

[5]The forced sterilization of Native American women in the 1970s is discussed further in the fertility results section as it provides important context for the study of fertility in this time period.

fertility rate for Native American women living in cities was almost half that of those living in the rural parts of states with large tribal land areas. While this finding does not imply causality, it suggests changes in fertility outcomes among the children of those who were affected by the Relocation Program. This result could have important implications for the future of the Native American population distribution across the US and for other programs that target or induce migration.

## Background

The Relocation Program began in 1952 during what is known as the termination era of US tribal policy. The joint policies of termination and relocation aimed to assimilate the Native American population and move them off tribal lands by terminating tribal trust relationships with the US federal government and urging Native Americans to relocate to urban centers, away from reservations. While this paper focuses on the effects of the latter policy, I also include a brief outline of the termination policy as the two were inextricably linked.

### *Relocation*

The Relocation Program originally began as part of an $88 million ten-year rehabilitation act passed by Congress in April of 1950 which aimed to aid the Navajo and Hopi Reservations after an extreme summer drought and severe winter at the end of the 1940s devastated the area. In addition to providing for a number of reservation development programs, the bill also allocated funds to provide financial aid to individuals for finding off-reservation employment. Specifically, the bill provided direct employment services for individuals from the Navajo and Hopi Reservations to Denver, Salt Lake City, or Los Angeles. The

program was almost immediately expanded by the Bureau of Indian Affairs[6] (BIA) to allow for individuals from any US tribe to participate. During this period, the BIA was run by Dillon S. Myer. It is important to note, that prior to being named Commissioner on Indian Affairs in 1950, Myer served as the director of the War Relocation Authority, the agency charged with the internment of Japanese Americans during WWII. He brought many of his colleagues from the War Relocation Services with him to work at the BIA in implementing the Relocation Program on a national scale. The first relocatees of what became the Relocation Program arrived at the newly opened relocation office of Chicago in February of 1952 (Fixico, 1980).

The Relocation Program grew throughout the early 1950s. In addition to the relocation offices in Denver, Salt Lake City, Los Angeles, and Chicago, offices were added in Cleveland, Cincinnati, Dallas, St. Louis, and San Francisco. The BIA chose these cities for relocation offices because of their "diversified, abundant industrial employment" (Madigan, 1956). The first step in the relocation process was to apply at a local BIA area offices. There were no demographic restrictions, such as age or gender, on who could apply, although Madigan (1956) suggests that acceptance was based on the relocation officer's expectations of an applicant's success in the program. Records show that drunkenness, physical or mental health issues, marital problems, or arrests were all grounds for disqualification of applicants. There were also complaints of prejudice among the relocation officers who were mostly non-Native American (Fixico, 1980).

[6]The Bureau of Indian Affairs was established in 1824 under the Department of War but became part of the Department of the Interior after its establishment in 1849. The BIA acts as a liaison between the federal government and sovereign tribes. The president and Congress are responsible for creating federal tribal policy, and the BIA is responsible for administering the policies.

Upon acceptance to the program, a relocatee and her family received one-way tickets to one of the relocation cities in the relocatee's top choices as well as money for the journey. In their relocation city, the relocatee met the relocation officer who would assist relocatees in finding temporary housing and provide financial assistance for the first month, including money for rent and some money for clothes and food. Additionally, the relocation officer would provide the relocatee with "intensive employment counseling" (Madigan, 1956).

Criticisms of the program, including complaints of slum housing, temporary and unstable job placements, and inadequate preparation for cultural differences, pushed Congress to reassess the program in 1955 and 1956. Congress responded with the passage of Public Law 959–also known as the Indian Relocation Act of 1956–which consisted of two additional components: adult vocational training and on-the-job training. The adult vocational training program trained participants in industrial skills such as plumbing or steel work and targeted individuals between the ages of 18 and 35. The on-the-job training program focused on finding employment opportunities near reservations where individuals could be trained in a specific industry while working. The BIA would subsidize wages of trainees during their training period with the possibility of the trainees gaining full employment upon completion of the training. These training programs were meant to serve as a precursor to relocation in an attempt to better prepare relocatees for life in the relocation cities (Fixico, 1980).

### *Termination*

Termination policy consisted of ending the trust relationship between tribes and the US federal government. This meant that if a tribe was officially

terminated, then any land that belonged to the tribe would be released from its trust status, which generally resulted in the land being appraised and sold to the highest bidder with the proceeds from the sale distributed to the members of the terminated tribe. Additionally, Native Americans who were part of the terminated tribe would be subject to state and federal laws and taxation from which they were previously exempt, and all special federal programs at the tribal and individual level were discontinued. Essentially, termination meant the end of tribal sovereignty as without land, tribal governments had nowhere to exert their jurisdiction (Wilkinson & Biggs, 1977). The stated aim of termination was to "make Indians within the territorial limits of the United States subject to the same laws and entitled to the same privileges and responsibilities as are applicable to other citizens of the United States, to end their status as wards of the United States and the grant them all of the rights and prerogatives pertaining to American citizenship.[7]" Advocates of termination had two main motivators. First, they wanted to "free" Native Americans from the federal trusteeship which proponents of termination believed was paternalistic and kept Native Americans dependent on government aid. Secondly, terminationists wanted to reduce the amount of federal funds allocated to tribal programs. Those opposed to termination cited the desire of many Native Americans to keep reservations intact to preserve tribal homelands and to avoid the loss of sovereignty that comes with loss of communal property. Additionally, termination opponents also condemned the meager payment on assets in the forced sale of tribal land and suggested that high poverty rates on reservations were evidence of continued need for federal education, health, and welfare services (Sorkin, 1971). One of the most critical opponents of termination,

[7]House Concurrent Resolution no. 108

former Commissioner of Indian Affairs, John Collier, asserted that termination was truly motivated by greed for land and natural resources owned by the tribes (Fixico, 1980).

In 1953, House Concurrent Resolution no. 108 was passed. It called for the termination of roughly 100 tribes throughout California, Florida, New York, Texas, Wisconsin, Oregon, Utah, Oklahoma, South Carolina, and Nebraska, affecting over 10,000 Native Americans and over 1 million acres of tribal land. Tribal consent was not deemed necessary, but tribes were nonetheless often coerced into providing consent. At the same time, Congress passed another law aimed at termination which granted states jurisdiction over civil and criminal offenses on tribal land in five states, while allowing for all other states to adopt similar practices if they chose. Strong opposition to these termination policies and poor outcomes among early terminated tribes led to a reversal of policy in the 1960s, though many of the terminated tribes did not regain tribal status until much later (Wilkinson & Biggs, 1977).

## Data

### *Migration*

The main data I use to measure migration comes from the U.S. Decennial Census. Specifically, I use population estimates for the American Indian and Alaska Native (AIAN) population by county and by metropolitan statistical area (MSA). For the years 1910-1940, I calculate county-level population by race, by aggregating the Integrated Public Use Microdata Series (IPUMS) complete count U.S. census microdata. For the years 1950 onward, county-level population estimates for the American Indian and Alaska Native population and the total population come from

U.S. Decennial Census Publications which have, in part, been digitized by Gardner and Cohen (1992) and Haines (2010). All of the county-level population estimates are corrected for changing county borders following Eckert et al. (2020). The final county population estimates follow 1990 county borders. I then match these counties to the 1990 U.S. Census MSAs to create MSA-level population counts. The final dataset contains population estimates for the American Indian and Alaska Native and total population for each MSA in the United States for every decennial year from 1910-1990.

In addition to the county-level population data, I also use the IPUMS full count data from 1930-1940 and the 1-5% samples from 1950-1980 to determine migration patterns for a small number of Native American individuals. Specifically, I use the place of birth and current place of residence variables to document where individuals who identified as American Indian or Alaska Native migrated during this period.

### *Fertility*

To examine fertility outcomes, I use the National Center for Health Statistic's (NCHS) Vital Statistics Natality Birth Data along with the Survey of Epidemiology and End Results (SEER) U.S. state and county population data. The natality data contains all births in the United States from 1985-2002 with information on the year of the birth as well as the mother's race, current place of residence, and age. The SEER population data gives population estimates at the county level by race, sex, and single year of age from 1990-2018. I combine these data from 1990-2002 to create general fertility rates by place of residence, year, and race.

I break up the fertility data into three types of locations: target MSAs, other MSAs, and the non-MSA parts of tribal states. I designate a state as "tribal" if in all years prior to 1952, at least 1% of the population identified as American Indian or Alaska Native in the decennial census.[8] Because the place of residence variable is censored in the publicly available natality data for counties with less than 100,000 people, I am unable to examine the fertility rates specifically for tribal lands. Instead, I use the tribal states as a proxy for people living on tribal lands as most of the Native American population lived on tribal land prior to the relocation program.

The general fertility rate equals the total number of births for a given location category in a given year divided by the total female population aged 15-45 for that location category and year multiplied by 1000. Therefore, the general fertility rate represents the total births per 1000 women aged 15-45 for the given location type in a given year.

## Methodology

### *Migration*

My main approach for answering the question of how the Relocation Program affected migration patterns to urban areas is a generalized difference-in-differences specification. Here, the two dimensions of the difference-in-differences approach are (1) geography: whether or not the MSA was targeted by the program and (2) time: when the MSA was targeted (if it was ever targeted). The main estimating equation takes the following form.

[8]These states are Arizona, Montana, Nevada, New Mexico, Oklahoma, and South Dakota

For MSA, $m$, in decennial year, $y$:

$$AIANPopulation_{my} = \beta(TargetMSA_m \times Post_y) + \gamma_m + \delta_y + \varepsilon_{my}$$

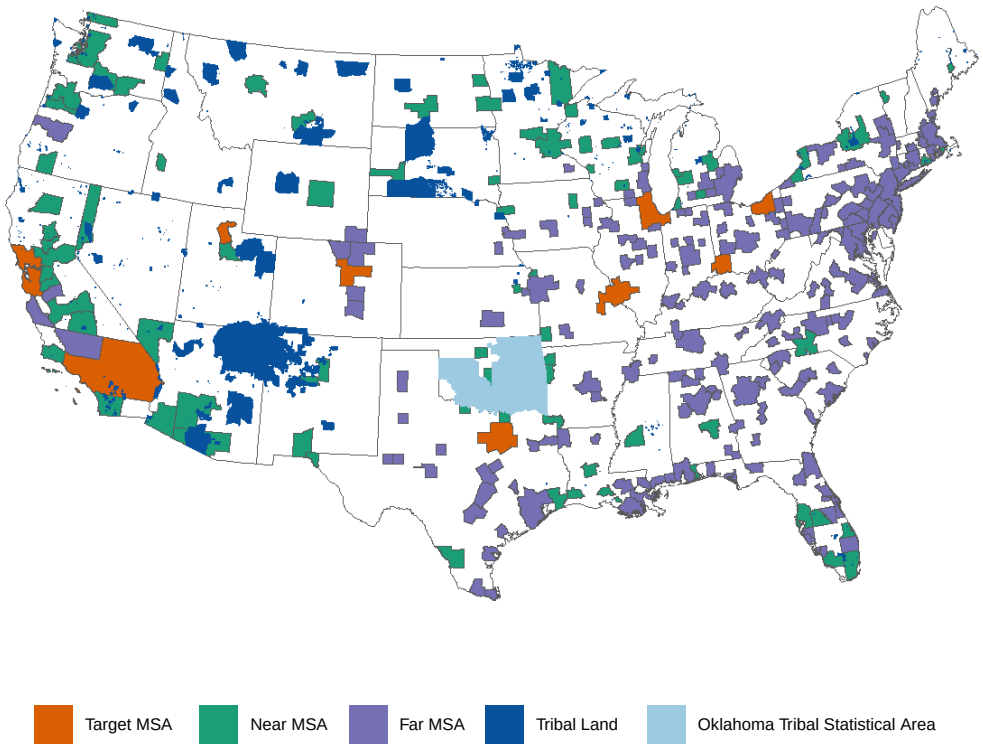
The dependent variable is the total Native American population in MSA, $m$, in a decennial year, $y$. For the main analysis, I include all MSAs. Thus, MSAs not targeted by the Relocation Program act as the control group. Note that non-MSA areas are dropped. In the map in Figure 9, the target cities appear in orange, while the main control group includes both the green areas (MSAs near reservations) and purple areas (MSAs that are not near reservations). The tribal lands and Oklahoma tribal statistical areas[9] are shown in dark blue and light blue, respectively, for reference.

The independent variable of interest is the interaction between $TargetMSA_m$ and $Post_y$. $TargetMSA_m$ is a dummy variable indicating that the MSA, $m$, is one of the cities targeted by the Relocation Program.[10] $Post_y$ is a dummy variable indicating that the decennial year, $y$, is after the start of the program in 1952. This means that for each of the decennial years from 1960 onwards, $Post_y$ takes on a value of 1, while for each of the decennial years from 1910-1950, $Post_y$ is 0. The interaction of these dummies gives us the program effect. $\gamma_m$ and $\delta_y$ represent fixed effects for the MSA and decennial year, respectively. $\varepsilon_{my}$ is the error term. The errors throughout the primary analysis are clustered at the MSA.

[9]Oklahoma Tribal Statistical Areas represent former reservation lands that existed prior to Oklahoma's statehood in 1907. The borders were determined by the U.S. Census Bureau in consultation with the American Indian tribes residing in Oklahoma.

[10]The target cities used for this analysis are Chicago, Cincinnati, Cleveland, Dallas, Denver, Los Angeles, San Francisco, San Jose, Salt Lake City, and St. Louis. An official list of target cities for the Relocation Program is not fully identified among historians. For this paper, I utilize the most consistent list of cities I was able to find in the available historical literature and administrative records.

FIGURE 9. Map of Relocation Program Target MSAs

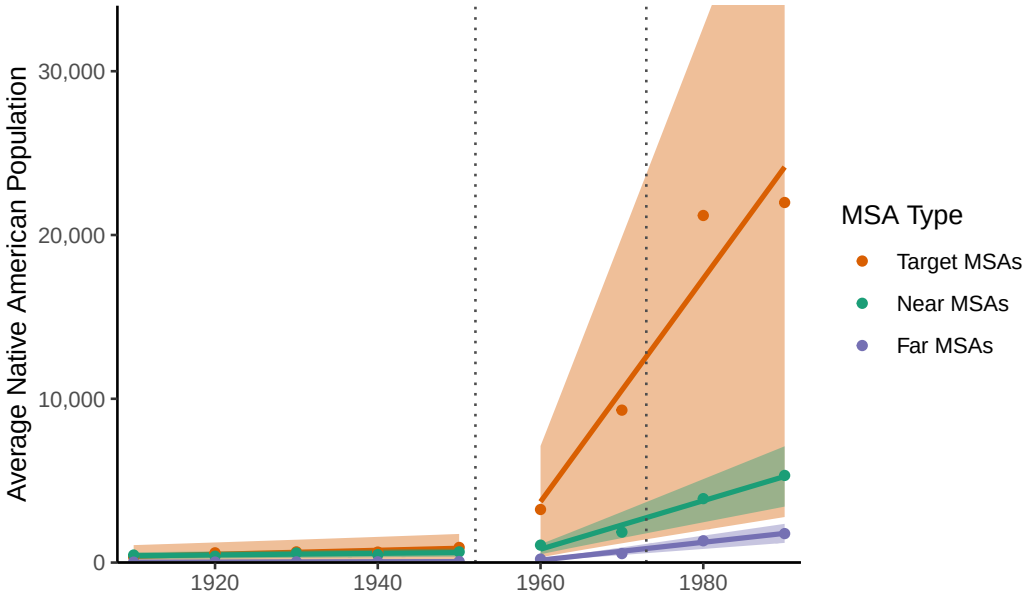



*Notes:* MSA borders are based on the 1990 census and tribal land borders are based on 2018 borders.
*Sources:* MSA: NHGIS; Tribal Lands: Bureau of Indian Affairs (BIA) Branch of Geospatial Support (BOGS) New Land Area Representation GIS dataset 2018

Identification for determining a causal effect of the program on migration relies on the assumption that absent the treatment, the control and treatment MSAs would have similar trends in Native American population counts. One way that this assumption is often supported is by showing parallel trends of the treatment and control units in the pre-treatment period. Figure 10 shows the trends in the Native American population for the three types of MSAs: the target MSAs, near tribal land MSAs, and all other MSAs. The colors in Figure 10 correspond to the map above as a reference for which MSAs fall into which of the three MSA types. Figure 10 suggests that prior to the introduction of the program,

all three types of MSAs had similar average levels (near zero) and trends (no growth) in the Native American population. After the introduction of the program in 1952, we see that the average Native American population in target MSAs quickly grows substantially larger than in the other two types of MSAs, providing a first indication that the program did have an effect on Native American migration patterns. To more carefully examine whether the parallel trends assumption likely holds, I also conduct an event study which supports the assumption that prior to the implementation of the Relocation Program, target MSAs and all other MSAs had similar trends in Native American population counts.

FIGURE 10. Native American Population Trends by MSA Type




*Notes:* Trends for the target and non-target MSAs from 1910-1990. Non-target MSAs are broken into two groups based on proximity to tribal land. The points represent average MSA Native American population. Best fit lines are estimated by MSA type for the pre and post periods. The dotted lines correspond to the start and end of the Relocation Program in 1952 and 1973, respectively. The standard errors are clustered at the MSA.
*Sources:* Author's calculations from Decennial Census Data

Using all other MSAs as the control group, however, has its drawbacks as the cities targeted by the program were mainly located in the West and Midwest, where there is more tribal land, while the majority the MSAs in the US are located in the South and on the East Coast. The geographic concentration of the target MSAs relative to the set of control MSAs could be a problem given that the start of the program coincided with the end of WWII which also may have been related to Native American migration to urban centers. If there was greater migration after the end of the war to cities near tribal lands, then this migration to target cities could in part be driven by post-WWII migration patterns rather than program effects. Therefore, I employ several more restrictive control groups in additional specifications. The first alternative control group includes only MSAs located near tribal land (MSAs colored green in Figure 9).[11] Near tribal land MSAs are chosen as they offer a plausible lowest cost alternative to migration through the Relocation Program as they were likely easier and cheaper to get to than other MSAs. One drawback of using this alternative control group is that with the expansion of the program in 1956 to include vocational and on-the-job training in areas closer to tribal land, it's possible that people chose to stay in these near tribal land cities where the job trainings took place rather than participating in the direct employment program after their training period ended. Thus, these MSAs may not be unaffected by the program. Therefore, the second alternative control group I consider is far from tribal land MSAs. This control groups includes only the MSAs that are not included in the near tribal land MSA group (shown in purple in Figure 9). This control group is considered because it is the least likely control group to be at all affected by the actual treatment from the program. The third alternative

[11] An MSA is considered a near tribal land MSA if it is the closest MSA to at least one tribal land area measuring from centroid to centroid.

control group includes only the MSAs that are in the same state as a target MSA. This sample includes all MSAs in California, Colorado, Illinois, Missouri, Ohio, Texas, and Utah. Finally, all but one of the target MSAs were among the top fifty most populous cities in the US at the time of the program's development. The fact that the target cities were more populous on average than the group of all other MSAs could have important consequences for my analysis since I am specifically looking at population changes. Therefore, the final alternative control group includes only the top fifty most populous MSAs in 1950.

### *Fertility*

To study the impacts of the Relocation Program on fertility, I compare second-generation fertility rates for women living in target cities to those of women living in other urban areas and the rural parts of tribal states. I focus specifically on second-generation outcomes for two reason. First, the previous migration program literature has suggested that the economic and health outcomes of such programs are greatest for those of the second-generation or the children of movers. Secondly, the publicly available data on births in the US that is disaggregated by geographic region and race is only available starting in 1990. Given the time of the program, the sample of women who are of childbearing age from 1990 onward is mainly second-generation women.

The main measure of fertility that I use is the generalized fertility rate which gives the number of births per 1000 women of childbearing age (15-45). Specifically, I calculate the generalized fertility rate for each year from 1990 to 2002 for three racial groups, namely, white, black, and Native American, and by three types of residence: target MSA, non-target MSA, and rural parts of tribal states. The rural

parts of tribal states area of residence includes all areas of tribal states that are not included in an MSA in 1990. I compare the general fertility rate across these three racial groups and the three types of residence to determine the unique fertility patterns among Native American women in target MSAs. While the fertility analysis does not provide causal estimates for the affect of the program on fertility outcomes, it does provide descriptive evidence into how fertility rates differ between these groups, providing valuable insight into how the program may impact long-term health and demographic outcomes.

## Results

### *Main Approach*

The results of the main analysis are presented in Table 8. Column 1 shows the results of the simple OLS specification without fixed effects, and column 2 adds year and MSA fixed effects. Both columns show a positive and statistically significant increase in the Native American population associated with the implementation of the Relocation Program. The estimates suggest that, on average, between 1960 and 1990, the Relocation Program increased the Native American population in target cities by almost 12,000 individuals relative to all other MSAs. This constitutes a major shift in the Native American population of these target cities considering that prior to the program the average MSA had a Native American population of about 1,000 people and the median MSA, less than 100 people.

TABLE 8. Baseline Native American Population Response to the Relocation Program

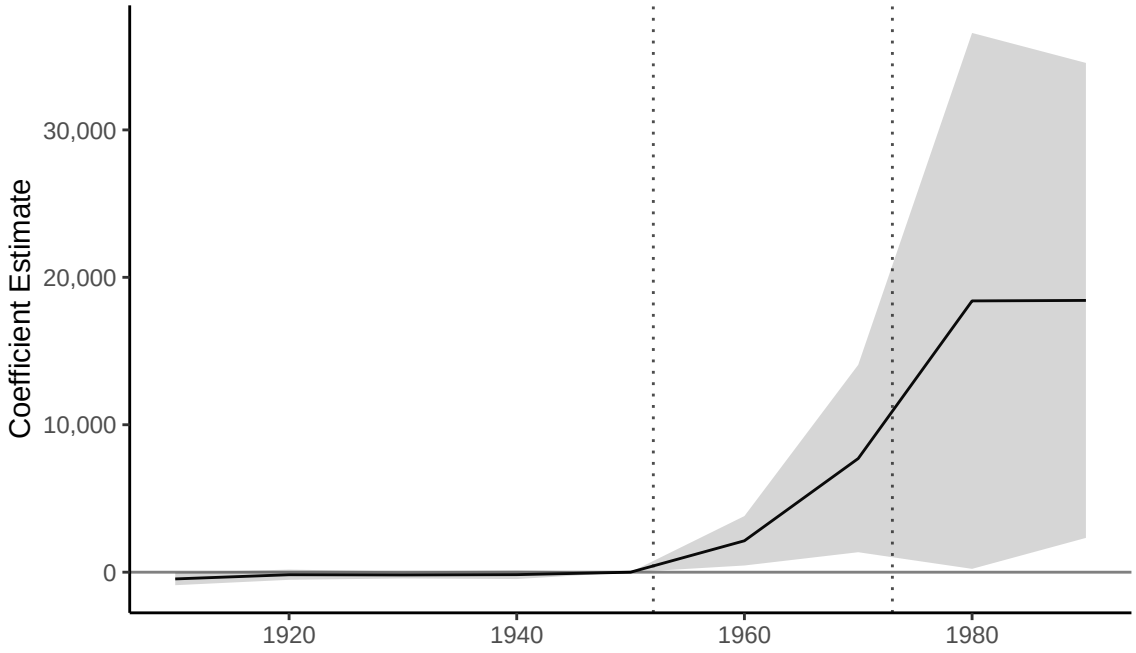
| *Dependent variable:* | Native American Population | |
|---|---|---|
| | (1) | (2) |
| Post | 1,446*** | |
| | (186) | |
| Target MSA | 432 | |
| | (362) | |
| Target MSA × | 11,856** | 11,866** |
| Post | (5,394) | (5,392) |
| Constant | 197*** | |
| | (40) | |
| MSA FE | No | Yes |
| Year FE | No | Yes |
| Observations | 2,526 | 2,526 |
| $R^2$ | 0.167 | 0.524 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01. The Native American population data is the total number of individuals registered by the U.S. Census Bureau as American Indian or Alaska Native for each decennial census year 1910-1990 by MSA. Standard errors are clustered at the MSA.

As a supplement to the main approach, I also run an event study analysis to examine how this average effect was distributed throughout the course of the program and to further establish the parallel trends assumption. The results of the event study are shown in Figure 11, which plots the interaction of the $TargetCity_m$ variable with each decennial year from 1910-1990. 1950 is the reference year. As noted in the methodology section, it is clear that prior to the implementation of the program, trends in Native American population for target and non-target MSAs are very similar. After the program was implemented in 1952, we can see that there is a sharp increase in the number of Native American individuals living in the target

cities relative to all other MSAs. This increase in the target MSA Native American population relative to the other MSAs continues through 1970 and 1980. After 1980, the effects of the program level off. This is not surprising as the program ended in 1973 (17 years prior to the 1990 census).

FIGURE 11. Estimates of the Relocation Program's Migration Effects over Time




*Notes:* The figure shows the coefficent estimates for the interaction of the target city dummy and each of the decennial years. The estimating equation includes MSA fixed effects, and the standard errors are clustered at the MSA. The gray ribbon represents the 95% confidence interval. The dotted lines represent the beginning and end of the program in 1952 and 1973, respectively

Finally, I also estimate the main regression with a number of different control groups as described in the methodology section. Table 9 shows the results of these regressions. For column 1, the control group is just the MSAs that are near tribal lands. These are the MSAs shown in green in Figure 9. For column 2, the control group is just the MSAs that are far away from tribal lands, as shown in purple in Figure 9. The control group in column 3 includes only the MSAs that are located in states with target MSAs. Column 4 uses a control group that consists of only

the 50 most populous MSAs around the time the target locations were being chosen in 1950. All four columns produce estimates that are similar in significance and magnitude to the baseline specification, further solidifying the main results of the primary approach. Notably, column 2, which consists of the control group that is least likely to have been partially treated, has the largest point estimate.

TABLE 9. Native American Population Response to the Relocation Program, Alternative Control Groups

| *Dependent variable:* | Native American Population | | | |
|---|---|---|---|---|
| | Near MSAs | Far MSAs | Target State MSAs | Top 50 MSAs |
| | (1) | (2) | (3) | (4) |
| Target MSA × Post | 10,780** | 12,394** | 12,043** | 10,206* |
| | (5,427) | (5,3956) | (5,437) | (6,058) |
| MSA FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 882 | 1,725 | 675 | 448 |

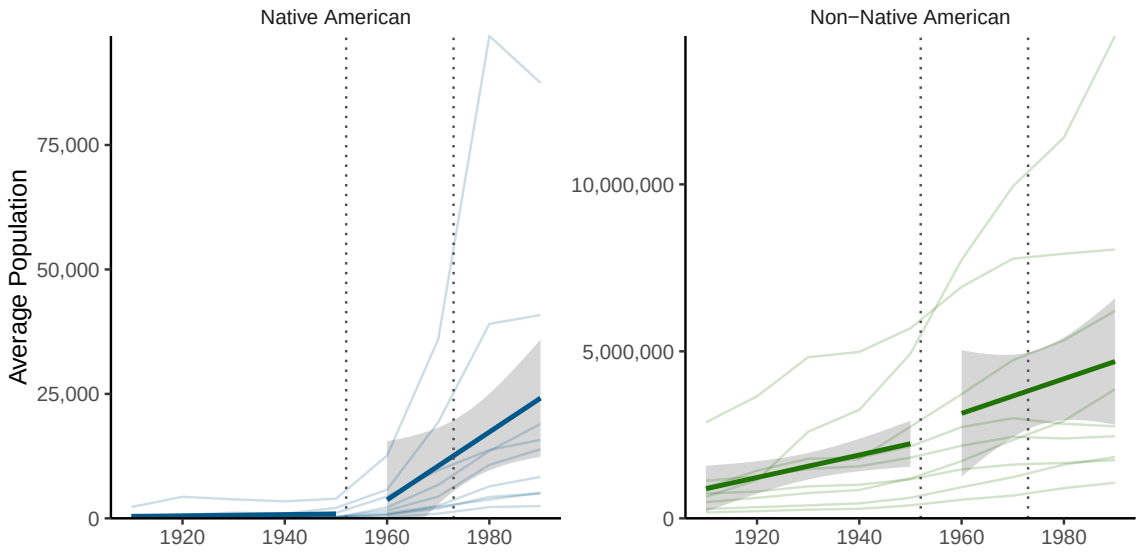
*Notes:* *p<0.1; **p<0.05; ***p<0.01. The Native American population data is the total number of individuals registered by the U.S. Census Bureau as American Indian or Alaska Native for each decennial census year 1910-1990 by MSA. Standard errors are clustered at the MSA.

*Additional Robustness Analysis*

One potential threat to identification in the main analysis would be if the program specifically targeted cities that were growing differentially at the time of the program's implementation for reasons other than the program itself. To examine this concern, I compare the change in Native American population to that of the non-Native American population in the target cities over the period of 1910-1990. Figure 12 shows the population trends in the target cities for the Native American population on the left and the non-Native American population on the

right. These graphs suggest that even with the large population growth in the target cities over the time period, there does seem to be differential trends for the Native American population following the start of the Relocation Program. This suggests that the changes in the Native American population in the target cities corresponding with the time of the Relocation Program seen in the main analysis are likely indeed a result of the program and not other attributes specific to the target cities from the 1950s onward.

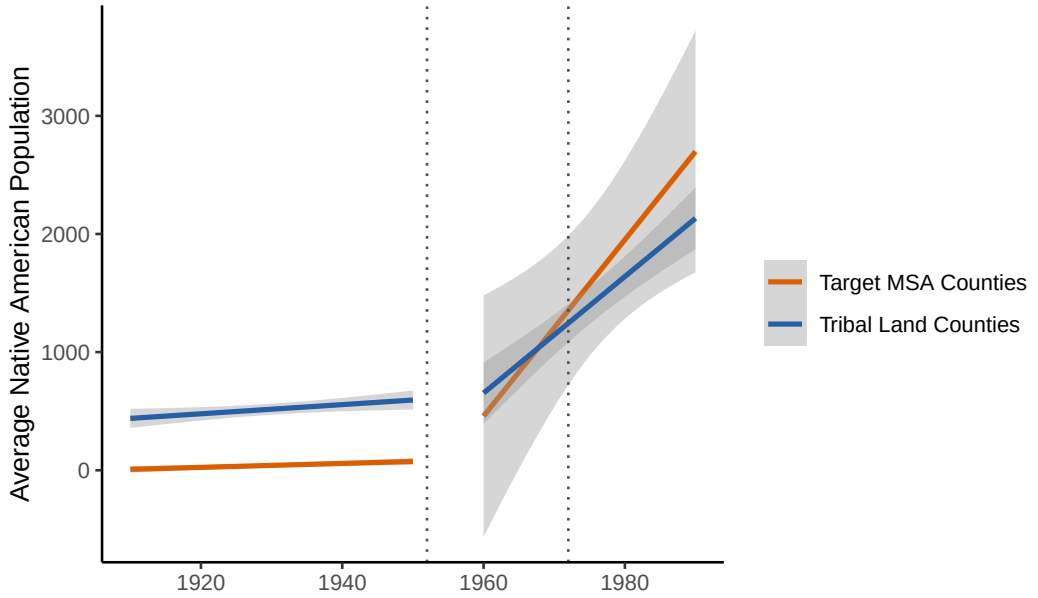
FIGURE 12. Target City Population Trends by Race




*Notes:* The bold lines estimate linear trends in MSA population by race for the pre and post periods. Background lines show individual MSA population totals by race for each of the target cities. The dotted lines depict the start and end of the Relocation Program in 1952 and 1973, respectively.
*Sources:* Author's calculations from Decennial Census Data

Given that one of the program's main aims was to move Native Americans off of tribal lands to urban centers, I also compare the average Native American population in target MSAs to the Native American population on tribal lands. Because there is no data available on Native American population counts by reservation throughout this time period, I examine Native American population

FIGURE 13. Native American Population Trends for Target MSAs versus Tribal Land




*Notes:* Trends in average county-level Native American population for the target MSA and tribal land counties from 1910-1990. Best fit lines are estimated by county type for the pre and post periods. The dotted lines correspond to the start and end of the Relocation Program in 1952 and 1973, respectively.

counts at the county level. Specifically, I compare average Native American population counts in counties that are in target MSAs to those counties that contain tribal land as shown in Figure 13. Figure 13 suggests that the increase in the Native American population in the target MSAs is so large that the average county-level target MSA Native American population may even exceed that of tribal land counties by the end of the relocation program.

I also verify my main results by using a doubly robust difference-in-differences specification. The second level of robustness relies upon the propensity-score methods literature (Rosenbaum & Rubin, 1983)—i.e., inverse probability weighting. Using logistic regression, I model the probability of an MSA being targeted by the program as a function of total population, percent of the population that is

non-white, percent change in the total population since 1940, percent change in the non-white population since 1940, the male and female employment rate, and the minimum distance to a tribal land area. Using the logit regression estimates, I calculate inverse probability of treatment weights using the formula: $w_i = \frac{TargetMSA_m}{p_m} + \frac{1-TargetMSA_m}{1-p_m}$, and apply these weights to my baseline difference-in-differences estimating equation to form a doubly robust estimator (Hirano et al., 2003). The results of the doubly robust inverse probability weighted regression are found in Table 10. While the estimate of the coefficient of interest on the interaction of $TargetMSA \times Post$ is smaller than the baseline estimate, it is well within the traditional confidence intervals and still suggests substantial movement of the Native American population as a result of the Relocation Program.

TABLE 10. Doubly Robust DiD using Inverse Probability of Treatment Weighting

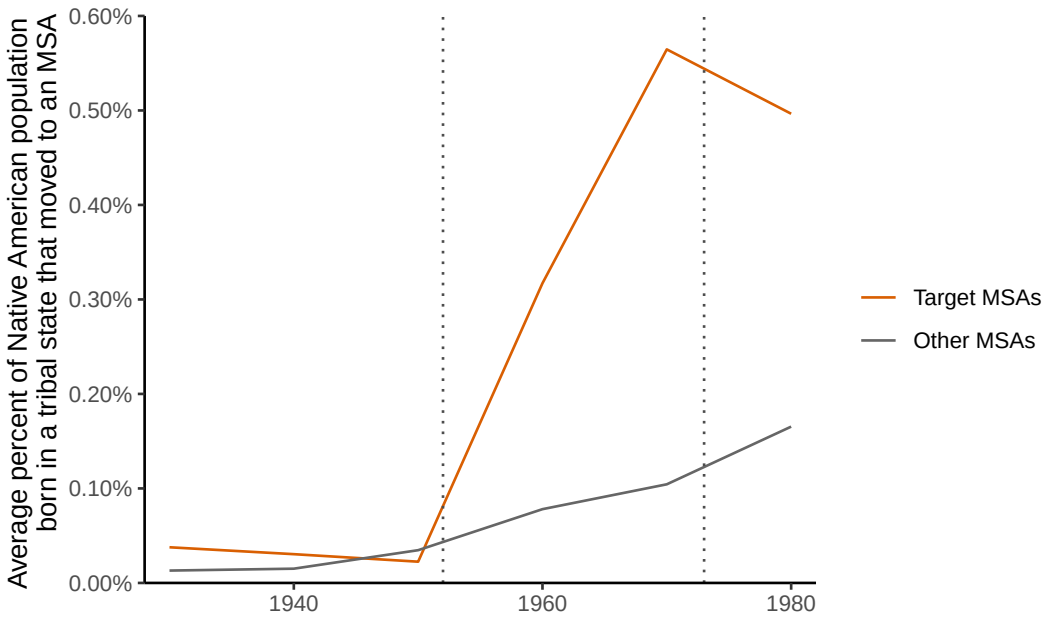
| *Dependent variable:* | Native American Population |
|---|---|
| Target MSA × Post | 6,714*** (2,273) |
| MSA FE | Yes |
| Year FE | Yes |
| Observations | 954 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01. The Native American population data is the total number of individuals registered by the U.S. Census Bureau as American Indian or Alaska Native for each decennial census year 1910-1990 by MSA. Standard errors are clustered at the MSA.

Finally, up to this point, my analysis has focused on total population counts rather than specific migration variables. This is mainly due to a lack of available data on Native American migration over the period during which the program ran. There is, however, a limited amount of data available in the IPUMS samples which I use to examine migration in Figure 14. Here I explore the place of residence by

year for Native Americans born in one of the six states with the highest Native American population prior to the start of the program in 1952.[12] Figure 14 shows the average percent of individuals who were born in a tribal state that are living in a target MSA versus the average percent living in a non-target MSA at the time of the census. While migration out of tribal states seems to be increasing for both groups in 1960 and 1970, there is a much higher average percent of people moving to the target MSAs than non-target MSAs. Overall, this supports the main findings that Native Americans were migrating to target cities more than other MSAs during the time of the Relocation Program.

FIGURE 14. Migration Trends for Native Americans Born in Tribal States




*Notes:* Lines show the average percent of the Native American population that was born in one of the six tribal states that now live in an MSA. The MSAs are broken into two types based on whether or not the MSA was a target MSA.
*Sources:* Author's calculations from IPUMS Data

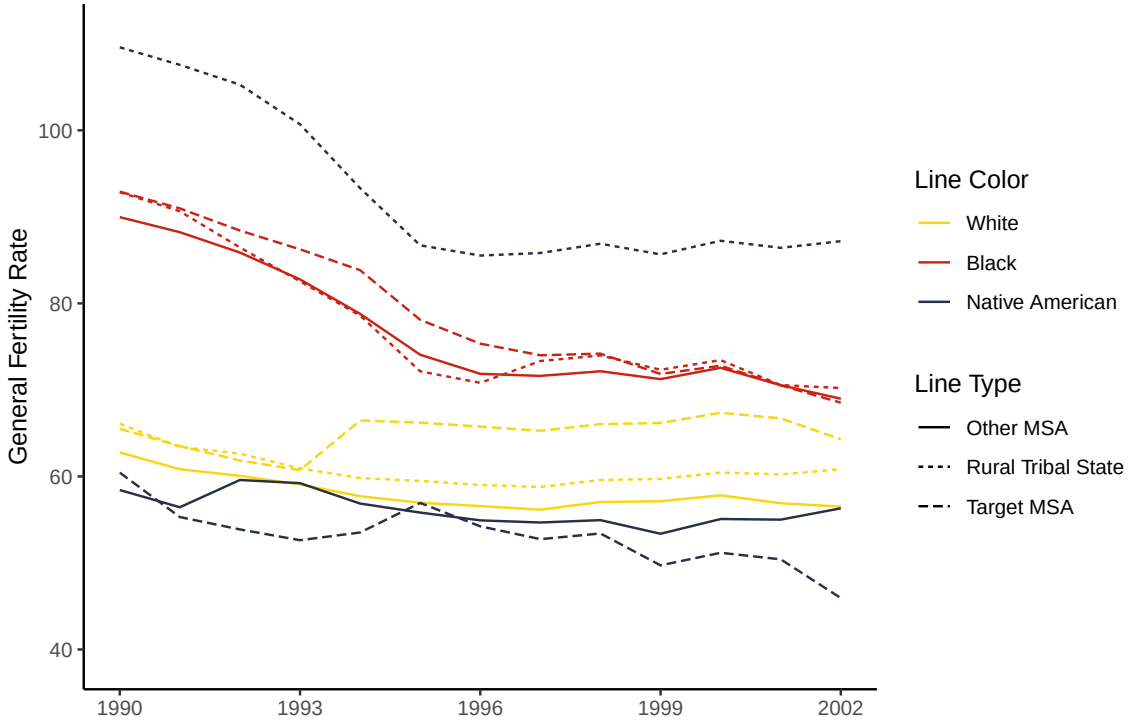
[12]The tribal states here are the same those chosen for the fertility analysis. As a reminder these states are those with at least 1% of the population identifying as American Indian or Alaska Native in the decennial census for all years 1910-1950.

## Fertility Differences

The fundamental aim of the Relocation Program was to alter the distribution of Native Americans in the US and to promote assimilation. In the preceding sections, I have documented the cross-sectional effects of the program, which show a shift in the Native American population from rural reservations to urban centers targeted by the Relocation Program. In this section, I examine the long-run fertility differences among Native American women living in target MSAs versus tribal states.

Before I present my fertility results, it is important to note that in the 1970s there was widespread forced sterilization of Native American women living on reservations. Because the Indian Health Services had such singular control over healthcare on tribal lands, Native American women were particularly vulnerable to this practice during this period. Estimates suggest that one in four Native American women of childbearing age were forcibly sterilized at this time (Lawrence, 2000). Given that I see even lower rates of fertility for Native American women living in urban areas compared to tribal land ares, this period of forced sterilization does not change how I understand these fertility data as they relate to the Relocation Program. It does, however, provide important context for this discussion.

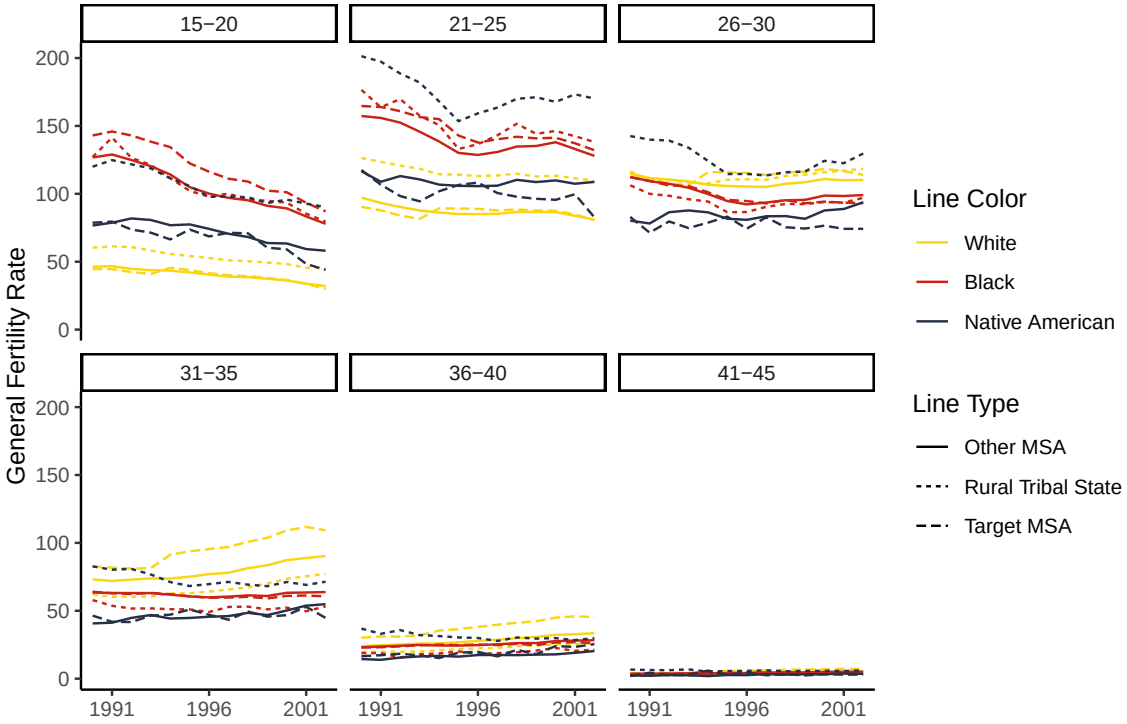
FIGURE 15. General Fertility Rate by Race and Current Residence




*Notes:* The general fertility rate represents the number of births per 1,000 women age 15-45. The lines above show the general fertility rate by race for each of three types of current residence: target MSA, other MSA, and rural tribal state.
*Sources:* Author's calculations from NVSS Natality and SEER population data

Figure 15 shows the general fertility rate for Native American women living in target MSAs, non-target MSAs, and the rural parts of tribal states from 1990 to 2002. Figure 15 suggests that there is a large gap in the fertility rate between those individuals living in an MSA versus those living in non-MSA areas of tribal states. This provides some preliminary evidence of how the Relocation Program may have contributed to shifting fertility outcomes for Native American women.

FIGURE 16. General Fertility Rate by Age




*Notes:* The general fertility rate by age represents the number of births to women within the specified age range per 1,000 women in the age group.
*Sources:* Author's calculations from NVSS Natality and SEER population data

Figure 16 disaggregates the fertility rate by age and shows that the gap is largest for women ages 21-25, with the gap shrinking steadily in the older age groups. This suggests that Native American women living in cities are likely both delaying fertility as well as having less children overall. Given the timing of the program, these women whose fertility I am examining here can most likely be considered part of the second generation of movers (i.e., the children of the original movers who were part of the program). Further research into the first generation's fertility outcomes and additional heterogeneity analysis may help

explain the mechanisms driving the disparity in fertility rates seen here.[13] The fact that these differences in fertility exist in the generation after the Relocation Program, however, suggests long-term, persistent demographic effects from the two-decade long program.

### Tribal Land Population Counterfactuals

To better understand the magnitude of these migration and fertility effects on the tribal land population, I calculate a set of back-of-the-envelope counterfactual population estimates for the tribal land areas. I consider four possible counterfactual populations:

(1) Adjusted Migration, Unadjusted Birth Rate: the tribal land population had those induced to migrate by the program remained on tribal land with no adjustment to the birth rate for the period of forced sterilization

(2) Adjusted Migration, Adjusted Birth Rate: the tribal land population had those induced to migrate by the program remained on tribal land and had there been a 33% increase in the birth rate on tribal lands after 1970 to correct for the estimated one quarter of women forcibly sterilized

(3) Unadjusted Migration, Adjusted Birth Rate: the tribal land population with no migration adjustment but with a 33% increase in the birth rate on tribal lands after 1970 to correct for the estimated one quarter of women forcibly sterilized

(4) Unadjusted Migration, Unadjusted Birth Rate: the tribal land population with no migration adjustment and with no adjustment made to the birth rate

[13] A lack of available Native American population data disaggregated by sex and age precludes me from analyzing these questions here.

I create this fourth counterfactual population estimate with no adjustments for migration or forced sterilization as a comparison group for the other three population estimates rather than using the census population estimates because there is a substantial increase in individuals identifying as "American Indian/Alaksa Native" in the census during this period. There is no evidence of a differential increase in identifying as Native American across my treatment and control MSAs, so this should not cause any problems for the identification of my main findings. It does, however, make it hard to compare my counterfactual population estimates. Thus, I will instead be using the population described in (4) above as a estimate of what the population growth on the tribal lands would have looked like under the actual historic conditions.

To estimate the counterfactual Native American population living on tribal land, I use the following population growth model:

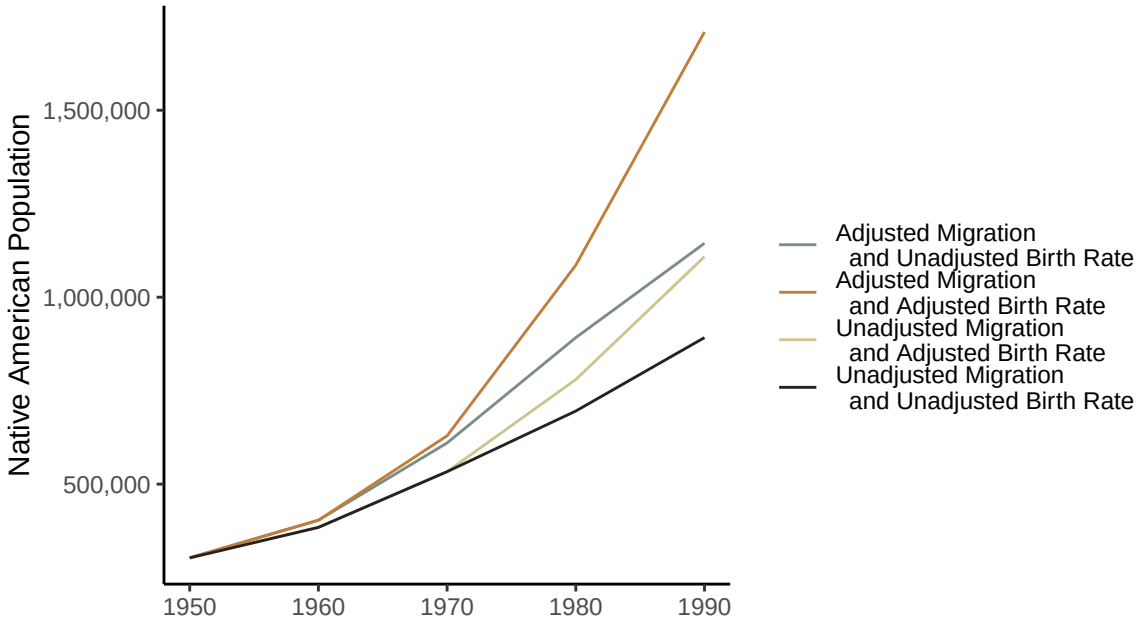
For decennial year, $y$:

$$AIANPopulation_{y+10} = AIANPopulation_y \times (1 + r)^{10} + Migration_{y+10}$$

Here, $AIANPopulation_y$ is the Native American population living on tribal land in decennial year $y$ and $AIANPopulation_{y+10}$ is the Native American population living on tribal land in the following decennial year, 10 years after $y$. The variable $r$ is the population growth rate. $r$ is equal to the per capita birth rate minus the per capita death rate for Native Americans living in the tribal states described in the data section. Finally, $Migration_{y+10}$ is the estimated total migration in decennial year $y + 10$ based on the event study estimates in Figure 11. I calculate total migration for each decennial year by multiplying the coefficient by the number of

treated units and subtracting the previous decennial year coefficient as the event study coefficients are cumulative.

FIGURE 17. Counterfactual Native American Population on Tribal Land




*Notes:* The four models in the figure match the descriptions at the beginning of this section. Birth and death rates are based on births and deaths in the tribal states as described in the data section. Population estimates are based on the Native American population living on tribal land. *Sources:* Author's calculations from NVSS Natality data and author estimates of decennial migration induced by the Relocation Program.

As you can see in Figure 17, there would have been a substantially larger Native American population living on tribal land in 1990 if individuals had remained on tribal lands rather than migrating as a result of the Relocation Program. My population growth model suggests that absent the Relocation Program, there would be over 250,000 additional Native Americans living on tribal lands in 1990. Thus, in addition to the increased population from returning the roughly 100,000 estimated migrants back to the tribal land area, there is a further increase of 150,000 Native Americans living on tribal lands associated with the tribal land population's natural growth rate. Moreover, this additional population

would have been even more substantial if the forced sterilization in the 1970s had not decreased the birth rate. In fact, model 2 suggests that absent these factors, the Native American population living on tribal lands in 1990 would have been almost double that of the unadjusted population estimates in model 4.

## Conclusion

This paper shows how government-sponsored migration programs can affect the migration patterns of targeted populations and potentially impact long-run fertility. The Relocation Program had large and significant impacts on the migration outcomes for Native Americans in the second half of the $20^{th}$ century. This policy led to a sorting of the Native American population into the program's target MSAs relative to all other MSAs, shaping the distribution of the Native American population across the United States today. The magnitude of these impacts for the tribal land area Native American population is made clear in the counterfactual population estimates, which suggest that absent the Relocation Program, there would have been an addition 250,000 Native American individuals living on tribal lands in 1990. The gap in the fertility rate between Native Americans living in MSAs versus traditionally high Native American population states suggests that there are likely important long-run consequences for those affected by the program.

The findings of this paper suggest some fulfillment of the program's stated aim–to relocate Native Americans away from tribal lands and into urban centers with the overarching goal of assimilation. There is strong evidence of relocation of the Native American population to the cities targeted by the program. Further,

the fertility findings provide some suggestive evidence of assimilation among those living in the target cities.

While I am able to document one important dimension of the Relocation Program by determining the effects on migratory outcomes, the full impacts of this movement from reservations to major urban centers have yet to be examined. By establishing the effect of the Relocation Program on migration, this paper sets up a first stage for further research into the long-term economic and health outcomes for those induced to move by the program.

# CHAPTER IV

# RIGHT TO CARRY LAWS AND INTIMATE PARTNER HOMICIDE

## Introduction

Gun control is a fiercely debated topic in the United States. The regulation of who and under what circumstances individuals can carry concealed handguns is a topic that has grown increasingly prominent within this discussion and which has seen substantial changes in recent years. At the state level alone, there have been more than fifty changes made to concealed carry regulations in the past four decades including a recent wave of eleven states adopting no permit[1] concealed carry in the past five years. In fact, the Supreme Court has just this year agreed to review its first major gun control case in the past decade on the topic of states rights to require permits for concealed carry weapons.[2] Given the perennial and divisive nature of this debate and the steady adoption of changes to concealed carry laws in recent years, understanding the effects of concealed carry laws on violent crime and homicide is vital.

While there is a vast literature examining these questions, there is still uncertainty about the overall impact of concealed carry laws on violent crime and homicide. One topic within this literature that is understudied is the effect of concealed carry on homicides committed by an intimate partner. Access to guns is known to be a major risk factor for intimate partner homicide (Bailey et al., 1997; Campbell et al., 2003). Therefore, it is important to understand how laws that ease

[1]No permit style concealed carry regulations mean that an individual can carry a concealed handgun in public without having to go through any type of permitting process.

[2]Scott Neuman and Nina Totenberg. "Supreme Court To Take Up 1st Major Gun Rights Case In More Than A Decade." *NPR*. April 26, 2021.

restrictions on gun access and increase the amount of time spent carrying a firearm may impact intimate partner homicide rates.

In this paper, I analyze how right to carry laws[3], which are the less restrictive set of concealed carry regulations, affect rates of intimate partner homicide using data from the Uniform Crime Reporting Program's Supplementary Homicide Reports. To answer this question, I employ three methods, namely: generalized difference-in-differences, event study, and synthetic control methods (SCM). Using these data and methodologies, I test a number of specifications with variation in sample, fixed effects, and controls. Across specifications, I find no evidence for an impact of right to carry laws on the rate of female victim intimate partner homicide. I find only limited evidence of an impact on intimate partner firearm homicide rates for male victims. Specifically, the adoption of right to carry laws are associated with a decrease in the rate of male victim intimate partner firearm homicides in some specifications using the generalized difference-in-differences methodology. However, these findings are sensitive to reasonable variation in specification and are not seen when using synthetic control methods. The event study specifications suggest a possible violation of the parallel trends assumptions which could be driving the difference-in-differences findings.

Overall, this paper shows that there is no strong evidence for an impact of right to carry laws on intimate partner homicide rates, and any limited evidence of a protective effect is solely realized for male victims. Given that 75% of male victim intimate partner homicides are predated by male perpetrated intimate partner violence, this suggests that the most likely mechanism driving these results is a deterrent effect of right to carry laws (Campbell et al., 2003). Specifically, female

[3]Right to carry laws include both "shall issue" and "no permit" concealed carry laws.

partners who would have otherwise resorted to lethal means to escape a violent relationship may be deterred from this extreme measure if they are concerned their male partner may be more likely to be carrying a firearm. Additionally, these findings suggest that when considering the impacts of right to carry laws, estimates based on a single or only a select few specifications should be approached with caution. Rather, researchers analyzing right to carry laws should provide readers with specification sensitivity analysis so that it is clear how researcher choice impacts treatment findings.

This paper primarily contributes to our understanding of the broad effects of concealed carry laws and the impacts of gun laws on intimate partner homicide. Because guns are known to be a significant risk factor for intimate partner homicide, there have been a number of studies examining the impacts of gun laws on rates of intimate partner homicides. Studies have found that certain domestic violence restraining order laws (Vigdor & Mercy, 2006; Zeoli & Webster, 2010), the expansion of the gun control act (Raissian, 2016), and laws which prohibit firearm possession for those convicted of violent misdemeanors (Zeoli et al., 2018), all reduce rates of intimate partner homicide. Overall, this body of work suggests that there are important protective effects against intimate partner homicides of restricting access to firearms for certain groups of people under specific circumstances. This paper contributes to this work by building up our understanding of how right to carry laws specifically impact rates of intimate partner homicide.

This paper also contributes to our understanding of how the loosening of restrictions on concealed carry impacts different types of violent crimes and homicide. There is still much debate within the concealed carry literature on the

impacts of concealed carry laws on violent crime. In their seminal paper on the topic, Lott and Mustard (1997) find that less restrictive concealed carry laws lead to lower rates of violent crime with some evidence of low levels of substitution to property crime. Considerable work since Lott and Mustard's initial findings suggests that their results are quite sensitive to model specification and that there is more work to be done to establish a clear understanding of the associations between concealed carry and crime. In 2004, the National Research Council, part of the National Academy of Sciences, investigated the early research and determined that there was not enough evidence to support the findings that loosening restrictions on concealed carry decreases violent crime (National Research Council, 2004). Since 2004, there have been a number of studies that have continued to shed light on this question (La Valle & Glover, 2012; Aneja et al., 2014; Donohue et al., 2019; Hamill et al., 2019, and many others) with many finding no significant or strong association between less restrictive concealed carry laws and rates of violent crime or homicide. There is still no consensus, however, with some papers finding positive or negative associations among certain crimes or within certain changes of the law. One recent paper that examines the impacts of concealed carry laws on intimate partner homicide is Roberts (2009). Roberts (2009) finds that there is a decrease in the overall rate of intimate partner homicide associated with less restrictive concealed carry laws, but he finds no effects on intimate partner firearm homicide rates. Given that concealed carry regulations are not the main focus of Roberts' paper, he ends his discussion of the findings by noting that he is unable to explain these counterintuitive results.

This paper extends the small literature examining the relationship between right to carry and intimate partner homicide to shed new light on the topic. I

contribute to the discussion in a number of ways. First, I apply new techniques and consider additional specifications to test the robustness of treatment effects to help further our understanding of inconsistencies in the concealed carry literature. Also, I analyze about twenty years of additional data. This is of particular importance given that the violent crime and concealed carry literature has shown results are sensitive to the time period studied. Specifically, I add fifteen more years of more recent data which are more relevant for new policy and suffer less from contemporaneous issues like the violent crime spike in the 1990s. Finally, I disaggregate my results by victim sex to better understand what might be driving findings. Disaggregating by victim sex is vital within this setting because the circumstances surrounding female versus male victim intimate partner homicides are generally quite different. Therefore the mechanisms driving results for each group are also likely to be different. For instance, roughly 70% of all intimate partner homicides are predated by male perpetrated violence (Campbell et al., 2003). Therefore, instances of female victim intimate partner homicide are typically thought of as an escalation of male partner violence whereas male victim intimate partner homicides are more often thought to be an extreme means by which female partners attempt to escape violent relationships.

In addition to contributing to the concealed carry and intimate partner homicide literatures, this paper also contributes to the literature analyzing researcher choice. I find that my difference-in-differences results are highly sensitive to defensible researcher choice of sample, fixed effects, and control variables. These findings solidify what much of the research within this literature suggests–that a more formal process for checking robustness to alternative specifications or sensitivity analysis should be common practice to provide transparency about the

impacts of the research design process on the results. In this paper, I provide a specification chart to formally illustrate specification sensitivity based on work by Harrison and Rubinfeld (1978), Leamer (1983), and Simonsohn et al. (2020) – using Ariel Ortiz Bobea's design and R code.

## Concealed Carry Laws and Intimate Partner Homicide

Concealed carry laws permit people to carry concealed handguns in public. Intimate partner violence and homicides are often thought of as occurring inside the home. Therefore, focusing on the impacts of concealed carry laws on intimate partner homicides may initially seem counterintuitive. There are, however, a number of ways by which the loosening of restrictions on concealed carry weapons could impact intimate partner homicide. Firstly, less restrictive concealed carry laws may increase the likelihood that a perpetrator has a gun on them when a violent incident occurs, thereby increasing the probability that the violent incident escalates to homicide. Secondly, I find some evidence that loosening restrictions on concealed carry weapons increases overall gun sales[4]. Thus, it may be the case that more permissive concealed carry laws could increase intimate partner homicides by simply increasing the overall number of guns in possession. Thirdly, it is possible that stricter concealed carry laws, which require more oversight in the permitting process, serve as an additional check on the illegal possession of firearms. Therefore, by loosening concealed carry laws, individuals who are prohibited from owning firearms may be less likely to have any illegally possessed firearms confiscated.

[4]The results for this analysis can be found in the Appendix in Table 1.

On the other hand, it is also possible that loosening restrictions on concealed carry weapons may have a protective effect. It is possible that loosening concealed carry laws could allow for a greater capacity for self-defense from intimate partners. It is also possible that less restrictive concealed carry laws could have a deterrent effect. For example, if people believe that it is more likely their partner will have a gun on their person under a less restrictive concealed carry law, then they may be less likely to engage in lethal violence.

## Data

### *Supplementary Homicide Report*

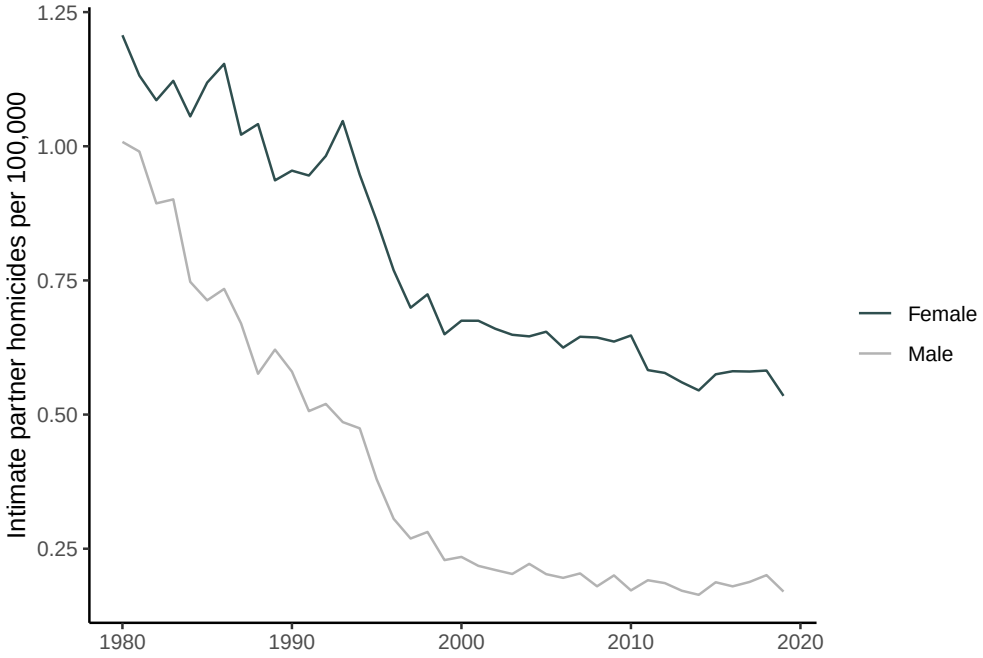
Data on intimate partner homicides come from the Uniform Crime Report's Supplementary Homicide Report (SHR). The SHR includes detailed information on homicides reported to the FBI by local law enforcement agencies starting in 1976. Importantly, the SHR includes information on the relationship between the victim and the offender which is used to determine instances of intimate partner homicide. A homicide is considered an intimate partner homicide if the victim-offender relationship is defined as spouse, ex-spouse, dating partner, or homosexual relationship. Because of the marked differences in the circumstances surrounding intimate partner homicides of males versus females, I also use the details on the sex of the victim to separately analyze intimate partner homicides by victim sex. In my main analysis, I include only homicides that were committed using a firearm given that I am examining the effects of firearm restrictions. I also limit the observations to only homicides that involved a single victim and single offender as is common in the intimate partner homicide literature because the relationship variable is only defined for the first victim. I combine the SHR data with the Surveillance,

Epidemiology, and End Results Program's (SEER) U.S. Population Data 1969-2019 to create a panel dataset of annual, state-wide intimate partner homicide rates.

One drawback of using the SHR is that reporting is voluntary and fluctuates over time. Further, there is no distinction made between instances of non-reporting and instances of zero homicides. Therefore, I consider both the entire sample of what is available from the SHR as well as a limited sample. The limited sample includes only state-year observations where the number of total homicides reported in the SHR is at least 50% of the number of homicides reported by vital statistics data. The information on SHR and vital statistics matching is available from 1980-2019 by the National Center for Juvenile Justice. To allow for a more direct comparisons between these two samples, I limit all analysis to 1980-2019. For state-year observations with the 50% match, I impute zeros on any subsets of the data which have no observations.

Figure 18 shows the trends in intimate partner homicide rates in the US separately for male and female victims. Overall, intimate partner homicide rates have decreased substantially in the past four decades, with the decline leveling off in the 2000s. Intimate partner homicide rates decreased more dramatically for male victims, falling by about 75% in the 1980s and 1990s.

FIGURE 18. Intimate Partner Homicides per 100,000 by Victim Sex



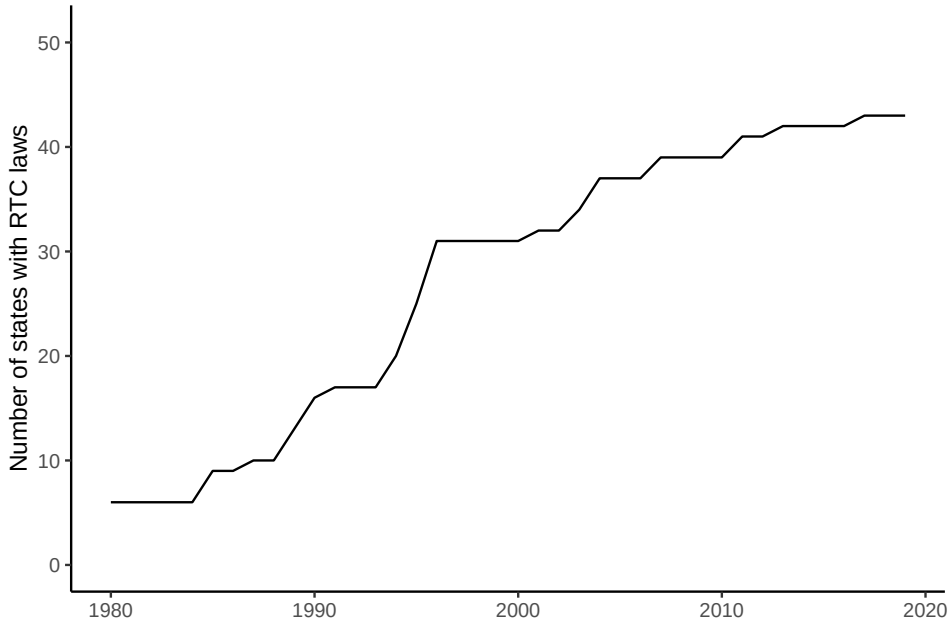

*Sources:* Author's calculations from the Supplementary Homicide Reports and SEER population data

*RAND State Firearm Law Database*

Information on state-level concealed carry laws comes from the RAND State Firearm Law Database. This database includes information on all state-level concealed carry laws including when the law was passed and the level of the law's restrictiveness. The concealed carry laws are broken down into four categories, namely: prohibited, may issue, shall issue, and permit not required. Prohibited means that it is unlawful to carry a concealed handgun. May issue means that if an individual meets certain criteria established by the law then a permit may or may not be issued based on the discretion of the police. In this case, individuals applying for permits usually must provide a reason for needing a concealed carry permit. Shall issue means that if an individual meets the criteria established by

the law then they will be granted a concealed carry permit. Finally, permit not required means that it is lawful to carry a concealed handgun without obtaining a permit. Prohibited is the most restrictive category while permit not required is the most permissive. States with concealed carry laws that are shall issue or permit not required are considered to have right to carry (RTC) laws. In this paper, I analyze the impacts of these right to carry laws on intimate partner homicide rates.

FIGURE 19. Number of States that have Adopted Right to Carry Laws over Time




*Sources:* Author's calculations from the RAND state firearm database data

Throughout the time period analyzed, all the changes that states made to their concealed carry laws served to make concealed carry less restrictive. Figure 19 shows how state adoption of right to carry laws has grown over time. The most significant period of state adoption of right to carry laws occurred between 1994 and 1996 when fourteen states switched to right to carry. By the end of the period 43 of 51 states (counting the District of Columbia) had right to carry laws in place.

*Control Variables*

I include a number of control variables across various specifications. My baseline controls are the unemployment rate and the percent of the population that lives in a metropolitan statistical area (MSA) as unemployment and urbanization have been shown to impact a state's likelihood of adoption of right to carry laws as well as the intimate partner homicide rate. Data on unemployment comes from the St. Louis Federal Reserve's FRED database and data on the percent of the state population living in an MSA is calculated using SEER population data and US Census MSA delineation files. I also consider a number of other economic, demographic, regulatory, and criminal justice system variables common in the concealed carry and violent crime literature and the intimate partner homicide and gun regulation literature. Information on what each measure means and where is comes from is provided below, while details on the inclusion of each measure across specifications is outlined in the methodology section.

Additional economic variables include information on real per capita personal income, the poverty rate, maximum AFDC/TANF benefit levels for a family of three, and the female to male employment ratio. Data on real per capita income is from the St. Louis Federal Reserve's FRED database. Information on the poverty rate and the maximum AFDC/TANF benefit levels for a family of three comes from the National Welfare Data that is made available through the University of Kentucky's Center for Poverty Research. The female to male employment ratio is calculated using information from the Current Population Survey (CPS) made available through IPUMS. I also use the CPS to calculate a number of the demographic variables including the proportions of the population that are white and black as well as the proportions of the population aged 15 and older who are

married, divorced, and widowed. The other demographic variables considered are the set used in the Donohue et al. (2019) paper and are calculated using the SEER population data. They consist of 6 age-sex-race groups that include white males 15-19, black males 15-19, other males 15-19, white males 20-39, black males 20-39, and other males 20-39. Data on incarceration and police employment are available through the FBI's Uniform Crime Report and are used to calculate lagged incarceration and police employment rates. I also control for the per capita level of ethanol consumption from beer using data made available by Jacob Kaplan from the Apparent per capita Alcohol Consumption: National, State, and Regional Trends, 1977-2016 report from the National Institute of Health.

Lastly, I also control for several laws related to intimate partner homicides. These include unilateral divorce laws, domestic violence misdemeanor firearm confiscation laws, laws for police seizure of firearms at scenes of domestic violence, and domestic violence restraining order firearm dispossession laws. Information on unilateral divorce laws comes from Gruber (2004) with updates from Ciacci (2018). Data on domestic violence misdemeanor firearm confiscation laws are from Raissian (2016). Information on laws for police seizure of firearms at scenes of domestic violence is available from Vigdor and Mercy (2006), which I updated with information from the Giffords Law Center to Prevent Gun Violence. Finally, data on domestic violence restraining order laws come from Zeoli et al. (2018).

## Methodology

I take three broad approaches to determine how right to carry laws impact intimate partner homicide rates. Firstly, I use a generalized difference-in-differences approach. Within the difference-in-differences framework, I consider a number of

specifications with different samples, weighting, and sets of time-varying state-level controls. I also employ an event study specification. This approach builds on the difference-in-differences method by explicitly illustrating pre-trends and by deconstructing the average treatment estimates from the difference-in-differences specifications to determine how treatment effects vary over time. Finally, I use a synthetic control methods approach to improve the pre-trend match between the treated and control units. The details of each methodology and the particulars of each specification considered are outlined below.

*Method 1: Generalized DD Regression*

For the difference-in-differences approach, the general estimating equation takes the following form:
For state, s, in census region, r, in year, y:

$$IPH_{sy} = \alpha + \beta RightToCarry_{sy} + \gamma \mathbf{X_{sy}} + \delta_y + \lambda_s + \mu_{ry} + \varepsilon_{sy}$$

Here, $IPH_{sy}$ is the rate of intimate partner homicide for a given state in a given year per 100,000 population. In my main specifications, I include only intimate partner homicides committed with a firearm in the calculation of the intimate partner homicide rate and calculate separate rates for male and female victims. As a robustness check, I also examine the rates of non-firearm intimate partner homicides for male and female victims. $RightToCarry_{sy}$ is an indicator for whether or not a given state has a right to carry law in place in year y. $\delta_y$ and $\lambda_s$ are fixed effects for year and state, respectively, and $\mu_{ry}$ is a fairly strict region by year fixed effect. $\varepsilon_{sy}$ is the error term. For all difference-in-differences specifications, standard

errors are clustered at the state. Across many of the specifications, I use a weighted least squares approach to adjust my estimates for the greater levels of noise in low population states.

$\mathbf{X_{sy}}$ is a vector of state and time varying controls that differs across specifications. The base controls are the state level unemployment rate and the percent of the state population that lives in a metropolitan statistical area (MSA). In additional specifications, I include sets of control variables that are common in the concealed carry literature and the intimate partner homicide literature. When considering the controls typical for the concealed carry literature, I use the set of controls from Donohue et al. (2019). I refer to this set of controls as the DAW controls. They include the one year lagged incarceration rate, the one year lagged police employment rate per 100,000 population, the poverty rate, the unemployment rate, per capita ethanol consumption from beer, the percent of the population living in an MSA, real per capita personal income and 6 demographic age-sex-race groups which consist of white males 15-19, black males 15-19, other males 15-19, white males 20-39, black males 20-39, and other males 20-39. When considering the controls common to the relevant intimate partner homicide literature, I include the following indicator variables for whether or not the state has a unilateral divorce law, a domestic violence misdemeanor firearm confiscation law, a law permitting or mandating police seizure of firearms at scenes of domestic violence, or a domestic violence restraining order firearm dispossession law that applies ex parte or has firearm relinquishment information. I also include information on the ratio of male to female employment, AFDC/TANF maximum benefit levels for a family of three, the poverty rate, the proportion of the population that is white, the proportion of the population that is black, and

the proportions of the population 15 years and older that are married, divorced, or widowed, individually.

*Method 2: Event Study*

The event study specifications look similar to the difference-in-differences specifications but demonstrate the impacts of treatment over time.
For state, s, in year, y:

$$IPH_{sy} = \alpha + \sum_{i=-10}^{-2} \beta_i PreRTC_{isy} + \sum_{i=0}^{10} \beta_i PostRTC_{isy} + \gamma \mathbf{X_{sy}} + \delta_y + \lambda_s + \mu_{ry} + \varepsilon_{sy}$$

$IPH_{sy}$ is still the rate of intimate partner homicide for a given state in a given year per 100,000 population. $\delta_y$ and $\lambda_s$ are again fixed effects for year and state, $\mu_{ry}$ is a region by year fixed effects, and $\mathbf{X_{sy}}$ is the same varying vector of state-level time-varying controls as outlined above. In the event study equation, though, instead of having a single indicator for whether or not a state has a right to carry law in a given year, the event study equation considers a set of indicator variables for how long it has been prior to or after the adoption of the right to carry law for ten year in each direction. All other years before and after adoption are included in the estimates of $\beta_{-10}$ and $\beta_{10}$, respectively. The reference year is the year immediately prior to adoption. $\varepsilon_{sy}$ is the error term, and standard errors are clustered at the state.

*Method 3: Synthetic Control Methods*

Finally, I also use synthetic control methods (SCM; Abadie et al., 2010, 2015) to answer the question of how right to carry laws impact intimate partner homicide

rates. The synthetic control method improves on the event study methodology by creating synthetic control groups through the weighting of untreated units to match pre-treatment outcomes for the treated units. SCM was originally designed for a setting with a single treated unit and multiple control units that are weighted to create the synthetic control. In this case, though, I am examining a number of treated units with varying treatment times. One method for using SCM in this setting is to simply find synthetic controls for each unit separately and then take the average of the treatment effect for each individually estimated SCM regression. However, this relies on finding good synthetic controls for each of the individual units. Because several states do not have good matches in the pre-period when estimated using the *gsynth* package, I use the partially pooled synthetic controls method developed by Ben-Michael et al. (2021) for the main analysis. This method balances the dual objectives of finding a good unit-level fit with finding a good pooled fit. This methodology is ideal for settings like this where there are some units that do not have good synthetic control fits at the individual level and where we are interested in average treatment effects. In the *augsynth* package developed by Eli Ben-Michael, which I use for this analysis, the hyperparameter $\nu$ determines the relative importance that is placed on each goal and is set to equal the ratio of the pooled fit to the average of the unit-level fit.

In this setting where treatment times are staggered, researchers have shown that the generalized difference-in-differences approach with two-way fixed effects can be misleading when treatment effects differ by treatment timing (Sun & Abraham, 2020; Borusyak & Jaravel, 2017; Goodman-Bacon, 2018). The results of the unit-level SCM found in the appendix suggest that treatment effects are likely varying by treatment timing and maybe also by region. Therefore, the partially

pooled SCM provides a way to better estimate the treatment effects than the generalized differences-in-differences method or the event study method described above.

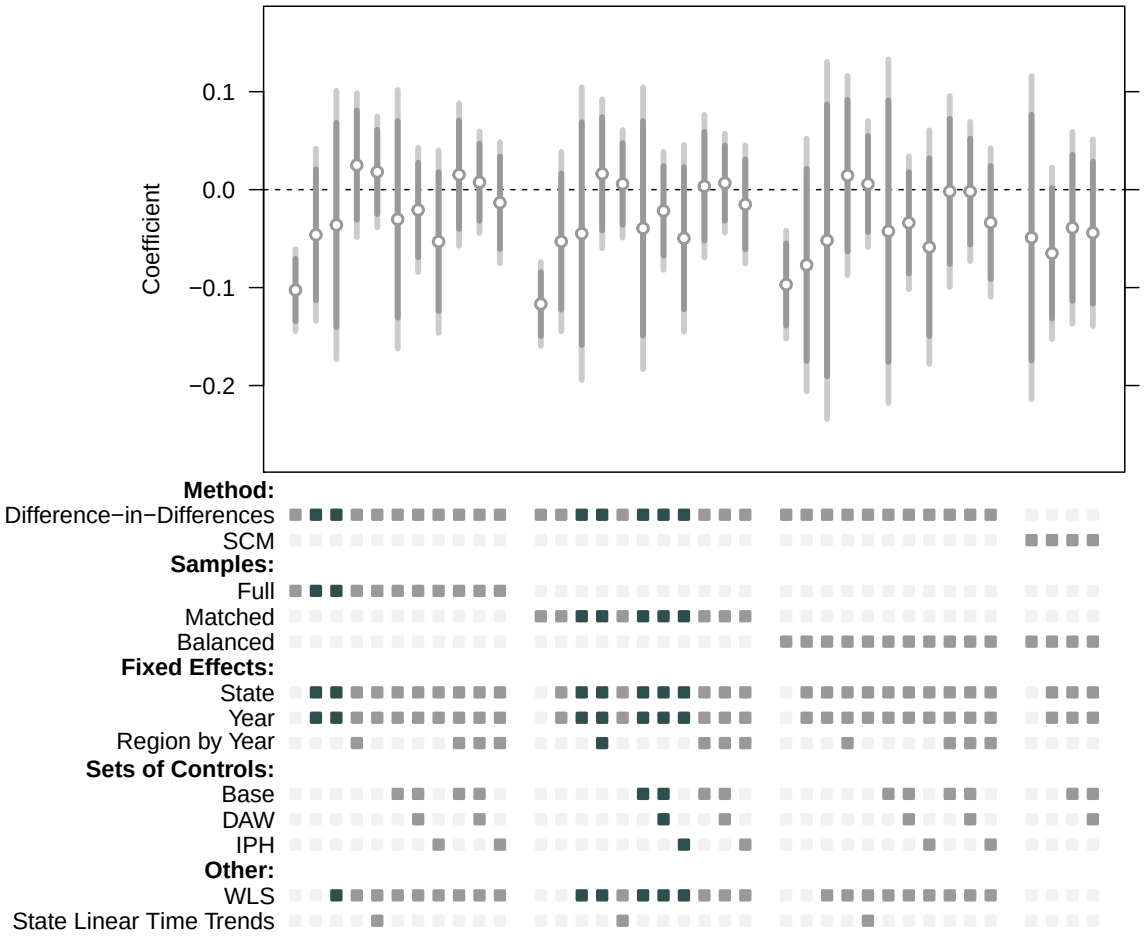
I generate the baseline SCM estimates by matching on the intimate partner homicide rate for the ten years prior to adoption of the right to carry law. In additional specifications, I also match on the 10 year lags of unemployment and the percent of the population living in an MSA as well as the full set of DAW controls described in the difference-in-differences methodology section.[5] One synthetic control is estimated for each time cohort–with a time cohort being the group of states that adopt RTC laws in the same year. Currently, the *augsynth* package only accepts balanced panels so all states with missing units are dropped. The *gsynth* package is able to analyze unbalanced panels, so states that are dropped in the main analysis are still included in the state-by-state estimates found in the appendix. The SCM estimates all have jackknife standard errors.

## Results

The main findings of the impacts of right to carry laws on intimate partner homicides can be found in Figures 20 and 22. Figure 20 shows the impacts of RTC laws on the rate of female victim intimate partner homicides where the weapon used is a firearm. Across all of the specifications shown in the specification chart, only the naive OLS estimates are significant at the 5% level. One SCM estimate is significant at the 10% level, but no other estimates are significant at traditional levels.

[5]I do not include in the results section a specification matching on the 10 year lags of the IPH controls because so many of the IPH controls are binary variables and the non-binary variables are not largely different from the DAW controls.

FIGURE 20. Specification Chart for the Impact of Right to Carry Laws on the Rate of Intimate Partner Homicides with a Firearm: **Female Victims**

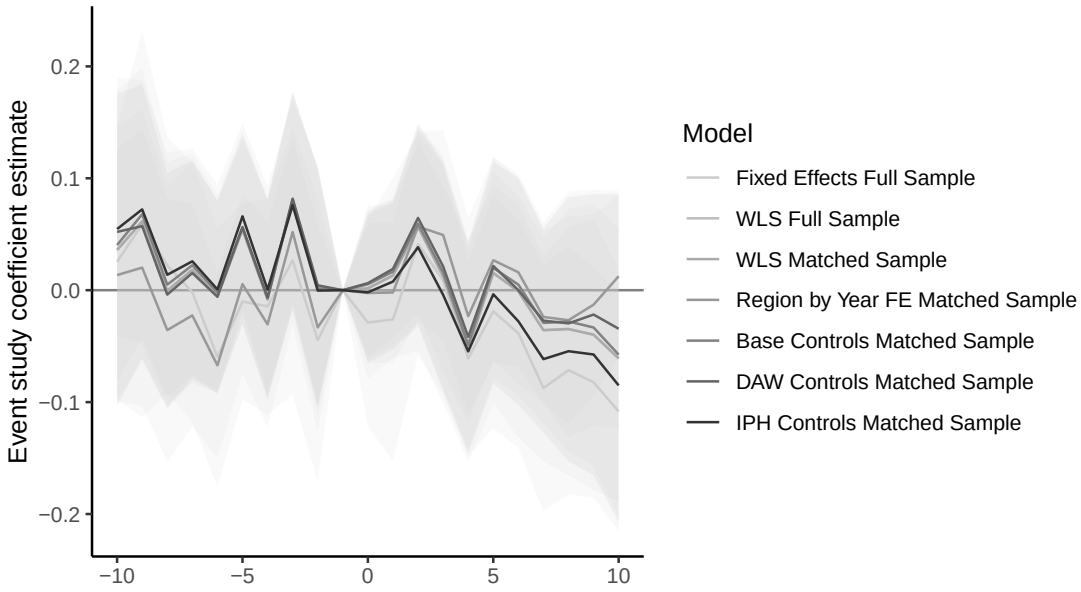



*Notes:* This specification chart was created using the 'spec_chart' function in R developed by Ariel Oritz Bobea. Each coefficient on the chart is an estimate of the impact of right to carry laws on the rate of female victim intimate partner homicides committed with a firearm from a different specification that varies by methodology, sample, controls, and fixed effects as described in the bottom half of the figure. The bars represent 95% and 99% confidence intervals. Standard errors for the difference-in-differences estimates are clustered at the state. SCM specifications have jackknife standard errors. The darkly shaded specification details correspond to the specifications analyzed further in the event study graphs and Table 1. The baseline (1980) population weighted mean for the rate female victim firearm intimate partner homicide is 0.828

Figure 21 shows the results of the event study specifications. Each line on the graph is a different event study specification and corresponds to one of the darkly

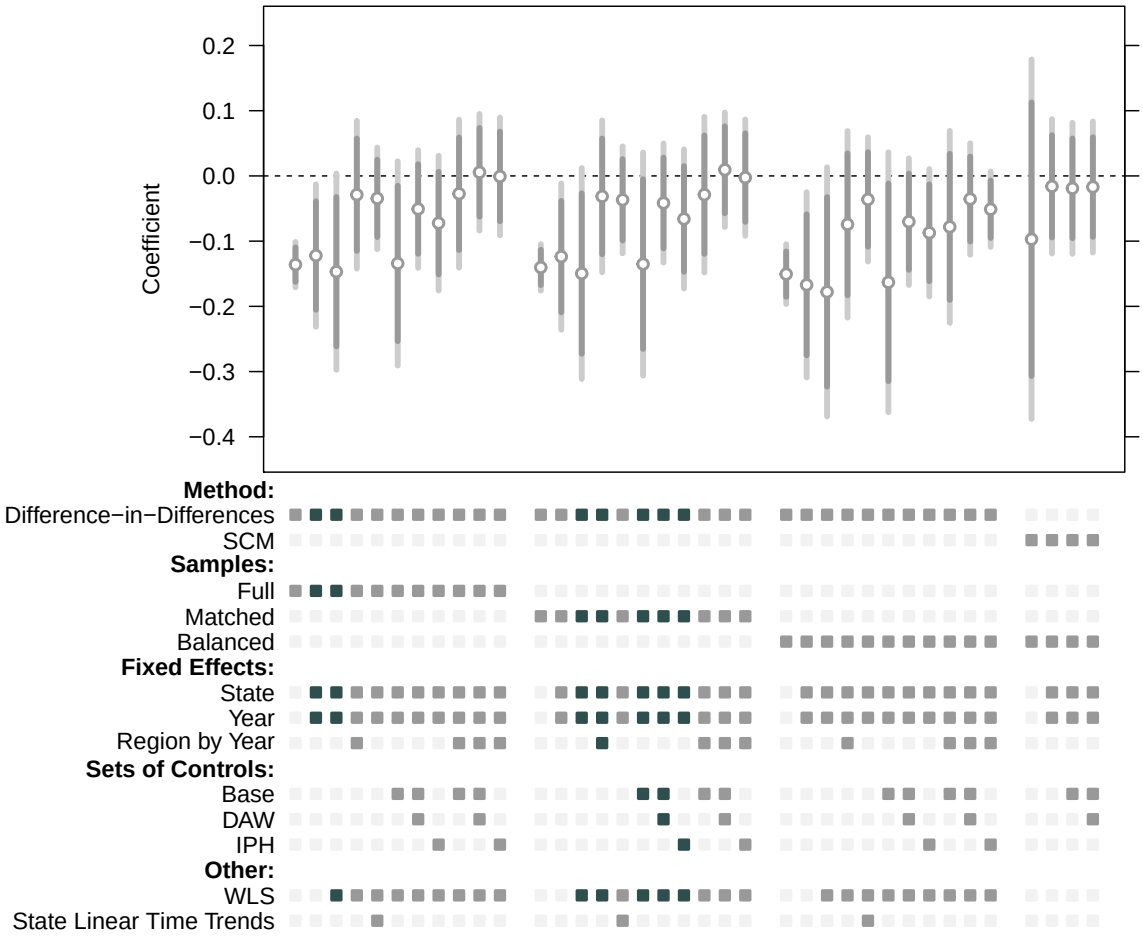
shaded columns in the specification chart. Only seven specifications are included here so that the results are still visible, but the results from other specifications look similar. The event study estimates verify the results from the other methods as they provide no evidence of an impact. None of the estimates are significant at traditional levels. Overall, these results suggest that there is no effect of right to carry laws on rates of female victim intimate partner firearm homicides.

FIGURE 21. Event Study Estimates for Female Victims, Weapon Type: Firearm




*Notes:* Standard errors are represented by the ribbons around the estimates and are all clustered at the state. Starting from the top of the event study graph legend the specifications included in the event study graphs match up to the darkly shaded models in the bottom half of the specification charts moving from left to right.

FIGURE 22. Specification Chart for the Impact of Right to Carry Laws on the Rate of Intimate Partner Homicides with a Firearm: **Male Victims**

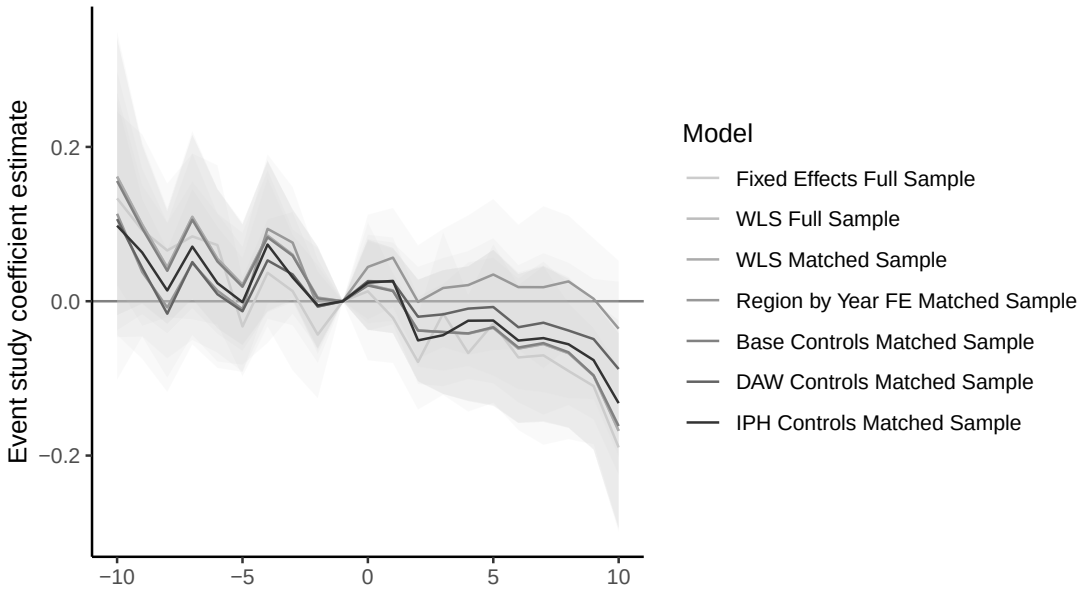



*Notes:* This specification chart was created using the 'spec_chart' function in R developed by Ariel Oritz Bobea. Each coefficient on the chart is an estimate of the impact of right to carry laws on the rate of male victim intimate partner homicides committed with a firearm from a different specification that varies by methodology, sample, controls, and fixed effects as described in the bottom half of the figure. The bars represent 95% and 99% confidence intervals. Standard errors for the difference-in-differences estimates are clustered at the state. SCM specifications have jackknife standard errors. The darkly shaded specification details correspond to the specifications analyzed further in the event study graphs and Table 11. The baseline (1980) population weighted mean for the rate male victim firearm intimate partner homicide is 0.689

Figure 22 shows the impacts of right to carry laws on the rate of male victim intimate partner homicides committed with a firearm. In Figure 22, we

see that there are a number of specifications with a negative coefficient that are significant at traditional levels, but the estimates are not stable. Stricter difference-in-differences specifications and synthetic controls method estimates suggest no significant impacts of right to carry on male victim intimate partner firearm homicide rates.

FIGURE 23. Event Study Estimates for Male Victims, Weapon Type: Firearm

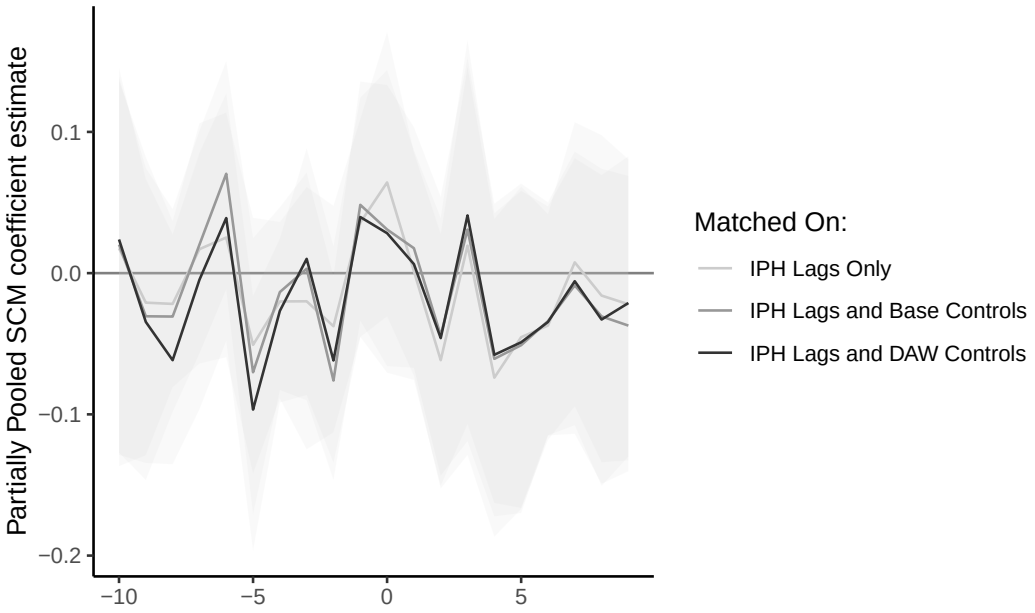



*Notes:* Starting from the top of the event study graph legend, the specifications match up to the darkly shaded models in the bottom half of the specification charts moving from left to right. The ribbons represent 95% confidence intervals based on state-level clustered standard errors.

Figure 23 shows the event study results for a subset of the difference-in-differences specifications in the specification chart with each line representing a different specification. I include a number of the specifications where the difference-in-differences coefficients are negative and significant. Here, the event study shows an overall negative trend in the coefficients over time, including during the period before treatment occurs. This trend is particularly pronounced among the less

restrictive difference-in-differences specifications. The event study pre-trends suggest a possible violation of the parallel trends assumption that may be one reason for the significant and negative coefficient estimates in the less restrictive specifications. Figure 24 shows the results of the partially pooled SCM estimates over time for all but the naive OLS specification. Through the use of synthetic controls, the SCM model is able to better match trends in the pre-period and correct for the overall downward trend. All together, the evidence of an effect of concealed carry on male victim intimate partner homicides is fairly weak.

FIGURE 24. Partially Pooled SCM for Male Victims, Weapon Type: Firearm




*Notes:* All models are matched on 10 lags and estimated for 10 time periods after treatment. All models are estimated with two-way factors, and one synthetic control is estimated for each group of states that adopted a right to carry law in the same year. The ribbons represent 95% confidence intervals based on jackknife standard errors.

As an additional check on the main analysis, I examine the impact of right to carry laws on rates of intimate partner homicides where the perpetrator used

TABLE 11. RTC Laws and Non-Firearm Intimate Partner Homicide Rates

| | *Dependent variable:* Rate of Intimate Partner Homicides per 100,000 | | | | | | |
|---|---|---|---|---|---|---|---|
| | Full SHR Sample | | SHR Sample with 50% Match to Vital Statistics | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: Female Victims** (1980 Population Weighted Mean: 0.380) | | | | | | | |
| Right to Carry | 0.035 | 0.039 | 0.038 | 0.051** | 0.032 | 0.029 | 0.002 |
| | (0.023) | (0.026) | (0.028) | (0.024) | (0.026) | (0.021) | (0.019) |
| **Panel B: Male Victims** (1980 Population Weighted Mean: 0.306) | | | | | | | |
| Right to Carry | −0.015 | −0.013 | −0.017 | 0.025 | −0.019 | −0.013 | −0.018 |
| | (0.019) | (0.018) | (0.019) | (0.016) | (0.019) | (0.011) | (0.018) |
| Two-Way FE | Y | Y | Y | Y | Y | Y | Y |
| WLS | N | Y | Y | Y | Y | Y | Y |
| Region x Year FE | N | N | N | Y | N | N | N |
| Unemployment | N | N | N | N | Y | Y | N |
| Percent MSA | N | N | N | N | Y | Y | N |
| DAW Controls | N | N | N | N | N | Y | N |
| IPH Controls | N | N | N | N | N | N | Y |
| Observations | 1,945 | 1,945 | 1,885 | 1,885 | 1,885 | 1,734 | 1,827 |

*Notes:* *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered at the state. The weighted least squares specifications are weighted by population. From left to right, these specifications correspond to the darkly shaded models in the bottom half of the specification charts from left to right.

a weapon other than a firearm. These results can be found in Table 11. This analysis serves as an additional robustness check of my primary samples as we would not expect right to carry laws to have an impact on non-firearm intimate partner homicides. Table 11 includes only the subset of specifications shown in the event study graphs that correspond to the darkly shaded specifications in the specification charts. I show only this subset for brevity, but these are reflective of the broader specification results. Here, we see no consistent evidence of an impact of right to carry laws on non-firearm intimate partner homicides for male

or female victims. The single specification that is statistically significant is small in magnitude relative to the baseline mean. Further, it is reasonable for one of these to show statistical significance even if there is no impact given the number of specifications and samples being tested.

## Conclusion

Overall, I find little evidence of any impact of right to carry laws on intimate partner homicide rates. Across three methodologies, multiple samples, and a variety of included control variables, I find few specifications with statistically significant results, almost all of which are found in specifications focusing on the male victim sample. My findings indicate that the generalized difference-in-differences approach may be inappropriate in this setting where the results are sensitive to model specification and event study estimates call into question the validity of the parallel trends assumption. This specification sensitivity suggests that previous estimates of the relationship between right to carry laws and intimate partner homicide based solely on difference-in-differences models should be considered carefully. These results have important implications for advocates and policymakers working in the areas of intimate partner violence and gun control as well as for researchers studying concealed carry regulations.

Firstly, the limited evidence of an impact is completely concentrated among male victims, while female victims do not see any protective effect of right to carry laws. This provides some new insights into the possible mechanisms driving the impacts of right to carry laws. Given that the majority of male victim intimate partner homicides are predated by male offender intimate partner violence, it is most likely that the right to carry laws serve as a deterrent. Specifically, women

may be deterred from resorting to lethal violence as a means of escaping a violent relationship if they believe it is more likely their partner will have a firearm easily accessible when a right to carry law is in place.

Additionally, the sensitivity of my results further the call for more formal means of testing specification sensitivity particularly for researchers studying the impacts of concealed carry. Results from the male victim sample shown in Figure 22 demonstrate this point well by showing just how sensitive the results are to the particulars of the model specification. This sensitivity across defensible but somewhat arbitrary researcher choices illustrates why more formal and extensive sensitivity analysis is necessary and should be commonplace in the concealed carry literature and in social science research more broadly. Further, these findings provide one possibility for why there is so little consensus within the literature on the effects of right to carry laws.

For policymakers and advocates seeking to reduce rates of intimate partner homicide, these results suggest that the regulation of concealed carry laws may not be a policy area with substantial impacts on this dimension. Therefore, advocates and policymakers may want to spend their time focused on other areas of gun control with larger documented protective effects such as restricting possession of firearms for individuals with past violent misdemeanor offenses.

Although this paper does not show any strong evidence of an impact of right to carry laws on intimate partner homicides, this does not mean that there are no impacts for intimate partner violence more broadly. Intimate partner homicides are just one extreme form of violence perpetrated by intimate partners. Further research into how right to carry laws may impact other dimensions of

intimate partner violence is needed as right to carry laws could affect other forms of aggression like controlling or threatening behavior.

While other research has shown right to carry laws have important impacts for other outcomes, I find little evidence of an impact on intimate partner homicide. These findings provide insight into possible mechanisms driving right to carry law impacts and highlight the need for greater sensitivity analysis in the concealed carry literature. Further, they suggest that policymakers and advocates seeking to reduce rates of intimate partner homicides may have greater impacts by focusing their efforts on other gun control legislation.

# CHAPTER V

# CONCLUSION

Across the three main chapters of this dissertation, I provide insight into how a number of policies have impacted public health as well as, for one policy, migration.

In Chapter II, "The Effect of Women's Access to Free Health Care on Breastfeeding Practices: Evidence from Armenia," I estimate the impacts of increased access to free health care for women on the prevalence and duration of breastfeeding. I find that providing free health care to women in this setting decreased the likelihood of ever breastfeeding among those exposed to the program. I find no impact of the program on the duration of breastfeeding. Analyzing data on hospital practices related to infant feeding suggests that a lack of support for breastfeeding in the treated region's medical community may be driving this decrease. Well-documented infant formula promotion in health care facilities throughout the treated region further supports this explanation. Overall, these results suggest that policymakers may want to take additional steps to ensure up-to-date practices at the clinics providing increased access to health care when implementing programs like this. In particular, facilitators of programs targeting female health care may want to pay special attention to the gender-specific practices at the clinics to curb these types of unintended consequences.

Chapter III analyzes the effects of the Relocation Program on Native American migration and fertility outcomes. Comparing the program's target cities to cities that were not targeted, I employ a difference-in-differences model to estimate the effect of the Relocation Program on Native American migration.

I find that the Relocation Program led to a significant 12,000 person increase in the Native American population in cities that were targeted by the Relocation Program relative to the non-targeted cities. This constitutes a considerable shift in the spatial distribution of the Native American population who prior to 1950 primarily resided on rural tribal lands. I also find that there is a substantial gap in the rates of second-generation fertility for Native American women living in urban areas compared to those living on tribal lands. This suggests that the program likely had lasting impacts across generations for those exposed to the program.

In Chapter IV, I examine the impact of right to carry laws on rates of intimate partner homicide. I analyze to what extend right to carry laws effect intimate partner homicide rates separately for male and female victims using three methodologies, namely: difference-in-differences, event study, and synthetic control methods. I find no indication of an effect of right to carry on female victim intimate partner homicide rates and only weak evidence of an effect for male victims. These limited effects are highly sensitive to model specification and vary considerably across the three methodologies. This specification sensitivity suggests that policymakers should approach estimated effects of right to carry laws with caution. Further, this highlights the need for more formal sensitivity analysis within this literature where sensitivity has been demonstrated but also more broadly as these results show that defensible but arbitrary researcher choices can produce misleading conclusions.

# APPENDIX

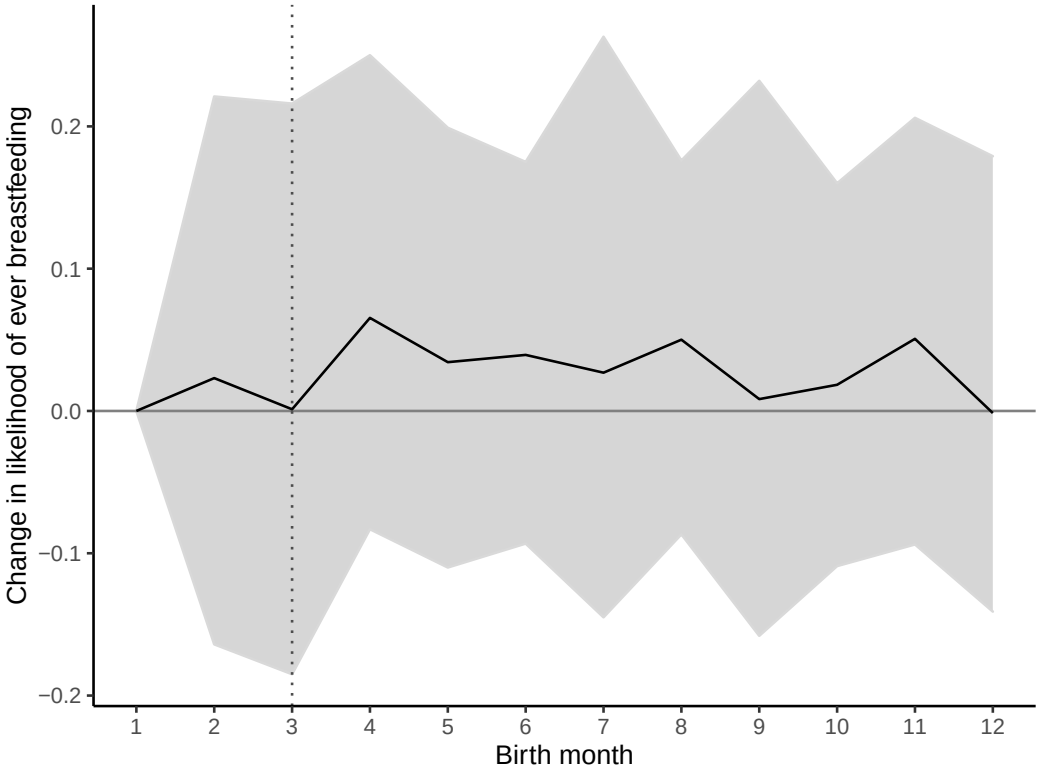
## ADDITIONAL TABLES AND FIGURES

### CHAPTER II: Additional Tables and Figures

TABLE A1. Effect of Access to Free Maternal Healthcare on Breastfeeding for the Full Sample

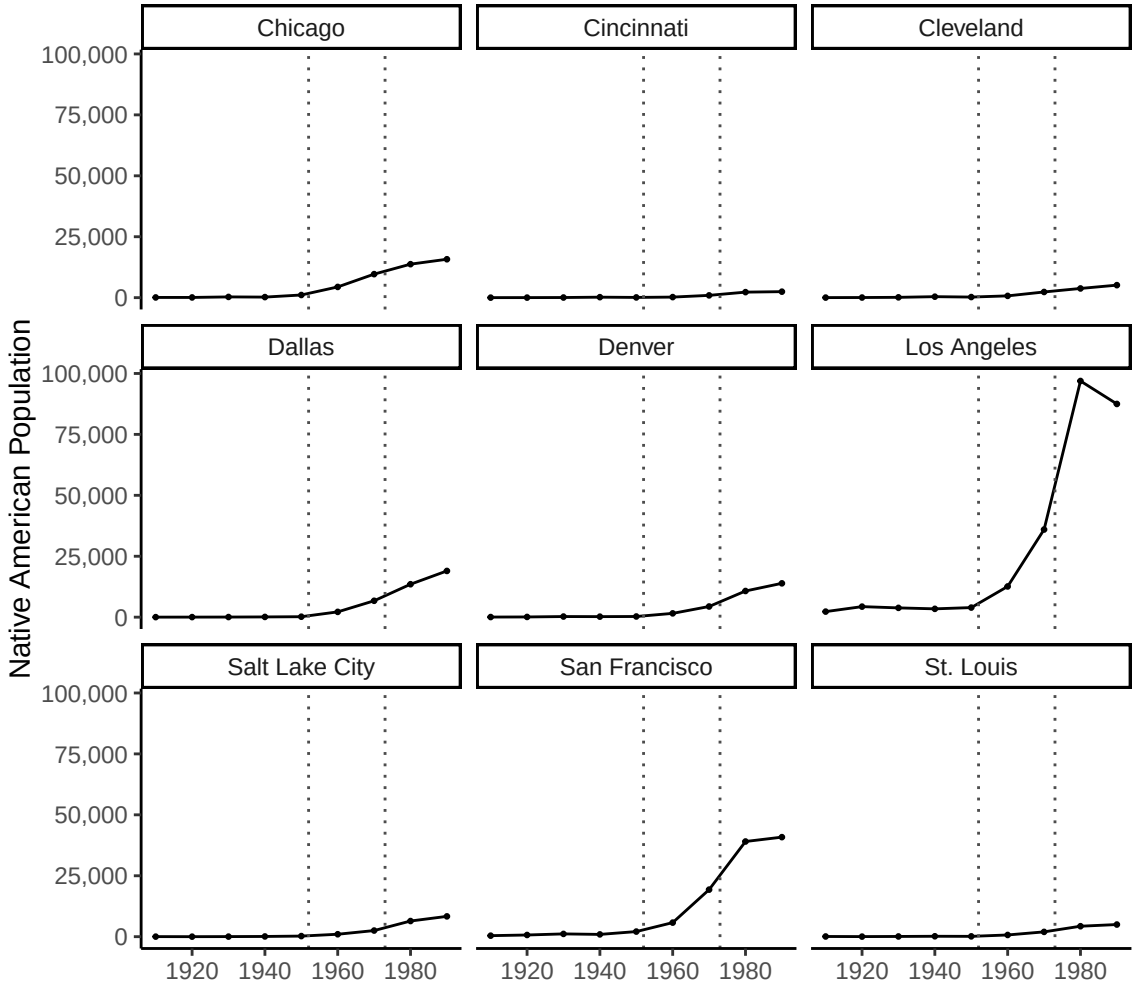
| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Months Breastfed | | Extensive Margin | Intensive Margin |
| | (1) | (2) | (3) | (4) |
| Post June08 | −0.424 | | | −0.515 |
| | (0.369) | | | (0.363) |
| Post March09 | | −0.552 | 0.006 | |
| | | (0.563) | (0.016) | |
| Number of | 0.008 | 0.008 | 0.002 | −0.010 |
| HH Members | (0.031) | (0.031) | (0.002) | (0.027) |
| Number of | −0.058 | −0.058 | 0.004* | −0.108* |
| Rooms in HH | (0.065) | (0.066) | (0.003) | (0.065) |
| Post June08 | −0.670 | | | −0.312 |
| × Yerevan | (0.864) | | | (0.857) |
| Post March09 | | −0.573 | −0.045** | |
| × Yerevan | | (0.773) | (0.023) | |
| Full Set FE | Yes | Yes | Yes | Yes |
| Household Controls | Yes | Yes | Yes | Yes |
| Mother Controls | No | No | No | No |
| Observations | 23,090 | 23,090 | 23,090 | 21,408 |
| $R^2$ | 0.083 | 0.083 | 0.021 | 0.102 |
| Adjusted $R^2$ | 0.081 | 0.081 | 0.019 | 0.100 |
| *Note:* | | | | *p<0.1; **p<0.05; ***p<0.01 |

FIGURE A1. Pre-Treatment Coefficient Estimates by Month of Birth



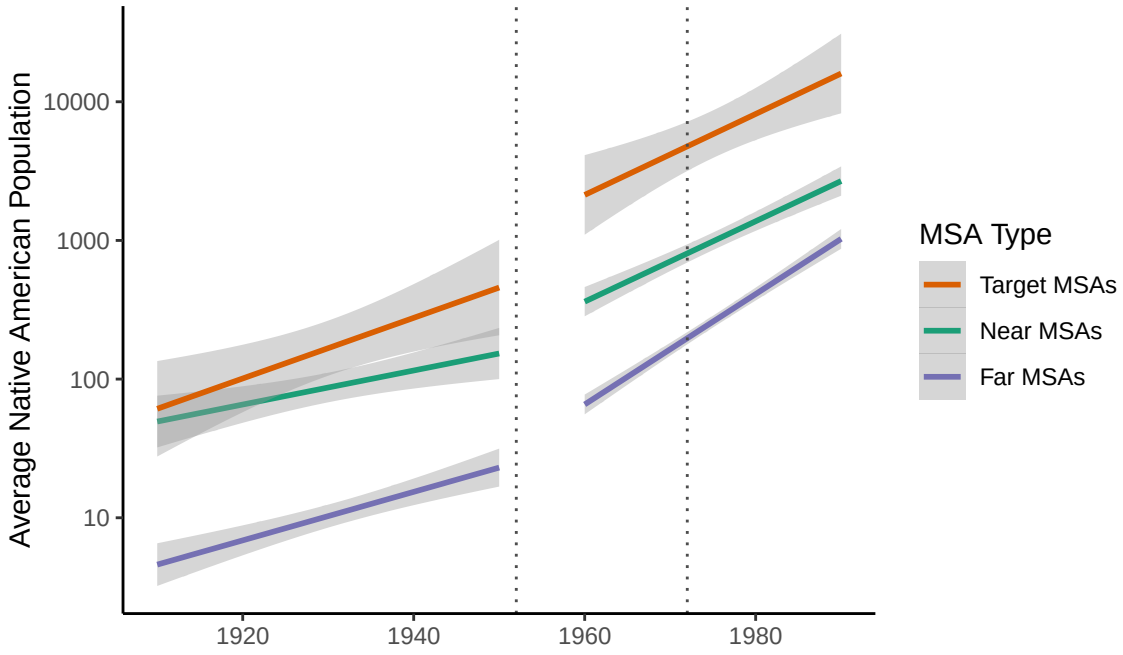

Note: Estimates for the coefficients on the indicator for Yerevan as the region of residence interacted with the child's month of birth in the per-treatment period, namely, prior to 2009. Regression includes year, region, and wave fixed effects. The ribbon reflects the 95% confidence intervals, and the vertical dashed line represents the beginning month of the intervention each year. Estimates are normalized to January.

FIGURE A2. Target City Native American Population Trends




*Notes:* Total Native American population for each of the target MSAs individually from 1910-1990. Dotted lines represent the start and end of the Relocation Program in 1952 and 1973, respectively.

FIGURE A3. Native American Population Trends by MSA Type, Log Scale



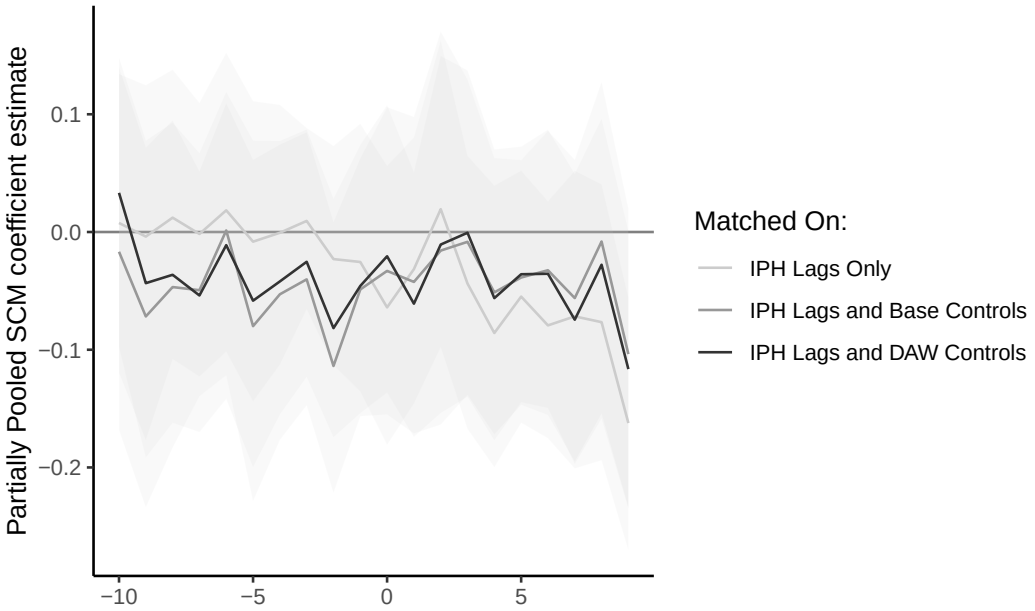

*Notes:* Population Growth for the target and non-target MSAs from 1910-1990. Non-target MSAs are broken into two groups based on proximity to tribal land. The points represent log average MSA Native American population. Best fit lines are estimated by MSA type for the pre and post periods. The dotted lines correspond to the start and end of the Relocation Program in 1952 and 1973, respectively.

TABLE A2. Impact of Right to Carry Laws on Rates of Handgun Sales

| | *Dependent variable:* |
|---|---|
| | Rate of Handgun Sales per 100,000 |
| Right to Carry | 479.041** |
| | (179.115) |
| Observations | 756 |
| $R^2$ | 0.870 |
| Adjusted $R^2$ | 0.859 |

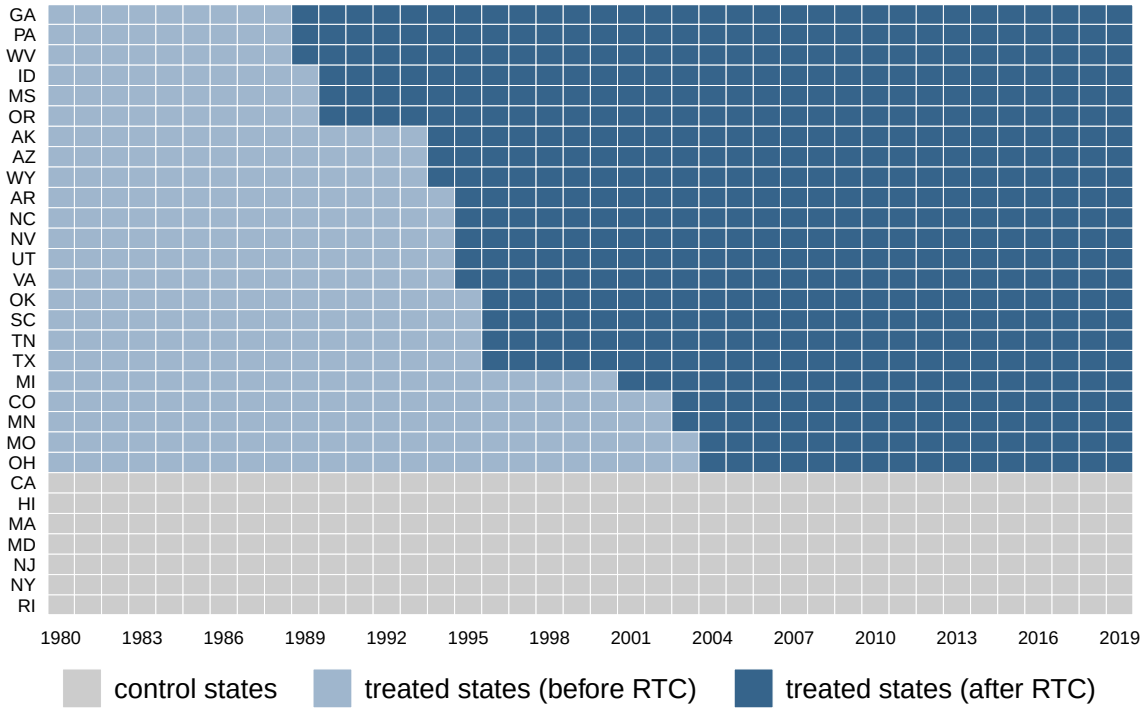
*Note:* *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered at the state. Data from the FBI's NICS background checks is used as a proxy for gun sales. These data are made available on GitHub by BuzzFeedNews.

FIGURE A4. Partially Pooled SCM for Female Victims, Weapon Type: Firearm



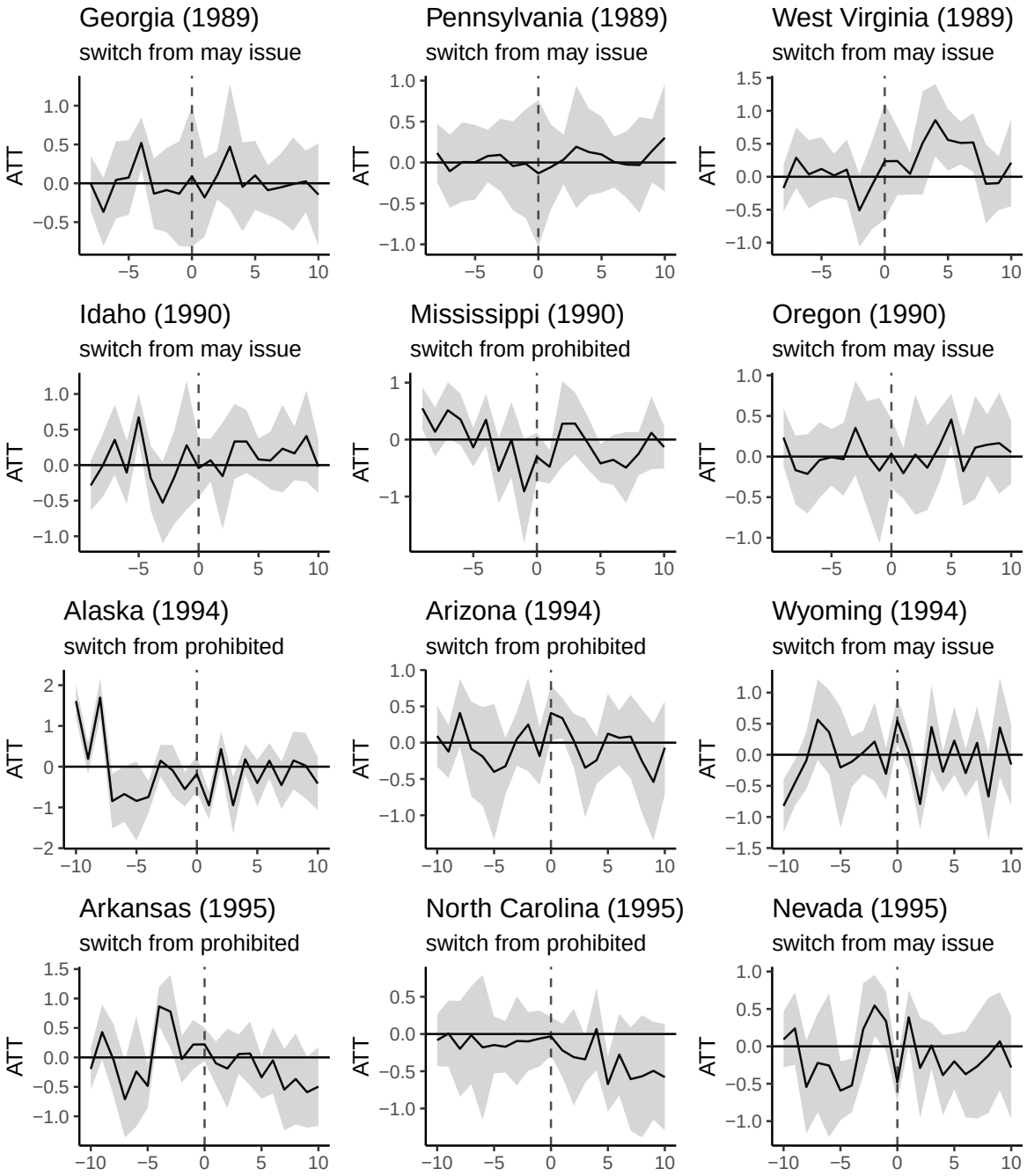

*Notes:* All models are matched on 10 lags and estimated for 10 time periods after treatment. All models are estimated with two-way factors, and one synthetic control is estimated for each group of states that adopted a right to carry law in the same year. The ribbons represent 95% confidence intervals based on jackknife standard errors.

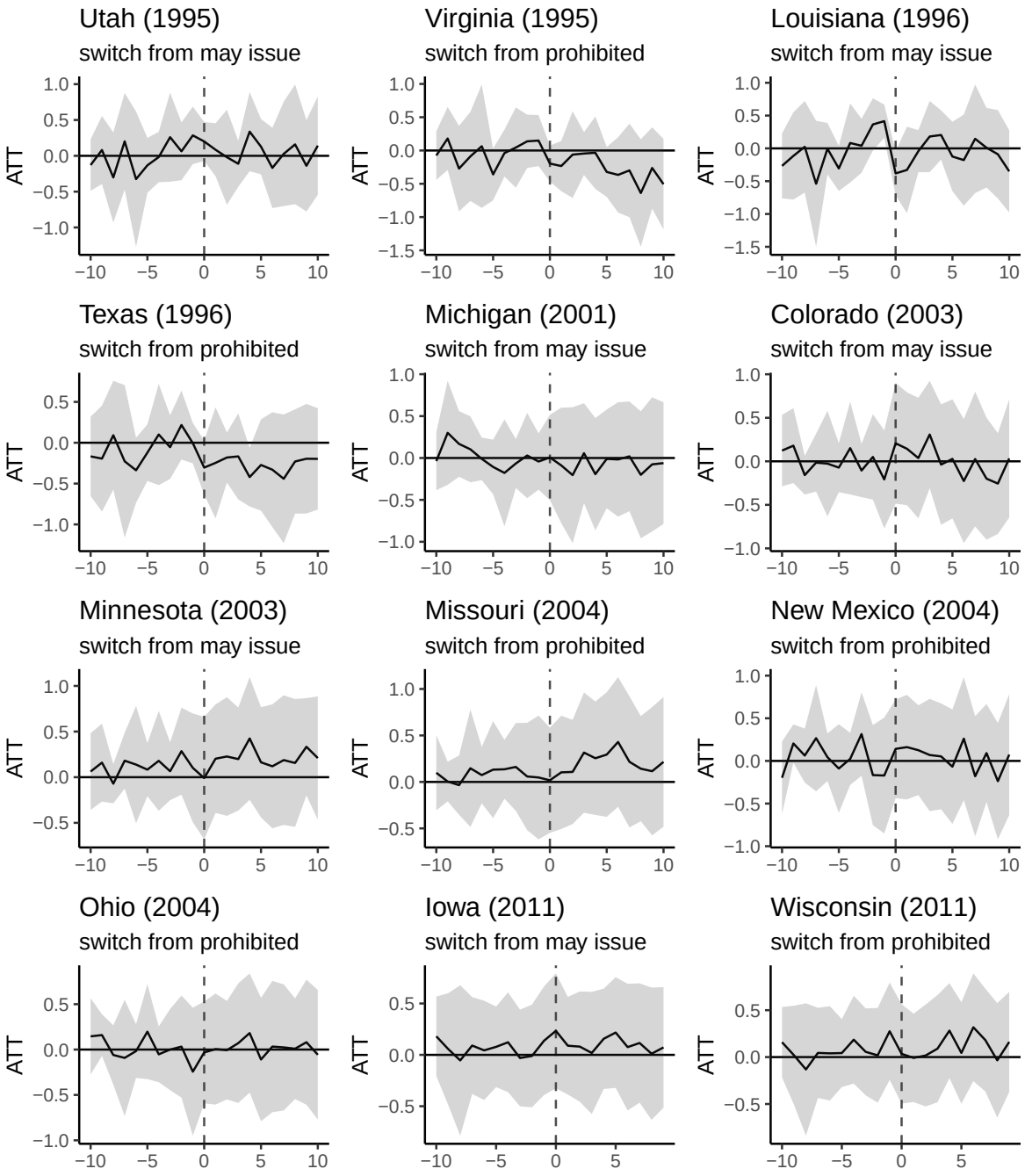
FIGURE A5. Adoption of Right to Carry Laws over Time




*Notes:* Includes only states from the balanced panel used in the synthetic control methods.

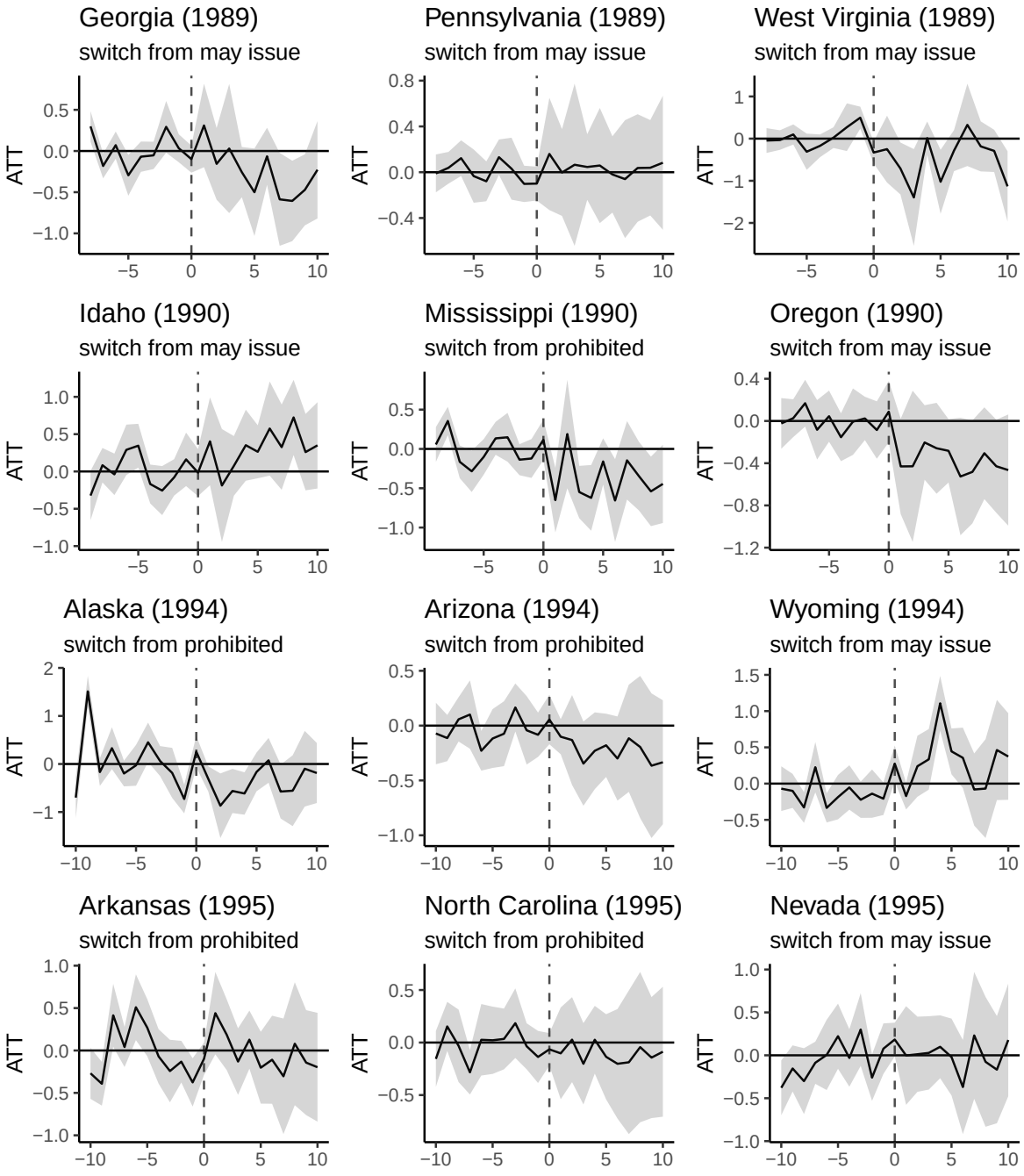
FIGURE A6. Individual State SCM Estimates
Female Victims, Weapon Type: Firearm

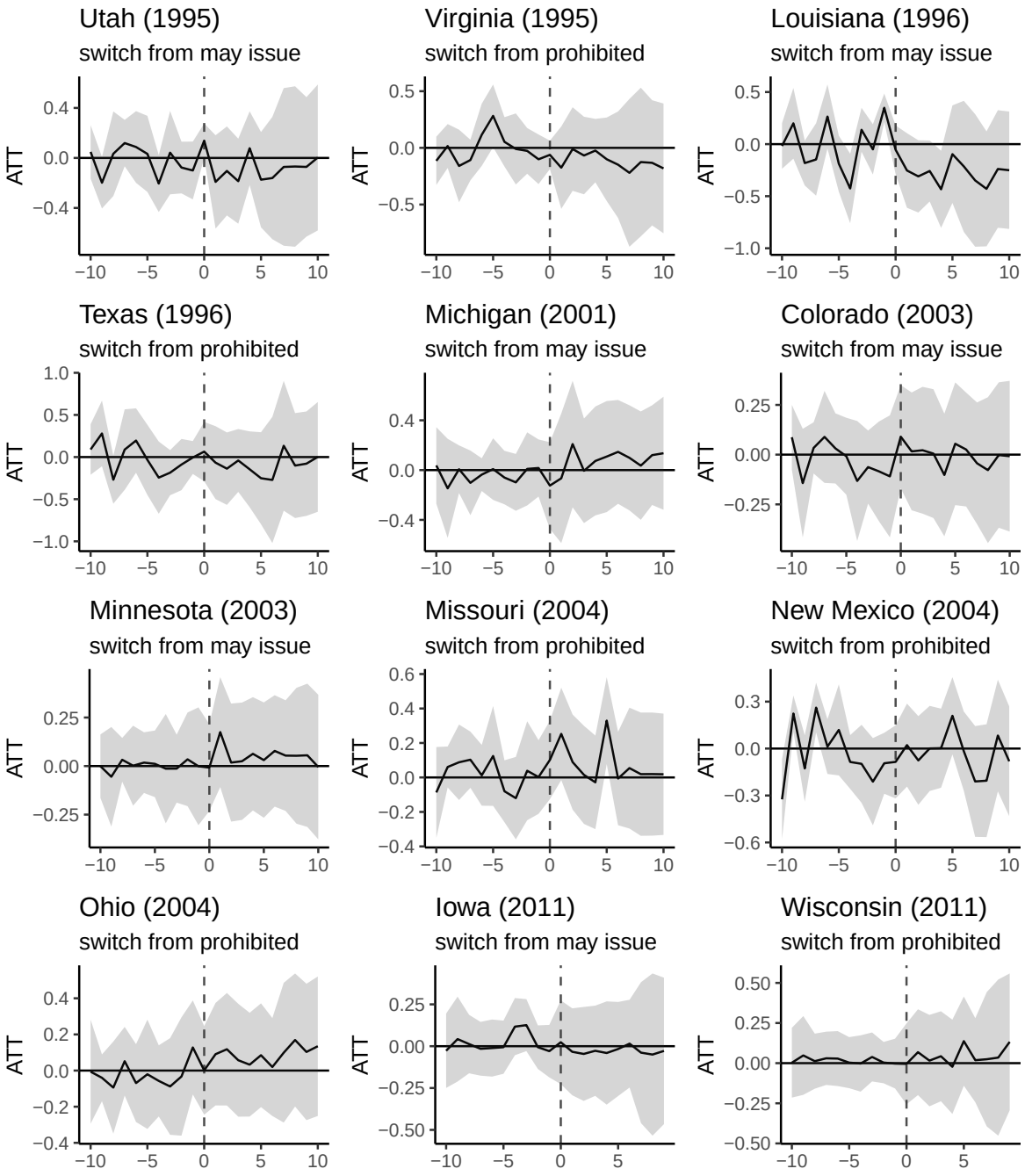





*Notes:* Each graph represents the estimate of the Average Treatment on the Treated from a SCM specification with inclusion of factors determined by cross-validation. Matches are based on 10 years of lagged intimate partner homicide rates as well as lagged unemployment and the percent of the state's population living in an MSA.

FIGURE A7. Individual State SCM Estimates
Male Victims, Weapon Type: Firearm






*Notes:* Each graph represents the estimate of the Average Treatment on the Treated from a SCM specification with inclusion of factors determined by cross-validation. Matches are based on 10 years of lagged intimate partner homicide rates as well as lagged unemployment and the percent of the state's population living in an MSA.

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