NBER WORKING PAPER SERIES

ABORTION, ECONOMIC HARDSHIP, AND CRIME

Erkmen G. Aslim
Wei Fu
Caitlin K. Myers
Erdal Tekin
Bingjin Xue

Working Paper 34245
http://www.nber.org/papers/w34245

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2025

In the past three years, Caitlin Myers has received a grant from the Society of Family Planning to support data collection on abortion access. She has also served as a paid consultant for the Urban Institute and the Planned Parenthood Federation of America on issues related to the effects of abortion policy and access. None of these parties reviewed this paper prior to its circulation. Erkmen Aslim, Wei Fu, Erdal Tekin, and Bingjin Xue have not received any financial or in-kind support from interested parties. The authors alone retained the right to review this paper prior to its circulation. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Abortion, Economic Hardship, and Crime
Erkmen G. Aslim, Wei Fu, Caitlin K. Myers, Erdal Tekin, and Bingjin Xue
NBER Working Paper No. 34245
September 2025
JEL No. I12, I18, J13, K42

ABSTRACT

We study how abortion access affects economic hardship and crime. Using a database of abortion provider locations and operations in Texas from 2009–2019, we exploit variation in travel distance to the nearest facility created by clinic closures following the enforcement of Texas HB-2 in 2013. We confirm previous evidence that increased distance to the nearest abortion facility reduces abortions and increases births. We provide novel evidence that reduced access to abortion also leads to significant economic hardship, reflected in lower labor force participation, rising debt, widening income inequality, and heightened housing insecurity. This financial strain translates into higher rates of financially motivated crime, such as theft and burglary, with no significant effect on violent crime. These effects extend beyond directly affected individuals, reflecting intrahousehold spillovers. These findings suggest far-reaching consequences of restricted access to reproductive healthcare.

Erkmen G. Aslim
University of Vermont
easlim@uvm.edu

Wei Fu
University of Louisville
wei.fu@louisville.edu

Caitlin K. Myers
Middlebury College
Department of Economics
and NBER
cmyers@middlebury.edu

Erdal Tekin
American University
School of Public Affairs
and IZA
and also NBER
tekin@american.edu

Bingjin Xue
University of New Hampshire
bingjin.xue@unh.edu

1 Introduction

Public debate on abortion policy often centers on moral, legal, and health considerations. Yet restrictions on reproductive autonomy can generate broader economic and social costs. By removing the ability to time or defer childbearing, abortion restrictions can result in unplanned or mistimed parenthood—circumstances that impose substantial financial strain, reduce labor force participation, and increase reliance on public assistance. These pressures can be especially destabilizing for households with limited resources (Miller, Wherry, and Foster, 2023; Wilkinson and Bernard, 2024). This paper asks whether limiting abortion access contributes to such hardship in ways that spill over into higher rates of criminal activity, providing evidence on an understudied outcome of reproductive policy.

We hypothesize that these disruptions in reproductive timing translate into measurable economic and social consequences. In particular, abortion restrictions are likely to heighten financial vulnerability among already disadvantaged populations, with effects observable in reduced employment, rising debt, and housing insecurity. Such economic strain, in turn, can increase the likelihood of engaging in financially motivated criminal behavior (Becker, 1968; Freeman, 1996; Machin and Meghir, 2004).[1] In this way, abortion restrictions can trigger a series of consequences, beginning with diminished reproductive autonomy and extending to broader economic and social outcomes such as elevated crime.

To test these hypotheses, we construct a panel of Texas counties from 2009–2019 that links administrative and demographic outcomes to variation in abortion access driven by clinic closures following a 2013 policy change. Our key measure is driving distance to the nearest abortion clinic, based on geocoded facility data from Myers (2025b). We estimate the effects of these shocks using a difference-in-differences strategy that compares counties experiencing large increases in travel distance to those with stable access. We examine outcomes in three domains: reproductive behavior (abortion and birth rates), household

[1] For instance, Collinson et al. (2024) show that eviction orders have adverse labor market consequences, including reduced earnings, particularly for female and minority tenants. The literature further demonstrates that labor market conditions play a significant role in shaping criminal behavior (Yang, 2017; Schnepel, 2018).

economic well-being (labor force participation, income, debt, and housing instability), and public safety (property and non-property crime).

Our findings confirm prior evidence (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo et al., 2020a; Myers, 2024) that increased travel distance to abortion facilities significantly reduces abortion rates and increases birth rates, with the largest effects observed among younger women. We provide novel evidence that increases in travel distance also result in significant declines in labor force participation, elevated debt-to-income ratios, rising income inequality, higher mortgage delinquency, and more evictions, all of which reflect increased economic hardship. In turn, we find that barriers to abortion access are linked to increased rates of property crime, particularly burglary, motor vehicle theft, and robbery. These effects are concentrated in counties experiencing the largest increases in travel distance and are not mirrored in non-financial or violent crimes.

This study contributes to several areas of research. First, it adds to a large literature on the consequences of abortion access, which documents short- and long-run effects on maternal health, fertility, educational attainment, employment, and poverty.[2] The liberalization of abortion policies in the 1970s reduced unintended births and maternal mortality, particularly among non-White populations (Levine et al., 1999; Myers, 2017; Farin, Hoehn-Velasco, and Pesko, 2024), and improved women's long-run economic trajectories by increasing labor force participation, reducing poverty, and facilitating higher educational attainment (Angrist and Evans, 1999; Abboud, 2019; Lindo et al., 2020b). More recently, Jones and Pineda-Torres (2024) show that post-Roe restrictions on providers reduce educational attainment for Black women, and Miller, Wherry, and Foster (2023) find that being denied an abortion leads to persistent financial instability. Our study complements this work by showing that these individual-level burdens can spill over into the community in the form of elevated property crime.

Second, it contributes to empirical research linking abortion access to family stability,

[2]For a review, see Myers (2025a).

child welfare, and risk of violence. Aslim, Fu, and Tekin (2024) find that greater distance to abortion services increases reports of child maltreatment and victimization suggesting that abortion restrictions affect not only women's health and economic prospects but also heighten risks for children born under constrained circumstances. Similarly, Dave et al. (2025) document that abortion restrictions in the post-Dobbs era led to significant increases in women's exposure to intimate partner violence. We build on this work by identifying another downstream consequence: a rise in financially motivated crime that may be rooted in the economic strain abortion restrictions impose on vulnerable households.

Finally, our study connects to the literature on the long-term effects of abortion access on children and societal outcomes. Gruber, Levine, and Staiger (1999) show that abortion legalization improved average living conditions for children by altering the composition of births, reducing the number of children born into disadvantage. Donohue and Levitt (2001) further argue that this selection effect contributed to the decline in U.S. crime during the 1990s.[3] However, our study differs from this literature by focusing on immediate economic mechanisms rather than long-run compositional effects, showing how restrictions on abortion access can destabilize household finances in the short run and elevate community-level crime.

The rest of the paper is structured as follows. Section 2 outlines our conceptual framework and details the mechanisms through which abortion restrictions may influence economic hardship and crime. Section 3 describes the natural experiment generating the variation in distance we exploit in our research design. Section 4 describes the data sources and the construction of key variables, including our measure of access to abortion services, and provides descriptive evidence. Section 5 presents the empirical strategy. Section 6 discusses the main results, examining the effects on crime. Section 7 explores mechanisms via fertility

[3]However, this hypothesis has faced challenges. Joyce (2004) argued that the models in Donohue and Levitt (2001) failed to account for key contemporaneous social trends, including the crack epidemic, while Foote and Goetz (2008) showed that the findings are highly sensitive to model specification and that the use of per capita crime rates and policy-based instruments instead of abortion rates weakens the evidence for a causal link. More recent international evidence from Hjalmarsson, Mitrut, and Pop-Eleches (2021) shows that while abortion policy shifts in Romania affected crime levels, they did not meaningfully change crime rates, highlighting the importance of separating compositional and cohort-size effects from behavioral mechanisms.

and economic outcomes. Section 8 concludes by discussing the broader policy implications of our findings.

2 Conceptual Framework

According to the economic theory of crime developed by Becker (1968), individuals weigh the expected utility of engaging in criminal activity against the utility of pursuing legal alternatives. Specifically, individuals assess the potential rewards of illegal behavior in relation to the probability of apprehension, the severity of punishment, and the opportunity cost of forgoing lawful income and long-term stability. Within this framework, the decision to commit a crime is not viewed as irrational, but rather as a calculated response to the individual's economic environment and perceived options.

Economic hardship plays a central role in this decision-making process. When individuals experience persistent financial stress or lack access to stable, legal sources of income due to factors such as unemployment, low wages, limited educational opportunities, or obstacles to labor force participation, the relative cost of engaging in criminal activity decreases. The appeal of property crime, in particular, increases when the legal labor market offers insufficient returns or when individuals lack the resources or support systems to weather economic shocks. In such contexts, the risks associated with criminal behavior may be outweighed by the immediate gains, especially when the perceived likelihood of detection or punishment is low. Becker's framework thus provides a compelling lens through which to understand how shifts in economic conditions, whether induced by policy, market forces, or access to social services, can influence patterns of criminal behavior.

Applying this framework to the context of reproductive policy, we posit a conceptual pathway linking restricted abortion access to crime. Reduced access to abortion services increases the probability of unintended births, especially among individuals with limited economic means. We hypothesize that these births often generate additional financial burdens through increased childcare responsibilities, reduced labor force attachment, and greater reliance on public assistance or informal support networks. The resulting financial strain lowers the opportunity cost of engaging in economically motivated crimes such as theft and burglary. In this way, abortion restrictions can trigger a cascade of consequences from diminished reproductive autonomy to intensified economic insecurity that ultimately shift incentives in a manner consistent with higher property crime rates. We introduce a formal theoretical model that captures these ideas in Appendix A.

3 The Texas HB-2 Natural Experiment

To estimate the effect of abortion access on economic hardship and crime, we exploit the natural experiment created by Texas House Bill 2 (HB-2), enacted in July 2013 during a special legislative session (Texas Legislature, 2013). HB-2 imposed several new restrictions on providers, most notably: (i) an admitting privileges requirement mandating that physicians hold admitting privileges at a hospital within 30 miles, and (ii) an ambulatory surgical center requirement obliging facilities to meet surgical-center building standards even when offering only medication abortion.

Reproductive rights advocates describe such measures as "Targeted Regulation of Abortion Providers (TRAP)" laws, contending that they impose medically unnecessary and costly requirements intended to force abortion facilities to close (Gold and Nash, 2013). When the admitting privileges requirement took effect on November 1, 2013, nearly half of Texas abortion facilities shut down (Lindo et al., 2020a). Providers challenged the law in federal court; during the litigation, the second major provision—the ambulatory surgical center requirement—was largely enjoined, aside from a brief enforcement period in October 2014. The case ultimately reached the U.S. Supreme Court, which in June 2016 struck down both provisions, concluding that "neither of these provisions offers medical benefits sufficient to justify the burdens upon access that each imposes" (Whole Woman's Health v. Hellerstedt, 2016). Despite this ruling, clinics were slow to resume services in Texas. Two years after the ruling, only three facilities had reopened, with news reports suggesting that providers were hindered by licensing hurdles, facility requirements, staffing shortages, and heightened security concerns (Lopez, 2019; Yaffe-Bellany, 2018).

Figure 1 illustrates the spatial and temporal variation in driving distances to the nearest abortion facility generated by HB-2.[4] Panel A shows that average statewide distance was stable until November 2013, when HB-2's admitting privileges requirement took effect. The average distance more than doubled, from 21 miles in July 2013 to 53 miles in July 2014. Panel B highlights substantial heterogeneity in this shock across counties. Distances rose sharply in areas where the sole provider closed—Corpus Christi, Lubbock, McAllen, Midland, San Angelo, and Waco—while remaining largely unchanged in areas where at least one facility remained open—Austin, Dallas, El Paso, Houston, and San Antonio.

Figure 1, Panel A also illustrates a noteworthy reduction in distances the following year, when the statewide average declined from 53 to 41 miles by the middle of 2014. This is driven by the resumption of services at the sole facility in McAllen after a district court exempted it from the admitting privileges requirement, causing distances to again decline in the Lower Rio Grande region. No other region saw comparable relief, and distances continued to remain elevated at twice their pre-HB-2 level through the end of the analyses period.[5]

4 Data

To estimate the effect of abortion access on crime, we combine data for Texas from multiple sources spanning 2009 to 2019. Below, we detail our data sources and sample.

[4]We generate this map using the Myers Abortion Facility Database (Myers, 2025b), which we describe in greater detail in the following section.

[5]Of the three clinics that resumed services after *Whole Woman's Health* ruling, only a re-opening in Waco in 2017 appreciably affected driving distances; the other two facilities to resume services did so in cities where at least one provider had remained open. We present robustness checks in Appendix B demonstrating that our results and conclusions are robust to excluding the McAllen and Waco service regions from the analyses.

4.1 Distances

We measure distances using the Myers Abortion Facility Database (Myers, 2025b), which provides a monthly panel of distance from each county's population centroid to the nearest open brick-and-mortar abortion facility. The identified facilities include private physician offices, hospitals, and freestanding clinics that publicly advertised abortion services or were otherwise readily identifiable to women seeking care.[6] To align with the annual frequency of our outcome data, we collapse the county-by-month panel to county-by-year, taking the average distance within each year.

4.2 Abortion and births

We obtain annual data on induced abortions, categorized by the mother's age and county of residence, from the Texas Induced Terminations of Pregnancy (ITOP) Statistics. This is supplemented with county-level data from the Texas Vital Statistics (VSTAT) public-use file, which provides annual figures on live births, disaggregated by the mother's age and marital status.

4.3 Crime

We obtain crime data from Kaplan's (2021) concatenated Uniform Crime Reporting (UCR) Program files. This dataset includes two key components: (i) a selective list of criminal offenses reported by law enforcement agencies across the U.S. and (ii) demographic and incident details of individuals arrested for resolved crimes. The dataset covers seven major offenses: murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault,

[6] During our analysis period, brick-and-mortar facilities were the only available option to obtain an abortion from a clinician operating in the formal healthcare system in Texas. Direct-to-patient telehealth did not become available in the United States until July 2020 when a court ordered the FDA to temporarily relax the in-person distribution rules for the abortion medication mifepristone during the early stages of the COVID-19 pandemic. However, even this regulatory change did not impact Texas because it had policies in place that effectively banned telehealth provision by licensed Texas providers (Ramaswamy et al., 2021).

burglary, larceny-theft, and motor vehicle theft.[7] The Federal Bureau of Investigation (FBI) collectively refers to these as index crimes and uses them as benchmarks for tracking crime trends in the United States. These crimes are considered serious, frequently occurring, and likely to be reported to law enforcement.

The FBI further divides index crimes into two categories: (i) property crimes, which consist of burglary, larceny-theft, and motor vehicle theft, and (ii) violent crimes, which consist of murder, forcible rape, robbery, and aggravated assault. In our main analyses, we add simple assaults to the violent crimes category, creating an expanded definition that aligns with prior studies and accounts for both severe and less severe forms of violent behavior (Boggess, Chamberlain, and Gill, 2022; DiIulio Jr, 1996). While the FBI does not classify simple assaults as index crimes, they constitute a substantial share of criminal activity and are frequently used as a proxy for broader trends in interpersonal violence (Cook and MacDonald, 2011). We also disaggregate crime types and examine changes in each category individually, allowing us to assess the extent to which specific crime types contribute to the observed changes in property and violent crime.

The non-mandatory participation in the UCR program introduces interruptions in reporting across agencies. To address this issue, we restrict our analysis to agencies that report data continually throughout the sample period (see, e.g., Bondurant, Lindo, and Swensen, 2018). Appendix Figure B.1 describes the number of continuously reporting agencies in each county in this balanced agency-year sample. There is broad geographic coverage across the state: 92% of counties have at least one agency reporting crime data consistently over the entire period and 71% have multiple reporting agencies.[8]

[7]Larceny-theft refers to unlawfully taking property from someone else's possession without involving force or unlawful entry, including theft types like pickpocketing, purse-snatching, shoplifting, theft of auto parts, bicycles, and items from vehicles or buildings; it excludes motor vehicle theft, embezzlement, fraud, and thefts involving lawful access. Motor vehicle theft, by contrast, specifically involves the theft or attempted theft of self-propelled vehicles designed for land travel (e.g., cars, motorcycles, buses), excluding vehicles like boats, aircraft, farm or construction equipment, and situations where the individual had lawful access to the vehicle. The UCR Program and some states use the terms "theft" and "larceny" interchangeably.

[8]As we show later, including all Texas agencies yields results that are similar in both magnitude and statistical significance.

4.4 Economic outcomes

We characterize county-level socioeconomic outcomes using data from three primary sources. Unemployment and labor force participation rates are obtained from the Bureau of Labor Statistics, Local Area Unemployment Program (BLS) (2025), while county-level per capita personal income (in 2010 US dollars) comes from the U.S. Bureau of Economic Analysis (BEA) (2024). We use individual-level income data from the American Community Survey (ACS) (Ruggles et al., 2025) to compute dispersion measures and examine distributional changes in income. For household indebtedness and financial distress, we rely on the debt-to-income ratio from the Federal Reserve's Enhanced Financial Accounts (FED: EFA; 2025). We also obtain the county-level mortgage delinquency rate from the Consumer Financial Protection Bureau (CFPB) (2024). Lastly, we retrieve county-level eviction filing data from Gromis et al. (2022), which is also available through the Eviction Lab at Princeton University.

4.5 Demographic covariates

Demographic information comes from the Surveillance, Epidemiology, and End Results Program (SEER) (2023). We include the population shares of white, black, non-Hispanic residents, as well as age structure: the shares aged 0–9, 10-19, 20–29, 30–39, 40–49, and 50–59. Additionally, we control for the share of women of reproductive age (15–44) to account for variation in the size of the at-risk population.

4.6 Descriptive evidence

Figure 1 illustrates that the Texas HB-2 natural experiment generated a dramatic increase in distances over a short period of time that persisted in most regions of Texas. Our primary identification strategy relies on classifying counties into a binary treatment group defined as a function of changes in travel distance exceeding three alternative thresholds: $\geq$25, $\geq$50, and $\geq$100 miles.[9] Figure B.2 illustrates the probability of treatment across the analysis period. Consistent with the sudden closures of facilities due to the enforcement of Texas HB-2, the probability of being classified as treated increased sharply between 2012 and 2014 for all three treatment definitions.

Table B.1 summarizes outcomes for the control group (no change in distance exceeding 25 miles) and the three alternative treatment groups. On average, crime levels are higher in control counties than in treated counties. In addition, property crimes are more prevalent than non-property crimes across Texas. The table also reports demographics between control and treated counties. The age distribution is fairly similar, including the share of women of childbearing age. Control counties have relatively higher shares of Black and Hispanic individuals, while treated counties have relatively higher shares of White individuals. We control for these demographic variables in our empirical analysis.

These are time-averaged figures and do not capture how different types of crimes evolved over time, and particularly whether the incidence of crime changed following the implementation of HB-2. In Figure 2, we explore this question further and uncover a striking pattern. While property crimes show a declining trend overall, this decline flattens in treated counties after the introduction of HB-2. In contrast, the downward trend continues in control counties. For non-property crimes, the trends between treated and control counties are largely similar between 2013 and 2014. If anything, control counties exhibit a more noticeable increase in non-property crimes over time. Taken together, these descriptive patterns motivate our empirical analysis.

[9]These are not mutually exclusive categories, but rather alternative definitions of treatment. For example, a county that experience a 100-mile increase in travel distance would be included in any of the three treatment groups, whereas a county that experienced a 25-mile increase would be considered treated only by the first and most expansive definition.

5 Empirical Approach

We implement a difference-in-differences (DID) strategy by comparing counties that experienced significant increases in driving distance to the nearest abortion clinic with those that did not. This approach builds on foundational work identifying the effects of Texas HB-2 on abortions and births (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo et al., 2020a). These early papers allow for nonlinear effects of distance using distance categories and additionally, in the case of Lindo et al. (2020a), distance entered as a quadratic function, and all find evidence of a diminishing marginal effect of distance such that increases in distance have the greatest effects on abortion and birth rates in counties that were initially close to an abortion facility.

Recent advancements in DID methodology highlight several identification and interpretation challenges related to the use of continuous treatment in a two-way fixed-effects model (Callaway, Goodman-Bacon, and Sant'Anna, 2024; De Chaisemartin and D'haultfœuille, 2023). To address these concerns, we follow the recommendations of Callaway, Goodman-Bacon, and Sant'Anna (2024) and Callaway and Sant'Anna (2021) by discretizing continuous access shocks to abortion services and defining cohort- and threshold-specific treatment and control groups. Using this approach, we estimate the *average level treatment effects* for each threshold under the standard parallel trends assumption. We outline our identification strategy below.

Specifically, let D_{ct} denote the driving distance from county c to the nearest abortion clinic in year t. We measure year-over-year access shocks to abortion services using ΔD_{ct}, defined as the change in distance to the nearest abortion clinic between consecutive years.[10]

A county c is assigned to the control group C_0 if its access shock never exceeds a baseline threshold $d^0 = 25$ miles over the sample period:

[10] Myers (2024) provides anecdotal and policy evidence suggesting the exogeneity of these changes in driving distance. Aslim, Fu, and Tekin (2024) further show that changes in travel distance during this period are not driven by the demand for abortion services in the previous period.

$$C_0 \equiv \{c \mid \forall t \in \{2010, \ldots, 2019\}, \Delta D_{ct} < d^0\}.$$

We later present alternative specifications in which we vary the threshold parameter d^0. A county c is assigned to a treatment-cohort group T_g if its access shock first surpasses a predetermined threshold d^1 ($\geq d^0$) in year t^g:

$$T_g \equiv \{c \mid \exists t^g \in \{2010, \ldots, 2019\} \text{ such that } \Delta D_{ct^g} \geq d^1, \text{ and } \forall t < t^g, \Delta D_{ct} < d^1\}.$$

The term t^g indicates the treatment year. By construction, we allow treated counties to have different treatment timing, each considered as different treatment cohorts. We designate the years $t < t^g$ as the pre-treatment period and the years $t \geq t^g$ as the post-treatment period.

For a given treatment threshold, our primary parameter of interest is the average treatment effect on the treated (ATT). This estimand is a weighted average of cohort-specific treatment effects (ATT_g), each estimated by comparing outcomes in the treatment group T_g with those in the control group C_0 before and after the treatment year t^g.

In our analysis, we assess the robustness of treatment group definition by using several values for d^1, specifically 25, 50, 100 miles. We select these thresholds ex-ante based on prior work by Fischer, Royer, and White (2018) and Myers (2024), which demonstrate that these cutoffs effectively capture the nonlinear effects of abortion clinic closures on abortion rates, birth outcomes, and contraceptive purchases. We further explore alternative thresholds as well as the use of a continuous treatment variable, finding that the results remain consistent across these specifications. Finally, we assess the robustness of the results to samples that exclude counties in the McAllen and Waco regions in which distances eventually decreased due to facilities re-opening in 2014 (McAllen) and 2017 (Waco).

In the spirit of Wooldridge (2023) and Chen and Roth (2024), we use a Poisson model with police agency and year fixed effects to implement the DID estimation strategy, when assessing the treatment effects on crime counts. We specify the conditional mean of the model as:

$$E[Y_{ict} \mid \cdot] = \kappa_{ict} \times exp(Treatment_{ict}\beta + \gamma'\vec{X}_{ict} + \theta_i + \tau_t), \tag{1}$$

where Y_{ict} represents the number of reported crimes (e.g., index crimes, property crimes, violent crimes, and total crimes) by agency i in county c and year t. The term κ_{ict} accounts for the number of people served by agency i in a given year — this is the exposure variable in our Poisson model.[11] $Treatment_{ict}$ is an indicator for whether the agency is located in one of the treatment-cohort counties (T_g).[12] For instance, when setting $d^1 = 50$ miles, we define treated groups as agencies in counties that have ever experienced an increase of more than 50 miles in driving distance between two consecutive years during our sample period, generating variation in treatment timing across cohorts. $\vec{X}_{ict}$ is a vector of time-varying county-level demographic variables, as detailed in the data section. The terms θ_i and τ_t denote police agency and year fixed effects, respectively.

Our primary focus is on β, which reflects the average treatment effect on the treated (in percentage terms). To ensure comparability in treatment effects across different thresholds, we adopt a "clean-control" strategy: regardless of the threshold d^1 used to define the treated group, we consistently use the same control group (C_0). This control group consists of agencies in counties that have never experienced an increase of more than 25 miles in driving distance (d_0) over two consecutive years. To account for spatial correlations, we cluster standard errors at the county level.

The key identification assumption in a DID model is that, in the absence of treatment, the

[11] Poisson regression with an exposure variable assumes that the expected count for each agency is proportional to its exposure (e.g., population served). Larger exposures lead to larger expected counts and, consequently, greater influence in the likelihood function. Population served is a UCR variable that measures the population under an agency's jurisdiction. This variable is often used to create crime rates that control for population. In cases of overlapping coverage (e.g., city police and county sheriffs), the population is assigned to the most local agency (Kaplan, 2025).

[12] It is possible for a single agency to serve multiple counties. In such cases, accounting for 10% of our sample, the UCR assigns the agency to the county with the largest population share under its jurisdiction. Our results remain consistent and robust when these agencies are excluded.

average evolution of outcomes for the treated group would parallel that of the control group. In our Poisson specification, the parallel trends assumption takes a ratio form, requiring the percentage change in mean outcomes to be parallel between treatment and control groups had the event not occurred (Wooldridge, 2023). Although this assumption cannot be directly tested, we provide evidence supporting parallel pre-treatment trends by conducting a fully specified event study. Specifically, we define the conditional mean of the outcome as:

$$E[Y_{ict} \mid \cdot] = \kappa_{ict} \times exp(\sum_{l=-5}^{5} Treatment_{ic} \times \mathbf{1}\{t = t_c^g + l\}\beta_l + \gamma' \vec{X}_{ict} + \theta_i + \tau_t), \tag{2}$$

where t_c^g denotes the treatment year, and $\mathbf{1}\{t = t_c^g + l\}$ is an indicator for whether an agency is observed l years before or after treatment. We set $l = -1$ as the baseline. Our coefficients of interest, β_l, capture the dynamic effects of increased driving distance to abortion clinics on the outcomes. Statistically insignificant and economically small β_l for $l < -1$ provide evidence of parallel pre-treatment trends, while β_l for $l \geq 0$ track the evolution of the treatment effect over the post-treatment period.

Heterogeneity-Robust DID Estimator. In Figure B.2, we show the proportion of agencies treated over the sample period using different threshold values of d^1. Across all choices of d^1, the onset of treatment occurs in either 2013 or 2014, coinciding with the enactment of HB-2. However, Figure B.2 also highlights variation in the timing of treatment adoption. In the presence of staggered adoption, TWFE estimates are likely to be biased due to heterogeneous treatment effects (Borusyak, Jaravel, and Spiess, 2024; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021), which applies to our empirical setting as well.

To address this concern, we leverage the fact that a sufficient proportion (70%–86%) of units remain never-treated across all samples, supporting the validity of imputation-based and interaction-weighted estimators that use never-treated units to infer counterfactual potential outcomes for treated units (Borusyak, Jaravel, and Spiess, 2024; Sun and Abraham, 2021).

Therefore, we present additional event-study and ATT estimates using the Borusyak, Jaravel, and Spiess (2024) estimator (henceforth BJS). We show that our baseline results are comparable to the BJS estimates and those from alternative heterogeneity-robust methods (e.g., the Sun and Abraham, 2021 estimator, or the S&A estimator). Specifically, the BJS estimator uses never-treated counties to impute counterfactual outcomes for treated counties, estimating the treatment effect by comparing actual observed outcomes with their imputed counterfactuals. This approach allows for time-varying controls and is BLUE under mild assumptions (Borusyak, Jaravel, and Spiess, 2024; Wooldridge, 2021). However, it is important to note that the BJS estimator measures a level rather than a percentage treatment effect and uses the average outcome prior to treatment as the baseline (see Roth et al., 2023 for a review of different DID estimators).

6 Distance to Abortion Providers and Crime

The raw data in our descriptive figures suggest that property crimes increased in counties that experienced a travel distance increase after 2013, relative to other counties (see Figure 2). In contrast, our descriptive analysis does not reveal similar patterns for non-property crimes. We formally test these descriptive patterns using our empirical framework.

Table 1 presents estimated results of the Poisson model in Equation 1, where we disaggregate crime into property and non-property categories.[13] The estimates show a clear increase in property crime as the distance to the nearest abortion provider grows, with the largest effects observed under the most conservative treatment definition—counties experiencing travel distance increases of 100 miles or more. Specifically, column (6) of Table 1 indicates

[13] Table B.2 reports results for pooled crime categories. The pattern mirrors the disaggregated results in Table 1, showing an overall increase in crime.

an approximate an 14.9% increase in property crimes ($p < 0.01$) under this threshold.[14]

By contrast, we find no consistent or statistically significant changes in non-property crimes. The estimates vary in sign and are small in magnitude, providing no evidence of a meaningful or systematic relationship between increased travel distance and non-property offenses. These patterns align with the hypothesis that restricted abortion access exacerbates economic hardship, which in turn increases financially motivated criminal behavior, rather than general or violent criminal activity.

Before presenting a range of robustness checks that support the validity of our findings, we further disaggregate the crime counts to explore the types of crimes most impacted and investigate whether individual characteristics, such as gender and age, contribute to the observed patterns.

6.1 Heterogeneity by crime type

To explore potential heterogeneous effects by crime type, we first disaggregate property crimes into burglary, motor vehicle theft, and larceny-theft (referred to interchangeably as "theft" by the UCR Program). These crimes typically involve unlawfully taking property without direct personal confrontation or violence and are primarily financially motivated. Among these, motor vehicle theft generally involves the highest-value assets, such as cars, motorcycles, and trucks, leading to substantial average monetary gains for offenders. Burglary, meanwhile, yields significant but less consistent financial returns, typically involving stolen electronics, jewelry, cash, or other valuable household items. Finally, larceny-theft usually represents the lowest-value crime category, often comprising petty offenses like shoplifting inexpensive goods, bicycle theft, or pickpocketing. In other words, motor vehicle theft is likely to generate the highest average financial value, followed by burglary and then larceny-theft.[15]

[14] To obtain the percentage change in the expected count of the outcome, we use the transformation $(e^{\hat{\beta}} - 1) \times 100$.

[15] This generalization is supported by average dollar loss data by crime category from the 2019 UCR: the average dollar loss per offense was \$8,886 for motor vehicle theft, \$2,661 for burglary, and \$1,162 for larceny-theft. See the following UCR reports by crime category: motor vehicle theft, burglary, and larceny-theft.

In Panel A of Figure 3, we report the estimates by crime type and find significant increases in motor vehicle theft and burglary across all thresholds for minimum travel distance increase. We also find some evidence of an increase in larceny-theft, though it is not salient across lower distance thresholds.

We further disaggregate violent crimes and identify robbery as a natural candidate within the category of financially motivated offenses. Robbery often involves the direct seizure of cash or valuables (e.g., from persons, banks, or businesses), but it is considered a more serious crime.[16] It may prompt immediate police response, increasing the detection probability, but can also yield high immediate monetary returns. We find a statistically significant increase in robbery when abortion provider distance exceeds 100 miles. Importantly, we do not find statistically significant changes in other violent crimes, including the most common form of assault, simple assault. These findings are consistent with our Beckerian framework: under substantial financial strain, caused by reduced access to abortion, individuals may shift toward high-gain, high-risk offenses. Robbery, for example, may offer substantial immediate (financial) rewards but is associated with higher detection risk, legal penalties, and moral or psychological costs. The model predicts such crimes become more attractive under acute hardship.

Similarly, motor vehicle theft and burglary may offer relatively high financial returns, but detection risk varies. Vehicles, for instance, are more likely to be traceable via GPS, traffic cameras, or license plate readers. In contrast, criminological research finds that detection risk for burglary may be lower when offenders selectively target homes, though such strategic behavior is typically associated with expertise (Nee and Meenaghan, 2006; Coupe and Blake, 2006). In our context, however, these are marginal offenders who may not have committed any crime in the absence of abortion access constraints.

[16] Robbery explicitly involves direct interaction with victims and entails the use of force, threat of force, violence, or intimidation.

6.2 Heterogeneity by offender characteristics

Next, we explore whether the observed increase in property crimes is driven by specific types of offenders. For instance, we hypothesize that although women may be directly affected by increased abortion provider distance, any constraints on abortion access could generate intrahousehold spillovers, as the entire household may bear the economic burden of childbirth rather than just the individual. In that case, we would also expect to observe increases in property crimes committed by males.

We further break down offender characteristics by age, using 18 as a natural cutoff for several reasons. First, age 18 marks the legal transition to adulthood in most jurisdictions, often coinciding with greater financial independence, legal responsibility, and direct exposure to economic hardship. Individuals over 18 are more likely to bear the financial consequences of unintended childbirth within their own households, either as parents or as financially responsible adults. In contrast, minors under 18 are generally financially dependent on their families, so any economic strain would more likely affect their parents' behaviors rather than their own.

Second, patterns of criminal behavior differ notably between minors and adults. Minors are generally processed through the juvenile justice system, which typically emphasizes rehabilitation over punishment and may result in more lenient penalties compared to the adult criminal justice system (Tanenhaus, 2004). Although these lower penalties could, in theory, reduce the opportunity cost of committing crimes, other factors likely constrain minors' criminal activity. In particular, minors are often subject to more intensive supervision environments, such as mandatory school attendance and parental control, which can limit both the opportunity and autonomy necessary to engage in crimes (Wright and Cullen, 2001; Cook and Kang, 2016). Furthermore, developmental and life-course criminology suggests that serious criminal behavior, particularly financially motivated offenses, tends to peak in late adolescence and early adulthood (Hirschi and Gottfredson, 1993), as individuals gain independence and face greater financial responsibilities. Therefore, splitting the sample at age 18 serves as an (indirect) mechanism test for the economic hardship channel: if increased financial strain is the primary driver of the observed rise in property crime, we would expect the effects to be more pronounced among individuals aged 18 and older.

In Panel B of Figure 3, we put these hypotheses to the test. Two novel findings emerge. First, property crimes increase among both females and males as abortion provider distance increases, supporting the presence of intrahousehold spillovers. That is, while women may be directly affected by abortion access constraints, the resulting economic hardship may be shared across the household, leading to increased financially motivated crimes committed by both genders. Second, and consistent with our hypothesis, this increase in property crimes is more pronounced among adults aged 18 and older. This finding aligns with the idea that adults are more directly exposed to the economic consequences of unintended childbirth, either as parents themselves or as financially responsible members of their households. In contrast, minors are more likely to be financially dependent on their families, and thus less directly impacted by the immediate economic hardship stemming from abortion access constraints.

6.3 Event study analyses

In this section, we explore possible dynamic treatment effects and assess the validity of our identification strategy by estimating alternative event study specifications using three estimators described in Section 5: Poisson pseudo-maximum likelihood (PML), BJS, and S&A. To address potential heterogeneous treatment effects arising from using later-treated counties as controls, the BJS and S&A estimates rely on the 516 never-treated agencies—representing 71 percent of all Texas agencies—to construct counterfactual crime outcomes.

In Figure 4, we report dynamic DID estimates for property crimes from three estimation strategies. Consistent with our benchmark analysis, we show pre- and post-trends in crime outcomes for alternative treatment groups defined by abortion provider distance (i.e., ≥ 25 miles, ≥ 50 miles, and ≥ 100 miles). The resulting patterns are closely aligned across treatment thresholds and all three estimation strategies. We do not find significant or systematic differences in pre-trends. In particular, there is no evidence of an upward trend in property crimes prior to the increase in travel distance. We find suggestive evidence of a modest increase in property crimes in the year of treatment, followed by a more noticeable rise the following year. This lag may reflect the time required for unintended births to occur and for any associated economic hardship to materialize. Over time, as the number of individuals affected by unintended births accumulates, the cumulative effect could contribute to higher property crime rates. In addition, the increase in property crimes is most pronounced for counties experiencing an increase of at least 100 miles in travel distance.

We repeated this exercise for non-property crimes, reporting the results in Figure 5. We do not detect any statistically significant changes in non-property crimes either before or after the increase in travel distance. This pattern is consistent with our earlier finding that effects are concentrated among property crimes, which are more tied to financial motives.

6.4 Alternative estimators and specifications

Appendix B reports the results of a series of additional robustness checks to ensure that our findings are not sensitive to specific modeling decisions, estimator choices, treatment definitions, or sample selections. In addition, we implement an alternative identification strategy to further strengthen the credibility of our findings. We briefly describe these exercises here.

Alternative estimators and specifications.

We obtain our baseline static DID estimates in Table 1 using the Poisson PML estimator. As a first robustness check, we re-estimate the static DID coefficients using the BJS estimator, with crime outcomes defined as rates per 10,000 population. In Table B.3, we find a persistent increase in the property crime rate across treatment groups using the BJS estimator. In column (6), property crime increases by approximately 50 incidents per 10,000 population ($p < 0.01$), corresponding to a 13.85% increase relative to the pre-treatment average. By contrast, we do not find any consistent increases in non-property crimes across specifications. These findings closely align with our baseline analysis.

Next, we take our benchmark specification and include a wide range of covariates to assess the robustness of our estimates. One concern is whether increased congestion at destination abortion providers could be driving the observed effects. For instance, crowding of providers in destination counties due to travel distance changes may attenuate the estimated treatment effect on crime. To account for this, we introduce additional controls for the size of the destination service population.[17] In Table B.4, column (2), we find that property crimes increase by approximately 14.57% ($p < 0.01$) when travel distance increases by 100 miles or more. Importantly, we do not observe any statistically significant changes in non-property crimes.

In Table B.4, column (3), we control for county-specific trends to account for economic and policy shocks at the county level that might influence crime trends over time. One potential concern is that differences in crime trends across treated and untreated counties may reflect systematic differences in law enforcement, such as variation in police staffing levels. To address this, in column (4), we additionally control for the number of police officers employed per 10,000 population covered by each agency. In columns (5) and (6), we control for the number of family planning clinics and the number of mental health centers in each county, respectively.[18] The latter is particularly important, as there is a well-established literature documenting a negative relationship between access to mental healthcare and criminal behavior (Jácome, 2020; Deza, Maclean, and Solomon, 2022).[19] If counties experiencing increased travel distances also saw closures of mental health providers, then our baseline

[17]This is the average number of women aged 15-44 served by each facility in the destination city (Myers, 2025b).

[18]Prior to the passage of HB-2, the Texas Department of State Health Services enacted substantial cuts in 2011 to funding for family planning clinics. Although these publicly funded clinics do not provide abortion services, they offer reproductive health care, such as pregnancy tests, preventive screenings, and contraceptive services, primarily to younger and socioeconomically disadvantaged women. Earlier research finds that these funding cuts increased teen birth rates in Texas by approximately 3.4% (Packham, 2017).

[19]In addition, Aslim et al. (2022) develop a theoretical framework showing how different types of crime may be influenced by access to mental health and addiction treatment through the health insurance channel, and empirically test this mechanism using administrative data (see, also, Aslim, Mungan, and Yu 2024).

estimates could be biased upward. Across all alternative specifications, we find a strong and positive relationship between travel distance (≥100 miles) and property crime. In contrast, we consistently find no evidence of substantial effects on non-property crimes.

Alternative treatment and control definitions.

We continue to assess the sensitivity of our estimates by turning to alternative definitions of treatment and control groups. To evaluate whether the observed increase in property crimes is sensitive to how we categorize travel distance, we conduct three additional exercises.

First, we redefine treatment using progressively higher travel distance thresholds in 30-mile intervals and examine changes in property and non-property crimes in Table B.5. We find pronounced increases in property crimes, particularly when travel distance increases by 90 miles or more. This reinforces our earlier finding that large travel distance shocks, especially those approaching or exceeding 100 miles, drive the observed relationship. Consistent with our earlier results, we find no meaningful changes in non-property crimes across these thresholds.

Next, we assess the robustness of our estimates to alternative definitions of the control group, as shown in Table B.6. Specifically, we redefine the control group using a more restrictive threshold of $d^0 = 10$ miles. That is, counties are assigned to the control group if their travel distance to the nearest abortion provider never exceeds 10 miles over the sample period. Our findings remain highly robust to this alternative definition. In column (6), we estimate an approximately 16% increase in property crimes ($p < 0.01$) following a travel distance increase of at least 100 miles. As before, our estimates for non-property crimes remain statistically insignificant.

Finally, we relax the binary treatment definition entirely and model travel distance changes as a continuous variable. In these specifications, we estimate the effect of both contemporaneous changes in travel distance (at time t) and lagged changes (at time $t-1$) on crime outcomes. In Table B.7, we continue to find a positive relationship between travel distance and property crime, although the magnitude of the effect is smaller. This relationship holds for both contemporaneous and lagged changes in travel distance. The smaller effect size in the continuous model is expected and consistent with our baseline analysis, which showed that the impact is heterogeneous: smaller distance changes yield smaller increases in crime, and averaging over the full travel distance distribution attenuates the estimated effect. Modeling travel distance continuously does not alter our conclusions regarding non-property crimes, which remain statistically insignificant and economically negligible, with coefficients consistently near zero.

Alternative sample selection.

The analytical sample in our baseline analysis includes all available border counties in Texas and a balanced panel of police agencies. We assess the sensitivity of our estimates to four alternative sample definitions: (i) an unbalanced panel including all Texas agencies, (ii) the exclusion of counties bordering New Mexico, (iii) the exclusion of counties near the Mexico border that experienced temporary abortion clinic closures, and (iv) the exclusion of all Texas–Mexico border counties. These exercises serve two purposes: (i) to assess whether sample composition affects our results and (ii) to test the robustness of our estimates to excluding counties where individuals may have had disproportionate access to self-managed abortion via pills obtained across the border.

Our first exercise pertains to the inclusion of all Texas agencies in the analysis. This creates an unbalanced panel of agencies, as we relax the consistent reporting constraint over the sample period. The estimates in Table B.8 suggest that our findings are not driven by the composition of agencies: the results are very similar both quantitatively and qualitatively, showing an increase in property crimes as travel distance increases, with no significant changes in non-property crimes.

Next, in Table B.9, we exclude counties bordering New Mexico, which became a hot spot for Texans seeking abortion services after 2013. This proximity may have generated information spillovers beyond travel distance if individuals who might not have otherwise known about alternative access were more likely to become aware of their options.[20] However, our estimates are highly robust to the exclusion of these counties. Notably, the estimate in column (6), reflecting an increase in property crimes following a travel distance increase of 100 miles or more, is nearly identical in magnitude to our baseline result.

We next exclude counties that experienced temporary clinic closures following the implementation of HB-2.[21] As discussed in Section 3, these counties initially faced an increase in travel distance due to the closure of the sole provider, followed by a reversal when clinics in the region reopened. The closure was more persistent in the Waco service region, where the provider did not reopen until 2017, than in McAllen. Nonetheless, we assess the robustness of our estimates by excluding all counties that experienced a reversal in travel distance during our sample period. As shown in Table B.10, our estimates remain remarkably robust to this alternative sample selection. We also present the corresponding event-study estimates using three different estimators for property and non-property crimes in Figure B.3 and Figure B.4, respectively. Consistent with the static DID results, the dynamic specifications show a persistent increase in property crimes, with no significant changes in non-property crimes.

In our final sample selection exercise, we consider the potential for disproportionate access to misoprostol (i.e., abortion pills) in counties neighboring Mexico.[22] If self-induced abortion was more common in treated border counties, then our benchmark estimates would

[20] Bhardwaj et al. (2020) document an approximately 11 percentage point increase in the share of abortions provided in New Mexico clinics to Texas residents.

[21] These counties are located in the McAllen and Waco service regions, including Cameron, Coryell, Falls, Hidalgo, Limestone, McLennan, Starr, Willacy, and Zapata.

[22] In addition to abortion pill access, there was also a surge of unaccompanied children arriving at the U.S.–Mexico border without a guardian in 2014. More than three-quarters of these minors came from poor and violence-affected communities in El Salvador, Guatemala, and Honduras. Data from the Office of Refugee Resettlement (U.S. Department of Health & Human Services, 2025) show that these children were not systematically released to sponsors in counties that experienced changes in abortion access due to clinic distance. In Texas, e.g., sponsors were concentrated mainly in Houston (Harris County) and Dallas (Dallas County) - counties that saw relatively small distance changes. Other states and counties also received large numbers of children awaiting immigration proceedings, including Los Angeles County (CA), Miami-Dade County (FL), Fairfax County (VA), and Suffolk County (NY). Taken together, these patterns indicate that the settlement of unaccompanied minors is unlikely to threaten our identification strategy.

be conservative, reflecting a lower bound. Table B.11 therefore examines the robustness of our results to excluding all counties bordering Mexico. We find that, as predicted, the estimates are slightly larger, with a consistent positive relationship between travel distance and property crimes. In a few specifications, we also observe increases in non-property crimes, though these are driven primarily by financially motivated offenses (e.g., robbery) rather than violent assaults or manslaughter.

Alternative statistical inference.

In our baseline analysis, we report robust standard errors clustered at the county level. To assess the sensitivity of our statistical inference, we implement a randomization inference procedure by randomly assigning treatment across counties, simulating this process 999 times. For each iteration, we estimate the treatment effect and plot the distribution of the placebo estimates. Importantly, we preserve the structure of our multiple treatment cutoffs by replicating the random assignment for each threshold (i.e., $\geq$ 25 miles, $\geq$ 50 miles, and $\geq$ 100 miles). We conduct this randomization inference separately for property crimes and non-property crimes.

In Figure B.5, we plot the distribution of placebo estimates for property crimes. We do not find statistically significant effects in Panels A and B, which correspond to smaller distance thresholds. However, the p-value in Panel B (50 miles or more) is 0.13, marginally above the conventional 10 percent significance level.

By contrast, in Panel C (100 miles or more), we find that only 20 out of 999 placebo estimates exceed the baseline estimate, yielding a Fisher p-value less than 0.01. In both the baseline and randomization inference approaches, we observe a statistically significant increase in property crimes when travel distance increases by 100 miles or more. This result provides additional reassurance regarding the validity of our baseline inference.

In our baseline analysis, we did not find any statistically significant impact of travel distance on non-property crimes. The randomization inference in Figure B.6 reinforces this finding. Across all treatment thresholds, we do not observe statistically significant changes in non-property crimes when treatment is randomly assigned.

Alternative identification strategy.
To further assess the robustness of our findings, we introduce an alternative estimation strategy by implementing the synthetic control method.

We compare trends in property and non-property crimes between treated agencies, located in counties where travel distance to the nearest abortion provider (ΔD_{ct}) ever exceeded 100 miles, and control agencies in counties where distance changes never exceeded 25 miles. To construct the synthetic controls, we use pre-treatment crime outcomes observed prior to 2013 as predictor variables. This approach allows us to create more tailored counterfactual trends for the treated agencies and provides an additional check on the validity of our baseline results. A detailed discussion of the synthetic control implementation is provided in Appendix C.

In Figure C.1, Panel A shows a clear divergence in property crime trends between treated units and their synthetic controls. Specifically, while property crimes decline in the synthetic control counties, we observe a sharp increase in treated counties beginning in 2013. This uptick continues through 2015 and remains elevated in 2016, followed by a decline thereafter. In contrast, Panel B shows that treated and control counties follow similar trends in non-property crimes both before and after 2013.

7 Mechanisms

While crime may serve as a means of funding the increased cost of obtaining an abortion, the event study evidence that the increase in crime begins to manifest one to two years following the decrease in abortion access runs counter to this explanation, instead suggesting that financial strain resulting from the arrival of an unplanned baby may lead to financially motivated crimes. To explore this possibility, we first confirm previous findings that the increase in distance results in decreases in abortions and increases in births and then turn to measures of resulting financial strain.

Changes in abortion and fertility rates.

First, we examine how travel distance is associated with abortion and fertility rates. This exercise replicates several seminal papers that study the impact of HB-2 on abortions and births (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo et al., 2020a), using a longer sample period but similar estimation strategies. We present our findings in the main text, with detailed explanations provided in Appendix D. In short, our results closely align with those in the existing literature. For example, Lindo et al. (2020a) find a 35% decline in abortion rates associated with a 100-mile increase in travel distance (Table 2, column (6)). Despite differences in sample period and specifications, we find a 34% decline in abortion rates (Table D.1, column (3)) for counties experiencing an increase in travel distance of 100 miles or more.[23] We also find an increase in fertility rates, particularly among younger women, as travel distance increases. This pattern is consistent with findings from existing studies.

In Panel A of Figure B.7, we examine the relationship between the number of property crimes in the preceding year ($t-1$) and the abortion rate. Of course, this is a descriptive analysis, and the abortion rate at time t could still be correlated with property crimes in $t-1$ through its effect on property crimes at time t. Even so, Panel A provides reassuring evidence: lagged property crimes are not positively correlated with the abortion rate. In fact, due to outliers at the lower and upper ends, the overall relationship appears slightly negative. To probe this further, we also fit a linear trend in the middle of the distribution, disregarding the extreme tails, and find it to be essentially flat, indicating no significant association between the number of property crimes (per 10,000) in $t-1$ and abortion rates

[23]The change in $log(abortion\ counts)$ reported in Lindo et al. (2020a) is -0.427; our estimate is -0.413.

in t.

Our hypothesis is that property crimes are a downstream outcome influenced by the economic strain of unintended births resulting from increased travel distances to abortion providers. If this is the case, we would expect to observe a positive relationship between the birth rate in time $t-1$ and the number of property crimes (per 10,000) in time t. Panel B of Figure B.7 confirms this expectation: we find a strong positive correlation, regardless of whether tail observations are included, between the birth rate in time $t-1$ and the number of property crimes (per 10,000) in time t.

So far, we have hypothesized that unintended births due to increased travel distance result in economic hardship. Thus, a natural next step is to explore whether economic outcomes change as travel distance imposes constraints on abortion access.

Changes in economic outcomes.

Existing evidence suggests that being denied an abortion and the consequent unwanted childbirth have severe economic consequences (Miller, Wherry, and Foster, 2023). A large literature also documents adverse labor market outcomes associated with motherhood, particularly for women—often referred to as the "child penalty" (Andresen and Nix, 2022; Kleven, Landais, and Leite-Mariante, 2024). The financial impact of pregnancy and childbirth can take many forms, ranging from medical expenses to the overall cost of raising a child. The latter has been estimated at over $12,000 per child per year for a middle-income family, adjusted for an inflation rate of 4%, during the period from 2015 to 2019.[24]

Against this backdrop, we explore whether distance to the nearest abortion provider affects labor market and financial outcomes. We begin with descriptive analyses to assess whether these potential relationships emerge in simplified frameworks. Figure B.8 plots labor market outcomes in time t, particularly the labor force participation rate and the unemployment rate, against travel distance in time $t-1$. We find that labor force participation is

[24]The estimate is obtained from a 2022 Brookings Report: `bit.ly/4dOgjdZ`.

negatively related to travel distance, while the unemployment rate is positively related.

Raising a child imposes significant time costs (e.g., child care, health visits, daily care needs). These time costs can crowd out labor market participation, particularly for mothers who might otherwise have worked (Juhn and Potter, 2006). As a result, the negative relationship between abortion access barriers and labor force participation rates is both intuitive and consistent with the broader literature on the economic costs of early motherhood (see, e.g., Bloom et al. 2009). The observed positive relationship between abortion access barriers and the unemployment rate could partially reflect an increase in frictional unemployment. Faced with the new demands of unintended births, mothers may leave jobs that no longer align with their childcare needs and spend time searching for more suitable employment (Amuedo-Dorantes and Kimmel, 2005; Laffērs and Schmidpeter, 2021). This transition period could temporarily elevate unemployment rates, even as overall labor force participation declines. But these transitions, even if temporary, could exacerbate existing financial challenges.

This naturally leads us to consider a set of financial outcomes. We begin by descriptively exploring the relationship between travel distance and measures of income, debt, and housing instability in Figure B.9. We do not find a significant relationship between personal income measures or debt-to-income ratio and travel distance, at least descriptively. However, we find that the income interquartile range (a measure for inequality), the debt-to-income ratio, the mortgage delinquency rate, and the number of eviction filings (per 1,000) are all positively related to travel distance. Notably, the associations for these housing instability measures are also statistically significant.

We extend our analysis using our baseline regression framework, though we employ different estimators (e.g., MLE for Poisson GLM versus OLS for linear regression) depending on the nature of the outcome variable. First, drawing on multiple data sources, we report estimates of the impact of travel distance changes on labor market outcomes in Table 2. Consistent with our descriptive analysis, we find a decline in the number of individuals in the labor force and an increase in the number of unemployed individuals.[25]

In Figure B.10, Figure B.11, and Figure B.12, we unpack these relationships by age, sex, marital status, and educational attainment, respectively. Overall, the observed changes in labor market outcomes associated with increased travel distance to abortion providers appear to be driven primarily by younger, unmarried, and less-educated individuals. While we find a decline in labor force participation among unmarried females, we also observe meaningful spillover effects on unmarried males, mirroring the rise in property crimes among this group. These patterns may reflect cohabiting couples who are not formally married, where abortion access shocks generate intrahousehold spillovers related to economic hardship and financially motivated criminal behavior.

Next, we turn our focus to the impact of travel distance changes on financial outcomes in Table 3. In Panel A, we find a negative but statistically insignificant relationship between travel distance and county-level per capita personal income. While the coefficients are not significant, the consistent negative signs across specifications are suggestive.[26] It is plausible that changes in abortion access do not substantially affect aggregate income at the county level. If those most affected by travel distance increases are disproportionately young and less educated, aggregate income effects may be muted, but income dispersion may still change within affected populations.

To explore this, we turn to individual-level data and estimate the interquartile range (IQR) of personal income as a measure of inequality among middle earners. Specifically, we employ a Recentered Influence Function (RIF) regression to estimate the effect of treatment on the IQR.[27] This method allows us to capture the unconditional distributional impact of travel distance shocks on income dispersion. We provide technical details of the RIF

[25]The exposure variable in the Poisson regression is the overall population. Our estimates are robust to using the working-age population as the exposure variable instead.

[26]We also obtain consistently negative, but statistically insignificant, effects when we define the outcome as the natural logarithm of personal income.

[27]We implement unconditional quantile regression following Firpo, Fortin, and Lemieux (2009) to estimate the effect of abortion provider distance on the IQR of personal income, using individual-level data from the American Community Survey (2009–2019) (Ruggles et al., 2025).

framework in Appendix E.

An increase in the IQR in response to travel distance shocks implies a widening of the income distribution between the 25th and 75th percentiles. That is, rising inequality among the middle 50% of earners. This supports the view that the policy disproportionately impacts individuals who are more economically vulnerable at baseline. The estimates in Panel B of Table 3 are consistent with this hypothesis: the RIF regression results indicate that an increase in travel distance to abortion providers is associated with an approximately $5,000 increase in the IQR of personal income ($p < 0.01$). This suggests a meaningful widening of the income distribution among middle earners, reinforcing concerns about rising inequality among those most likely to be affected by the policy.

In addition, we find increases in the debt-to-income ratio, mortgage delinquency rate, and the number of eviction filings, reinforcing our descriptive patterns. These effects are most pronounced in counties experiencing travel distance increases of 100 miles or more, where we also observe sharp rises in property crimes. Rising debt and housing instability likely capture the economic hardship families face as a result of unintended childbirth.

These patterns, particularly the increases in debt and evictions, are consistent with existing evidence on the economic consequences of abortion restrictions, including the Turnaway Study. Unlike prior work focused on legal bans or gestational limits, our setting captures changes in geographic access and highlights downstream impacts on financial distress and housing instability.

Our findings suggest that increased travel distance imposes additional economic burdens, potentially crowding out labor force participation due to the time costs of child care, and elevating frictional unemployment through job disruptions or transitions. These mechanisms likely underlie the observed increases in financial hardship and may also help explain the rise in financially motivated crimes.

8 Conclusion

This paper demonstrates that restrictions on abortion access can have substantial downstream effects, shaping not only reproductive outcomes but also economic conditions and patterns of criminal behavior. Using detailed panel data from Texas counties between 2009 and 2019, we show that increased travel distance to abortion providers, driven largely by clinic closures, leads to a reduction in abortion rates and a corresponding rise in live births. These shifts are concentrated among young adults, particularly women aged 20–24, who are also most vulnerable to the economic burdens of unintended parenthood.

We further show that these shifts in fertility patterns set off broader economic impacts, leading to financial strain that spreads throughout affected communities. Specifically, limited access to abortion is associated with declines in labor force participation, increases in debt-to-income ratios, higher income inequality, and greater housing instability, as evidenced by rising mortgage delinquency rates and the number of eviction filings. These developments reflect growing financial stress, especially among populations with limited resources and few alternatives.

Importantly, we find that this economic strain significantly contributes to financially motivated illegal activity, as reflected in increased rates of property crimes, particularly burglary, motor vehicle theft, and robbery, in counties facing the greatest increases in travel distance to abortion providers. Moreover, these increases are concentrated among adults, rather than minors, and among both male and female offenders, consistent with a household-level transmission of financial stress. In contrast, we detect no meaningful changes in non-property crimes, indicating the selective and economically driven nature of the observed crime effects.

To put our estimates into context, we conduct a back-of-the-envelope calculation. Specifically, we aggregate crime at the county level so that both the ATT and the first stage are measured at the same level, and then estimate the local average treatment effect (LATE).[28]

[28] This empirical exercise yields coefficients similar to our benchmark analysis. While this is reassuring, as

The results suggest that property crimes increase by 181 incidents as abortions decline by 64 cases, implying that one prevented abortion corresponds to approximately 2.8 additional annual crime counts.

This estimate is likely plausible for several reasons we uncover in this paper and that are consistent with the literature on property crime and criminal behavior. First, we find evidence that crimes can spill over within households. For example, we observe increases in property crimes committed by males, suggesting that incidents may involve multiple offenders from the same household. Second, the crime literature consistently shows that individuals who enter the illegal labor market accumulate "illegal human capital" while their legal human capital depreciates, making it increasingly difficult to return to lawful employment (Mocan, Billups, and Overland, 2005; Loughran et al., 2013). Abortion, by contrast, is a one-time event, whereas property crimes can be repeated within a single year, creating many more opportunities for offenses.[29] This dynamic could be amplified by the fact that property crimes, especially motor vehicle theft (where we find particularly salient increases), have disproportionately low clearance ("hit") rates.[30] When detection rates are low, the opportunity cost of committing property crimes declines, which may incentivize multiple offenses by the same offender within a given year.

Taken together, our findings reveal that abortion restrictions have consequences that extend well beyond reproductive health. By increasing the likelihood of unintended childbirth among economically vulnerable populations, policies that limit access to abortion generate material hardship that can alter individual behavior and community dynamics in consequential ways. The rise in property crime highlights one of the unintended spillovers that can result from curtailing access to reproductive services. As such, our study highlights the need

with any LATE-style back-of-the-envelope calculation, the estimates rely on stricter identifying assumptions, so the caveat regarding the exclusion restriction applies here as well.

[29] This does not necessarily imply that individuals who enter the illegal labor market commit more crimes each year, only that crime may not be a one-time event.

[30] According to 2023 FBI data, the clearance rate for motor vehicle theft was around 8%, while the average for property crimes was about 14%. This means that for every 100 reported property crimes, roughly 14 are solved through an arrest or exceptional means. For comparison, the average clearance rate for violent crimes in 2023 was about 41%.

for policymakers to consider the broader economic and social costs of abortion restrictions, costs that are often borne not just by individuals but by families, communities, and the public at large.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program." *Journal of the American Statistical Association* 105 (490):493–505.

Abboud, Ali. 2019. "The Impact of Early Fertility Shocks on Women's Fertility and Labor Market Outcomes." URL https://papers.ssrn.com/abstract=3512913.

Amuedo-Dorantes, Catalina and Jean Kimmel. 2005. "The motherhood wage gap for women in the United States: The importance of college and fertility delay." *Review of Economics of the Household* 3:17–48.

Andresen, Martin Eckhoff and Emily Nix. 2022. "What causes the child penalty? Evidence from adopting and same-sex couples." *Journal of Labor Economics* 40 (4):971–1004.

Angrist, J.D. and W.N. Evans. 1999. "Schooling and labor market consequences of the 1970 state abortion reforms." *Research in Labor Economics* 18:75–113. ISBN: 9780762305841.

Aslim, Erkmen G, Wei Fu, and Erdal Tekin. 2024. "Abortion Access and Child Maltreatment." National Bureau of Economic Research, Working Paper 32771.

Aslim, Erkmen G, Murat C Mungan, Carlos I Navarro, and Han Yu. 2022. "The effect of public health insurance on criminal recidivism." *Journal of Policy Analysis and Management* 41 (1):45–91.

Aslim, Erkmen G, Murat C Mungan, and Han Yu. 2024. "A welfare analysis of Medicaid and recidivism." *Health Economics* 33 (11):2463–2507.

Becker, Gary S. 1968. "Crime and punishment: An economic approach." *Journal of Political Economy* 76 (2):169–217.

Bhardwaj, Neha R, Cristina Murray-Krezan, Shannon Carr, Jamie W Krashin, Rameet H Singh, Alicia L Gonzales, and Eve Espey. 2020. "Traveling for rights: abortion trends in New Mexico after passage of restrictive Texas legislation." *Contraception* 102 (2):115–118.

Bloom, David E, David Canning, Günther Fink, and Jocelyn E Finlay. 2009. "Fertility, female labor force participation, and the demographic dividend." *Journal of Economic Growth* 14:79–101.

Boggess, Lyndsay N, Alyssa W Chamberlain, and Lexi Gill. 2022. "Deconstructing neighborhood effects across aggravated, domestic, and simple assault." *Journal of Crime and Justice* 45 (5):567–587.

Bondurant, Samuel R, Jason M Lindo, and Isaac D Swensen. 2018. "Substance abuse treatment centers and local crime." *Journal of Urban Economics* 104:124–133.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2024. "Revisiting event-study designs: robust and efficient estimation." *Review of Economic Studies* :rdae007.

Bureau of Labor Statistics, Local Area Unemployment Program (BLS). 2025. "Labor Force data by county and state, 1990-2024." Online tables [dataset].

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna. 2024. "Event Studies with a Continuous Treatment." In *AEA Papers and Proceedings*, vol. 114. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, 601–605.

Callaway, Brantly and Pedro HC Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics* 225 (2):200–230.

Chen, Jiafeng and Jonathan Roth. 2024. "Logs with zeros? Some problems and solutions." *Quarterly Journal of Economics* 139 (2):891–936.

Collinson, Robert, John Eric Humphries, Nicholas Mader, Davin Reed, Daniel Tannenbaum, and Winnie Van Dijk. 2024. "Eviction and poverty in American cities." *Quarterly Journal of Economics* 139 (1):57–120.

Consumer Financial Protection Bureau (CFPB). 2024. "Mortgages 90 or More Days Delinquent." URL https://www.consumerfinance.gov/data-research/mortgage-performance-trends/mortgages-90-or-more-days-delinquent/. Retrieved August 2, 2025.

Cook, Philip J and Songman Kang. 2016. "Birthdays, schooling, and crime: Regression-discontinuity analysis of school performance, delinquency, dropout, and crime initiation." *American Economic Journal: Applied Economics* 8 (1):33–57.

Cook, Philip J and John MacDonald. 2011. "Public safety through private action: an economic assessment of BIDS." *The Economic Journal* 121 (552):445–462.

Coupe, Timothy and Laurence Blake. 2006. "Daylight and darkness targeting strategies and the risks of being seen at residential burglaries." *Criminology* 44 (2):431–464.

Cowell, Frank A and Emmanuel Flachaire. 2015. "Statistical methods for distributional analysis." In *Handbook of Income Distribution*, vol. 2. Elsevier, 359–465.

Dave, Dhaval M, Christine Durrance, Bilge Erten, Yang Wang, and Barbara L Wolfe. 2025. "Abortion Restrictions and Intimate Partner Violence in the Dobbs Era." National Bureau of Economic Research, Working Paper 33916.

De Chaisemartin, Clement and Xavier D'haultfœuille. 2023. "Two-way fixed effects and differences-in-differences estimators with several treatments." *Journal of Econometrics* 236 (2):105480.

Deza, Monica, Johanna Catherine Maclean, and Keisha Solomon. 2022. "Local access to mental healthcare and crime." *Journal of Urban Economics* 129:103410.

DiIulio Jr, John J. 1996. "Help wanted: Economists, crime and public policy." *Journal of Economic Perspectives* 10 (1):3–24.

Donohue, John J and Steven D Levitt. 2001. "The impact of legalized abortion on crime." *Quarterly Journal of Economics* 116 (2):379–420.

Essama-Nssah, Boniface and Peter J Lambert. 2012. "Chapter 6 influence functions for policy impact analysis." In *Inequality, mobility and segregation: Essays in honor of Jacques Silber*. Emerald Group Publishing Limited, 135–159.

Farin, Sherajum Monira, Lauren Hoehn-Velasco, and Michael F Pesko. 2024. "The impact of legal abortion on maternal mortality." *American Economic Journal: Economic Policy* 16 (3):174–216.

Federal Reserve Board. 2025. "Enhanced Financial Accounts - Household Debt Visualization." URL https://www.federalreserve.gov/releases/z1/dataviz/household_debt/. Accessed January 2025.

Firpo, Sergio, Nicole M Fortin, and Thomas Lemieux. 2009. "Unconditional quantile regressions." *Econometrica* 77 (3):953–973.

Firpo, Sergio P, Nicole M Fortin, and Thomas Lemieux. 2018. "Decomposing wage distributions using recentered influence function regressions." *Econometrics* 6 (2):28.

Fischer, Stefanie, Heather Royer, and Corey White. 2018. "The impacts of reduced access to abortion and family planning services on abortions, births, and contraceptive purchases." *Journal of Public Economics* 167:43–68.

Foote, Christopher L and Christopher F Goetz. 2008. "The impact of legalized abortion on crime: Comment." *Quarterly Journal of Economics* 123 (1):407–423.

Freeman, Richard B. 1996. "Why do so many young American men commit crimes and what might we do about it?" *Journal of Economic Perspectives* 10 (1):25–42.

Gold, Rachel Benson and Elizabeth Nash. 2013. "TRAP Laws Gain Political Traction While Abortion Clinics—and the Women They Serve—Pay the Price." *Guttmacher Policy Review* 16 (2):7–12.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225 (2):254–277.

Gromis, Ashley, Ian Fellows, James R Hendrickson, Lavar Edmonds, Lillian Leung, Adam Porton, and Matthew Desmond. 2022. "Estimating eviction prevalence across the United States." *Proceedings of the National Academy of Sciences* 119 (21):e2116169119.

Gruber, Jonathan, Phillip Levine, and Douglas Staiger. 1999. "Abortion legalization and child living circumstances: Who is the "marginal child"?" *Quarterly Journal of Economics* 114 (1):263–291.

Hirschi, Travis and Michael Gottfredson. 1993. "Rethinking the juvenile justice system." *Crime & Delinquency* 39 (2):262–271.

Hjalmarsson, Randi, Andreea Mitrut, and Cristian Pop-Eleches. 2021. "The impact of abortion on crime and crime-related behavior." *Journal of Public Economics* 200:104468.

Jácome, Elisa. 2020. "Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility." Working Paper, Princeton University.

Jones, Kelly M and Mayra Pineda-Torres. 2024. "TRAP'd teens: Impacts of abortion provider regulations on fertility & education." *Journal of Public Economics* 234:105112.

Joyce, Ted. 2004. "Did legalized abortion lower crime?" *Journal of Human Resources* 39 (1):1–28.

Juhn, Chinhui and Simon Potter. 2006. "Changes in labor force participation in the United States." *Journal of Economic Perspectives* 20 (3):27–46.

Kaplan, Jacob. 2021. *Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020.*

———. 2025. "Population (UCR Overview) — Decoding FBI Crime Data." `https://ucrbook.com/index.html#population`. Population section from "Decoding FBI Crime Data" (UCR Book).

Kleven, Henrik, Camille Landais, and Gabriel Leite-Mariante. 2024. "The child penalty atlas." *Review of Economic Studies* :rdae104.

Kuziemko, Ilyana, Jessica Pan, Jenny Shen, and Ebonya Washington. 2018. “The mommy effect: Do women anticipate the employment effects of motherhood?” National Bureau of Economic Research, Working Paper 24740.

Lafférs, Lukás and Bernhard Schmidpeter. 2021. “Mothers’ job search after childbirth.” IZA, Working Paper 14593.

Levine, P B, D Staiger, T J Kane, and D J Zimmerman. 1999. “Roe v Wade and American fertility.” *American Journal of Public Health* 89 (2):199–203. Publisher: American Public Health Association.

Lindo, Jason M, Caitlin Knowles Myers, Andrea Schlosser, and Scott Cunningham. 2020a. “How far is too far?: New evidence on abortion clinic closures, access, and abortions.” *Journal of Human Resources* 55 (4):1137–1160.

Lindo, Jason M., Mayra Pineda-Torres, David Pritchard, and Hedieh Tajali. 2020b. “Legal Access to Reproductive Control Technology, Women’s Education, and Earnings Approaching Retirement.” *AEA Papers and Proceedings* 110:231–235.

Lopez, Ashley. 2019. “Despite Supreme Court Win, Texas Abortion Clinics Still Shuttered.” KFF Health News.

Loughran, Thomas A, Holly Nguyen, Alex R Piquero, and Jeffrey Fagan. 2013. “The returns to criminal capital.” *American Sociological Review* 78 (6):925–948.

Machin, Stephen and Costas Meghir. 2004. “Crime and economic incentives.” *Journal of Human Resources* 39 (4):958–979.

Miller, Sarah, Laura R Wherry, and Diana Greene Foster. 2023. “The economic consequences of being denied an abortion.” *American Economic Journal: Economic Policy* 15 (1):394–437.

Mocan, H Naci, Stephen C Billups, and Jody Overland. 2005. “A dynamic model of differential human capital and criminal activity.” *Economica* 72 (288):655–681.

Myers, Caitlin. 2024. “Forecasts for a post-Roe America: The effects of increased travel distance on abortions and births.” *Journal of Policy Analysis and Management* 43 (1):39–62.

———. 2025a. “From Roe to Dobbs: 50 Years of Cause and Effect of US State Abortion Regulations.” *Annual Review of Public Health* 46 (Volume 46, 2025):433–446. Publisher: Annual Reviews.

———. 2025b. “Myers Abortion Facility Database.” Data vintage: July 1, 2025.

Myers, Caitlin Knowles. 2017. “The power of abortion policy: Reexamining the effects of young women’s access to reproductive control.” *Journal of Political Economy* 125 (6):2178–2224.

Nee, Claire and Amy Meenaghan. 2006. “Expert decision making in burglars.” *British Journal of Criminology* 46 (5):935–949.

Packham, Analisa. 2017. “Family planning funding cuts and teen childbearing.” *Journal of Health Economics* 55:168–185.

Quast, Troy, Fidel Gonzalez, and Robert Ziemba. 2017. “Abortion Facility Closings and Abortion Rates in Texas.” *Inquiry: A Journal of Medical Care Organization, Provision and Financing* 54:46958017700944.

Ramaswamy, Amrutha, Gabriela Weigel, Lauren Sobel, and Alina Salganicoff. 2021. "Medication Abortion and Telemedicine: Innovations and Barriers During the COVID-19 Emergency."

Rios-Avila, Fernando. 2020. "Recentered influence functions (RIFs) in Stata: RIF regression and RIF decomposition." *The Stata Journal* 20 (1):51–94.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's trending in difference-in-differences? A synthesis of the recent econometrics literature." *Journal of Econometrics* 235 (2):2218–2244.

Ruggles, Steven, Sarah Flood, Matthew Sobek, Daniel Backman, Grace Cooper, Julia A. Rivera Drew, Stephanie Richards, Renae Rodgers, Jonathan Schroeder, and Kari C.W. Williams. 2025. "IPUMS USA: Version 16.0." [dataset].

Schnepel, Kevin T. 2018. "Good jobs and recidivism." *The Economic Journal* 128 (608):447–469.

Sun, Liyang and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* 225 (2):175–199.

Surveillance, Epidemiology, and End Results Program (SEER). 2023. "SEER*Stat Database: Populations-State-level population files (1969-2023)."

Tanenhaus, David S. 2004. *Juvenile justice in the making*. Oxford University Press.

Texas Department of State Health Services. 2024. "Texas County-level Birth Data." `https://healthdata.dshs.texas.gov/dashboard/births-and-deaths/live-births`. Accessed on June 27, 2024.

Texas Legislature. 2013. "H.B. No. 2, 83rd Special Session (2013): An Act relating to the regulation of abortion procedures, providers, and facilities; providing penalties." Enacted during a special session of the 83rd Texas Legislature.

U.S. Bureau of Economic Analysis (BEA). 2024. "Personal Income by County, Metro, and Other Areas." Retrieved August 2, 2025.

U.S. Department of Health & Human Services. 2025. "Unaccompanied Children Released to Sponsors by County." `https://www.acf.hhs.gov/orr/grant-funding/unaccompanied-children-released-sponsors-county`. Accessed: 2025-09-26.

Whole Woman's Health v. Hellerstedt. 2016. U.S. Supreme Court, 579 U.S. 582. Decided June 27, 2016. Case No. 15-274.

Wilkinson, Tracey A and Caitlin Bernard. 2024. "Abortion restrictions and the impact on families." *JAMA Pediatrics* 178 (1):15–16.

Wooldridge, Jeffrey M. 2021. "Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators." *Available at SSRN 3906345* .

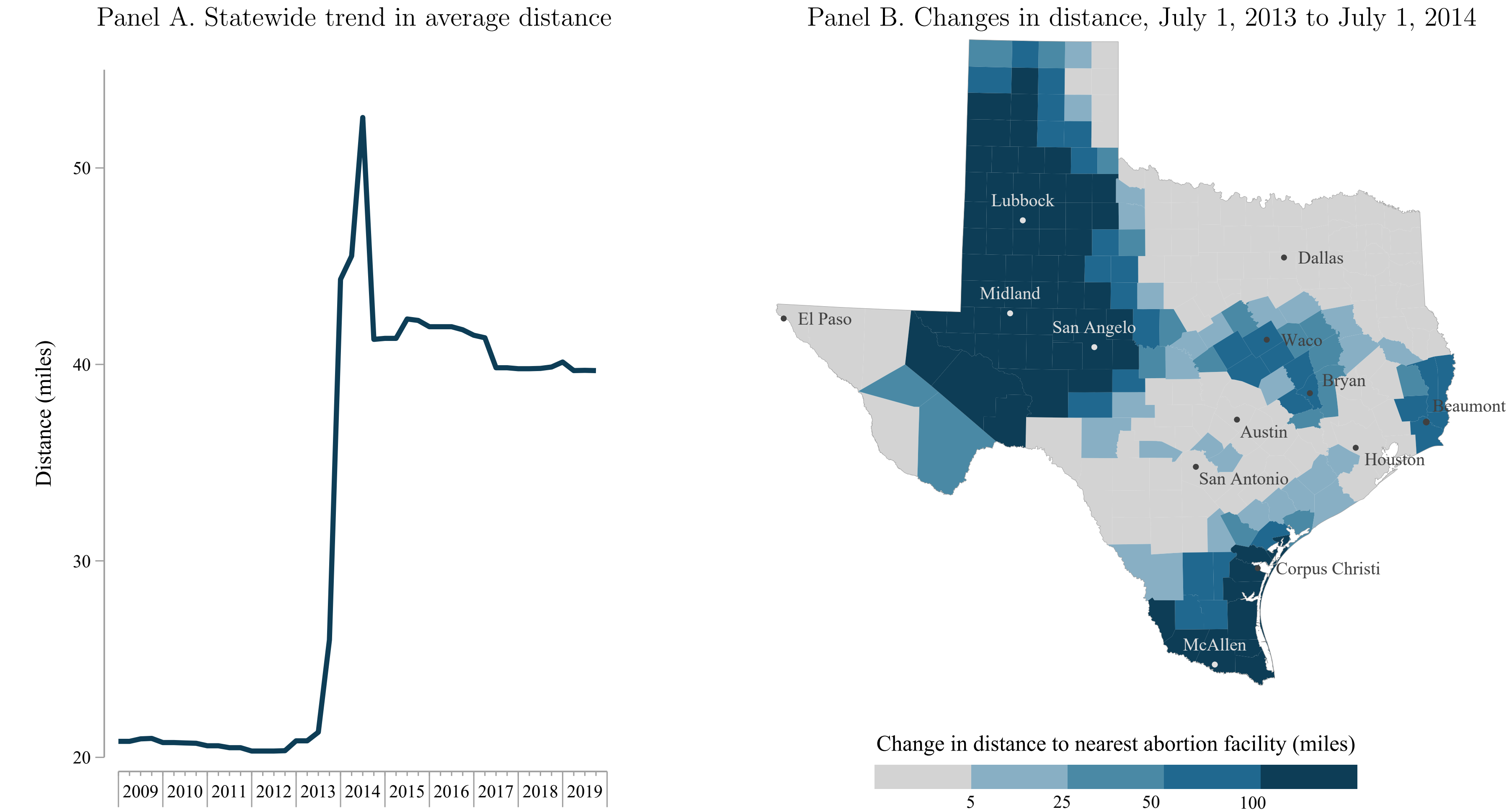
———. 2023. "Simple approaches to nonlinear difference-in-differences with panel data." *The Econometrics Journal* 26 (3):C31–C66.

Wright, John Paul and Francis T Cullen. 2001. "Parental efficacy and delinquent behavior: Do control and support matter?" *Criminology* 39 (3):677–706.

Yaffe-Bellany, By David. 2018. "Five years after Wendy Davis filibuster, Texas abortion providers struggle to reopen clinics."

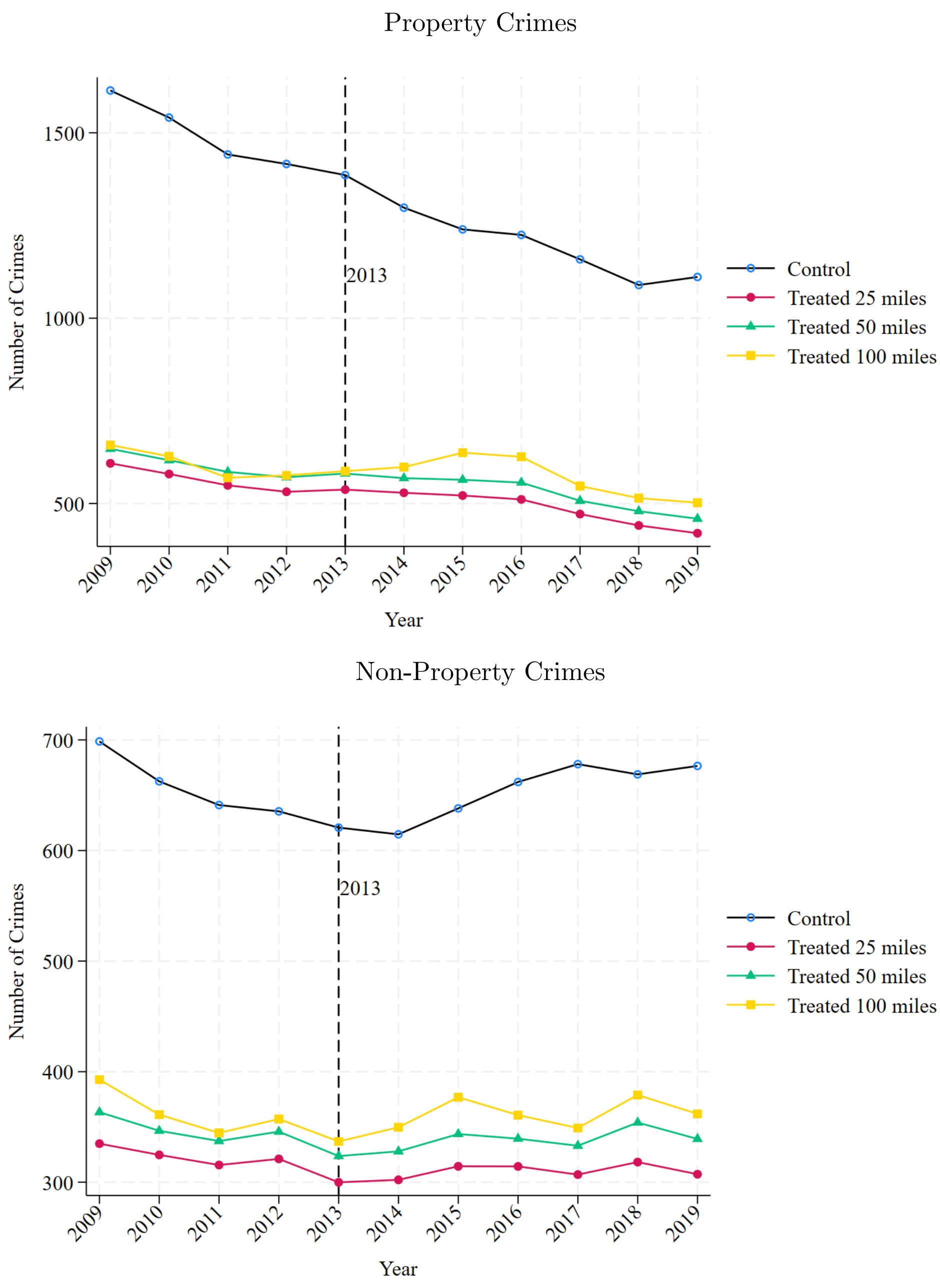
Yang, Crystal S. 2017. "Local labor markets and criminal recidivism." *Journal of Public Economics* 147:16–29.

Figure 1. Variation in driving distance to the nearest abortion facility



Notes Panel A illustrates the trend in travel distance to the nearest abortion facility faced by the average Texas resident. This is produced by calculating population-weighted average county travel distances on the first day of each quarter from 2009 through 2019. Panel B shows the change in travel distance between July 1, 2013 and July 1, 2014, illustrating variation across counties generated by facility closures in response to enforcement of the admitting privileges requirement of Texas HB-2 beginning on November 1, 2013. *Data:* Distances obtained from the Myers Abortion Facility Database (Myers, 2025b) and populations from (Surveillance, Epidemiology, and End Results Program (SEER), 2023).

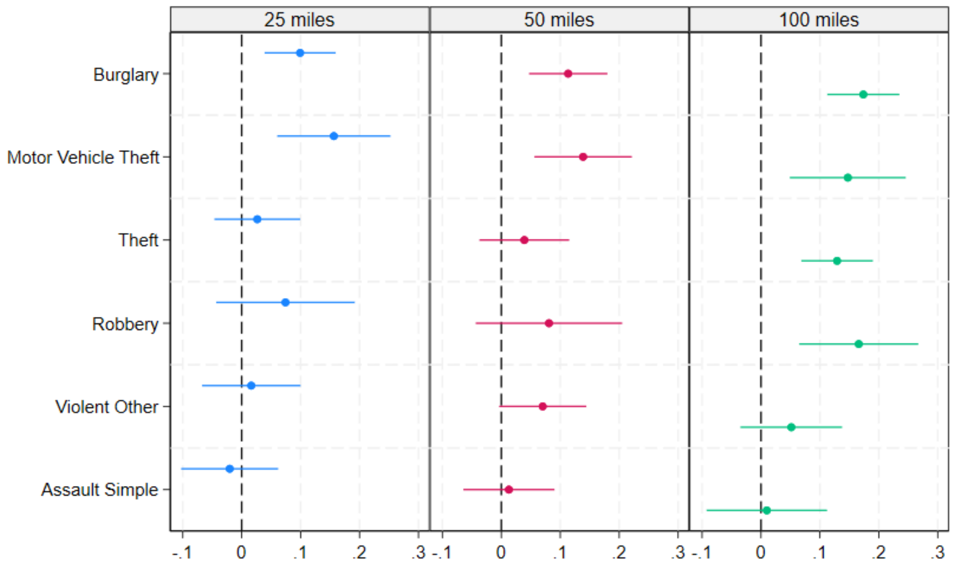
Figure 2. Average Number of Reported Crimes per Agency by Treatment Status, 2009–2019



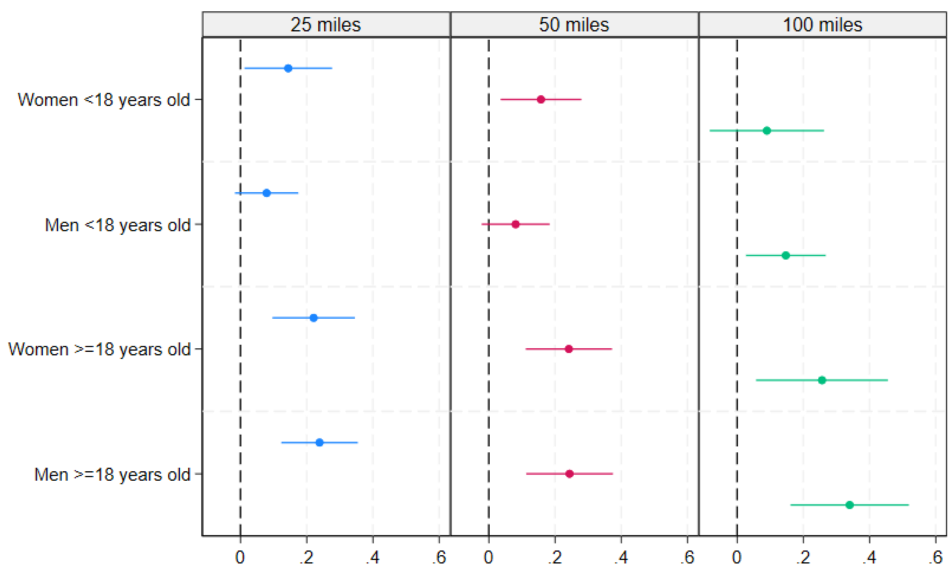
Notes: These figures show trends in the number of reported crimes at the agency level from 2009 to 2019. The top panel displays property crimes—burglary, larceny-theft, and motor vehicle theft—while the bottom panel shows non-property crimes, including murder, forcible rape, robbery, aggravated assault, and simple assault. *Data:* Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019 (Kaplan, 2021). Distances obtained from Myers (2025b).

Figure 3. Impact of Abortion Provider Distance on Crime by Type and Offender Characteristics

A. Different Crime Types



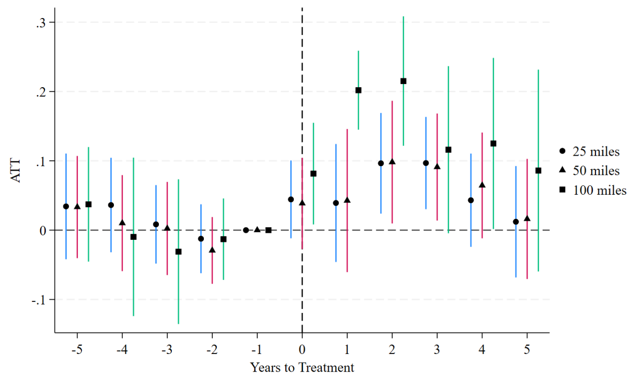
B. Offender's Gender and Age



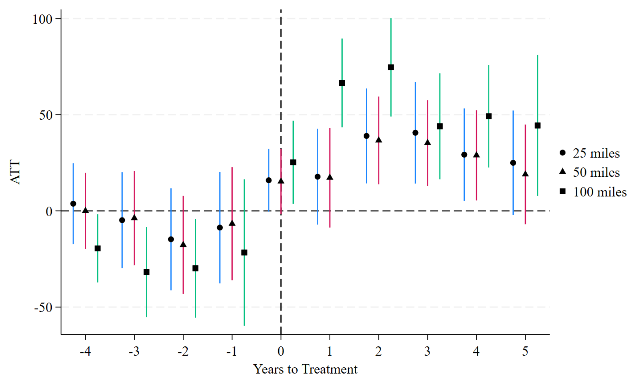
Notes: This figure presents heterogeneity in treatment effects by crime type (Panel A) and gender-by-age subgroups among property crime offenders (Panel B). Each point represents an estimate from the Poisson estimator (1) with the full set of covariates. The 95% confidence intervals are shown alongside the point estimates. The columns in each panel correspond to different definitions of the treatment group (e.g., $d_1 = 25$, 50, and 100 miles).

Figure 4. Dynamic Impact of Abortion Provider Distance on Property Crime

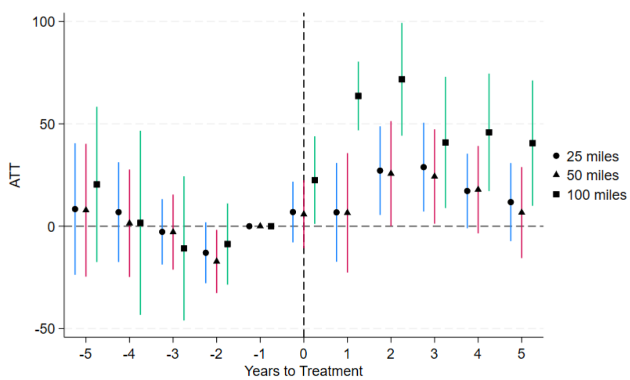
A. Poisson Estimates



B. BJS Estimates



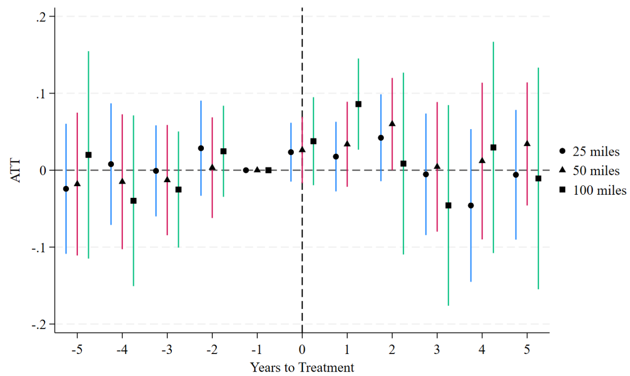
C. S&A Estimates



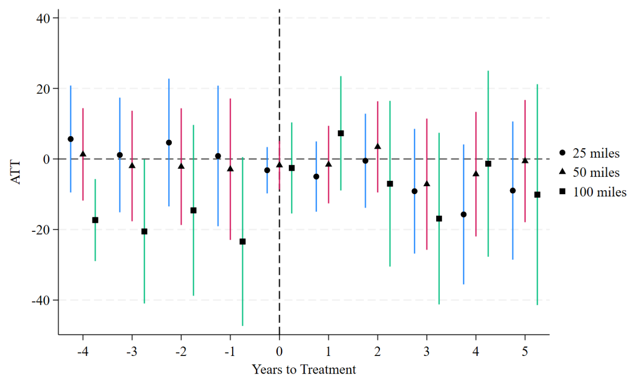
Notes: This figure shows event-study estimates of the effect of abortion provider distance on property crime rates (per 10,000 population) using a Poisson model (Panel A) and two heterogeneity-robust DID estimators: Borusyak, Jaravel, and Spiess (2024) (BJS, Panel B) and Sun and Abraham (2021) (S&A, Panel C). All models include the controls from Equation 1 and are weighted by agency-covered population. The Poisson and S&A estimators use the year before treatment as the base period, while the BJS estimator averages across all pre-treatment periods. Treatment effects are shown for three distance thresholds: 25, 50, and 100 miles. Points denote estimated ATTs and vertical bars 95% confidence intervals.

Figure 5. Dynamic Impact of Abortion Provider Distance on Non-Property Crime

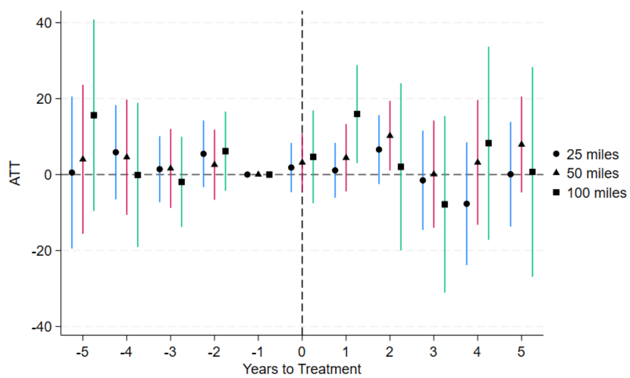
A. Poisson Estimates



B. BJS Estimates



C. S&A Estimates



Notes: This figure shows event-study estimates of the effect of abortion provider distance on non-property crime rates (per 10,000 population) using a Poisson model (Panel A) and two heterogeneity-robust DID estimators: Borusyak, Jaravel, and Spiess (2024) (BJS, Panel B) and Sun and Abraham (2021) (S&A, Panel C). All models include the controls from Equation 1 and are weighted by agency-covered population. The Poisson and S&A estimators use the year before treatment as the base period, while the BJS estimator averages across all pre-treatment periods. Treatment effects are shown for three distance thresholds: 25, 50, and 100 miles. Points denote estimated ATTs and vertical bars 95% confidence intervals.

Table 1. Impact of Abortion Provider Distance on Crime, Baseline

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.094***	0.044	0.113***	0.058*	0.186***	0.139***
	(0.033)	(0.031)	(0.034)	(0.032)	(0.028)	(0.027)
AME	102.574	48.321	128.433	66.630	226.427	169.678
AME S.E.	35.450	33.675	38.940	36.251	34.248	33.188
N	7,953	7,953	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	4492.185	4492.185	5056.232	5056.232	4940.145	4940.145
Panel B. Non-Property Crime						
Treatment	-0.008	-0.010	0.014	0.027	0.015	0.024
	(0.037)	(0.038)	(0.036)	(0.034)	(0.047)	(0.046)
AME	-4.682	-5.397	8.032	15.455	8.942	14.862
AME S.E.	20.400	21.075	20.914	19.794	28.898	28.092
N	7,942	7,942	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	2557.761	2557.761	2873.337	2873.337	2808.166	2808.166
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1). The treatment is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic in two consecutive years, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. The control group is composed of counties that never exceed a basleine distance threshold of 25 miles (ΔD_{ct} < 25 miles). For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table 2. Impact of Abortion Provider Distance on Labor Market Outcomes

	Treated group defined by travel distance ever increases by						
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles		
	(1)	(2)	(3)	(4)	(5)	(6)	
Panel A. Number of Individuals in the Labor Force							
Treatment	-0.026***	-0.021***	-0.025***	-0.019***	-0.015**	-0.011**	Poisson GLM, MLE
	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	
AME	-1432.314	-1132.635	-1501.780	-1145.385	-985.897	-715.551	
AME S.E.	222.542	194.203	274.940	231.929	413.886	361.568	
N	2,585	2,585	2,321	2,321	1,991	1,991	
$E[Y \mid Pre, Treat]$	80886.413	80886.413	79459.708	79459.708	67788.625	67788.625	
Panel B. Unemployment Rate							
Treatment	0.455***	0.240***	0.438***	0.206**	0.838***	0.512***	Linear OLS
	(0.097)	(0.093)	(0.110)	(0.103)	(0.117)	(0.120)	
N	2,585	2,585	2,321	2,321	1,991	1,991	
$E[Y \mid Pre, Treat]$	7.175	7.175	7.174	7.174	5.798	5.798	
County Demographics		✓		✓		✓	
County FE	✓	✓	✓	✓	✓	✓	
Year FE	✓	✓	✓	✓	✓	✓	

Notes: This table presents estimates from the Poisson model (if the outcome is a count variable; using county population as exposure) and the linear probability model (if the outcome is binary; weighted by county population), both of which share the same control variables as in (1). The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Data on the county-level labor force size and unemployment rate are sourced from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) 2009-2019.

Table 3. Impact of Abortion Provider Distance on Financial Outcomes

	Treated group defined by travel distance ever increases by						
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles		
	(1)	(2)	(3)	(4)	(5)	(6)	
Panel A. Personal Income (per capita)							
Treatment	-689.209	-607.055	-326.330	-208.224	-295.821	-883.394	Linear OLS
	(541.116)	(397.130)	(622.045)	(431.943)	(1085.614)	(820.391)	
N	2,585	2,585	2,321	2,321	1991	1991	
$E[Y\|Pre, Treat]$	36625.742	36625.742	37062.667	37062.667	40974.503	40974.503	
Panel B. Personal Income Interquartile Range (IQR)							
Treatment	5553.123***	5686.123***	5929.411***	5709.051***	5104.183***	5569.692***	Linear OLS
	(776.663)	(774.438)	(912.033)	(878.451)	(916.744)	(770.114)	
N	769,964	769,964	751,273	751,273	721,985	721,985	
$E[Y\|Pre, Treat]$	32963.716	32963.716	33024.777	33024.777	33090.275	33090.275	
Panel C. Debt-to-Income Ratio							
Treatment	0.083***	0.031	0.086***	0.013	0.140***	0.081**	Linear OLS
	(0.023)	(0.027)	(0.024)	(0.029)	(0.026)	(0.036)	
N	2,435	2,435	2,179	2,179	1,865	1,865	
$E[Y\|Pre, Treat]$	1.329	1.329	1.228	1.228	1.065	1.065	
Panel D. Mortgage Delinquency Rate ($\geq$ 90 days)							
Treatment	0.625***	0.326*	0.485***	0.259**	0.625***	0.326*	Linear OLS
	(0.155)	(0.190)	(0.097)	(0.119)	(0.155)	(0.190)	
N	275	275	308	308	275	275	
$E[Y\|Pre, Treat]$	2.063	2.063	4.463	2.505	4.046	2.063	
Panel E. Number of Evictions							
Treatment	0.168***	0.082**	0.191***	0.079*	0.186***	0.086**	Poisson GLM, MLE
	(0.027)	(0.033)	(0.033)	(0.040)	(0.027)	(0.039)	
AME	176.459	85.675	217.351	89.483	238.612	109.923	
AME S.E.	28.475	35.060	37.593	45.869	35.230	49.392	
N	2,167	2,167	1,942	1,942	1,666	1,666	
$E[Y\|Pre, Treat]$	1452.165	1452.165	1193.957	1193.957	957.088	957.088	
Demographic Controls		✓		✓		✓	
County FE	✓	✓	✓	✓	✓	✓	
Year FE	✓	✓	✓	✓	✓	✓	

Notes: This table presents estimates from the Poisson model (if the outcome is a count variable; using county population as exposure) and the linear probability model (if the outcome is binary; weighted by county population), both of which share the same control variables as in (1). The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: County-level personal income data in Panel A are sourced from the Bureau of Economic Analysis (BEA) for the period 2009–2019. Individual-level income data in Panel B come from the American Community Survey (ACS) for the same years. All income measures in Panels A and B are expressed in 2010 US dollars. County-level debt-to-income ratio data are obtained from the Federal Reserve's Enhanced Financial Accounts (EFA), covering the years 2009–2019. Mortgage delinquency rate data at the county level come from the Consumer Financial Protection Bureau (CFPB) for the same period. Data on the number of county-level eviction filings (2009–2018) are retrieved from the Eviction Lab at Princeton University.

A Theoretical Model

We develop a rational-choice model to understand how abortion access affects economic hardship and financially motivated criminal behavior. The model is grounded in the theory of crime developed by Becker (1968), which assumes that individuals weigh the expected utility of criminal activity against legal alternatives.

In this model, we define D as the distance to the nearest abortion provider. Let $A(D)$ be the probability of accessing abortion and $U(D) = 1 - A(D)$ be the probability of "unintended" or "unwanted" childbirth. Economic hardship resulting from unintended childbirth is $H(D) = H(1 - A(D))$. Monetary gain from financially motivated crime increases in hardship and is defined as $G(H)$. We assume individuals act as rational agents maximizing their expected utility by comparing outcomes across available choices. Therefore, the expected utility from crime is:

$$U^C(D) = (1 - p)G(H(D)) - pF - \theta, \tag{3}$$

where p is the probability of apprehension for committing a crime, the severity of punishment is F, and the moral cost of committing a crime is θ.[31] The utility from legal labor market opportunities is:

$$U^L = w, \tag{4}$$

such that individuals choose to commit a financially motivated crime if

$$U^C(D) > U^L \text{ or } U^C(D) > w. \tag{5}$$

There are a few additional technical assumptions underpinning our model.

[31]The parameter θ captures internal or socialized deterrents to crime, such as personal ethical beliefs, social stigma, or fear of reputational consequences.

Assumption 1. Abortion access $A(D)$ is continuously differentiable and strictly decreasing in distance D, i.e., $A'(D) < 0$.

This assumption that abortion access declines as distance increases is empirically supported by multiple studies, including Lindo et al. (2020a); Myers (2024); Aslim, Fu, and Tekin (2024). While our analysis does not rely on second-order derivatives (since we do not seek to characterize an optimal policy), one could, without loss of generality, assume diminishing marginal effects of distance; that is, access is twice continuously differentiable and concave in distance ($A''(D) > 0$). The same idea applies to the assumptions below.

Assumption 2. Economic hardship is continuously differentiable and strictly increasing in unintended childbirths U, i.e., $H'(U) > 0$.

Assumption 3. Criminal gain from financially motivated crime $G(H)$ is continuously differentiable and strictly increasing in hardship H, i.e., $G'(H) > 0$.

Given these assumptions, we examine how the distance to the nearest abortion provider (D) shapes economic hardship resulting from unintended childbirths ($H(U)$), and how this, in turn, affects the expected utility from committing financially motivated crimes.

Proposition 1. *Economic hardship increases with distance to the nearest abortion provider.*

Proof. By the chain rule: $\frac{dH}{dD} = H'(U)(-A'(D)) > 0$. ■

Proposition 2. *Expected utility from financially motivated crime increases with distance to the nearest abortion provider.*

Proof. Substitute $H(D) = H(1 - A(D))$ into the utility function $U^C(D) = (1-p)G(H(1 - A(D))) - pF - \theta$. Differentiate with respect to D:

$$\frac{dU^C}{dD} = (1-p)G'(H(U))H'(U)(-A'(D)) > 0. \tag{6}$$

Since all components are strictly positive (by assumptions 1 - 3), the utility from crime increases with distance. Legal utility is fixed, so the net gain from crime increases with D. ■

We demonstrate this idea in Figure A.1. Specifically, we assume a stylized, concave-increasing utility function of the form $U^C(D) = 3 + 3(1 - e^{-0.04D})$, represented by the solid line. This functional form captures diminishing marginal gains from distance-induced hardship. We set the utility from legal work to a constant value of $w = 5$, shown by the dashed line. The parameters used in this functional form are illustrative and intended to capture the theoretical shape of the relationship, rather than reflect empirically estimated values. Solving for $U^C(D) > U^L$ yields a threshold distance of approximately 27.5 miles. The shaded region indicates where crime becomes the utility-maximizing choice. This stylized example highlights how even modest increases in access barriers can shift the relative returns to illegal activity, especially for individuals facing tight economic constraints.

A.1 Crime-Type Selection and Detection Risk

We extend the Beckerian framework by modeling an offender's decision over multiple types of financially motivated crimes that differ in detection risk and monetary reward. The central tradeoff involves selecting a crime type that balances potential gains against the probability of apprehension and punishment severity.

Let $j \in \mathcal{J}$ index the set of possible financially motivated crimes. The offender chooses the crime type j^* that maximizes expected utility:

$$U^C(D)_j = (1 - p_j)G_j(H(D)) - p_j F_j - \theta_j, \tag{7}$$

subject to the participation constraint:

$$U_j^C > w. \tag{8}$$

Therefore, the decision rule is:

$$j^* = \arg\max_{j \in \mathcal{J}} \left\{ U_j^C \right\}, \quad \text{only if } \max_j U_j^C > w. \tag{9}$$

Proposition 3. *Among two crime types j and k, if $p_j > p_k$ and $G_j(H) > G_k(H)$, then there exists a threshold level of hardship H^* above which crime j yields higher expected utility than crime k.*

Proof. The difference in utility between crimes j and k is:

$$\Delta U(H) = U_j^C(H) - U_k^C(H) = (1 - p_j)G_j(H) - p_j F_j - \theta_j - [(1 - p_k)G_k(H) - p_k F_k - \theta_k]$$

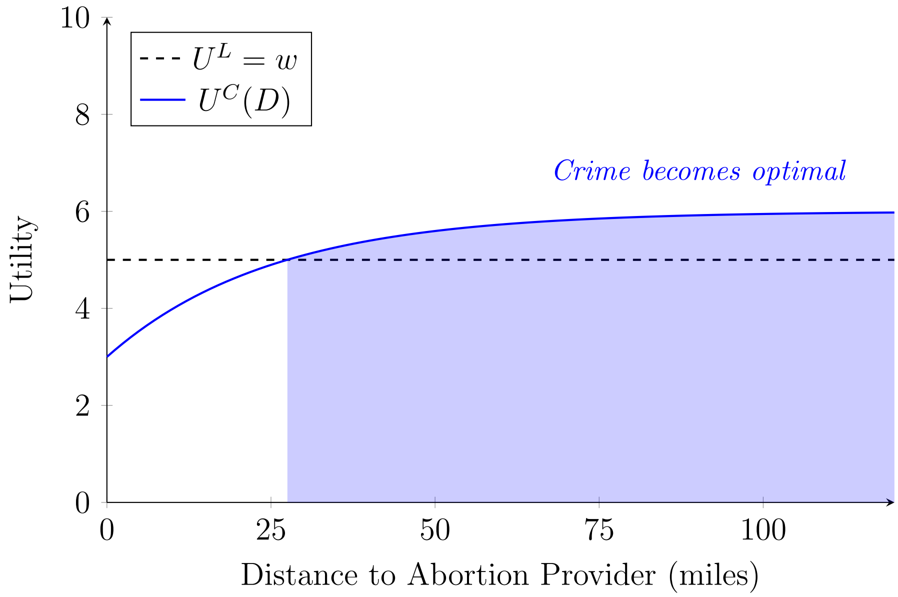
Assume $G_j'(H) > G_k'(H)$ and that $\Delta U(H)$ is continuous in H.

For low values of H, if the detection risk for j is sufficiently high and $G_j(H)$ not large enough, we may have $\Delta U(H) < 0$. However, as H increases, the gain $G_j(H)$ increases faster than $G_k(H)$, implying that $\Delta U(H)$ will eventually cross zero and become positive.

Hence, by the Intermediate Value Theorem, there exists $H^* > 0$ such that $\Delta U(H^*) = 0$, and for $H > H^*$, $U_j^C(H) > U_k^C(H)$. ■

This result implies that as economic hardship increases, offenders are more likely to choose crime types with greater financial returns, even if those crimes involve higher detection risks. The selection margin thus shifts toward high-gain, high-risk offenses under greater financial pressure.

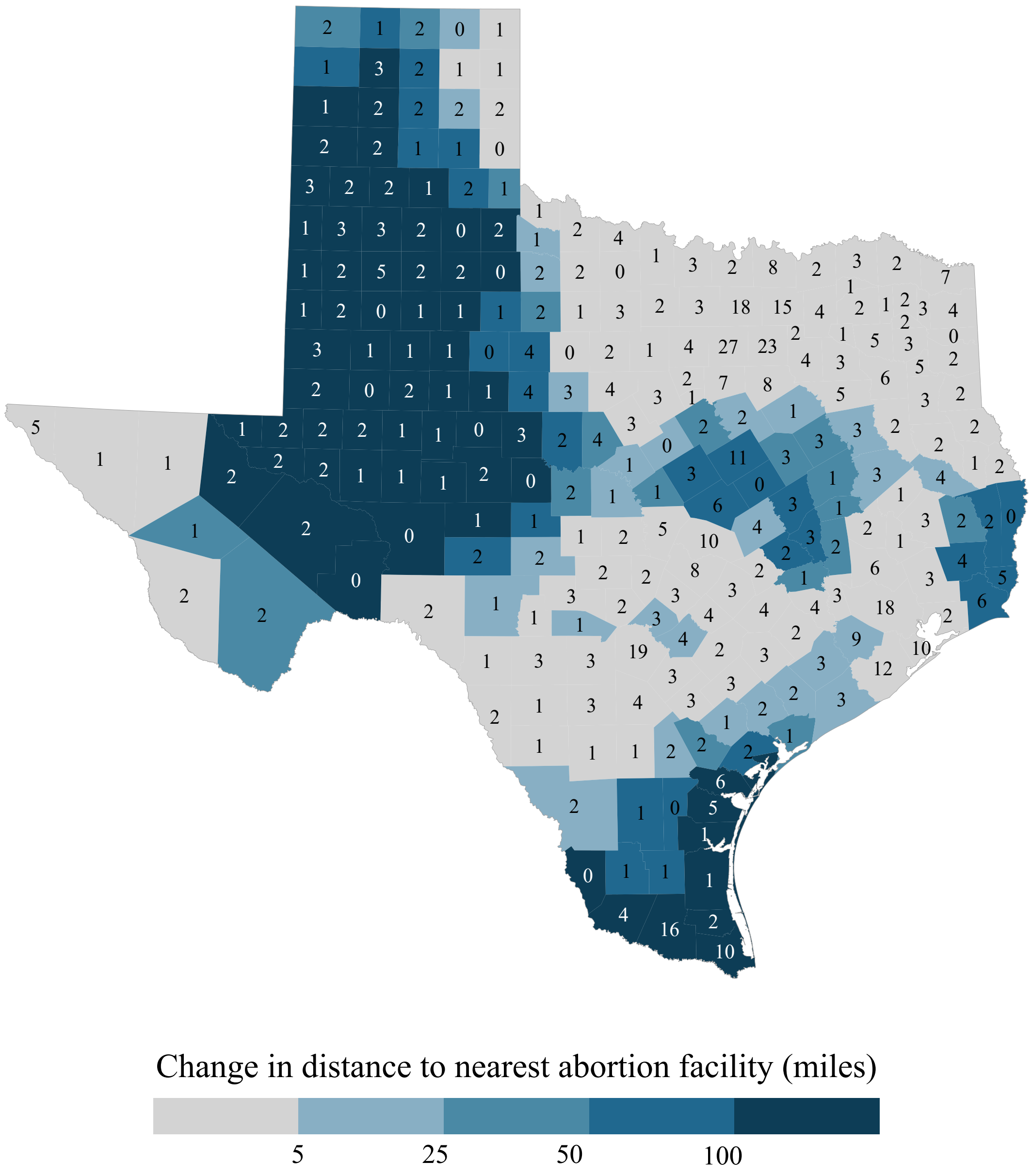
Figure A.1. Expected Utility from Crime and Legal Work as a Function of Distance (miles)



Notes: We assume a stylized utility function of the form $U^C(D) = 3+3(1-e^{-0.04D})$, represented by the solid line, and fix $U^L = w = 5$, shown as the dashed line. This implies that crime becomes the utility-maximizing choice when distance exceeds approximately 27.5 miles. The shaded region indicates where crime yields higher utility than legal alternatives.

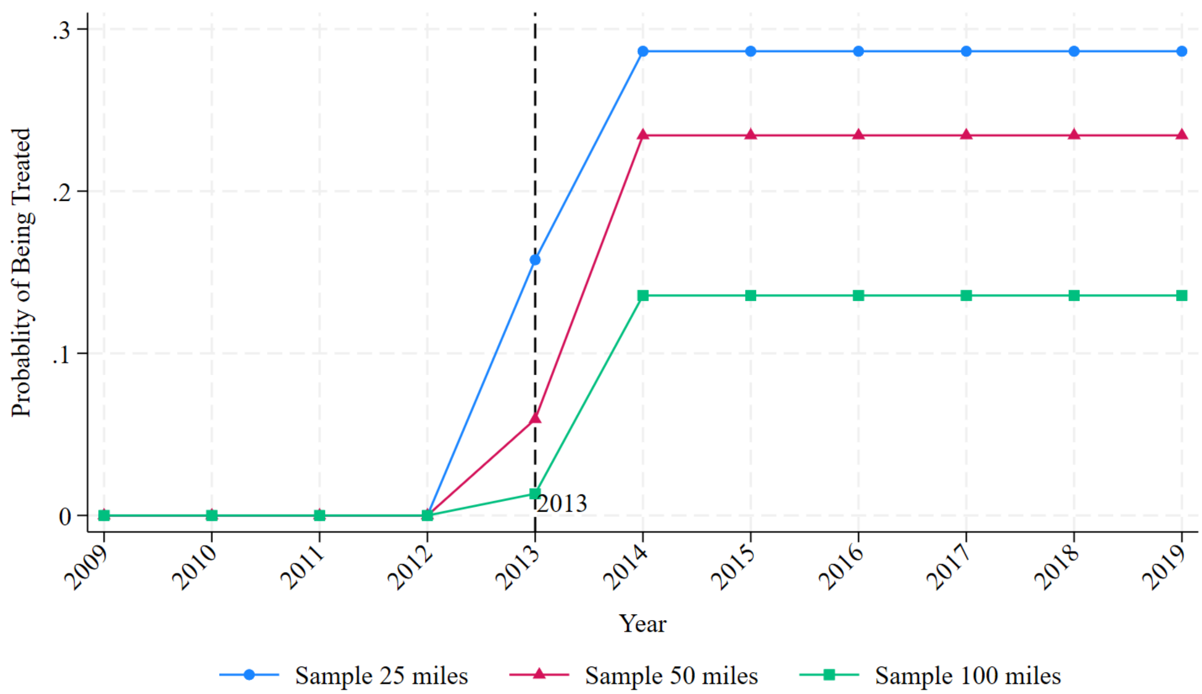
B Additional Figures and Tables

Figure B.1. Number of continuously reporting agencies and distance changes by county



Notes: Numbers overlaid on each county are the number of agencies in our balanced sample of continuously reporting agencies over the analysis period of 2009 to 2019. Each county is shaded by the change in distance between July 1, 2013 and July 1, 2014, which is identical to the variation presented in Figure 1.

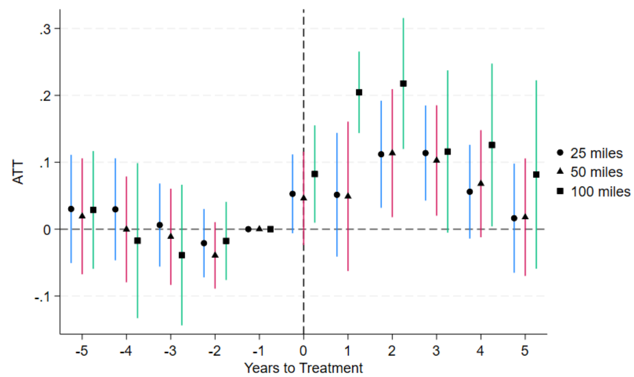
Figure B.2. Probability of Treatment Across Distance Cutoffs Cutoffs



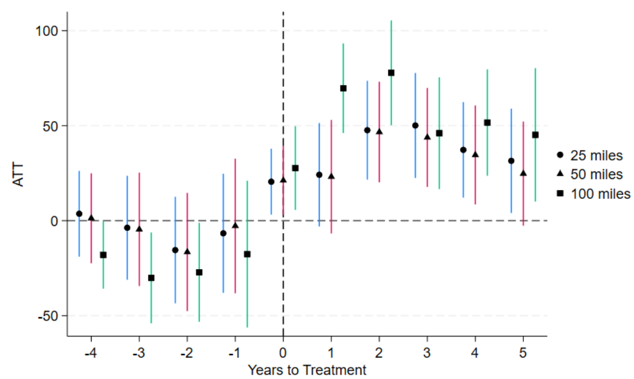
Notes: These figures illustrate trends in average distance levels and treatment probability over time from 2009 to 2019. The top figure presents the evolution of average travel distance ($\overline{D}_t$) to the nearest abortion facility, highlighting the level changes across years. The bottom figure depicts the probability of being treated as a function of changes in travel distance ($f(\Delta D_t)$) across different cutoffs, d^1 (e.g., $\geq$25, $\geq$50, and $\geq$100 miles). For instance, a county experiencing a 100-mile increase in travel distance is assigned to treatment in all three samples, whereas a 25-mile increase is captured only in the first sample. *Data:* Travel distance data are obtained from the Myers Abortion Facility Database 2009-2019.

Figure B.3. Dynamic Impact of Abortion Provider Distance on Property Crime, Excluding Temporary Closure Counties

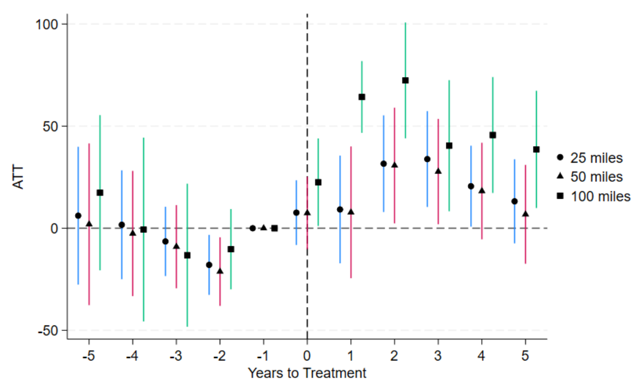
A. Poisson Estimates



B. BJS Estimates



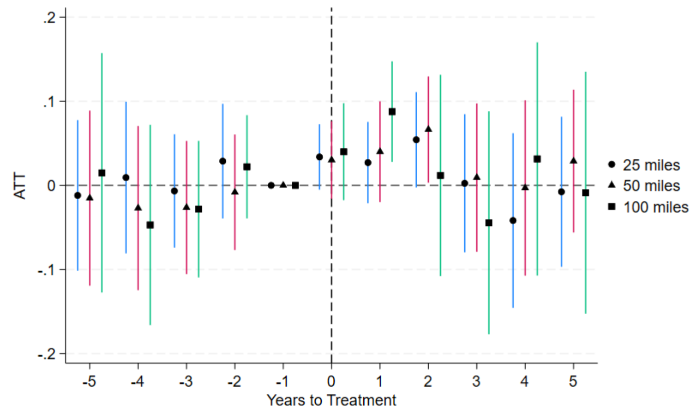
C. S&A Estimates



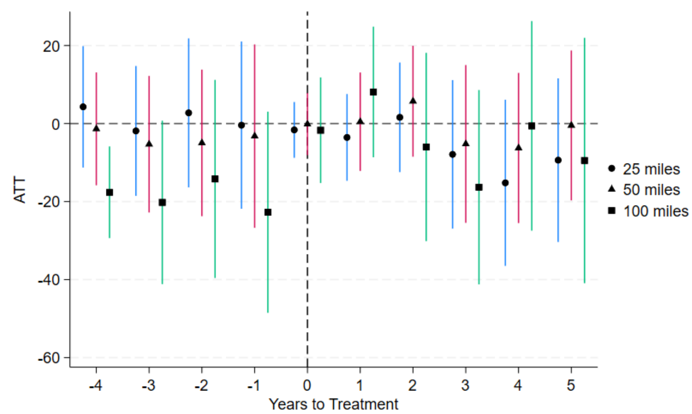
Notes: Alternative estimates the specifications in Figure 4 using an alternative sample that excludes counties affected by temporary abortion clinic closures in McAllen and Waco.

Figure B.4. Dynamic Impact of Abortion Provider Distance on Non-Property Crime, Excluding Temporary Closure Counties

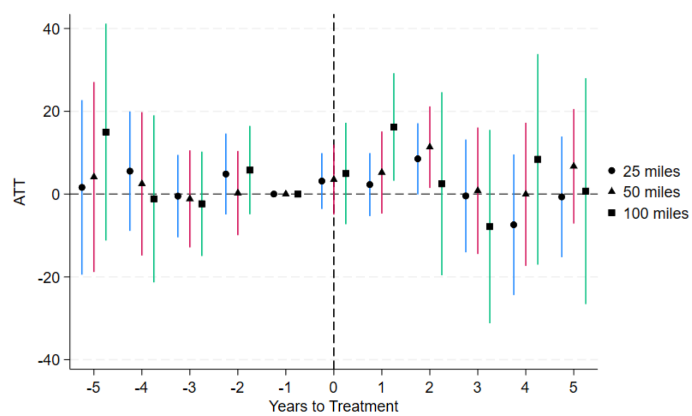
B. Poisson Estimates



A. BJS Estimates



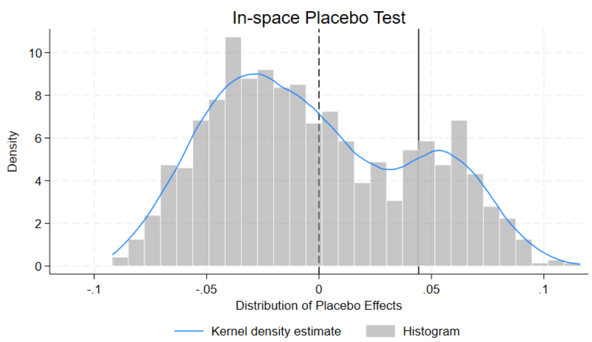
B. S&A Estimates



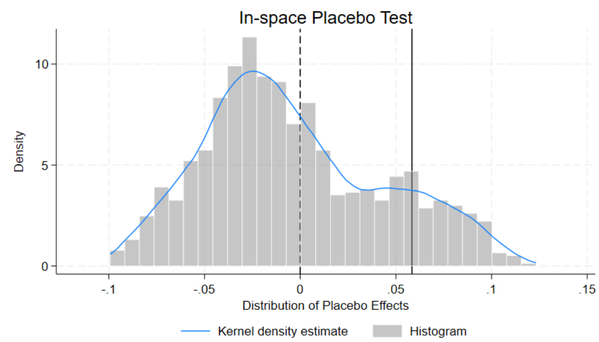
Notes: Alternative estimates the specifications in Figure 5 using an alternative sample that excludes counties affected by temporary abortion clinic closures in McAllen and Waco.

Figure B.5. Randomization Inference, Property Crimes

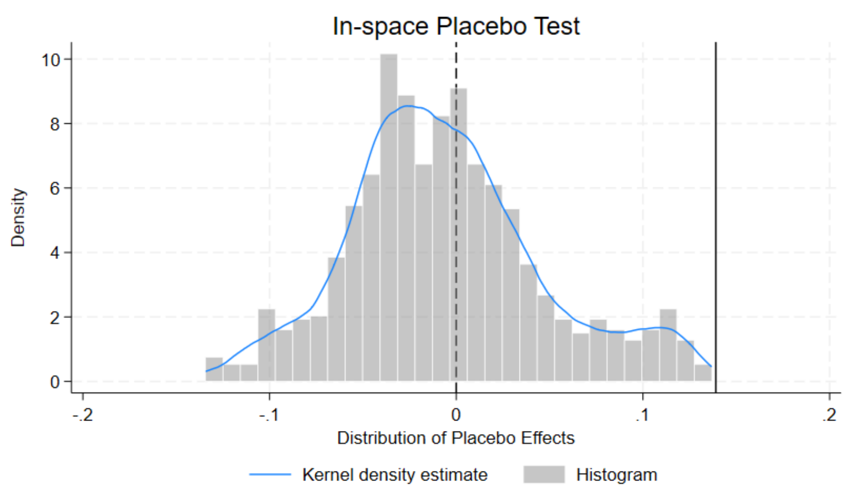
Panel A. ≥ 25 miles



Panel B. ≥ 50 miles



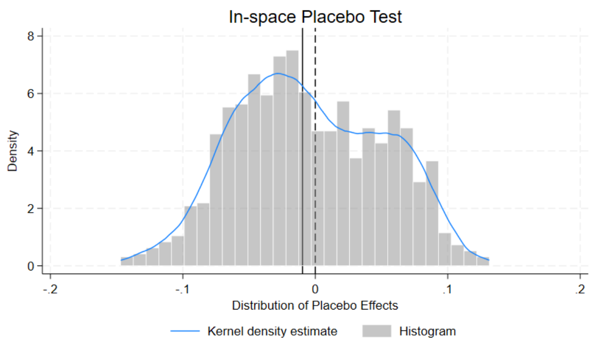
Panel C. ≥ 100 miles



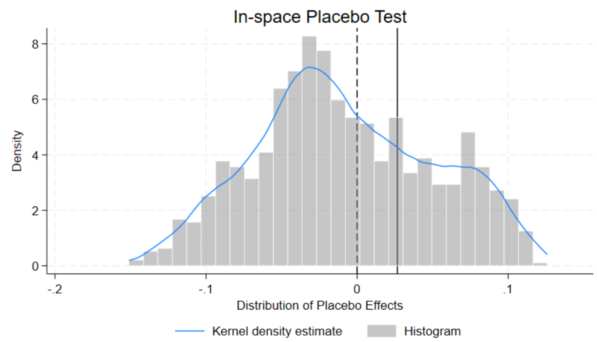
Notes: This figure displays the distribution of placebo treatment effect estimates for property crimes, generated from 999 repetitions of the baseline Poisson model with randomly assigned treatment units. The solid vertical line in each panel represents the baseline estimate using the actual treatment assignment. Treatment effects are shown for three binary treatment definitions based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (Panel A), 50 miles (Panel B), or 100 miles (Panel C) during the study period. The corresponding Fisher p-values for Panels A, B, and C are 0.21, 0.13, and <0.01, respectively. *Data:* Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Figure B.6. Randomization Inference, Non-Property Crimes

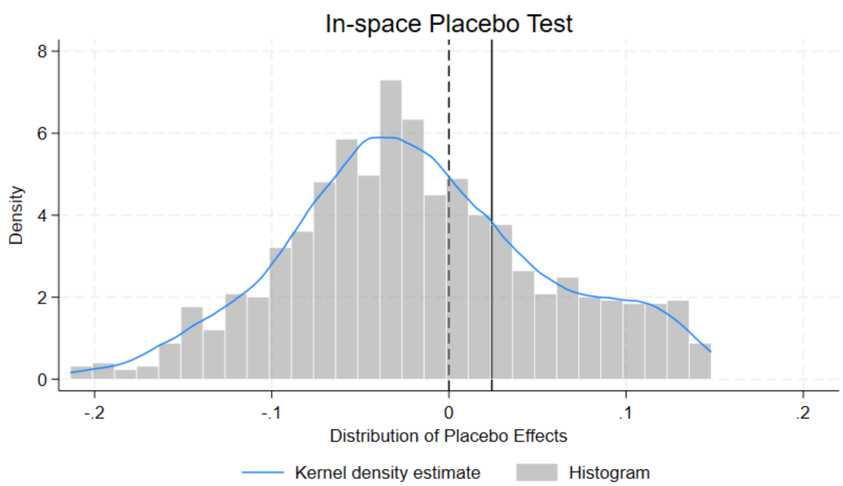
Panel A. $\geq$ 25 miles



Panel B. $\geq$ 50 miles



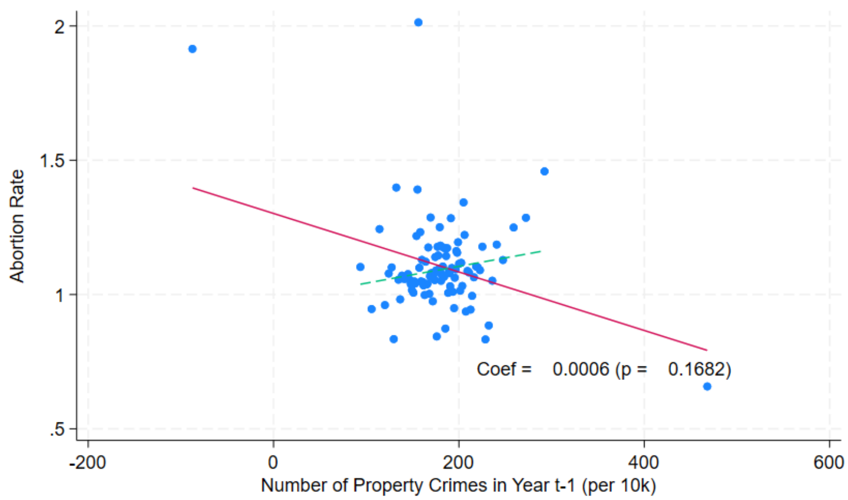
Panel C. $\geq$ 100 miles



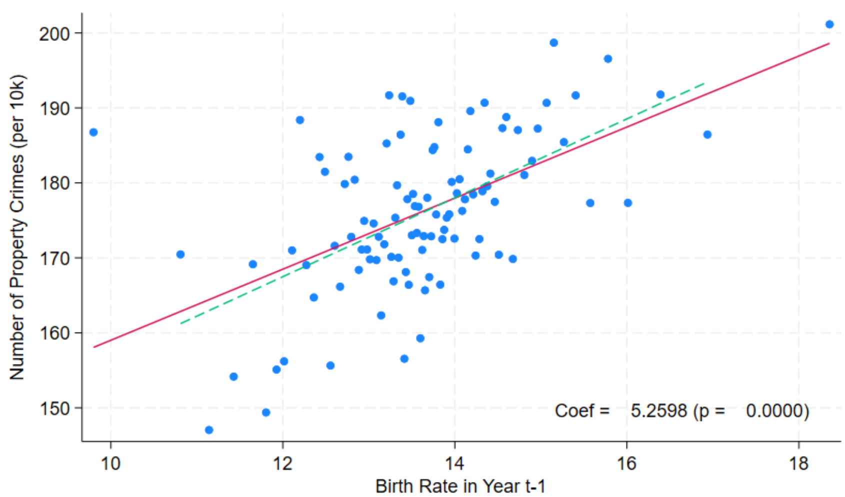
Notes: This figure displays the distribution of placebo treatment effect estimates for non-property crimes, generated from 999 repetitions of the baseline Poisson model with randomly assigned treatment units. The solid vertical line in each panel represents the baseline estimate using the actual treatment assignment. Treatment effects are shown for three binary treatment definitions based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (Panel A), 50 miles (Panel B), or 100 miles (Panel C) during the study period. The corresponding Fisher p-values for Panels A, B, and C are 0.50, 0.29, and 0.26, respectively. *Data:* Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Figure B.7. Descriptive Relationships Between Abortion Rates, Birth Rates, and Crime

Panel A. Abortion against Property Crime (t-1)



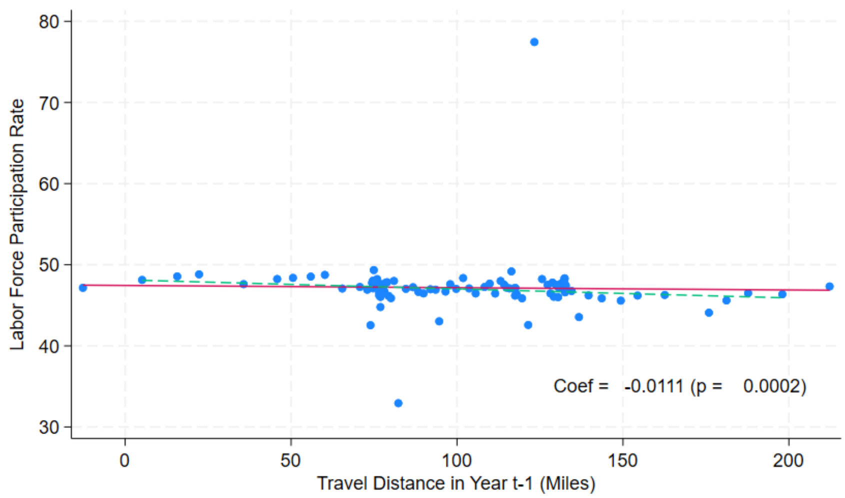
Panel B. Property Crime against Birth (t-1)



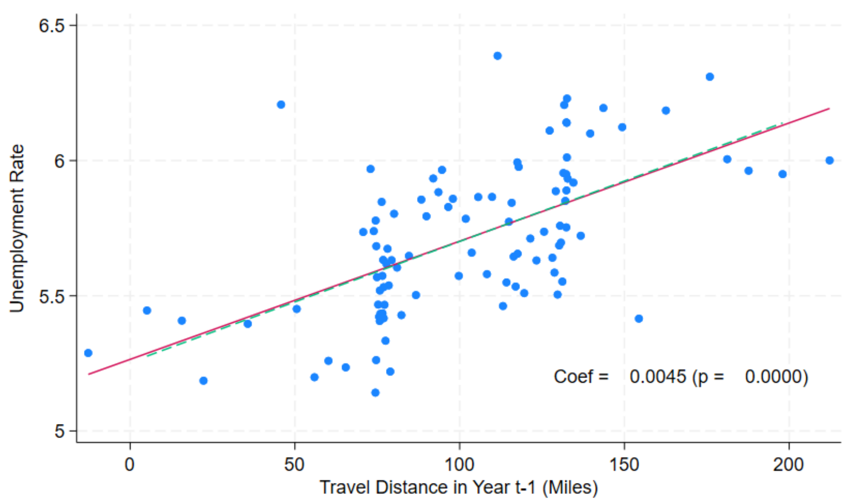
Notes: This figure presents bin-scatter plots illustrating the relationships among abortion, birth, and crime rates at the county level. Each point represents the average value of the y-axis variable within a percentile bin of the x-axis variable. Both variables are residualized using county and year fixed effects. The red solid line represents a linear fit using all data points, while the green dashed line represents a linear fit excluding outliers—observations in the top or bottom 1% of either variable's distribution. The slope and corresponding p-value for the green dashed line are reported in the figure.

Figure B.8. Descriptive Relationships Between Labor Market Outcomes and Abortion Provider Distance

Panel A. Labor Force Participation Rate



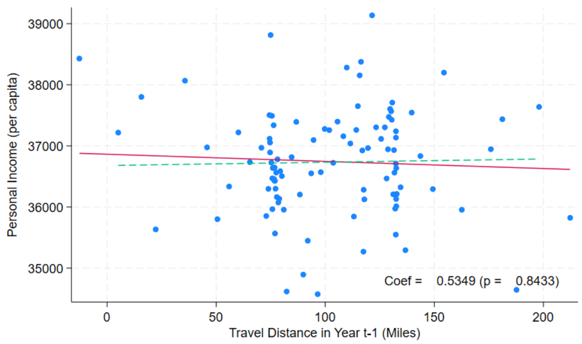
Panel B. Unemployment Rate



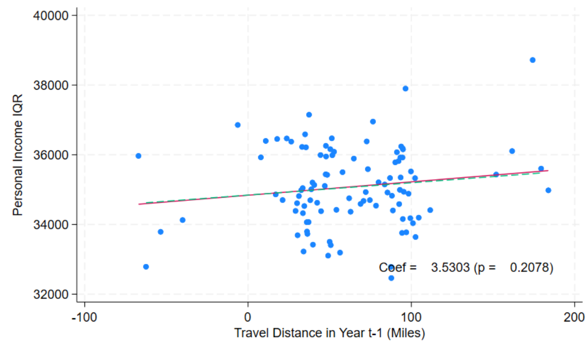
Notes: This figure presents bin-scatter plots illustrating the relationship between the labor market outcomes in a county in year t and the county's travel distance to the nearest abortion clinic in year $t-1$. Each point represents the average value of the y-axis variable within a percentile bin of the x-axis variable. Both variables are residualized using county and year fixed effects. The red solid line represents a linear fit using all data points, while the green dashed line represents a linear fit excluding outliers—observations in the top or bottom 1% of either variable's distribution. The slope and corresponding p-value for the green dashed line are reported in the figure.

Figure B.9. Descriptive Relationships Between Financial Outcomes and Abortion Provider Distance

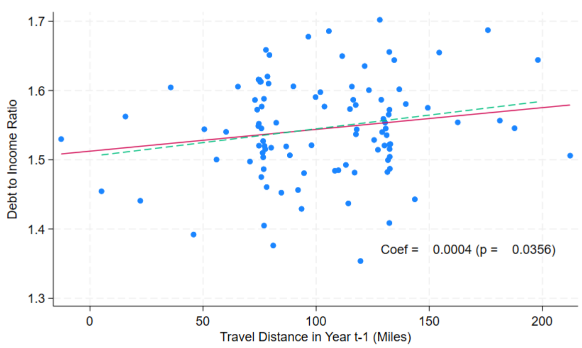
Panel A. Personal Income (per capita)



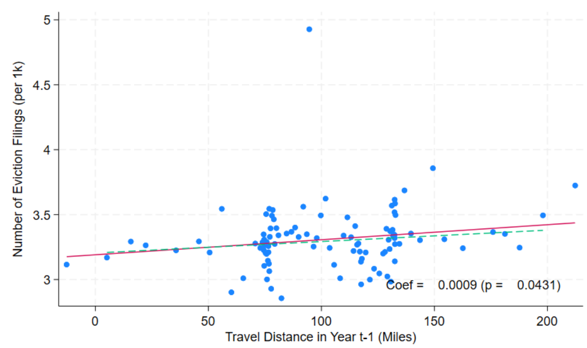
Panel B. Personal Income IQR



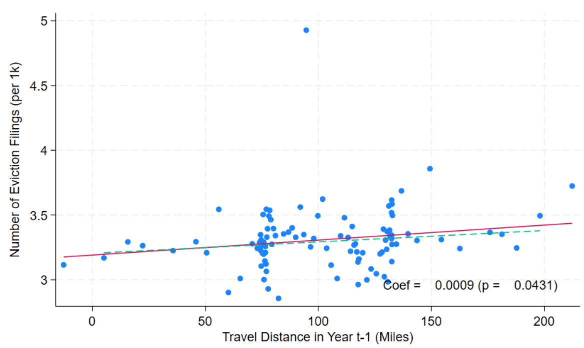
Panel C. Debt-to-Income Ratio



Panel D. Mortgage Delinquency Rate ($\geq$ 90 days)



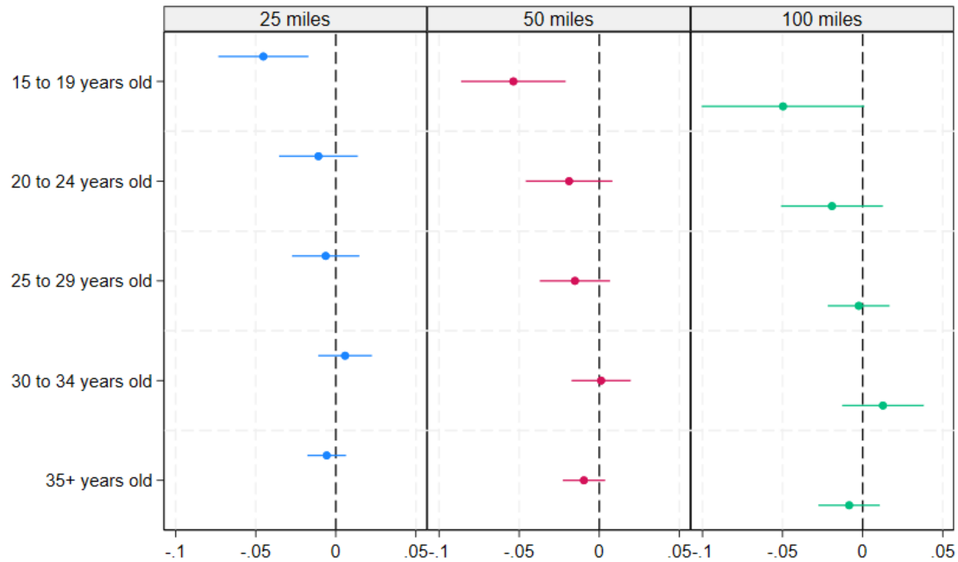
Panel D. Eviction (per 1,000 population)



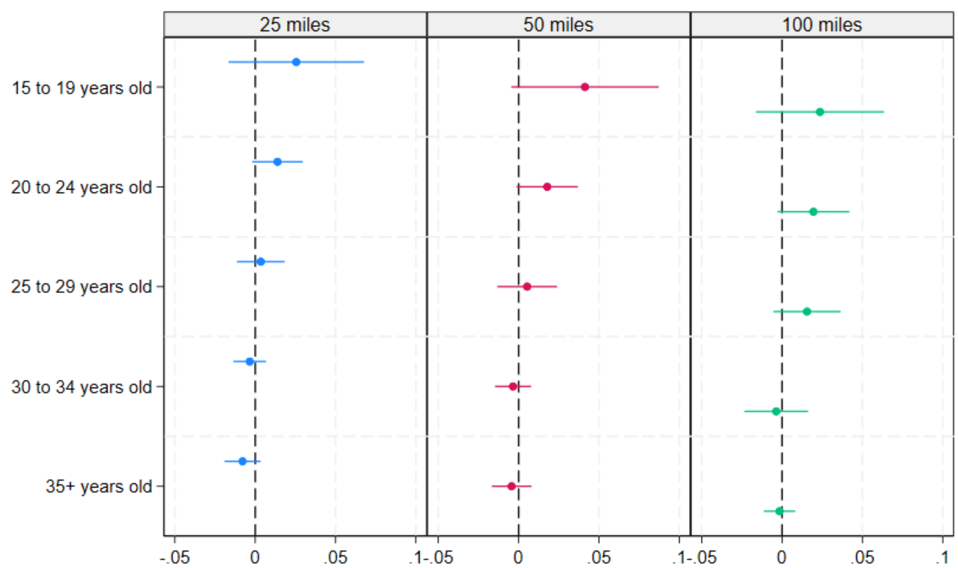
Notes: This figure presents bin-scatter plots illustrating the relationship between the financial outcomes in a county in year t and the county's travel distance to the nearest abortion clinic in year $t-1$. Personal income outcomes in panels A and B are measured in 2010 US dollars. Each point represents the average value of the y-axis variable within a percentile bin of the x-axis variable. Both variables are residualized using county and year fixed effects. The red solid line represents a linear fit using all data points, while the green dashed line represents a linear fit excluding outliers—observations in the top or bottom 1% of either variable's distribution. The slope and corresponding p-value for the green dashed line are reported.

Figure B.10. Impact of Abortion Provider Distance on Labor Market Outcomes by Age

A. Likelihood of Being in the Labor Force



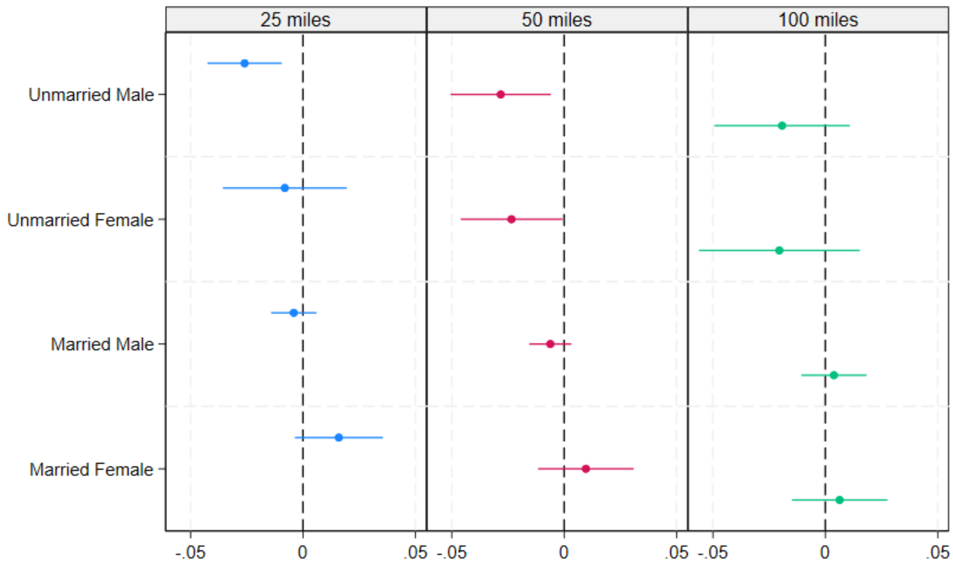
B. Likelihood of Being Unemployed (Conditional on Being in the Labor Force)



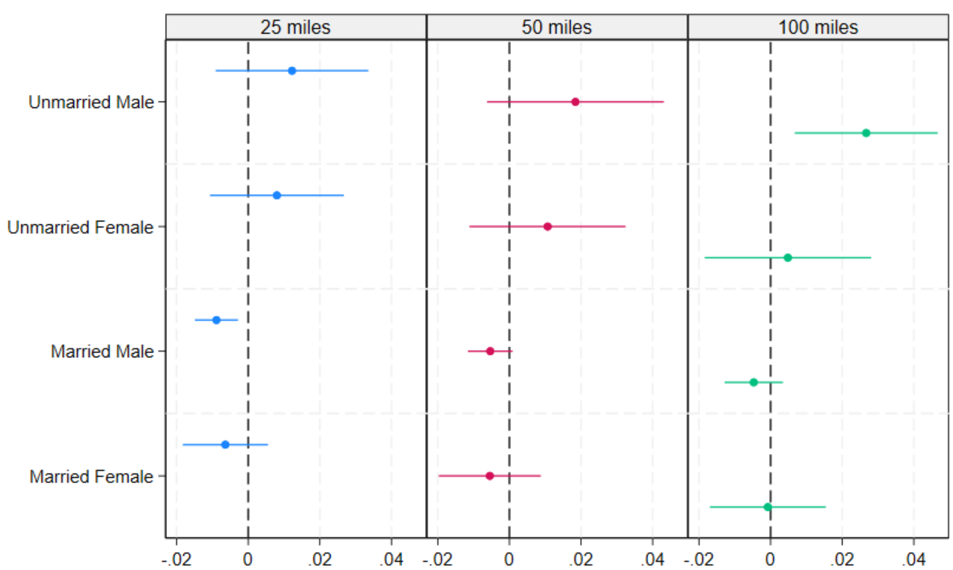
Notes: This figure presents heterogeneity in treatment effects by age subgroups on labor force participation (Panel A) and unemployment (Panel B). Each point represents an estimate from the linear OLS estimator with the same set of covariates as equation (1) and weighted by survey weight. The 95% confidence intervals are shown alongside the point estimates. The columns in each panel correspond to different definitions of the treatment group (e.g., $d_1 = 25$, 50, and 100 miles).
Data: Individual-level labor market survey data are obtained from the American Community Survey (ACS) 2009-2019.

Figure B.11. Impact of Abortion Provider Distance on Labor Market Outcomes by Sex and Marital Status

A. Likelihood of Being in the Labor Force



B. Likelihood of Being Unemployed (Conditional on Being in the Labor Force)

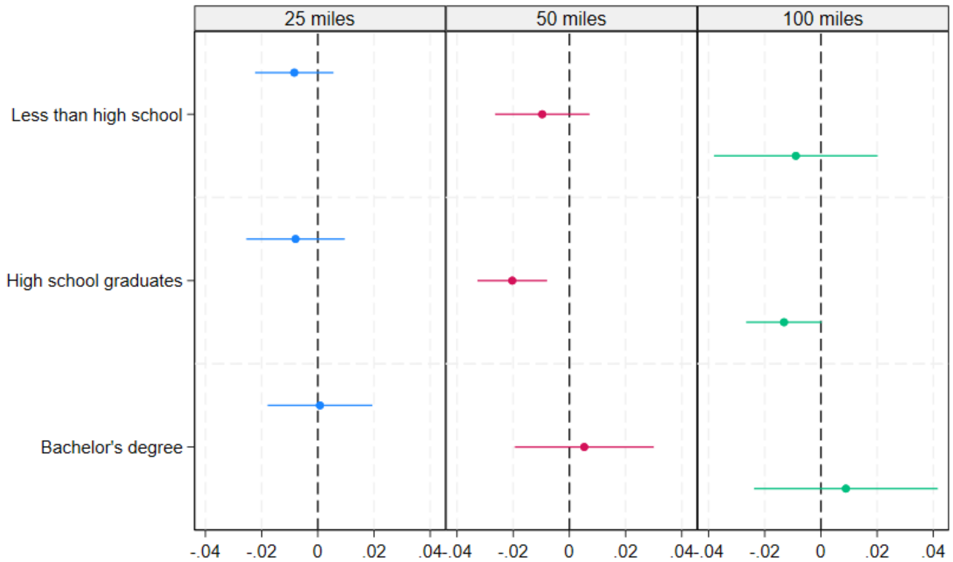


Notes: This figure presents heterogeneity in treatment effects by sex and marital status subgroups on labor force participation (Panel A) and unemployment (Panel B). Each point represents an estimate from the linear OLS estimator with the same set of covariates as equation (1) and weighted by survey weight. The 95% confidence intervals are shown alongside the point estimates. The columns in each panel correspond to different definitions of the treatment group (e.g., $d_1 = 25$, 50, and 100 miles).

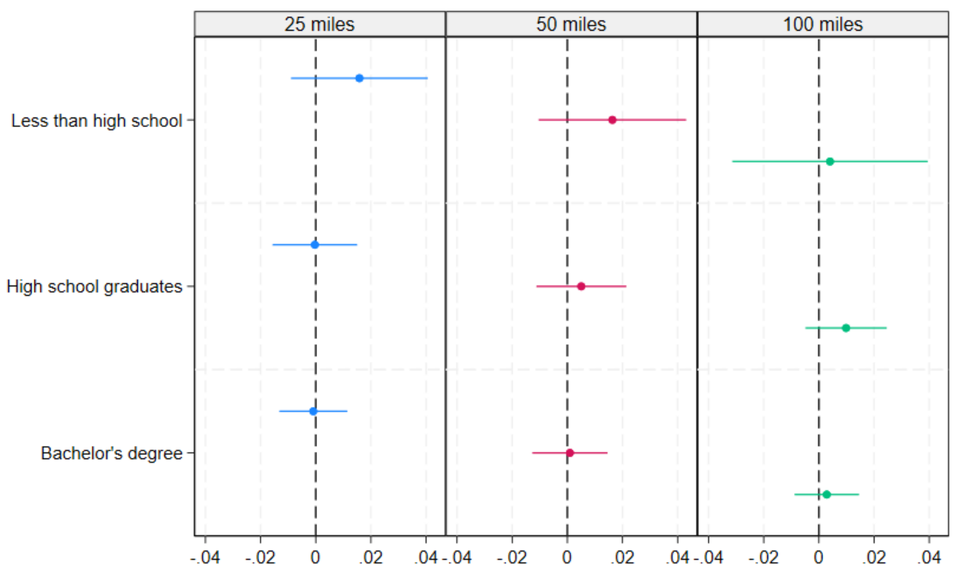
Data: Individual-level labor market survey data are obtained from the American Community Survey (ACS) 2009-2019.

Figure B.12. Impact of Abortion Provider Distance on Labor Market Outcomes by Educational Attainment

A. Likelihood of Being in the Labor Force



B. Likelihood of Being Unemployed (Conditional on Being in the Labor Force)



Notes: This figure presents heterogeneity in treatment effects by education-level subgroups on labor force participation (Panel A) and unemployment (Panel B). Each point represents an estimate from the linear OLS estimator with the same set of covariates as equation (1) and weighted by survey weight. The 95% confidence intervals are shown alongside the point estimates. The columns in each panel correspond to different definitions of the treatment group (e.g., $d_1 = 25$, 50, and 100 miles).
Data: Individual-level labor market survey data are obtained from the American Community Survey (ACS) 2009-2019.

Table B.1. Crime Type and Demographic Composition in Texas

	(1)	(2)	(3)	(4)
	Control	Treated group defined by travel distance ever increases by ...		
		25 miles	50 miles	100miles
Crime Type				
Number of property crimes	27047.87	4112.85	4740.64	4785.02
	(35347.52)	(4599.21)	(4952.24)	(4448.97)
Number of non-property crimes	14098.07	2535.77	2925.93	2885.02
	(19511.21)	(2765.56)	(2958.04)	(2478.30)
Demographics				
Share White	79.88	84.42	86.00	89.05
	(10.25)	(10.08)	(9.65)	(4.01)
Share Black	13.53	11.70	10.45	7.21
	(7.57)	(9.15)	(8.92)	(2.73)
Share non-Hispanic	60.64	65.61	63.28	60.11
	(21.16)	(16.92)	(17.28)	(11.61)
Share aged 0-9	14.76	14.36	14.43	15.16
	(1.62)	(1.83)	(1.64)	(1.80)
Share aged 10-19	14.80	14.45	14.39	14.67
	(1.35)	(1.18)	(1.12)	(1.04)
Share aged 20-29	14.27	16.53	15.56	16.37
	(2.09)	(4.41)	(2.70)	(2.60)
Share aged 30-39	14.35	13.09	13.12	13.22
	(1.68)	(1.40)	(1.35)	(1.25)
Share aged 40-49	13.49	11.71	11.89	11.55
	(1.23)	(1.13)	(1.02)	(1.01)
Share aged 50-59	12.34	12.07	12.42	11.95
	(1.20)	(1.58)	(1.26)	(1.03)
Share women aged 15-44	21.11	20.61	20.11	20.46
	(1.86)	(2.78)	(2.02)	(1.91)
N (agency-year obs)	5,676	2,277	1,738	891

Notes: This table reports the means and standard deviations (in parentheses) of the outcome and control variables in the baseline specification. The unit of observation is policy agency by year. Column (1) presents summary statistics for control counties—those where the increase in travel distance to the nearest abortion clinic (ΔD_{ct}) never exceeded 25 miles between 2009 and 2019. Columns (2) through (4) correspond to treatment counties, defined using progressively higher cutoff values: counties where ΔD_{ct} ever exceeded 25 miles, 50 miles, and 100 miles, respectively.

Table B.2. Impact of Abortion Provider Distance on Other Crime Categories

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All Crime						
Treatment	0.066**	0.030	0.086***	0.051*	0.132***	0.105***
	(0.031)	(0.031)	(0.031)	(0.030)	(0.031)	(0.031)
AME	108.251	49.099	147.500	87.439	241.905	192.224
AME S.E.	51.675	51.365	54.018	52.456	57.685	57.298
N	7,953	7,953	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	7049.410	7049.410	7929.688	7929.688	7748.405	7748.405
Panel B. Total Index Crime						
Treatment	0.091***	0.043	0.113***	0.061**	0.179***	0.135***
	(0.033)	(0.031)	(0.033)	(0.031)	(0.029)	(0.028)
AME	113.410	53.278	146.634	79.765	249.292	186.996
AME S.E.	40.678	38.416	43.406	39.838	40.146	38.540
N	7,953	7,953	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	5123.185	5123.185	5767.852	5767.852	5650.594	5650.594
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1) for alternative crime categories. Panel A reports results for all crimes (indexed + non-indexed crimes) and Panel B for all indexed crimes (property + violent crimes). The treatment is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic in two consecutive years, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. The control group is composed of counties that never exceed a basleine distance threshold of 25 miles ($\Delta D_{ct} < 25$ miles). For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.3. Impact of Abortion Provider Distance on Crime, BJS Estimates

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	29.315***	29.154**	33.219***	26.928**	53.466***	50.063***
	(9.776)	(11.352)	(9.322)	(10.616)	(8.549)	(11.495)
N	7,953	7,953	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	351.518	351.518	363.331	363.331	361.468	361.468
Panel B. Non-Property Crime						
Treatment	-5.509	-7.975	-2.176	-3.154	-1.652	-5.011
	(5.169)	(7.162)	(5.574)	(7.085)	(8.088)	(10.326)
N	7,942	7,942	7,414	7,414	6,567	6,567
$E[Y\|Pre, Treat]$	200.555	200.555	207.004	207.004	214.897	214.897
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates using the BJS estimator, with the same set of controls as those in equation (1) and weighted by agency-covered population size. Since BJS estimator is a linear estimator, we normalized the outcome variables to crime rates per 10,000 agency-covered population. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to ATT estimates. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.4. Impact of Abortion Provider Distance on Crime, Alternative Specifications

	Treated group defined by travel distance ever increases by $\geq$ 100 miles					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.139***	0.136***	0.176***	0.139***	0.138***	0.150***
	(0.027)	(0.028)	(0.042)	(0.027)	(0.028)	(0.032)
AME	169.678	166.475	214.691	169.664	168.947	183.045
AME S.E.	33.188	33.863	51.346	33.023	33.613	38.862
N	6,567	6,567	6,567	6,567	6,567	6,567
$E[Y\|Pre, Treat]$	4940.145	4940.145	4940.145	4940.145	4940.145	4940.145
Panel B. Non-Property Crime						
Treatment	0.030	0.025	0.055	0.024	0.027	0.034
	(0.058)	(0.045)	(0.052)	(0.046)	(0.045)	(0.048)
AME	18.590	15.620	33.684	14.671	16.861	20.750
AME S.E.	35.865	27.710	31.821	28.051	27.831	29.311
N	6,567	6,567	6,567	6,567	6,567	6,567
$E[Y\|Pre, Treat]$	2808.166	2808.166	2808.166	2808.166	2808.166	2808.166
County Demographics	✓	✓	✓	✓	✓	✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dest. Service Population		✓				
County-specific Trends			✓			
Police per 10,000				✓		
N. Family Planning Clinics					✓	
N. Mental Health Centers						✓

Notes: This table presents estimates from the Poisson model in equation (1), incorporating additional covariates. The treatment group is defined as counties where ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 100 miles during the study period. All specifications include the baseline covariates. Column (1) presents the baseline effects. Column (2) further controls for the number of women of reproductive age in the destination county. Columns (3) add county-specific linear time trends for the focal counties. Column (4) controls for the number of police officers employed by the reporting agency. Column (5) includes the number of family planning clinics in the county. Column (6) includes the number of mental health facilities in the county. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of reduced abortion access on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.5. Impact of Abortion Access on Crime, Different Treatment Cutoffs

	Treated group defined by travel distance ever increases by					
	$\geq$ 30 miles		$\geq$ 60 miles		$\geq$ 90 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.095***	0.045	0.116***	0.061*	0.184***	0.139***
	(0.033)	(0.031)	(0.036)	(0.035)	(0.028)	(0.027)
AME	104.591	50.073	134.067	70.830	221.745	166.958
AME S.E.	36.080	34.365	41.540	40.032	33.726	32.571
N	7,832	7,832	7,282	7,282	6,666	6,666
$E[Y\|Pre, Treat]$	4588.027	4588.027	5479.858	5479.858	4798.279	4798.279
Panel B. Non-Property Crime						
Treatment	-0.008	-0.008	0.011	0.020	0.014	0.024
	(0.037)	(0.038)	(0.037)	(0.036)	(0.047)	(0.045)
AME	-4.533	-4.765	6.715	11.540	8.407	14.619
AME S.E.	20.640	21.459	21.956	20.898	28.358	27.559
N	7,821	7,821	7,282	7,282	6,666	6,666
$E[Y\|Pre, Treat]$	2606.330	2606.330	3098.238	3098.238	2727.375	2727.375
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1). The treatment group is defined using progressively higher cutoff values (different from those used in the baseline model) based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 30 miles (columns 1–2), 60 miles (columns 3–4), or 90 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.6. Impact of Abortion Access on Crime, $d_0 = 10$ miles

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.095***	0.048	0.114***	0.063*	0.187***	0.151***
	(0.033)	(0.031)	(0.034)	(0.033)	(0.028)	(0.028)
AME	108.030	54.991	135.485	75.323	239.541	193.566
AME S.E.	37.163	35.665	40.937	38.993	36.353	35.325
N	7,590	7,590	7,051	7,051	6,204	6,204
$E[Y\|Pre, Treat]$	4492.185	4492.185	5056.232	5056.232	4940.145	4940.145
Panel B. Non-Property Crime						
Treatment	-0.008	-0.008	0.014	0.029	0.014	0.029
	(0.037)	(0.038)	(0.036)	(0.035)	(0.047)	(0.047)
AME	-4.924	-4.702	8.347	17.397	9.361	18.625
AME S.E.	21.343	22.211	21.956	21.147	30.479	30.174
N	7,579	7,579	7,051	7,051	6,204	6,204
$E[Y\|Pre, Treat]$	2557.761	2557.761	2873.337	2873.337	2808.166	2808.166
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1), using an alternative cutoff value (10 miles) to define the control group. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.7. Impact of Abortion Access on Crime, Continuous Treatment

	Travel Distrance in t		Travel Distrance in $t-1$	
	(1)	(2)	(3)	(4)
Panel A. Property Crime				
$Treatment\ (Continuous)$	0.065***	0.040***	0.067***	0.046***
	(0.015)	(0.015)	(0.014)	(0.017)
AME	70.682	43.584	71.210	48.751
$AME\ S.E.$	16.413	16.561	14.856	18.506
N	7,953	7,953	7,220	7,220
$E[Y\|Pre, Treat]$	1090.471	1090.471	1068.370	1068.370
Panel B. Non-Property Crime				
$Treatment\ (Continuous)$	0.000	0.005	-0.001	-0.003
	(0.021)	(0.021)	(0.022)	(0.024)
AME	0.094	2.552	-0.759	-1.449
$AME\ S.E.$	11.977	12.101	12.454	13.431
N	7,942	7,942	7,220	7,220
$E[Y\|Pre, Treat]$	557.744	557.744	553.986	553.986
County Demographics		✓		✓
Agency FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model using continuous treatment variables. In columns (1) and (2), we use the continuous travel distance (in 100 miles) to the nearest abortion clinic as the treatment variable. In columns (3) and (4), we use the travel distance of the preceding year. For each treatment variable, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies (as defined in the baseline model) in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.8. Impact of Abortion Provider Distance on Crime, Unbalanced Panel

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.095***	0.050*	0.113***	0.064**	0.187***	0.145***
	(0.031)	(0.030)	(0.033)	(0.031)	(0.027)	(0.026)
AME	86.035	45.564	107.774	61.406	190.673	147.767
AME S.E.	28.557	27.040	31.768	29.662	27.990	27.083
N	9,810	9,810	9,097	9,097	8,019	8,019
$E[Y\|Pre, Treat]$	4194.087	4194.087	4768.600	4768.600	4724.622	4724.622
Panel B. Non-Property Crime						
Treatment	-0.013	-0.015	0.012	0.025	0.017	0.025
	(0.037)	(0.037)	(0.036)	(0.034)	(0.046)	(0.044)
AMES.E.						
AME	-6.116	-6.884	6.014	12.105	8.729	13.095
AMES.E.	17.023	17.341	17.425	16.248	23.783	22.741
N	9,795	9,795	9,093	9,093	8,015	8,015
$E[Y\|Pre, Treat]$	2389.096	2389.096	2710.076	2710.076	2685.878	2685.878
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1), including all Texas agencies with or without missed reporting years. The treatment is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic in two consecutive years, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. The control group is composed of counties that never exceed a basleine distance threshold of 25 miles ($\Delta D_{ct} < 25$ miles). For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.9. Impact of Abortion Access on Crime, Excluding Counties Bordering New Mexico

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
$Treatment$	0.091***	0.041	0.110***	0.055*	0.186***	0.142***
	(0.033)	(0.031)	(0.035)	(0.032)	(0.028)	(0.028)
AME	100.504	45.865	127.959	63.812	229.476	175.709
AME $S.E.$	36.512	34.741	40.209	37.437	35.192	34.370
N	7,634	7,634	7,117	7,117	6,325	6,325
$E[Y\|Pre, Treat]$	4626.023	4626.023	5255.418	5255.418	5243.822	5243.822
Panel B. Non-Property Crime						
$Treatment$	-0.014	-0.011	0.009	0.026	0.011	0.033
	(0.036)	(0.038)	(0.035)	(0.034)	(0.047)	(0.046)
AME	-7.937	-6.355	5.254	14.943	7.030	20.205
AME $S.E.$	20.136	21.140	20.589	20.052	28.781	28.141
N	7,623	7,623	7,117	7,117	6,325	6,325
$E[Y\|Pre, Treat]$	2634.125	2634.125	2986.929	2986.929	2981.323	2981.323
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1), using an alternative sample that excludes counties bordering New Mexico. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. AME denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and AME $S.E.$ is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.10. Impact of Abortion Access on Crime, Excluding Counties Affected by Temporary Abortion Clinic Closure

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.096***	0.060*	0.116***	0.075**	0.178***	0.145***
	(0.033)	(0.033)	(0.034)	(0.036)	(0.027)	(0.034)
AME	104.621	65.964	133.448	86.322	216.428	177.165
AME S.E.	36.033	36.458	39.535	41.359	33.340	40.917
N	7,414	7,414	6,908	6,908	6,215	6,215
$E[Y\|Pre, Treat]$	4685.819	4685.819	5368.306	5368.306	4940.145	4940.145
Panel B. Non-Property Crime						
Treatment	-0.010	-0.005	0.013	0.033	0.008	0.029
	(0.037)	(0.040)	(0.036)	(0.037)	(0.047)	(0.048)
AME	-5.825	-2.546	7.886	19.273	4.822	18.006
AME S.E.	20.613	22.230	20.994	21.596	28.754	29.560
N	7,403	7,403	6,908	6,908	6,215	6,215
$E[Y\|Pre, Treat]$	2683.866	2683.866	3078.095	3078.095	2808.166	2808.166
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1), using an alternative sample that excludes counties affected by temporary abortion clinic closures in McAllen and Waco. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

Table B.11. Impact of Abortion Access on Crime, Excluding All Counties Bordering Mexico

	Treated group defined by travel distance ever increases by					
	$\geq$ 25 miles		$\geq$ 50 miles		$\geq$ 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Property Crime						
Treatment	0.081**	0.076**	0.100***	0.094***	0.173***	0.221***
	(0.032)	(0.030)	(0.034)	(0.033)	(0.027)	(0.024)
AME	86.880	81.366	111.383	104.415	206.881	264.430
AME S.E.	34.068	31.646	37.426	36.928	32.581	29.032
N	7,436	7,436	6,919	6,919	6,072	6,072
$E[Y\|Pre, Treat]$	4506.099	4506.099	5056.232	5056.232	4940.145	4940.145
Panel B. Non-Property Crime						
Treatment	-0.027	0.021	-0.005	0.060**	-0.005	0.109***
	(0.034)	(0.032)	(0.033)	(0.030)	(0.045)	(0.040)
AME	-14.588	11.545	-2.880	33.632	-2.828	65.009
AME S.E.	18.328	17.356	18.694	17.111	26.777	23.654
N	7,425	7,425	6,919	6,919	6,072	6,072
$E[Y\|Pre, Treat]$	2565.647	2565.647	2873.337	2873.337	2808.166	2808.166
County Demographics		✓		✓		✓
Agency FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1), using an alternative sample that excludes all Texas-Mexico border counties. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (columns 1–2), 50 miles (columns 3–4), or 100 miles (columns 5–6) during the study period. For each cutoff, we report estimates from two specifications: one without covariates and a fully specified baseline model. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

C Synthetic Controls Strategy

In this section, we implement the synthetic control method (SCM) to visually assess the effect of HB2 on crime outcomes (Abadie, Diamond, and Hainmueller, 2010).

We focus on the treatment definition that produced the most pronounced effects in our baseline difference-in-differences analysis: agencies in counties where the distance to the nearest abortion provider increased by more than 100 miles following the enactment of HB2 in 2013. The donor pool (control group) consists of agencies in counties where distance changes never exceeded 25 miles over the study period. To facilitate visual and analytical tractability, we aggregate all treated agencies into a single composite unit by taking the average number of crimes in each year. This yields a "representative treated unit," which we compare to a synthetic control unit constructed from a weighted average of the donor pool.

We define the pre-treatment period as 2009–2012 and the post-treatment period as 2013–2019. The predictor set includes only the annual crime count for each agency during the pre-treatment years. This minimalist specification aligns with the SCM framework's emphasis on matching based solely on lagged outcomes (Abadie, Diamond, and Hainmueller, 2010).

Formally, let Y_{it} denote the number of crimes in agency i in year t, with control units indexed by $i = 1, 2, \ldots, J$ and the composite treated unit indexed by $i = J + 1$. Define $X_1 \in \mathbb{R}^4$ as the vector of pre-treatment crime outcomes (2009–2012) for the treated unit, and $X_0 \in \mathbb{R}^{4\times J}$ as the corresponding matrix for the control units.

The SCM determines weights $W = (w_1, w_2, \ldots, w_J)'$ that minimize the mean squared prediction error (MSPE) between the treated and synthetic units in the predictor space:

$$\min_W (X_1 - X_0 W)' V (X_1 - X_0 W),$$

$$\text{subject to } w_j \geq 0 \text{ and } \sum_{j=1}^{J} w_j = 1,$$

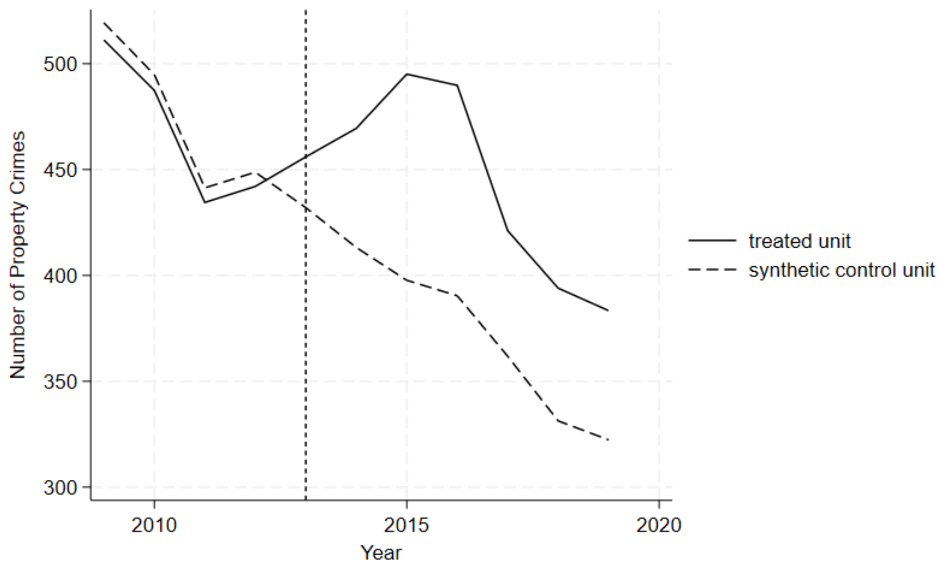
where V is a symmetric, positive semi-definite matrix that assigns relative importance to the predictors. In our implementation using the `synth` package in Stata, the predictor importance matrix V is selected through a data-driven nested optimization procedure.

Panel A of Figure C.1 displays the evolution of property crime in the treated and synthetic control units. The synthetic control closely follows the treated unit prior to 2013, confirming the quality of the match. After HB2's enactment in 2013, we observe a sharp divergence: property crimes increase in the treated counties while continuing to decline in the synthetic control. This elevated trend persists through 2016 before declining, suggesting a durable, though not permanent, treatment effect.

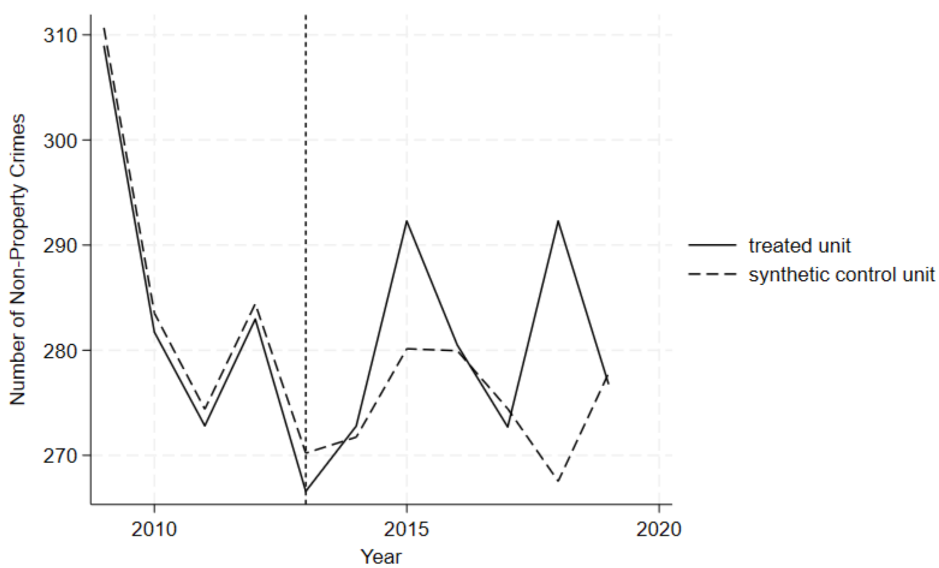
Panel B presents the results for non-property crimes. In contrast to property crime, we observe no meaningful divergence post-2013, and the trends for the treated and synthetic control units remain largely parallel throughout the study period.

Figure C.1. Impact of Abortion Access on Crimes, Synthetic Control

A. Property Crime



B. Non-Property Crime



Notes: This figure presents synthetic control estimates comparing the number of property and non-property crimes between treated agencies (from counties where ΔD_{ct} ever exceeded 100 miles) and control agencies (from counties where ΔD_{ct} never exceeded 25 miles). Predictor variables used in the construction of the synthetic controls are pre-treatment outcomes measured prior to 2013.

Data: Crime data are obtained from the FBI's Uniform Crime Reporting (UCR) Program 2009-2019.

D Abortion and Fertility Rates

To study the first-stage effect of distance on abortions, we residualize the abortion rate using county and year fixed effects. Panel A of Figure D.1 shows a negative association between travel distance and the abortion rate.

A key outcome in this context is live births. If financially motivated crime is used to fund abortions, we would not necessarily observe a change in the number of live births. To test this, we leverage birth data from the Texas Vital Statistics (Texas Department of State Health Services, 2024). Panel B of Figure D.1 shows a strong positive relationship between travel distance ($t-1$) and the birth rate (per 1,000 county population).

We further unpack these relationships using our formal Poisson regression framework. In addition, we are interested in exploring whether changes in abortion and birth rates vary across the age distribution of mothers. Therefore, we examine not only overall changes in the total number of intentional pregnancy terminations and live births, but also disaggregate these outcomes by five-year maternal age bins.

Table D.1 reports the estimates showing the impact of travel distance changes on the number of abortions. First, we observe a substantial decline in the total number of abortions across different travel distance thresholds. Consistent with our crime analysis, the reduction in abortions is more pronounced when travel distance increases by 100 miles or more. Specifically, in column (6) of Panel A, we find that the number of abortions decreases by 34% ($p < 0.01$) when travel distance increases by 100 miles or more. The heterogeneity analysis reveals that the impact is more pronounced among younger women, particularly those aged 20–29, although we find significant changes in abortion rates across the entire age distribution.

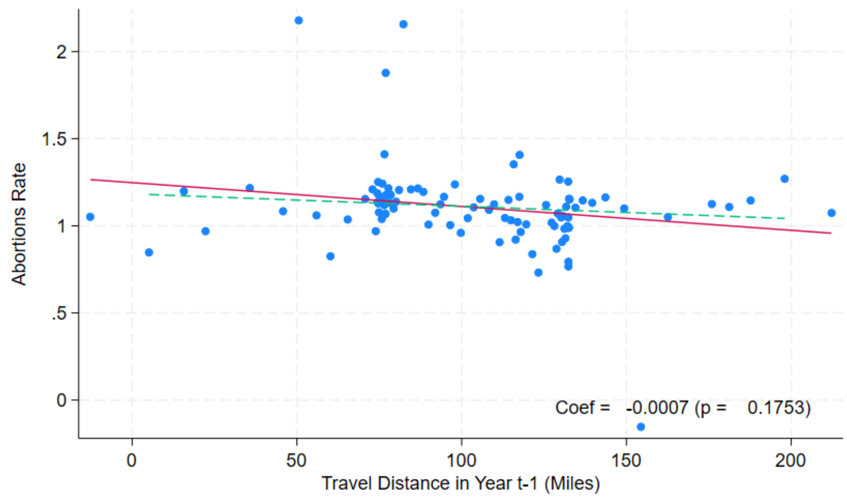
In Table D.2, we conduct a similar analysis using the number of live births as the outcome. We find some evidence of a decline in total live births, although the effect is less persistent over time. This decline is primarily driven by women at the older end of the age distribution. Disaggregating the impact by maternal age reveals that the aggregate birth outcome masks substantial heterogeneity, helping explain why the overall change in births appears less pronounced.

Specifically, we find that young mothers experience a significant increase in the number of live births as travel distance increases, with the effect concentrated among women under age 20 and those aged 20–29. For women under 20, column (3) shows an approximately 5% increase in live births ($p < 0.01$) following a travel distance increase of 100 miles or more.

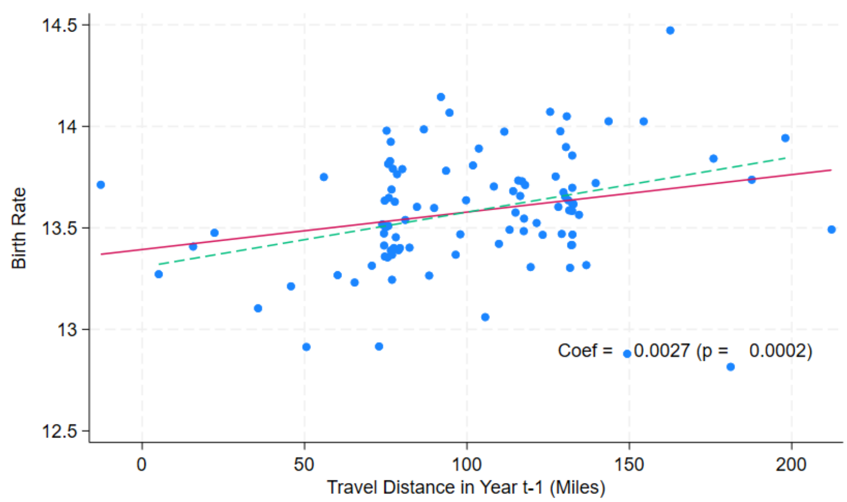
It is worth noting that the data sources for abortion and fertility outcomes differ, yet both corroborate a similar story, particularly for younger mothers. This finding also aligns with the idea that the economic strain of unintended births may be more salient for younger mothers, as the costs of early motherhood can be disproportionately high for women with less work experience and lower accumulated human capital (Kuziemko et al., 2018).

Figure D.1. Descriptive Relationships Between Abortion and Fertility Rates and Abortion Provider Distance

Panel A. Abortion per 1,000 County Population



Panel B. Live Births per 1,000 County Population



Notes: This figure presents bin-scatter plots illustrating the relationship between the abortion (and birth) rate in a county in year t and the county's travel distance to the nearest abortion clinic in year $t-1$. Each point represents the average value of the y-axis variable within a percentile bin of the x-axis variable. Both variables are residualized using county and year fixed effects. The red solid line represents a linear fit using all data points, while the green dashed line represents a linear fit excluding outliers—observations in the top or bottom 1% of either variable's distribution. The slope and corresponding p-value for the green dashed line are reported in the figure.

Table D.1. Impact of Abortion Provider Distance on the Number of Pregnancy Terminations

	Treated group defined by travel distance ever increases by		
	$\geq$ 25 miles	$\geq$ 50 miles	$\geq$ 100 miles
	(1)	(2)	(3)
Panel A. Number of Intentional Termination of Pregnancy, Total			
Treatment	-0.247***	-0.281***	-0.413***
	(0.026)	(0.033)	(0.052)
AME	-64.150	-79.026	-130.522
AME S.E.	6.869	9.378	16.531
N	2,530	2,277	1,958
$E[Y \mid Pre, Treat]$	417.490	355.777	208.300
Panel B. Mother Aged under 20			
Treatment	-0.203***	-0.241***	-0.244***
	(0.035)	(0.041)	(0.065)
AME	-6.200	-7.927	-8.872
AMES.E.	1.064	1.364	2.374
N	2,486	2,233	1,936
$E[Y \mid Pre, Treat]$	63.956	58.397	32.712
Panel C. Mother Aged 20-29			
Treatment	-0.234***	-0.264***	-0.414***
	(0.027)	(0.035)	(0.051)
AME	-35.352	-43.048	-75.949
AME S.E.	4.145	5.707	9.366
N	2,530	2,277	1,958
$E[Y \mid Pre, Treat]$	257.192	213.835	130.486
Panel D. Mother Aged 30-39			
Treatment	-0.250***	-0.292***	-0.394***
	(0.032)	(0.039)	(0.062)
AME	-17.540	-22.233	-33.817
AME S.E.	2.248	2.984	5.286
N	2,486	2,244	1,936
$E[Y \mid Pre, Treat]$	85.014	72.637	36.204
Panel E. Mother Aged 40+			
Treatment	-0.143**	-0.169**	-0.340***
	(0.064)	(0.069)	(0.127)
AME	-1.492	-1.934	-4.363
AME S.E.	0.666	0.794	1.634
N	2,145	1,925	1,672
$E[Y \mid Pre, Treat]$	8.987	8.242	4.394
County Demographics	✓	✓	✓
Agency FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: This table presents estimates from the Poisson model in equation (1) for total abortion and abortion by mother-age subcategories. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (column 1), 50 miles (column 2), or 100 miles (column 3) during the study period. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. *Data:* Pregnancy termination data are obtained from Induced Terminations of Pregnancy (ITOP) for Texas Residents 2009-2019, Texas Health and Human Services.

Table D.2. Impact of Abortion Provider Distance on the Number of Live Births

	Treated group defined by travel distance ever increases by		
	$\geq$ 25 miles	$\geq$ 50 miles	$\geq$ 100 miles
	(1)	(2)	(3)
Panel A. Number of Live Births, Total			
Treatment	-0.004	-0.002	-0.022***
	(0.005)	(0.005)	(0.007)
AME	-6.504	-3.959	-42.781
AME S.E.	8.110	9.752	14.528
N	2,552	2,288	1,969
$E[Y\|Pre, Treat]$	2627.504	2375.270	2032.170
Panel B. Mother Aged under 20			
Treatment	0.054***	0.058***	0.050***
	(0.011)	(0.013)	(0.018)
AME	8.235	9.618	9.000
AME S.E.	1.708	2.101	3.246
N	2,552	2,288	1,969
$E[Y\|Pre, Treat]$	332.912	331.115	286.675
Panel C. Mother Aged 20-29			
Treatment	0.012*	0.014**	0.000
	(0.006)	(0.007)	(0.008)
AME	10.088	13.544	0.382
AME S.E.	5.344	6.276	8.705
N	2,563	2,299	1,969
$E[Y\|Pre, Treat]$	1604.628	1415.607	1234.391
Panel D. Mother Aged 30-39			
Treatment	0.004	0.004	-0.009
	(0.008)	(0.008)	(0.011)
AME	2.489	2.400	-6.281
AME S.E.	4.434	5.410	8.007
N	2,563	2,299	1,969
$E[Y\|Pre, Treat]$	652.926	593.666	485.458
Panel E. Mother Aged 40+			
Treatment	0.031*	0.025	0.003
	(0.018)	(0.023)	(0.026)
AME	1.387	1.215	0.153
AME S.E.	0.810	1.105	1.442
N	2,552	2,288	1,969
$E[Y\|Pre, Treat]$	36.993	34.820	25.647
County Demographics	✓	✓	✓
Agency FE	✓	✓	✓
Year FE	✓	✓	✓

Notes: This table presents treatment effect estimates from the Poisson model in equation (1) for total live births and live births by mother-age subcategories. The treatment group is defined using progressively higher cutoff values based on whether ΔD_{ct}, the change in travel distance to the nearest abortion clinic, ever exceeded 25 miles (column 1), 50 miles (column 2), or 100 miles (column 3) during the study period. *Treatment* refers to the coefficient on the DID term in the Poisson model. *AME* denotes the average marginal effect of abortion provider distance on the number of crimes reported by treated agencies, and *AME S.E.* is the corresponding standard error. $E[Y|Pre, Treat]$ shows the mean outcome among treated agencies in the pre-treatment period. Standard errors (in parentheses) are clustered at the county level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. *Data:* Birth data are obtained from Texas Vital Statistics (VSTAT) public-use data files 2009-2019.

E Estimating Distributional Effects Using RIF Regressions

In this section, we detail the methodology used to produce the interquartile range estimates shown in Panel B of Table 3.

To assess the distributional consequences of abortion access, we estimate the effect of changes in travel distance to the nearest abortion clinic on the interquartile range of personal income using the recentered influence function (RIF) regression framework. Originally introduced by Firpo, Fortin, and Lemieux (2009) and extended by Firpo, Fortin, and Lemieux (2018); Rios-Avila (2020), this approach allows for simple estimation of unconditional partial effects on distributional statistics based on regression.

In our context, the outcome of interest is individual annual personal income in 2010 dollars, denoted Y. The target statistic is the interquartile range:

$$\mathrm{IQR}(Y) = q_{0.75}(Y) - q_{0.25}(Y)$$

where $q_\tau(Y)$ denotes the unconditional τ-quantile of the income distribution. The IQR captures the spread of the central 50% of the distribution and is a standard measure of income inequality.

The RIF regression methodology transforms the outcome variable into its recentered influence function, such that the expectation of the transformed variable recovers the target distributional statistic, enabling standard linear regression techniques to estimate effects on distributional statistics. For a given statistic $v = v(F_Y)$, where F_Y is the cumulative distribution function of Y, the influence function (IF) captures the impact of an infinitesimal contamination at point Y on v. For quantiles, the influence function is given by:

$$\mathrm{IF}(Y; q_\tau) = \frac{\tau - \mathbb{1}\{Y \leq q_\tau\}}{f_Y(q_\tau)}$$

where $\mathbb{1}\{\cdot\}$ is an indicator function and $f_Y(q_\tau)$ is the probability density function of Y evaluated at q_τ. The RIF adds the quantile itself to the influence function:

$$\text{RIF}(Y; q_\tau) = q_\tau + \text{IF}(Y; q_\tau)$$

This transformation satisfies the property $\mathbb{E}[\text{RIF}(Y; q_\tau)] = q_\tau$, which ensures that the expectation of the RIF recovers the quantile of interest (Cowell and Flachaire, 2015; Essama-Nssah and Lambert, 2012).

To construct the RIF for the interquartile range, we take the difference of the RIFs for the 75th and 25th percentiles:

$$\text{RIF}(Y; \text{IQR}) = \text{RIF}(Y; q_{0.75}) - \text{RIF}(Y; q_{0.25})$$

This transformed variable becomes the dependent variable in a standard linear regression. Specifically, we estimate the following model, which parallels our baseline difference-in-differences specification in Equation (1):

$$\text{RIF}(Y_{ict}; \text{IQR}) = \beta \cdot \text{Treatment}_{ct} + \boldsymbol{\gamma}'\mathbf{X}_{ict} + \theta_c + \tau_t + \varepsilon_{ict}$$

Here, Y_{ict} is the personal income of individual i residing in county c and year t; Treatment_{ct} denotes the abortion access shock as defined in Section 5; $\mathbf{X}_{ict}$ is a vector of individual-level covariates, including indicators for race/ethnicity (White, Black, non-Hispanic), age group (in 10-year bins), and whether the individual is a female aged 15–44; θ_c and τ_t are county and year fixed effects; and ε_{ict} is the error term. Standard errors are clustered at the county level to account for spatial correlation in treatment exposure.

The coefficient β captures the average treatment effect of increased travel distance on the interquartile range of income, after adjusting for covariates. A positive value of β implies that greater travel distance is associated with wider income dispersion (i.e., increased inequality), whereas a negative value suggests a compression in the income distribution.

We implement this approach using the `rifhdreg` command in Stata, developed by Rios-Avila (2020), which facilitates RIF estimation with high-dimensional fixed effects and covariates. The command computes the necessary density values at the quantiles of interest using kernel density estimation and adjusts standard errors accordingly. This framework provides an efficient and interpretable means to estimate how changes in access to abortion services influence income inequality within Texas counties over time.