THE LONG RUN EFFECTS OF TRANSFORMATIONAL FEDERAL POLICIES: REDLINING, THE AFFORDABLE CARE ACT AND HEAD START

A Dissertation

by

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ABSTRACT

My dissertation research spans several subfields of applied microeconomics, including public, health and urban economics. In particular, my dissertation is concerned with identifying the long-run effects of large, transformational Federal policies. My research shows how increases in access to credit markets, early childhood education and medical care can influence the course of a person's life.

In the first chapter, my Job Market Paper, I show how racially motivated restrictions to credit markets implemented in the 1930s, which are colloquially called "redlining", influence the present day distribution of crime. I employ two regression discontinuity (RD) designs. First, I use a spatial RD to show that redlining influenced the present day distribution of crime across neighborhoods in Los Angeles, California. Secondly, I use a city-level RD design that relies on an unannounced population cutoff used to determinate which cities were redline-mapped. I find *both* that redlining increased crime in predominantly Black and Hispanic neighborhoods in Los Angeles *and* that redline-mapping a city increased Black and Hispanic crime victimization in that city. I also find that redline-mapping increased city-level racial segregation, which suggests a mechanism through which credit-access restrictions could have influenced long-run crime volume.

In the second chapter, which is a separate sole-authored paper, I exploit the staggered state-level expansion of the Medicaid program (as allowed under the Affordable Care Act) as a natural experiment to ascertain whether increased access to medical services, including prescription drugs, increased opioid-related deaths. I also exploit the staggered stage-level legalization of marijuana to see whether the increased availability of an opioid substitute decreased opioid-related deaths. The state-level decision to expand Medicaid increased both opioid prescriptions and opioid-related deaths. These results vary strongly by demography, being driven largely by deaths of white men without college degrees. Overall, opioid accessibility shocks explain about 12,000 opioid deaths

per year, or nearly a third of the death toll. The state-level decision to legalize recreational marijuana (a substitute painkiller) reduced opioid-related deaths. Overall, these opioid-substitute accessibility shocks also explain about 12,000 opioid deaths per year. I conclude that policy-makers can achieve reductions in opioid mortality without restricting access to opioids.

Lastly, in the third chapter, a joint paper with Andrew Barr and Alex Smith, we use the staggered county-level implementation in the 1960s of a national early childhood education program called "Head Start" to show that access to early childhood education influences the likelihood of adulthood criminal behavior. We produce difference-in-difference estimates of the effect of Head Start availability in a child's birth county on the likelihood of adulthood criminal conviction. Head Start availability reduces the likelihood of a serious conviction by age 35 by 1.3 percentage points, but only in high-poverty counties. This paper is the first to (1) provide large-scale evidence that early childhood education reduces later criminal behavior, (2) provide estimates that rely on administrative crime data to determine the effects of Head Start availability on later criminal behavior, and (3) estimate that, in high poverty counties, the discounted benefits generated by Head Start's later crime reduction were greater than the costs of the program itself. Our results indicate a meaningful connection between targeted, large-scale early childhood education interventions and criminal behavior. These results provide evidence in support of recent state efforts to expand early childhood education, but point to large potential gains from targeting these efforts toward higher poverty areas.

DEDICATION

To my wife, Kathy, who somehow put up with me while I constructed this monster:

> ... like someone attempting a portrait by assembling hands, feet, a head and other parts from different sources. These several bits may be well depicted, but they do not fit together to make up a single body. Bearing no genuine relationship to each other, these fragments, joined together, produce a monster rather than a man

And to my parents, who, for some reason, allowed me to take Philosophy courses at the community college while I was in high school. Thank you.

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Contributors

This work was supervised by a dissertation committee consisting of Professor Andrew Barr (advisor), and Mark Hoekstra of the Department of Economics and Jason Lindo of the Department of Economics, as well as Bethany DeSalvo of the Texas Federal Research Data Center.

The data analyzed for Chapter 3 was provided by Professor Andrew Barr. The analyses conducted in Chapter 3 was conducted under the guidance of Andrew Barr.

All other work conducted for the dissertation was completed by the student independently, under the advisement of Dr. Andrew Barr.

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1.1 Introduction

"There is no such thing really as was because the past is"
(William Faulkner, quoted in *Faulkner in the University* pg. 84)

Today in the United States the social costs of crime exceed 2 trillion dollars.[1] These welfare costs are not distributed evenly across racial and ethnic categories: nearly 60% of murder victims, for example, are either African-American or Hispanic[2]. These welfare costs are also unevenly distributed across neighborhoods. Predominantly African-American neighborhoods have 5 times as many violent crimes as predominantly non-Hispanic white neighborhoods; predominantly Latino neighborhoods have about 2.5 times as many violent crimes as predominantly non-Hispanic white neighborhoods[3] ((2)).

Because variation in crime at the neighborhood level is likely associated with a vast number of neighborhood level factors including income, racial segregation ((3)), school quality ((4)) and pollution ((5)), researchers face a significant challenge in trying to identify the causes of these inequities in the distribution of crime. In this paper I use two regression discontinuity designs to show that Federal housing policies established in the wake of the Great Depression make present day contributions both to this inequity in the distribution of crime within cities, and to the overall volume of crime in a city. To stabilize housing markets in the 1930's, a newly formed Federal agency, the Home Owner's Loan Corporation (HOLC), constructed maps of 239 US cities; these maps purported to grade neighborhoods in terms of lending risk, the riskiest neighborhoods being

[1] United States. Senate Committee on the Judiciary. Hearing on The Costs of Crime. September 19, 2006 (cited (1))

[2] Author calculations from NIBRS 2010 Crime Victimization data

[3] A "predominantly African-American neighborhood" is defined as a census tract in which 70% or more of the residents are African-American. Similar definitions are used for "Latino" and "Non-Hispanic white"

labeled in red and colloquially said to have been “redlined”. Neighborhoods assigned low grades faced decades of reduced credit access relative to neighborhoods assigned higher grades ((6)). Thus, redlining policy provides a context in which a researcher can identify the long run effects of restricting credit access to a neighborhood.

Beginning in 1936 HOLC surveyors and administrators classified neighborhoods on the basis of housing characteristics such as home value, home age, construction-type and rental values, as well as demographic characteristics such as the occupation of residents and, most controversially, the race and ethnicity of residents. In particular, HOLC surveyors were asked to detail whether or not it was expected that certain "inharmonious" or "subversive" groups were likely to move into the neighborhood (see Figure 1.18). Because surveyors recorded demographics, expected demographics and explicitly expressed preferences about which races and ethnicities were more or less advantageous to neighborhood quality and less risky to lenders, many researchers have claimed that redlining not only reflected existing racial discrimination but further institutionalized this racial animus in the public and private credit market, and have also suggested that redlining had a long-run effect on neighborhood formation ((6)). Accordingly, the term “redlining" has come to denote the practice of credit-market discrimination on the basis of neighborhood characteristics such as racial demographics, rather than individual loan-applicant credit-worthiness. The use of these maps and associated discriminatory practices have since been made illegal, first in the 1968 Fair Housing Act (FHA), but later in the 1974 Equal Credit Opportunity Act (ECOA) as well as later revisions to the FHA such as the Fair Housing Amendments Act of 1988, which strengthened penalties for discriminatory lending practices (See Figure 1.1). Whether or not there still exist *de facto* forms of discrimination, the legal use of HOLC maps from roughly 1938 to 1968 created widespread *de jure* racially discriminatory practices in the credit market. This *de jure* discrimination restricted credit access to neighborhoods which were given low grades for at least this 30 year period.

This paper contributes to the growing literature on redlining as well as to the larger literatures on the determinants of crime and the effects of credit access. Concerning redlining, in particular, this paper is (1) the first to show quantitative evidence that racial animus motivated redlining assignments and (2) the first to estimate the causal effect of redlining on crime. In particular, this paper is the first to estimate the causal influence of redlining *within* a city using a spatial regression discontinuity design, the first to document a population cutoff that determined whether a city would be redline-mapped, and the first to use this cutoff to produce a *between*-city estimate of the causal impact of redline-mapping on crime and associated outcomes at the city-level, and finally the first to show suggestive evidence that redline-mapping *improved* crime outcomes in some neighborhoods, increasing overall city-level crime in part by transferring would-be crimes from predominantly White neighborhoods into redlined neighborhoods. Concerning the literature on the determinants of crime and the effects of credit-access more broadly, this paper is the first to show the long-run, persistent effects of credit access on crime.

First, I use a city-level regression discontinuity design that relies on variation in which cities were redline-mapped to show that these credit access restrictions increased the overall volume of city level crime. This identification strategy relies upon an unannounced population cutoff HOLC used to determinate which cities to redline-mapped.

Secondly, I show that the racially motivated credit access restrictions implemented in redlining causally influence the present day distribution of crime across neighborhoods in Los Angeles, California. Using crime data from the city of Los Angeles, I employ a spatial regression discontinuity design to show that redlining increased crime in redlined neighborhoods. Then, using an administrative dataset, I also provide the first quantitative evidence that racial animus seemed to drive the implementation of these 1930 housing policies; this evidence derives from showing robust associations between a HOLC surveyors expectations about racial demography and the quality grade awarded to a neighborhood.

Lastly, I use the city-level design to provide evidence that redline-mapping increased crime by increasing racial segregation, decreasing educational attainment and harming housing markets.

1.2 Background

1.2.1 Institutional History of Redlining

Prior to the housing policies enacted in Franklin Roosevelt's "New Deal", homeownership was difficult for most middle class households. Home loans were neither amortized nor federally insured, and consequently most lenders offered home loans that were between 5 and 10 years in duration and required down payments of 30% or more ((6) p.204). Moreover, the terms of these loans needed to be renegotiated every five years, leaving would-be homeowners subject to fluctuating interest rates. In the midst of the Great Depression, the home ownership market contracted even further as financially strapped families lost their homes and vacancies increased ((7)). In an effort to stabilize the housing market, the Roosevelt administration created the Home Owner's Loan Corporation (HOLC) in 1933. HOLC bought up billions of dollars of mortgages which were on the brink of foreclosure and renegotiated 15 to 25 year mortgages with uniform, amortized loan schedules; nearly 40% of eligible Americans sought HOLC assistance ((6) p.196). In order to make such a large volume of loans, HOLC needed to gauge the riskiness of these new loan offers, and part of the risk inherent in the loan was the expected future value of the home and the homes in its vicinity. Accordingly, HOLC hired local real estate agents to survey parts of a given city, dividing the city into neighborhoods and assigning to each of these neighborhoods a color-coded "security risk" grade. These HOLC-neighborhoods were not based on pre-existing Census designations such as Wards or Ennumeration Districts and were drawn at the discretion of the agency.

These "Residential Security Maps" contained four risk-grades: A(green), B(blue), C(yellow), D(red) (e.g. Figure 1.9). Ranked from best to worst A(green) were described as "new, homogeneous", while D(red) were described as "hazardous" ((6) p.198). HOLC surveyors assigned quality categories and accordingly classified neighborhoods on the basis of housing characteristics such as home value, home age, construction-type and rental values as well as demographic characteristics such as the occupation of residents and, most controversially, the race and ethnicity of residents. In particular, HOLC surveyors were asked to detail whether or not it was expected that certain "inharmonious" or "subversive" groups were likely to move into the neighborhood (see the“Shifting or Infiltration” item in Figure 1.18, as well as Table 1.10). Because surveyors recorded demographics, expected demographics and explicitly expressed preferences about which races and ethnicities were more or less advantageous to neighborhood quality and more or less risky to lenders, many observers and researchers have claimed that this practice and its associated maps not only reflected existing racial discrimination but further institutionalized this racial animus in the public and private credit market ((6)). Thus the term “redlining" has come to denote the practice of credit-market discrimination on the basis of neighborhood characteristics such as racial demographics, rather than individual loan-applicant credit-worthiness.

Alongside these efforts to reduce foreclosure, the Roosevelt administration took further measures to increase homeownership by creating the Federal Housing Administration (FHA) in 1934. The FHA was tasked with insuring private home loans so long as they were amortized, had a long enough term and were deemed to be suitably low risk by the FHA. When a bank applied for FHA insurance on a prospective loan, the FHA hired an appraiser and guided the appraisal process by detailing procedures in its 1935 “*Underwriting Manual*”. The “*Underwriting Manual*” instructed appraisers to rate loan risk partly based on current and expected racial composition of the surrounding neighborhood, since the presence or introduction of “adverse influences” such as “inharmonious racial or nationality groups” were likely to lead to “instability and a reduction in values” (**(author?)** (7))[4]. While insuring loans incentivized lenders to make more loans and increased access to credit[5], it did so differentially by race, leading many observers to see in FHA loan-insurance practices explicit discrimination against African-Americans loan-seekers even conditional on applicant creditworthiness ((6), (7)). Because the Department of Veteran Affairs later adopted FHA appraisal practices as it gave out millions of loans to veterans returning from World

[4]This concern with neighborhood racial composition remains in later editions of the Manual through the 1950’s ((7) p.67)

[5]Jackson claims that this increased access occurred while interest rates fell several percentage points (6) p.205

War II, these FHA practices affected a substantial volume of loans for decades[6]. Mostly importantly, it crucial to note that because the HOLC security maps were very widely distributed to FHA appraisers and FHA appraisers were explicitly encouraged to use them, the pervasive and longstanding influence of FHA insurance practices were themselves influenced by the decisions of HOLC surveyors as they delineated and graded neighborhoods[7].

In short, even though HOLC did influence neighborhood credit access through its own loan-granting practices, its long-run influence is due to its influence on other institutions. In particular, HOLC and its Residential Security Maps influenced loan access in two ways: (1) by influencing private lenders[8] and (2) influencing FHA loan-insurance appraisals. Through these two channels HOLC maps influenced credit access in hundreds of US cities for decades.

The use of these maps and loan practices have since been made illegal, first in the 1968 Fair Housing Act (FHA), but later in the 1974 Equal Credit Opportunity Act (ECOA) as well as later revisions to the FHA such as the Fair Housing Amendments Act of 1988, which strengthened penalties for discriminatory lending practices (See Figure 1.1). Nevertheless, there exists an active debate about whether or not such discriminatory practices still take place[9]. Whether or not there still exist *de facto* forms of discrimination, the *legal* use of HOLC maps from 1938 to 1968 created

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[6]One historian notes that "by 1950 the FHA and VA together were insuring half of all new mortgages nationwide" ((7) p.70). Jackson goes further: "No agency of the United States government has had a more pervasive and powerful impact on the American people over the past half-century than the FHA" ((6)).

[7]Jackson notes "[The FHA] Examiner was specifically instructed to refer to the Residential Security Maps"((6) p.209) and "The FHA cooperated with HOLC and followed HOLC appraisal practices"((6) p.215)

[8]Jackson notes that "During the late 1930s, the Federal Home Loan Bank Board circulated questionnaires to banks asking about their mortgage practices. Those returned by the savings and loan associations and banks in Essex County (Newark), New Jersey indicated a clear relationship between public and private "red lining" practices. One specific question asked "What are the most desirable lending areas?" The answers were often "A and B" or "Blue" or "FHA only". Similarly, to the inquiry, "Are there ary areas in which loans will not be made?" the responses included "Red and most yellow", "C and D", "Newark", "Not in red" and "D areas". Obviously, private banking institutions were privy to and influenced by the governments' Residential Security Maps" ((6) p.203). Hillier offers a dissenting view according to which private lenders were not very aware of the maps ((8)). On Hillier's view, HOLC maps could influence outcomes in any substantial way only through influencing FHA loan insurance practices. (9) offer an extended discussion of the debate in the literature concerning how widely the HOLC maps were used.

[9]See (10) and references. (9) points out that even today there are substantial lawsuits which allege this sort of discrimination in major cities to the extend that the Consumer Financial Protection Bureau and the Department of Justice have open investigations concerning lending discrimination (footnote 1).

widespread *de jure* racially discriminatory practices in the credit market. This *de jure* discrimination restricted credit access to neighborhoods which were given low grades for at least the duration of this 30 year period.

1.2.2 Existing Evidence on Redlining and Crime

While there is a large, interdisciplinary body of work exploring how housing policy in the 1930's may have shaped present day neighborhood characteristics, the literature has not yet identified the effects of redlining on crime nor has it used used this massive Federal policy to address questions about the the determinants of crime and the effects of credit-access more broadly.

Jackson's seminal book, "*Crabgrass Frontier*" chronicles the activities of the HOLC and FHA in relation to several broader narratives he weaves together which include urbanization, suburbanization and the racially motivated history of United States housing policy. More recently (11) uses a spatial regression discontinuity design to show that homes just across the border of a lower HOLC security grade have less value in 1990. To establish the identification assumption that home values did not exhibit jumps prior to the policy, the paper uses home value data from 1940, which is soon after the maps were constructed.

Most recently (9) engages in a groundbreaking and ambitious project to chart the effects of redlining maps in over one hundred US cities across decades. Using a variety of empirical approaches including the construction of counterfactual boundaries that experienced the same pre-existing trends, they identify the causal effect of the HOLC maps on the racial composition and housing development of urban neighborhoods. In particular, this paper shows that being on the lower graded side of D-C (red-yellow) boundaries increased racial segregation from 1930 until about 1970 or 1980 before starting to decline thereafter, even though some gaps persist until 2010. They find that the effects on homeownership rates and house values dissipate over time along the D-C (red-yellow) boundary but remain highly persistent along the C-B (yellow-blue) boundaries. This work is the first to explore and identify causal effects for a vast number of outcomes across over one hundred cities over three quarters of a century. Their work is also the first to highlight the importance of the C-B (yellow-blue) boundary and identify the long-run effects of "yellow-lining".

There still remain many gaps in the literature on redlining. I complement the existing literature by identifying the impact of redlining on crime and quantifying the extent to which racial animus may have motivated neighborhood grades. Furthermore, I add to the literature by estimating the causal influence of redlining *within* a city using a spatial regression discontinuity design, and finally, by documenting a population cutoff that determined whether a city would be Redline-mapped and using this cutoff to produce a *between* city estimate of the causal impact of redline-mapping on crime and other outcomes at the city-level.

Concerning the literature on the determinants of crime and the effects of credit-access more broadly, this paper is the first to show the long-run, persistent effects of credit access on crime. Previous studies have identified effects of childhood exposure to credit access on adulthood credit scores ((12)), and the local effects of reduced local competition between banks on property crime ((13))

1.3 Data

1.3.1 HOLC Administrative Data

To analyze the determinants of redline mapping assignments at both the neighborhood level (within-city) and city-level (between-city), I compiled two novel datasets from HOLC Administrative documents. For the city-level study, I generated a dataset featuring a variable that indicates whether HOLC constructed a redlining map for a given city alongside a robust set of pre-period city characteristics. To create this dataset, I obtained archival data which lists the cities for which HOLC maps were drawn.[10] As I discuss in detail below (Section 1.4), an analysis of this dataset reveals an unannounced population cutoff that nearly completely determined whether or not a city were redline-mapped (See Figure 1.2). Accordingly, I use this city level data to construct a run-

[10]These documents reside in National Archive Group 31.

ning variable based on pre-period city population and exploit this variation in a city-level regression discontinuity design in Section 1.4. Secondly, for the within-city or neighborhood level study, I generated a geocoded dataset of HOLC security-grades and their purported determinants. To create this dataset, I obtained all 416 surveyor "area description" documents for Los Angeles[11], coded the information contained in each document and assigned the resulting data to a georeferenced HOLC map of Los Angeles. Figure 1.18 shows an example of such a document, while Figure 1.9 shows the georeferenced HOLC map of Los Angeles. Of key interest to my analysis of the extent to which neighborhood grade assignments were motivated by racial animus (Sections 1.5.5) are the text responses in the field "1.e. Shifting or Infiltration", which details the HOLC surveyor's expectations about future neighborhood demography (See Figure 1.18). The neighborhood level "Security Grade" given at the bottom of each document is an ordinal ranking ranging from "1st" (colored green) to "4th" (colored red) and constitutes the neighborhood level HOLC mapping assignment.

1.3.2 Census Data

For the between-city study, I utilize decennial Census data from 1890 to 2010 (Ruggles et al. (2018)). For the within-city study, I use 1920 and 1930 address-level Census micro data, to obtain within-neighborhood pre-period covariates. I observe housing variables such as self-reported home-value and rental amounts as well as demographic information such as the race and ethnicity of residents[12]. I geocode these addresses and assign them to the existing map of Los Angeles and corresponding HOLC administrative data.

1.3.3 Crime Data

For the between-city study, I use individual level National Incident Based Reporting System (NIBRS) crime victimization data from 2015 and collapse it by reporting agency, assigning each

[11] T-RACES (http://salt.umd.edu/T-RACES/) publishes HOLC administrative documents for many cities in California.

[12] I do not observe migration or education attainment, since these were not introduced to the Census surveys until 1940.

agency to the city which it polices. Summary statistics are reported in Table 1.2. I also use agency-month level FBI Uniform Crime Reports (UCR) from Kaplan (2018), which I collapse to obtain city-year and city-decade level crime counts and rates. Summary statistics are reported in Table 1.3.

For the within-city or neighborhood level study, I use geocoded crime data from the city of Los Angeles. These data contain exact location of the crime as well as a description of the crime for the universe of crimes in Los Angeles beginning in 2010. I then use string searches over crime descriptions to classify crimes as UCR Part 1 Property Crimes and UCR Part 1 Violent Crimes[13]. I assign these data to the georeferenced map that contains HOLC administrative data. Summary statistics are reported in Table 1.1.

Crime is distributed unevenly across Los Angeles in 2010. Figure 1.10 presents a standard Gini coefficient diagram which displays how crime in Los Angeles in 2010 is distributed across the neighborhoods HOLC delineated in 1939. Because the Gini curve departs from a 45 degree straight line which would represent an equal distribution, we can see that the burden of criminal victimizations are not born evenly across all neighborhoods. Figure 1.10, in particular, shows that the most dangerous 10 percent of neighborhoods bear 80 percent of the total crime burden. I will explain some of this inequity by studying the effects of redlining practices.

1.4 Between-City: City-Level Effects of Redline-Mapping on Crime

In this section, I utilize an unannounced population cutoff which determined whether or not a city was redline-mapped to show that redline-mapping increased city level crime. Later, in Section 1.7 I use this same between-city variation to examine possible mechanisms for how redline-mapping increased crime and find that, in addition to increasing crime, redline-mapping also increased racial segregation, decreased educational attainment, and harmed housing markets.

[13]Property crimes include burglary and motor vehicle theft, while violent crimes include murder, robbery as well as physical and sexual assault.

1.4.1 Which Cities Were Mapped and Why?

HOLC residential security maps were made for 239 US cities including every every modern, major metropolitan area. Despite the broad coverage of the maps, hundreds of cities and smaller towns were never mapped. I obtained archival data that lists all cites for which HOLC maps were constructed and folded this data into Census data from 1930 and beyond. In doing this, I discovered an unannounced population cutoff which nearly perfectly determines mapping status. As Figure 1.2 shows, having a 1930 population above 40,000 nearly guaranteed that a city would be mapped, while having a population below 40,000 nearly guaranteed that a city would not be mapped.

While smaller in population than the largest and most often studied metropolitan areas, cities within a reasonable bandwidth about the 1930 population cutoff of 40,000 are still home to significant numbers of US residents. In 1930 approximately one third of the US population lived in cities with 50,000 or less people.[14] In California, representative cities (whose 1930 population was near the cutoff) include Stockton, Fresno and San Jose (which were redline-mapped) as well as Santa Barbara, Santa Monica and San Bernardino (which were not redline-mapped); In Texas, representative cities include Austin, Galveston and Waco (which were redline-mapped) as well as Lubbock, Laredo and Corpus Christi (which were not redline-mapped). Table 1.4 contains a list of representative cities. Figure A2 shows the regional breakdowns of cities near the threshold. Midwestern and Northeastern cities are slightly overrepresented, but there are cities from each region in the main bandwidths I consider.[15]

[14]In 1930, 44% of the population resided in areas classified as rural.*1930 Decennial Census Factbook* (Ch.2 V2 pgs.5-6)

[15]Conditional on 1930 city population, city region does not predict whether or not a city was redline-mapped.

1.4.2 Estimation: City Level

I use a regression discontinuity model to identify the city level impact of being redline-mapped on crime. I estimate regressions of the form:

$$Crime_c = \tau Above_c + \beta Pop30_c + \gamma Above_c \times Pop30_c + \epsilon_c. \tag{1.1}$$

where $Crime_c$ is the count of crimes in city c, $Pop30$ is the 1930 population of city c. This regression uses 1930 city population as the running variable variable and fits a local linear polynomial on either side of the mapping population cutoff of 40,000 people.[16] I am primarily interested in τ, the coefficient on $Above$, an indicator variable which equals 1 when city's population is above the population mapping cutoff (40,000 people) and zero otherwise; τ, the coefficient on $Above$, measures the average jump that occurs at the population cutoff conditional on the local linear polynomials.

1.4.3 City-Level Crime Effects

Figure 1.3 shows evidence that HOLC mapping increased the total city level volume of crime victimization in 2015. I use National Incident Based Reporting System (NIBRS) data on crime victimizations, restrict to UCR classified Part 1 Property and Violent crimes and further break down crime victimization outcomes by race and ethnicity. I then estimate whether cities whose 1930 city population was just above the cutoff have significantly higher volumes of crime victimization than cities whose 1930 population was just below the cutoff. I find that Black crime victimizations appear to nearly double across the mapping threshold, while Hispanic crime victimizations increase by more than 70%, although this latter estimate is significant only at the 15 percent level. The estimates reported in Figure 1.3 (and obtained by estimating Equation 1.1) imply that 176 Black and 65 Hispanic crime victimzations are attributable to redline-mapping. Figure 1.4 shows that

[16] I use the methods in (14) to find optimal number of bins and the optimal bandwidth.

these estimates are robust across a wide array of bandwidths.[17]

Figure 1.5 shows that these results are comparable to those obtained using the FBI, Uniform Crime Reports, which measure arrests by city by race. The estimates reported in Figure 1.5 (obtained by estimating Equation 1.1 on UCR data) imply that 61 additional Black arrests per city in 2015 are attributable to redline-mapping.[18] Thus, if we assume that the additional Black arrests are for crime perpetrated against Black victims, these estimates would suggest an arrest rate of roughly 35%, which is not far from the national average for UCR Type 1 crimes.[19]

Aside from providing a useful comparison to measures in the NIBRS dataset, the main reason to utilize the UCR measures of crime is that the UCR dataset has robust city coverage that spans many decades and hence the UCR data allows me to understand the dynamics of the effects of redline-mapping on crime. Figure 1.6 shows the dynamics of the impact of redline-mapping on crime over the entire period after the Fair Housing Act. The estimates across decades which are reported in Figure 1.6 (and obtained by estimating Equation 1.1 over UCR decadal data) show that the effects of redline-mapping peaked in the period around the passage of the Fair Housing Act of 1968, and, while having been mitigated to some extent in subsequent decades, nevertheless persist into the present day.[20]

1.4.4 City-Level Pre-Period Balancing

In order to interpret the estimates we just discussed as causal effects, it is necessary to show that before the HOLC redlining-mapping was done, the cities about the threshold did not already

[17] Figure 1.3 displays a regression discontinuity diagram using the optimal numbers of bins according to (14). To display more of the variation, Figure A4 gives the same regression discontinuity diagrams as Figure 1.3 but with more than the optimal number of bins.

[18] Preliminary tests on the distribution of crime as measured by NIBRS and UCR reveal that these datasets give consistent measures of criminality by race. To test consistency, for example, I construct a variable that measures the difference between Black (UCR Type 1) crime victimizations reported to NIBRS and Black (UCR Type 1) criminal arrests reported to the UCR. This variable, which measures consistency between the two datasets, is very nearly mean zero, and, more importantly, does not jump at the 40,000 population threshold. This suggests that whatever noise there is in these data at the city level is an instance of classical measurement error or at least not connected to redline-mapping.

[19] https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/clearances

[20] The same estimates in rates per 1,000 persons are given in Appendix Figure A5.

exhibit jumps across the threshold for any covariate which could reasonably be said to be connected to contemporary crime volumes. Ex ante, it seems unlikely that cities with slightly more than 40,000 people and those with slightly less than 40,000 would systematically differ from each other, however, to be cautious, I use 1920-1930 Census data to show that observable city covariates are smooth across the threshold.[21]

As in the neighborhood-level balancing tests (Section 1.5), I focus my balancing tests on pre-period measures of the percent of households that are Black, the percent that are Hispanic, as well as self-reported home values and rent values. I am most concerned about these covariates because any pre-period discontinuity in racial composition or socio-economic status could be used to construct a plausible endogeneity story in which the pre-existing racial or socio-economic difference could be claimed as the common cause of both the choice of the population-cutoff and the future crime volume. Figure 1.7 shows RD diagrams for these four covariates. We can see that they do not exhibit significant jumps about the threshold introduced by the population cutoff. Figure 1.8 shows RD estimates for these same covariates across a range of bandwidths. To pass covariate smoothness tests, I should not observe a statistically significant, nonzero estimate; indeed, Figure 1.8 shows that these four covariates pass the test for a wide range of bandwidths.

1.5 Within-City: Neighborhood-Level Effects of Redlining on Crime

Section 1.4 shows that redline-mapping increased city level crime. In this section, I complement the between-city analysis with a within-city or neighborhood level analysis for 3 reasons: (a)

[21] To the best of my knowledge there does not exist crime data from 1930 which covers large numbers of cities. (UCR historical data, which predates 1960, contains at most 400 agencies, all of which lie in large metropolitan areas.) Thus I cannot directly test for jumps in crime at the city level in the pre-period. However, in Figure A7 I use the group quarters variable from the 1930 Census to test for city level pre-period differences in the share residing in institutional group quarters. Because this variable measures not only individuals who are incarcerated, but also many non-incarcerated individuals (see note to Figure A7) it does not constitute an ideal variable to measure pre-period criminal activity. Nevertheless, if more individuals in cities just above the population threshold are incarcerated in the pre-period, and the institutional group quarters variable measures this difference across the threshold, this could be an indication of higher pre-existing crime rates in the cities just above the threshold, if we believe that the percent incarcerated is an increasing function of criminal perpetration. However, the results in Figure A7 show that, if anything, mapped cities had a smaller share of incarcerated individuals and incarcerated black individuals compared to non-mapped cities. Both because there is considerable variation in these bins and the group quarters variable in 1930 measures certain non-incarcerated individuals together with the incarcerated, these estimates should be treated with caution.

to better understand where within the city these increased crimes occurred, (b) to better understand the determinants of neighborhood quality designations found on redline-maps and (c) to assess whether or not neighborhood quality designations on redline-maps were partly motivated by racial animus.

1.5.1 Why use a Spatial RD?

One motivation for trying to identify the long run effects of restricting credit access on crime comes from considering the extent to which contemporary crime phenomena can be explained by demographic persistence: neighborhoods tend to retain similar demographies over time and neighborhood demography is correlated with crime volume. The first column of Table 1.7 shows that in Los Angeles having a significant Mexican population in a neighborhood in 1939 is associated with 380 more violent crimes in 2010, which could motivate an explanation of the distribution of crime in terms of demographic persistence. However, column 2 of Table 1.7 shows that when we control for HOLC color assignments the association attributable to demographic persistence no longer holds; we also see that 2010 crime is monotonic in HOLC security grade, red neighborhoods having the most crime, yellow the next most, etc. These estimates are not causal, but they suggest that what could appear to be the effects of demographic persistence could actually be due to housing policy.

I use contemporary crime data from the city of Los Angeles in a spatial regression discontinuity framework to estimate the causal effect of credit access restrictions on crime. A spatial RD is especially well suited to estimate these effects. In the absence of HOLC mapping, whatever taste based discrimination there was in the loan market would have still existed and the racial composition of neighborhoods would likely still have impacted credit access. However, it is also likely that individual lenders in the private market would have had heterogeneous beliefs about *exactly* where the “good” and “bad” neighborhoods began and ended. This variation would, in all likelihood, have led to credit access being smooth across the would-be HOLC borders. Therefore, when HOLC created its borders, it aligned lenders' beliefs and expectations and introduced a sharp discontinuity where one likely would not have existed before. As Hanchett puts it:

"The HOLC's work served to solidify practices that had previously only existed informally. As long as bankers and brokers calculated creditworthiness according to their own perceptions, there was considerable flexibility and a likelihood that one person's bad risk might be another's acceptable investment. The HOLC **wiped out that fuzziness** by getting Charlotte's leading real estate agents to compare notes, and then publishing the results. The handsomely printed map with its **sharp-edged boundaries** made the practice of deciding credit risk on the basis of neighborhood seem objective and put the weight of the U.S. government behind it." ((15) p. 231, bolding added)

Figure 1.13 depicts exactly this relationship: while prior to the discretionary creation of HOLC's borders, there would not exist discontinuously different level of credit access across the border, after HOLC mapping choices were made, there would likely result decreased credit access inside the redlined regions and increased access outside the redlined regions since these neighborhoods would enjoy a newly bolstered credit market with amortized loan schemes and federal lending insurance (See Section 2.2 for more details about relevant policy efforts to strengthen the credit market and encourage lending.)

1.5.2 Estimation: Neighborhood-Level

I use a spatial regression discontinuity model to estimate the neighborhood level impact of being redlined (assigned a security grade "red") on crime. I estimate regressions of the form:

$$Crime_{nd} = \tau Redlined_d + \beta DtoRedline_n + \gamma Redlined_d \times DtoRedline_n + \epsilon_{nd}. \quad (1.2)$$

where $Crime_{nd}$ is the count of crimes at a given distance d miles away from a given redlined neighborhood n, $DtoRedline$ is the running variable constructed as the distance from a given location in the city to the nearest redline on the map; $DtoRedline$ is zero on the redline itself. This regression uses distance to the nearest redline as the running variable and fits a local linear polynomial on either side of the redline-cutoff, which is where the distance away from from the redline equals zero.[22] I am primarily interested in τ, the coefficient on $Redlined$, an indicator variable which equals 1 when the point falls inside a redlined neighborhood, but is zero elsewhere; τ, the coefficient on $Redlined$, measures the average jump that occurs at the redline conditional on the local linear polynomials.

1.5.3 Neighborhood-Level Crime Effects: Contemporary Los Angeles

Figure 1.14 presents regression discontinuity diagrams for property and violent crime counts respectively in Los Angeles in 2010. These diagrams show that, inside redlined neighborhoods, we find a higher volume of crime than in neighborhoods that received some other color grade. This confirms the monotonic pattern we already saw in Table 1.7: neighborhoods awarded lower grades by HOLC in 1939 have higher 2010 crime volumes.

Table 1.8 shows estimates of the discontinuity at the redlining threshold[23]. I find that that, on average, crime jumps by approximately 34 property crimes and 35 violent crimes at the border of redlined neighborhoods. These represent increases of over 50% relative to the mean crime volume within the bandwidth, and increases of 17% and 29% respectively relative to the mean crime volume of the neighborhoods which were graded something other than red. Lastly, Figure 1.15 shows that these estimates are robust to a large set of bandwidth choices.

1.5.4 Neighborhood-Level Pre-Period Balancing

In order to interpret the estimates we just discussed as causal effects we must assume that, other than the redlining of a neighborhood, no determinant of crime is discontinuous at the redlining threshold. In order to test this assumption, I check whether before the HOLC maps were put in place these neighborhoods did not already exhibit jumps across the threshold for any covariate which could reasonably be said to be connected to contemporary crime volume. Using geocoded

[22] I use the methods in (14) to find optimal number of bins and the optimal bandwidth.

[23] All estimations are done by fitting local linear polynomials on either side of the threshold and calculating the jump at the threshold. I use the methods in (14) to find optimal number of bins and the optimal bandwidth.

Census data from 1920 and 1930, I show smoothness across the threshold for a large set of covariates including measures of property value, family structure, demography, labor force participation and literacy. Of course, many of these pre-period covariates differ across the neighborhoods *as a whole* because HOLC graded neighborhoods based on some of these very characteristics (see Figure 1.18); I wish to show only that the covariates are smooth as we zoom into the region around the borders of the redlined neighborhoods.

I focus my balancing tests on pre-period measures of the percent of households that are Black, the percent that are Hispanic, as well as home values and rent rates. I am most concerned about these covariates because pre-existing differences in racial composition or socio-economic characteristics would suggest an alternative explanation for the association between color-assignments and future crime volumes across the threshold. Figure 1.16 shows RD diagrams for these four covariates. We can see that they do not exhibit significant jumps about the threshold introduced by the redline. Figure 1.17 shows that these four covariates pass a balancing test for a wide range of bandwidths. For completeness, I estimate analogous balancing tests for every available covariate including measures of measure household demography, family formation, as well as education and labor market outcomes. Table 1.9 shows that nearly all of these pass the balancing test. When I use multiple inference methods to correct the p-values to account for the fact that I am testing for large numbers of covariates, I find that no covariate is statistically significant[24].

1.5.5 Were Redlining Assignments Motivated by Racial Animus?

To better understand the determinants of the assignments of neighborhood quality on HOLC Security Maps, I use a novel dataset of HOLC administrative data (See Figure 1.18). In particular,

[24] (9), who are looking across over one hundred cities, find evidence of discontinuous jumps in several covariates which I find to be smooth. For example, they show (in their Figure 4 and Figure A3) that there are gaps across the red-yellow border in percent black as well as homeownership and home values (they do so using a bandwidth of .25 miles). There could several explanations for why I do not find these jumps. First, my running variable is distance from any non-red neighborhood to the nearest redline, whereas they are considering distance from any yellow neighborhood to a redline. Secondly, from examining the diagrams, it seems that some of the smaller jumps diagrams *might* turn out to be statistically insignificant after a multiple inference correction. Lastly, and most importantly, (9) are looking at these covariates for over one hundred cities, whereas I am considering only Los Angeles.

in this section, I provide the first quantitative evidence that HOLC assignments were partly driven by racial animus. To show this I focus on the "1.e Shifting or Infiltration" response field on the HOLC survey sheets (see Figure 1.18). This response field of the survey sheet is where surveyors were asked to record their expectations concerning future racial composition of the neighborhood they surveyed. In Los Angeles, each of the 416 HOLC delineated neighborhoods received a survey sheet, with this field response. Table 1.10 shows a sample of text responses surveyors in Los Angeles made on this line. It is not difficult to see that the language is racially charged and shows a clear stated preference for white, nationally-born households.

To test whether or not the racial animus apparent in these stated preferences is associated with differential neighborhood risk grades, I run an ordered logit regression where the ordinal HOLC security grade is the outcome variable and a rich set of indicators drawn from the "Shifting or Infiltration" responses are independent variables. Table 1.11 reports the marginal effects derived from these regressions; all estimates are conditional on expectations about population increases and future wealth levels.. They show that a HOLC surveyor expressing his view that the black population in the neighborhood is likely to increase is associated with a 5% greater probability that the neighborhood would be redlined (graded "red"). The generic declaration that that the surveyor expected an increase in the presence of some "subversive" group in the neighborhood is associated with nearly a 2% increase in the likelihood of being redlined. Lastly, the surveyor noting the existence of a restrictive covenant in the neighborhood (which would prevent racial and ethnic minorities from moving into the neighborhood) decreased the likelihood of a neighborhood being redlined by nearly 4%[25]. Similar marginal effects can be obtained for the likelihood of being assigned a green, blue or yellow grade and the story that emerges is consistent: surveyor expectations of an increase in Black, Hispanic or other so-called "subversive" groups raised the risk score, while contrary expectations lowered it[26]. While these results do not clearly disentangle

[25] All results are conditional on expectations about overall population increased and expectations about the future wealth of residents who may move into the neighborhood

[26] An increase in the risk score means that a neighborhood is more likely to be colored red or yellow, while a decrease

statistical and taste based discrimination, they contribute quantitative evidence supporting the view that HOLC color assignments were at least partly driven by racial preferences for neighborhood composition. This evidence combined with the large body of existing anecdotal evidence (see Section 2.2) suggests that neighborhood assignments were partly driven by racial animus.

Showing that the policy was partly driven by racial animus influences how we interpret causal effects of HOLC assignments. If HOLC neighborhood assignments had long run effects on neighborhood crime, for example, showing racial animus behind the assignments strongly suggests that these neighborhood level effects are not simply an effective transfer of the crime burden from one arbitrary group of residents to another, but constitutes a transfer of crime burden away from one racial and ethnic group towards another.

1.6 Comparing Within-City to Between-City Estimates

1.6.1 Within-City and Between-City Estimators

This paper employs two regression discontinuity (RD) estimators to determine the effects of credit access on crime: the first RD estimates the between-city effects of a city being redline-mapped by HOLC (having a map constructed with red assignments) as compared to cities not redline-mapped (not having a map constructed at all), and the second RD estimates the within-city effects of neighborhoods being redlined (assigned grade "red") as compared to neighborhoods not redlined (assigned "non-red"). Broadly speaking, the within-city estimates show that redlining increased crime in redlined neighborhoods and the between-city estimates show that the construction of a redlining map increased overall city level crime. This section derives a general framework that allows me to compare the within-city and between-city estimates. This framework demonstrates that if the size of the difference between the estimates is large enough, redlining *increased* overall city level crime in redline-mapped cities while at the same time *decreasing* would-be crime levels in neighborhoods in the redline-mapped cities which were not redlined (assigned a "non-red"

means that a neighborhood is more likely to be colored blue or green grade).

Imagine an experimental environment in which there are c cites, each of which has n neighborhoods. Let a credit restriction ("redline-mapping") be randomly assigned to k of the n neighborhoods for l of the m cities ("mapped" cities); for $m - l$ cities no restrictions are placed on the credit market ("non-mapped cities"). At a later time period, we measure neighborhood-specific crime levels, y_{ij}, for neighborhood i and city j. I distinguish three distributions of the outcome, y_{ij}, based on these randomly assigned treatments:

$$\begin{cases} y_{H,ij} & \text{Redlined Neighborhood } i \text{ in Mapped City } j \\ y_{L,ij} & \text{Non-Red Neighborhood } i \text{ in Mapped City } j \\ y_{0,ij} & \text{Neighborhood } i \text{ in Non-Mapped City } j \end{cases}$$

(See Figure 1.19 for a diagrammatic represenation of the cases.) A positive within-city estimate would entail that, on average, crime is higher in redlined neighborhoods than in non-red neighborhoods ($Ey_{H,ij} > Ey_{L,ij}$). A positive between-city estimate would mean that, on average, crime is higher in mapped cities than in non-mapped cities ($E(\sum_{i=1}^{k} y_{H,ij} + \sum_{i=1}^{n-k} y_{L,ij}) > E\sum_{i=1}^{n} y_{0,ij}$). However, even if redlined neighborhoods had higher crime than non-red neighborhoods and mapped cities had higher crime than non-mapped cities, the relationship between crime in the non-red neighborhood of a mapped city and a neighborhood in an unmapped city ($Ey_{L,ij}$ and $Ey_{0,ij}$) is theoretically ambiguous. This relationship can help answer the question: did redlining increase overall city-level crime by increasing crime *only* in redlined neighborhoods, or did it also transfer some of the would-be crime from the non-red neighborhoods to the redlined neighborhoods? To address this question I derive the conditions under which we would find evidence of such a transfer, namely, when, on average, crime in redlined neighborhoods is higher than crime in neighborhoods in non-mapped cities, which is itself higher than crime in the non-red neighborhoods of mapped cities ($Ey_{H,ij} \geq Ey_{0,ij} \geq Ey_{L,ij}$).

Because I assume random assignment of credit restrictions in this Section, the between city and within city treatment effect estimates can be computed as straightforward differences of means. For a random assignment of credit restrictions to k of the n neighborhoods inside l of the m cities, the estimators are:

$$\hat{\beta}_{w/in} = \frac{1}{k}\sum_{i=1}^{k} y_{H,ij} - \frac{1}{n-k}\sum_{i=1}^{n-k} y_{L,ij} \tag{1.3}$$

$$\hat{\beta}_{b/t} = \frac{1}{l}\sum_{j=1}^{l} \underbrace{\left[\sum_{i=1}^{k} y_{H,ij} + \sum_{i=1}^{n-k} y_{L,ij}\right]}_{\substack{\text{Total Crime}\\ \text{In Mapped}\\ \text{City } j}} - \frac{1}{m-l}\sum_{j=1}^{m-l} \underbrace{\sum_{i=1}^{n} y_{0,ij}}_{\substack{\text{Total Crime}\\ \text{In Non-Mapped}\\ \text{City } j}} \tag{1.4}$$

for neighborhood i and city j. Substituting Equation 1.3 into Equation 1.4, and normalizing k to 1,[27] we discover the conditions under which the within-city estimate would exceed the between-city estimate:

$$\hat{\beta}_{w/in} > \hat{\beta}_{b/t} \iff \underbrace{E(y_{0,ij})}_{\substack{\text{Average Crime}\\ \text{In Neighborhood}\\ \text{Inside Non-Mapped City}}} > \underbrace{n \times E(y_{L,ij})}_{\substack{n\,\times\,\text{Average Crime}\\ \text{In Non-Red Neighborhood}\\ \text{Inside Mapped City}}} \tag{1.5}$$

If, on average, crime in a neighborhood in a non-mapped city were larger than crime in a non-red neighborhood in a mapped city (being appropriately scaled up by the fraction of neighborhoods redlined[28]) then this would imply that non-red neighborhoods in the mapped cities benefited from the mapping process: crimes that would have been in those non-red neighborhoods had the city not been mapped were transferred into the redlined neighborhoods because of the redline-mapping.[29]

[27]This would render the effective number of neighborhoods to be $\frac{n}{k}$. Intuitively, instead of having k redlined neighborhoods and $n-k$ non-red neighborhoods, we would now have 1 large redlined area and $\frac{n}{k}$-1 non-red neighborhoods.

[28]Intuitively, multiplying by $\frac{n}{k}$ simply scales up the crimes in the non-red areas of the mapped cities to account for the fact that all neighborhood in non-mapped cites are being compared to only the non-red share of neighborhoods in the mapped cities.

[29]This result also shows that it would be rational for a person living in a would-be highly ranked neighborhood whose preferences do not involve neighborhoods other than her own, to prefer her city to be mapped.

1.6.2 Within-City and Between-City Estimates

In Section 1.6.1 just above, I showed that, in an experimental context with random assignment of redline-mapping both within and between cities, if the within-city estimate were larger than the between-city estimate this would constitute evidence that crimes that would have been in those non-red neighborhoods had the city not been redline-mapped were transfered into the redlined neighborhoods because of the redline-mapping. In this section, I take my quasi-experimental within-city and between-city estimates and compare their sizes to test for evidence of such crime transfers. Evidence for a crime transfer would suggest that redline-mapping *decreased* crime in neighborhoods not graded red by transfering crime that would have been in these predominantly White neighborhoods into redlined neighborhoods.

The within-city estimate imply that, on average, redlining caused 67 more crimes per redlined area, implying that for Los Angeles as a whole redlining caused there to be 6968 more crimes to be in redlined neighborhoods compared to neighborhoods not redlined (Section 1.5). The between-city estimates imply that, on average, redlining caused 241 more crimes to occur in a city that was redline-mapped than in a city not redline-mapped. Scaling these estimates based on differences in city population between Los Angeles and cities in the bandwidth for being redline mapped, I find that the between-city estimates are 30% the size of the within-city estimates. This, together with the result in Equation 1.5, provides evidence that, on average, crime in a neighborhood inside a city not redline-mapped is greater than crime in a neighborhood inside a city that was redline-mapped but was not itself redlined (was assigned a grade other than "red"). This suggests that redline-mapping *reduced* crime in neighborhoods not graded "red" by transferring crime that would have been in these neighborhoods if a redlining-map had *not* been drawn into the redlined neighborhoods.

1.7 Mechanisms: How Did Redlining Increase Crime?

1.7.1 Racial Segregation as a Mechanism

There exists evidence that present-day racial segregation is correlated with reduced intergenerational mobility ((3)), is associated with increases in the black-white SAT test score gap ((16)), and that racial segregation is causally responsible for lower income and educational attainment for blacks as well as increased crime ((17), (18)).[30] Thus, one way redline-mapping may have increased crime is by increasing racial segregation.

To empirically test the hypothesis that racial segregation is a channel through which redline-mapping increased crime, I consider racial segregation as an outcome variable in Equation 1.1. Figure 1.20 shows a panel of city-level regression discontinuity diagrams where the outcome is White-Black racial segregation in a given year as measured by the White-Black Dissimilarity Index (a standard measure of racial segregation in cities).[31] Figure 1.20, subfigure (a), shows a placebo test for White-Black segregation in the period just before redline-mapping was implemented: I find no significant difference in White-Black racial segregation across the population threshold in 1930. Figure 1.20, subfigures (b)-(c), show estimates of the impact of redline-mapping on White-Black segregation in 1980 and 1990, respectively. (By 1980, cities which were redline-mapped had been subject to *de jure* discrimination in the credit market for approximately 30 years.) I estimate that in 1980 redline-mapping was responsible for an increase of 11 dissimilarity points, approximately a 24% increase off the mean. This estimate is significant at the ten percent level (See Figure 1.20, subfigure (b)). I separately estimate that in 1990 redline-mapping was responsible for an increase of 8 dissimilarity points, approximately a 19% increase off the mean. This estimate is significant at the fifteen percent level (See Figure 1.20, subfigure (c)). The estimate for 1980, for

[30](19) and (20) highlight the importance of studying racial segregation in the pre-World War II period (1900-1930): while the relocation decisions of white households from 1900-1930 ("White Flight") explain a large share of racial segregation, policies concerning zoning and public transit infrastructure have also affected racial segregation in the prewar era.

[31]A Dissimilarity Index of n implies that n percent of one race would have to move within the city and between neighborhoods in order for the neighborhood composition to reflect the overall city demography.

example, suggests that, as a result of being redline-mapped, in redline-mapped cities 11% *more* White households would have to move neighborhoods in order for each neighborhood to have the same racial composition as the city as a whole. Taken together, subfigures (a)-(c) of Figure 1.20 suggest that redline-mapping caused increases in racial segregation by slowing the rate at which racial segregation was otherwise declining at the national level. In other words, redline-mapping seems to have allowed racial segregation to persist longer than it would have in the absence of mapping.

To better understand the dynamics of the effects of redline-mapping on racial segregation, I also consider specifications in which I pool decadal measures of racial segregation from 1890-2010 based on whether they were (a) in the period prior to any redline-mapping (1890-1930), (b) in the period after redline-mapping was first implemented (1940-2010) or (c) in the period after both redline-mapping and the Fair Housing Act (1970-2010).[32] If the Fair Housing Act (and subsequent anti-discrimination laws), which ended *de jure* discrimination, mitigated the increases in racial segregation due to redline-mapping, we would expect the estimates from (c) to be attenuated versions of those from (b).[33] Figure 1.21, subfigure (a), presents a placebo test similar to that in Figure 1.20, subfigure (a), with the same result: there is not a significant difference in Black-White racial segregation across the population threshold prior to redline-mapping. Figure 1.21, subfigure (b), presents a regression discontinuity diagram that uses pooled city-decade level data from the entire period after redline-mapping was implemented (1940-2010); the reported estimate suggests that redline-mapping is responsible for an increase in racial segregation of 11.4 dissimilarity points (a 24% increase off the mean). Figure 1.22 shows that the estimates from Figure 1.21, subfigure (b), are robust to a wide array of bandwidths. Figure 1.21, subfigure (c), presents a regression discontinuity diagram that uses pooled city-decade level data from the period after both redline-

[32]For more on the timing of redline-mapping, the Fair Housing Act and other anti-discriminatory legislation, see the timeline in Figure 1.1.

[33]It is important to note that this strategy does *not* identify the causal effect of the Fair Housing Act nor the full interactive effect of the Fair Housing Act and redline-mapping.

mapping was implemented and the Fair Housing Act was passed (1970-2010). During this period (1970-2010), even though there was no *de jure* discrimination, there may have been *de facto* discrimination as well as lagged effects of prior *de jure* discrimination. The estimates from 1970-2010 (reported in Figure 1.21, subfigure (c)) are both smaller in magnitude and less strongly significant than those from 1940-2010 (reported in Figure 1.21, subfigure (b)).[34] If we attribute this reduction in the estimate (namely, the redution from Figure 1.21, subfigure (b), to Figure 1.21, subfigure (c)) to the Fair Housing Act (and other subsequent anti-discriminatory legislation), then we would conclude that the Fair Housing Act may have mitigated as much as 34% of the increase in racial segregation brought about by redline-mapping.

Taken together, the results from Figures 1.20 through 1.22 provide evidence that increases in racial segregation are a channel through which redline-mapping increased crime. I do *not* claim that increases in racial segregation are the *only* channel through which redline-mapping increased crime, however, comparing the magnitude of the effect of redline-mapping on segregation to the magnitude of the effect of redline-mapping on crime can give a useful back of the envelope estimate of the impact of racial segregation on crime. My estimates suggest that an 11.15 dissimilarity point increase in 1980 (see Figure 1.20, subfigure (b)) is associated with 11.42 additional black arrests per one thousand people in 2000 (see Figure A5). This suggests that, for black individuals born into a racially segregated neighborhood, a 10 percentage point increase in percent black is associated with a 1.02 percentage point increase in likelihood of arrest by adulthood.[35] These

[34] In subfigure (c), I find an 18% effect as opposed to a 24% effect in subfigure (b). The estimate in subfigure (c) is significant only at the 15 percent level, as opposed to the estimate in subfigure (b) which is significant at the 10 percent level

[35] Cohorts born in 1980 who commit crimes are likely to commit offenses that would be observed in 2000, thus my comparison is intended to be a back of the envelope estimate showing, for a black individual, the effects of being born into a city with more racial segregation on the likelihood of being arrested in adulthood. This back of the envelope calculation requires several strong assumptions to be taken as a causal estimate: (1) Racial segregation is the only channel through which redline-mapping increased crime, (2) all the new crimes observed in 2000 come from black individuals who were exposed to more segregation in 1980 (3) An increase in 10 dissimilarity points in a city (which, by definition, means that a city now has 10 percent more black residents who would have to move in order for the neighborhood-level racial distribution to match the city distribution) implies that at least some black children grew up with 10 percent more black children in their neighborhoods.

estimates are very close to those found in (18), and build on an existing body of evidence that shows that grouping together individuals who are at a high risk of committing crime increases the overall level of crime.[36]

1.7.2 Education as a Mechanism

Because there exists evidence that racial segregation is causally responsible for lower educational attainment for blacks ((17), (18)) and that lower educational attainment is causally responsible for increased crime ((21)), and, as we just saw in Section 1.7.1, evidence that redline-mapping increased racial segregation, reductions in educational attainment are a channel through which redline-mapping may have increased crime.

To empirically test whether reductions in educational attainment are a channel through which redline-mapping increased crime, I consider various measures of educational attainment as outcome variables in Equation 1.1. Figure 1.23 shows evidence that prior to redline-mapping there were not significant differences in education levels across the population threshold; in Figure 1.23 education levels are measured by the share literate, the best available measure of education in the 1930 Census. Figure 1.24 tests whether redline-mapping and the increases in racial segregation it caused influenced educational attainment at the city level.[37]. The estimates in Figure 1.24 imply that redline-mapping caused black individuals to be 4.4 percentage point less likely to finish high school (an 11% reduction off the mean) and 5.3 percentage points less likely to attend at least some college (a 25% reduction off the mean).

Thus, the estimates reported in Figures 1.20 through 1.24 suggest that redline-mapping decreased educational attainment for black individuals (possibly in part by increasing Black-White

[36] See citations in (18). (18) reports: "Our estimates suggest that a 10 percentage point increase in assigned school share minority led to an increase among minority males in the probability of ever being arrested and ever being incarcerated of about 1.3 percentage points, about a 7 percent increase relative to the mean for minority males in the sample". My back of the envelope calculation suggests that a 10 percentage point increase in percent black is associated with a 1.02 percentage point increase in the likelihood of being arrested for a black individual.

[37] Cohorts born in the 1980s would likely commit crimes observed in present day (2010-2015) crime data. Thus by choosing a 1980 measure, I am likely measuring parental educational attainment, which is strongly correlated with the educational attainment of the children whose criminal activity would be recorded in data from 2010-2015

racial segregation), which, in turn, increased crime. I do *not* claim that decreases in educational attainment (possibly occasioned by increases in racial segregation) are the *only* channel through which redline-mapping increased crime, however, comparing the magnitude of the effect of redline-mappping on educational attainment to the magnitude of the effect of redline-mapping on crime can give a useful back of the envelope estimate of the impact of educational attainment on crime. My estimates suggest that a 4.4 percentage point reduction in the likelihood of a black individuals completing high school in a city in 1980 (See Figure 1.24) is associated with 11.42 additional black arrests per one thousand people in 2000 (see Figure A5), which suggests that for every additional black high school graduate, .26 fewer black arrests will occur. When scaling for the share of arrests that result in incarceration, this estimate is larger than but consistent with (21), which finds that graduating high school reduces the likelihood of incarceration by 3.4 percentage points for blacks.[38]

1.7.3 Housing as a Mechanism

Because there exists evidence that home vacancies are causally responsible for violent crime ((22)) and that mortgage lending is responsible for decreased crime ((23)), harm to local housing markets that resulted in increased vacancies and decreased home ownership rates is a channel through which redline-mapping may have increased crime.

To empirically test whether harm to present day housing markets is a channel through which redline-mapping increased crime, I consider various measures of present day housing market strength as outcome variables in Equation 1.1. Table A1 shows regression discontinuity estimates with three such measures from the 2010 Census. These estimates suggest that redline-mapping increased home vacancy by 5 percentage points (a 43% increase off the mean), decreased the per-

[38](21)" "Overall, the estimates suggest that completing high school reduces the probability of incarceration by about .76 percentage points for whites and 3.4 percentage points for blacks". My estimate suggests that graduating high school reduces the likelihood of being arrested for blacks by 26 percentage points, which implies a reduction of incarceration by blacks of 12 percentage points. (I convert arrests to incarcerations using BJS averages (https://www.bjs.gov/index.cfm?ty=tp&tid=23)). The fact that my estimates are roughly four times larger than (21) is likely due to the fact that redline-mapping worked through channels other than educational attainment.

centage of homes underwritten by a mortgage by 7 percentage points (a 10% decrease off the mean) and decreased average monthly rental amounts by \$121 (a 15% decrease off the mean).

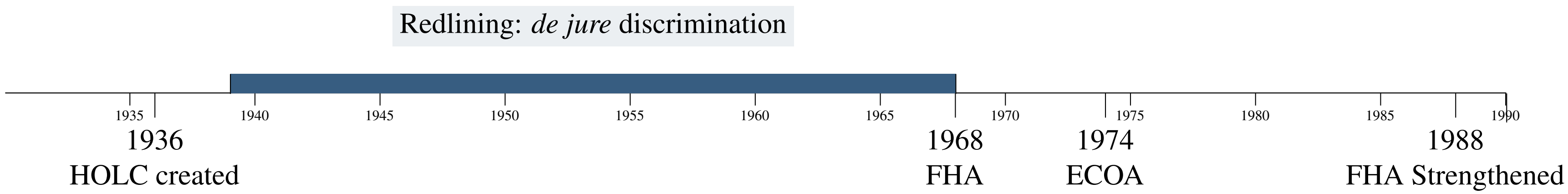
One way in which redline-mapping could have changed local housing markets is by changing the composition of housing stock. If would-be minority home buyers were prevented from accessing the credit market in redlined neighborhoods, there could arise an incentive for developers to favor large multi-family housing units over single family homes in these redlined neighborhoods. Table A2 reports estimates obtained by using various housing stock measures in various decades as outcome variables in Equation 1.1. The estimates in Table A2 provide little evidence that redline-mapping changed the composition of housing stock at the city level. Hence further research at the neighborhood level is necessary to identify whatever effects there may be of redlining on housing stock, and, more generally, to further elaborate how redline-mapping did lasting harm to local housing markets.

1.8 Conclusion

In the United States today, the welfare costs of crime are disproportionately born by households living in predominately African-American or Latino neighborhoods. This paper uses two regression discontinuity designs to show that Federal housing policies established in the wake of the Great Depression make present day contributions both to this inequity in the distribution of crime within cities and to the overall volume of crime in a city.

First, I use a regression discontinuity design that relies on a populatin cutoff that determined whether a city was redline-mapped to show that redline-mapping a city increased overall city-level crime. Secondly, I use a spatial regression discontinuity design to show that these neighborhood color-assignments and the restrictions to credit-access they initiated in the late 1930s causally influence the present day distribution of crime across neighborhoods in Los Angeles, California. Using neighborhood level archival data, I also provide the first quantitative evidence that racial animus seemed to drive the implementation of these 1930 housing policies. Next I compare the within-city and between-city estimates to show that that redline-mapping increased crime in redlined neighborhoods *both* by redistributing crime from predominantly White neighborhoods into redlined neighborhoods *and* by increasing the overall city-level of crime. Lastly, I use between-city variation in which cities were redline-mapped to identify mechanisms through which redlining increased crime. In particular, I show that redline-mapping increased city-level racial segregation, decreased education attainment for blacks and did harm to housing markets.

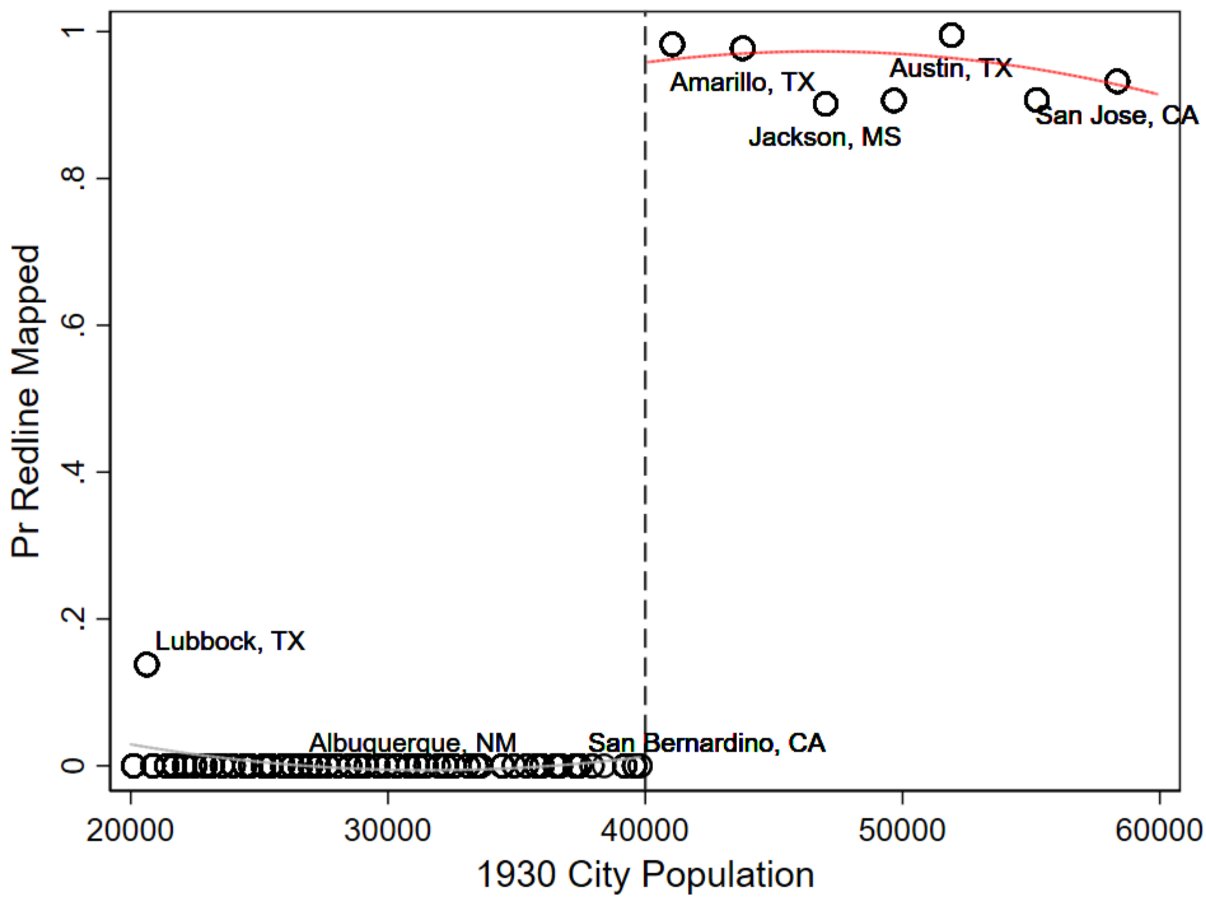
Figure 1.1: Timeline of *de jure* Discrimination Implemented by Redlining



Fair Housing Act (FHA) outlawed discrimination. Anti-discriminatory laws strengthened in 1974 (Equal Credit Opportunity Act) and in 1988.

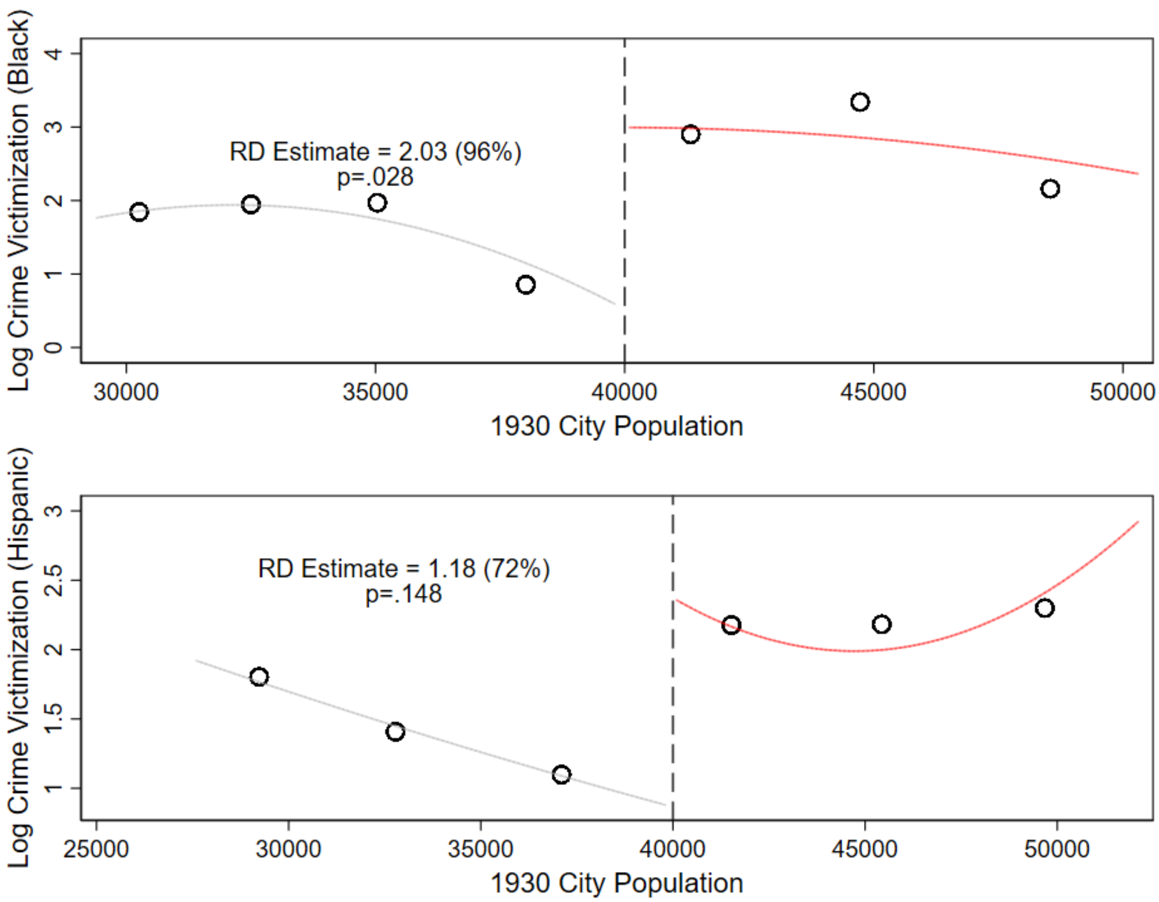
Note: Figure shows the period during which it was legal to discriminate ("*de jure* discrimination") in the loan market based on neighborhood demographics rather than applicant creditworthiness.

Figure 1.2: 1930 Population and Redline-Mapping: Between-City First Stage



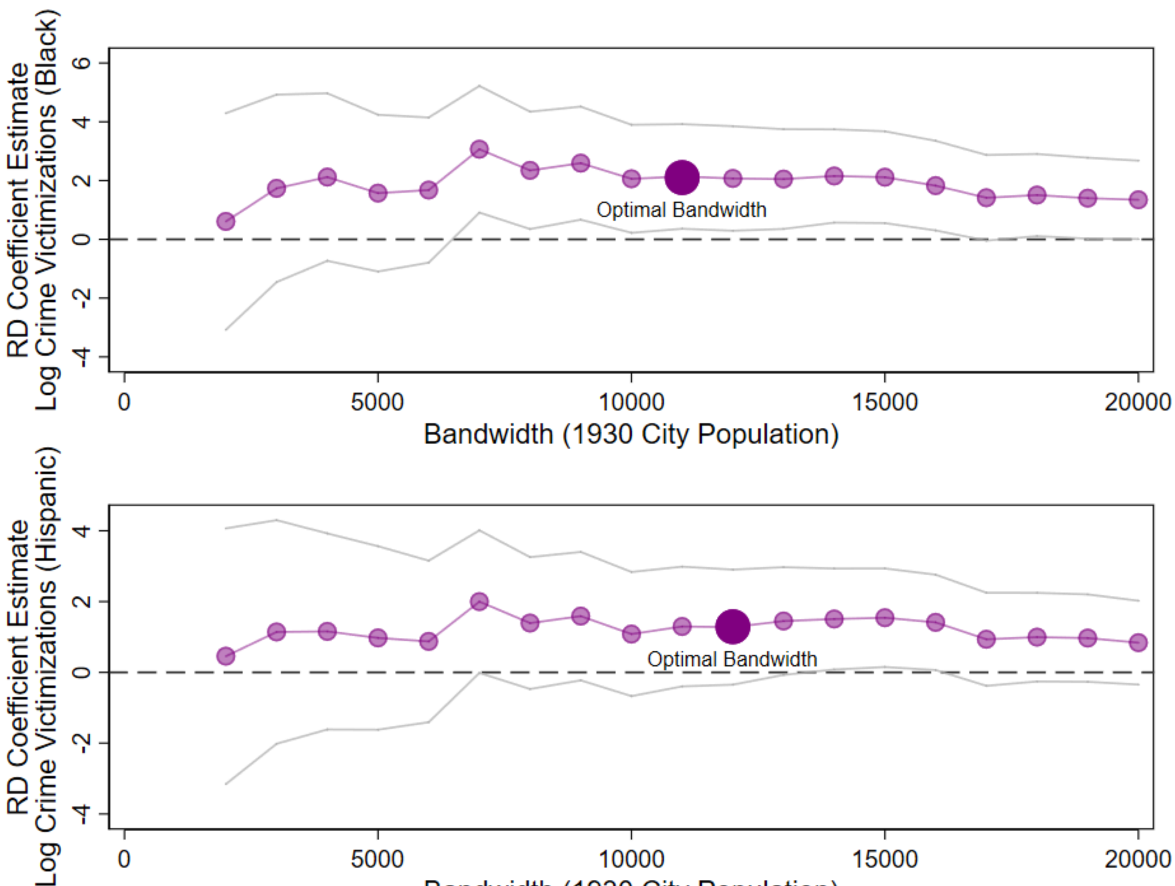
Note: Each figure shows a regression discontinuity diagram where the outcome variable is the likelihood that HOLC constructed a Residential Security Map ("Pr Redline-Mapped") for a given city in the 1930's. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size is chosen to be 20,000 people. Bin numbers are chosen optimally following (14). Data sources are Home Owner Loan Corporation (HOLC) archival records.

Figure 1.3: Impact of Redline-Mapping on Crime: Between-City Estimates



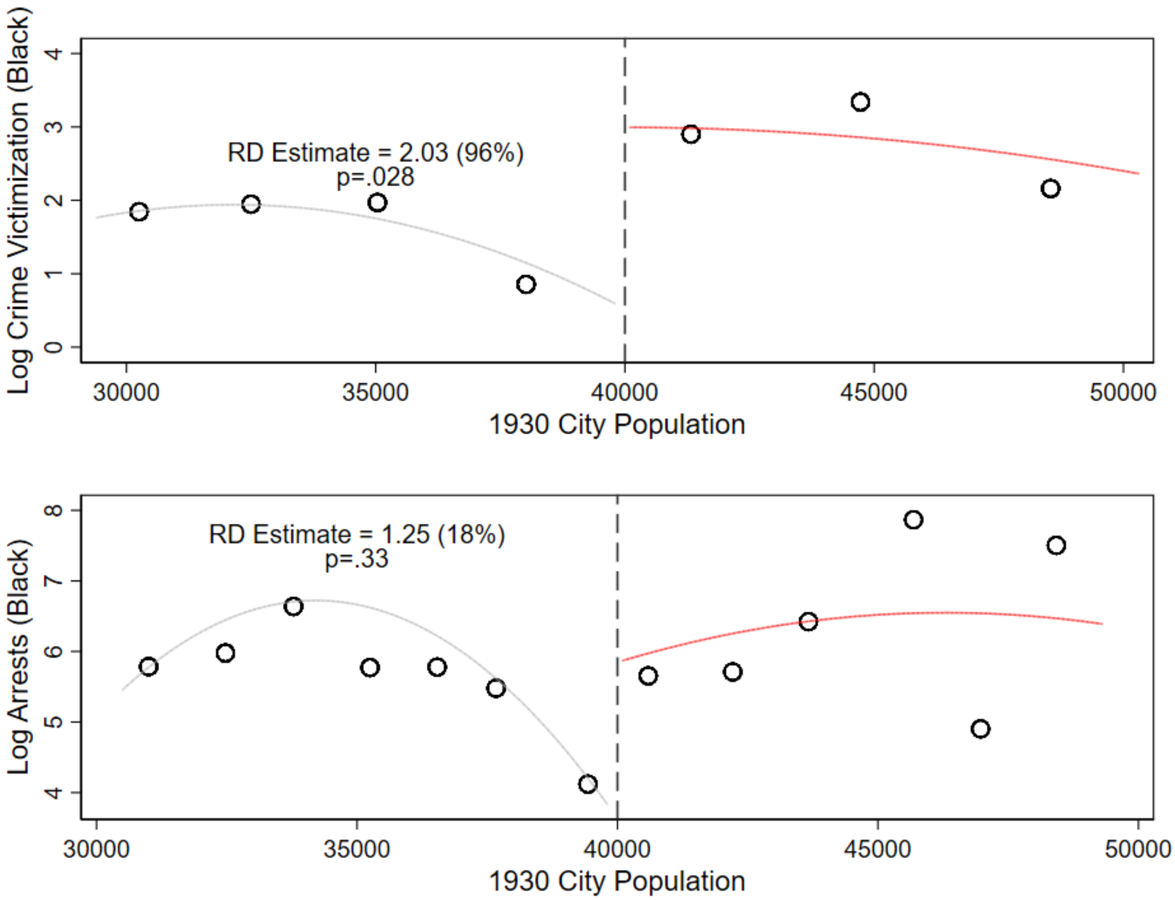
Note: Each figure shows a regression discontinuity diagram where the outcome variable is the log of crime victimizations in a given city in 2015. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). There are 133 agencies included in NIBRS 2015 data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates imply that 176 Black and 65 Hispanic crime victimizations per city in 2015 are attributable to redline-mapping. Data sources are individual-level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.4: Impact of Redline-Mapping on Crime: Between-City Estimates, by Bandwidth



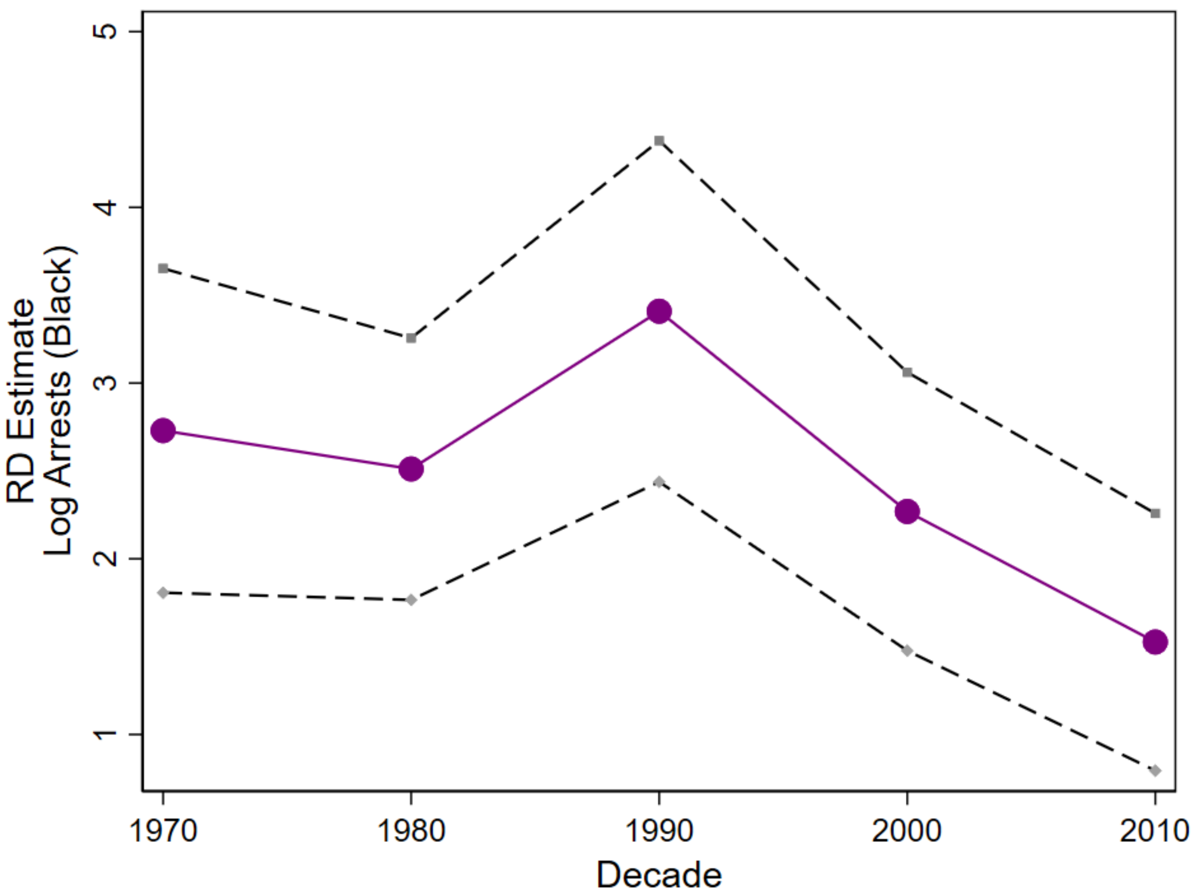
Note: Each figure shows a profile of regression discontinuity coefficent estimates and 95% confidence intervals across a range of bandwidth selections. The outcome variable is the log of crime victimizations in a given city in 2015. The top panel show results for the log of Black crime victimizations, while the bottom panel shows results for the log of Hispanic crime victimizations. The running variable is always 1930 city population. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth. Data sources are individual-level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.5: Impact of Redline-Mapping on Crimes and Arrests: Between-City Estimates



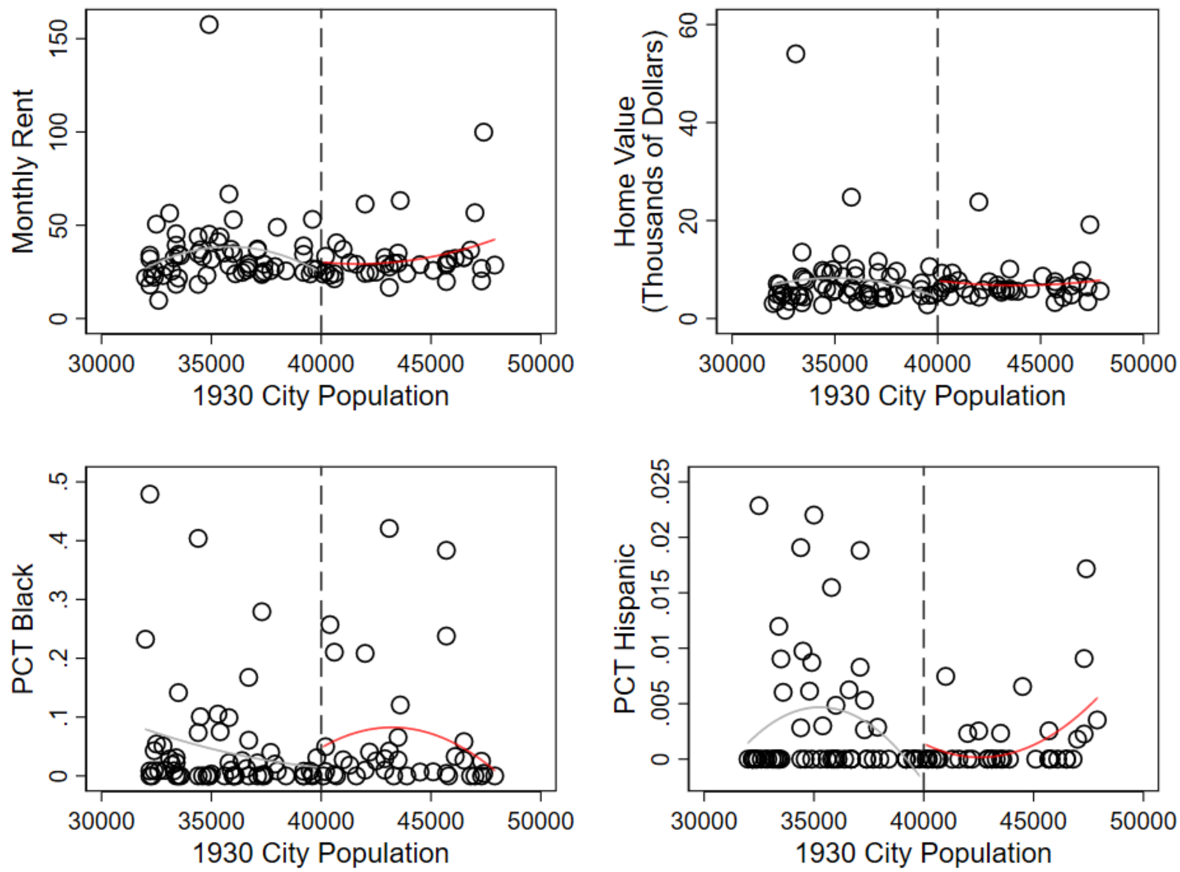
Note: Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the log of crime victimization in a given city in 2015, while in the bottom panel the outcome variable is the log of arrests in a given city in 2015. In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). In the top panel, there are 133 agencies included in NIBRS 2015 victimization data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates in the top panel imply that 176 Black crime victimizations per city in 2015 are attributable to redline-mapping. In the bottom panel, there are 131 agencies included in UCR 2015 arrest data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 82 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates in the bottom panel imply that 61 Black arrests per city in 2015 are attributable to redline-mapping. Data sources are UCR arrest data and NIBRS victimization data, as well as and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.6: Impact of Redline-Mapping on Arrests: Between-City Estimates Over Decades



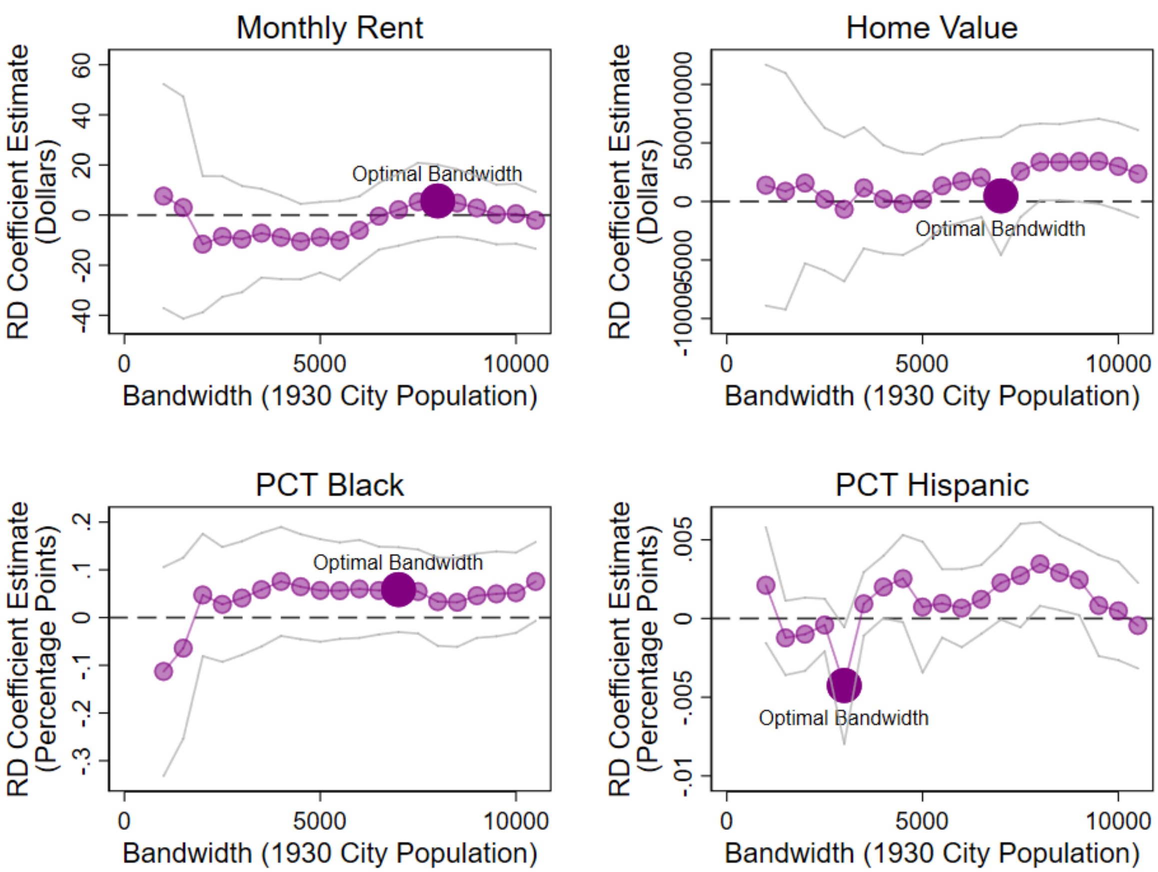
Note: The figure shows a profile of regression discontinuity estimates and 95% confidence intervals obtained by estimating Equation 1.1 on decadal UCR data. (Decadal UCR data is obtained by pooling monthly UCR data across decades.) In each estimate the outcome variable is the log of black arrests in a given city in a given decade. Data sources are UCR arrest data (1974-2016) and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.7: Balancing Tests: Between-City 1920-1930 Covariates



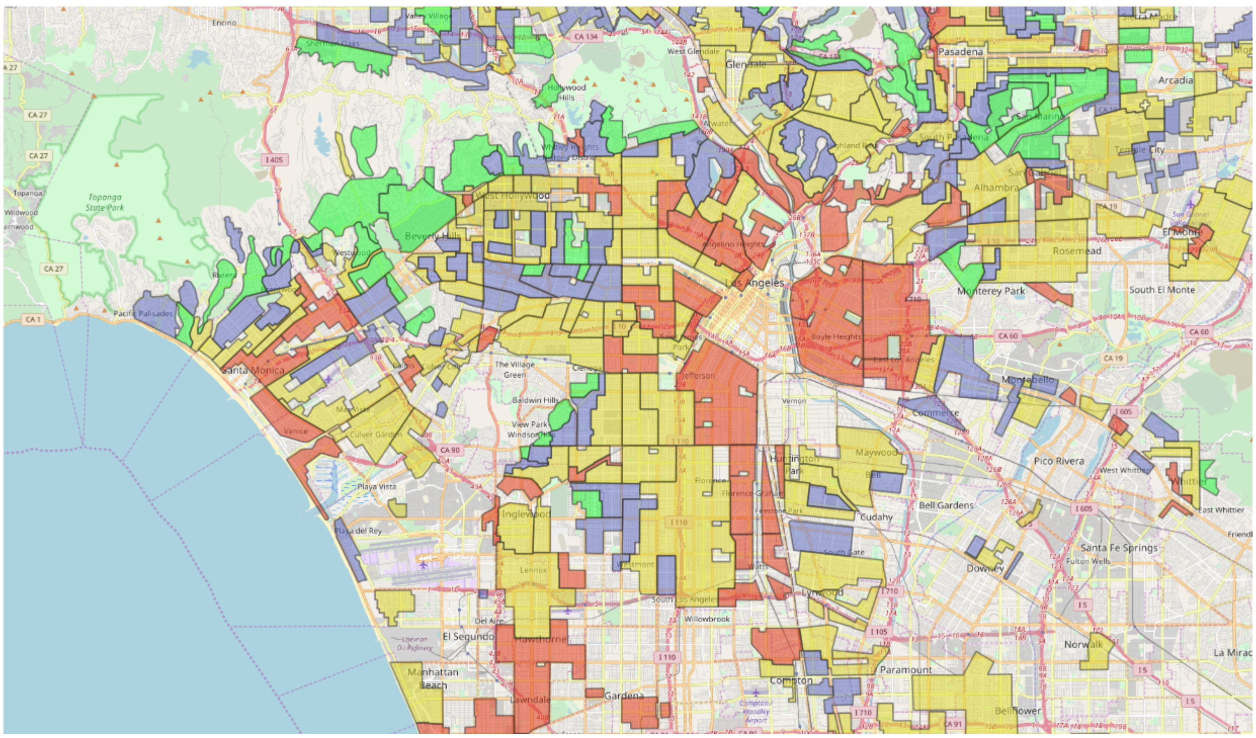
Note: Each figure shows a regression discontinuity diagram where the dependent variable is given city-level pre-period covariate measured in 1920-1930. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a city's population that is Black and the percent that is Hispanic, respectively. Circles represent bin means, while lines represent fitted quadratic curves. Bin number is fixed at 80 cities to ease comparison. The running variable is always 1930 city population. Bandwidth size is fixed at 7,000 people to ease comparison. Data sources are 1920-1930 Census and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.8: Balancing Tests: Between-City 1920-1930 Covariates, Bandwidth Sensitivity



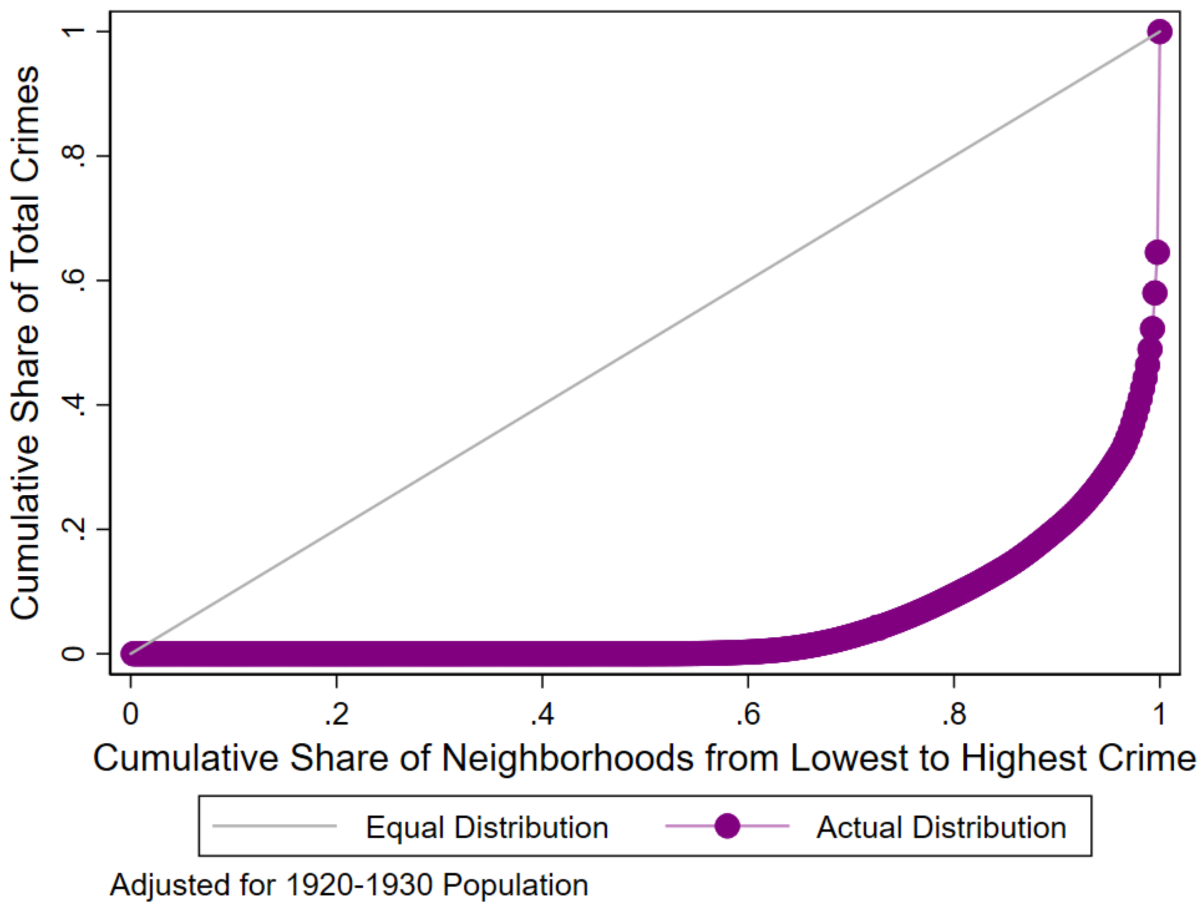
Note: Each figure shows a profile of regression discontinuity coefficient estimates across a range of bandwidth selections. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of an area that is Black and the percent that is Hispanic, respectively. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth. The running variable is always 1930 city population. Data sources are 1920-1930 Census and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.9: Residential Security Map of Los Angeles



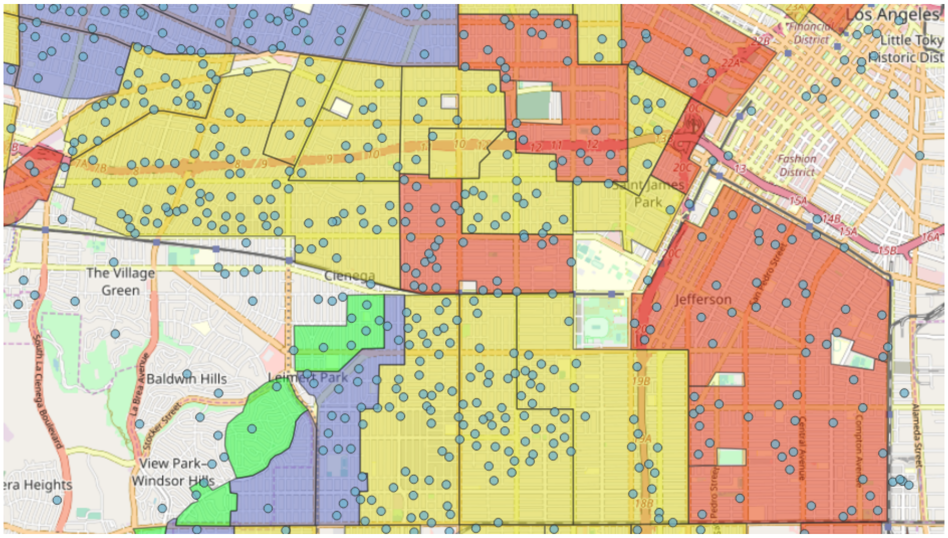
Note: Figure shows a georeferenced version of the Residential Security Maps constructed for Los Angeles by the Home Owners Loan Corporation (HOLC) in 1939. Neighborhoods were assigned ranked security risk categories which correspond to colors on the maps. Areas colored green were considered the best and to bear the least risk; blue were considered next best, followed by yellow and finally red. Areas colored red were considered the most risky and least deserving of credit access and, accordingly are said to have been "redlined".

Figure 1.10: Inequality in the Distribution of Crime in Los Angeles



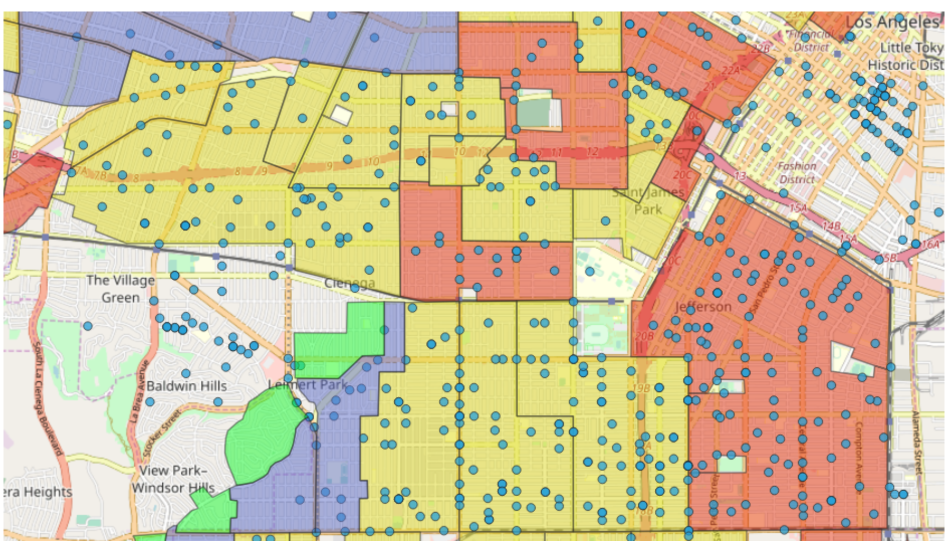
Note: Figure shows a Gini or Inequality Curve for neighborhood-level crime in Los Angeles in 2010. The sample is restricted to neighborhoods that received some Home Owners Loan Corporation (HOLC) color grade in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.

Figure 1.11: Hypothetical Murders in LA (Evenly Spaced by Population)



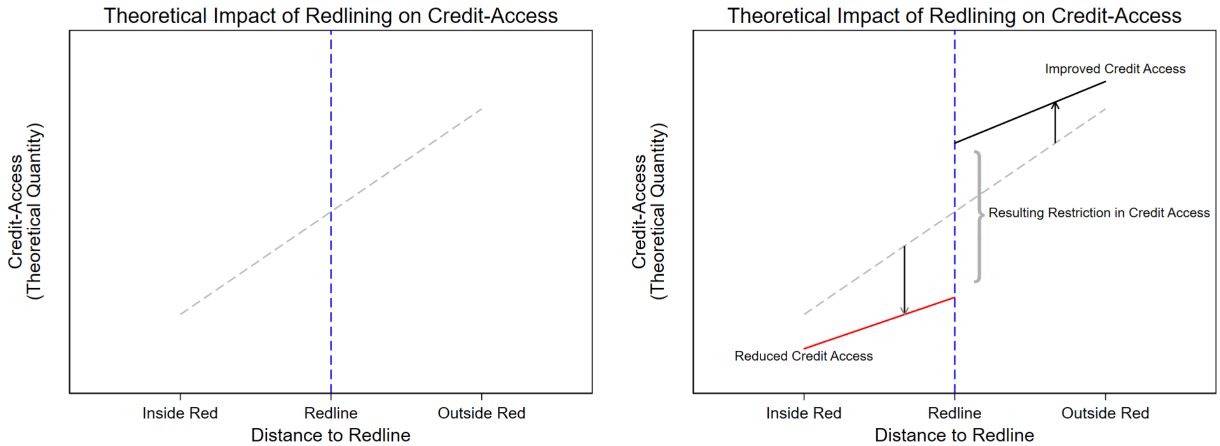
Note: Figure shows a hypothetical spatial distribution of murders across an area of Los Angeles in 2010. Distribution is weighted by block-level 2010 Census population data. The map displays a region of approximately 100 square miles of Central Los Angeles. Home Owners Loan Corporation (HOLC) neighborhood color grades are superimposed for comparison.

Figure 1.12: Murders in LA (2010 Actual)



Note: Figure shows the actual spatial distribution of murders across an area of Los Angeles in 2010. The map displays a region of approximately 100 square miles of Central Los Angeles. Home Owners Loan Corporation (HOLC) neighborhood color grades are superimposed for comparison.

Figure 1.13: Impact of Redlining on Credit Access: Within-City Theoretical Diagrams

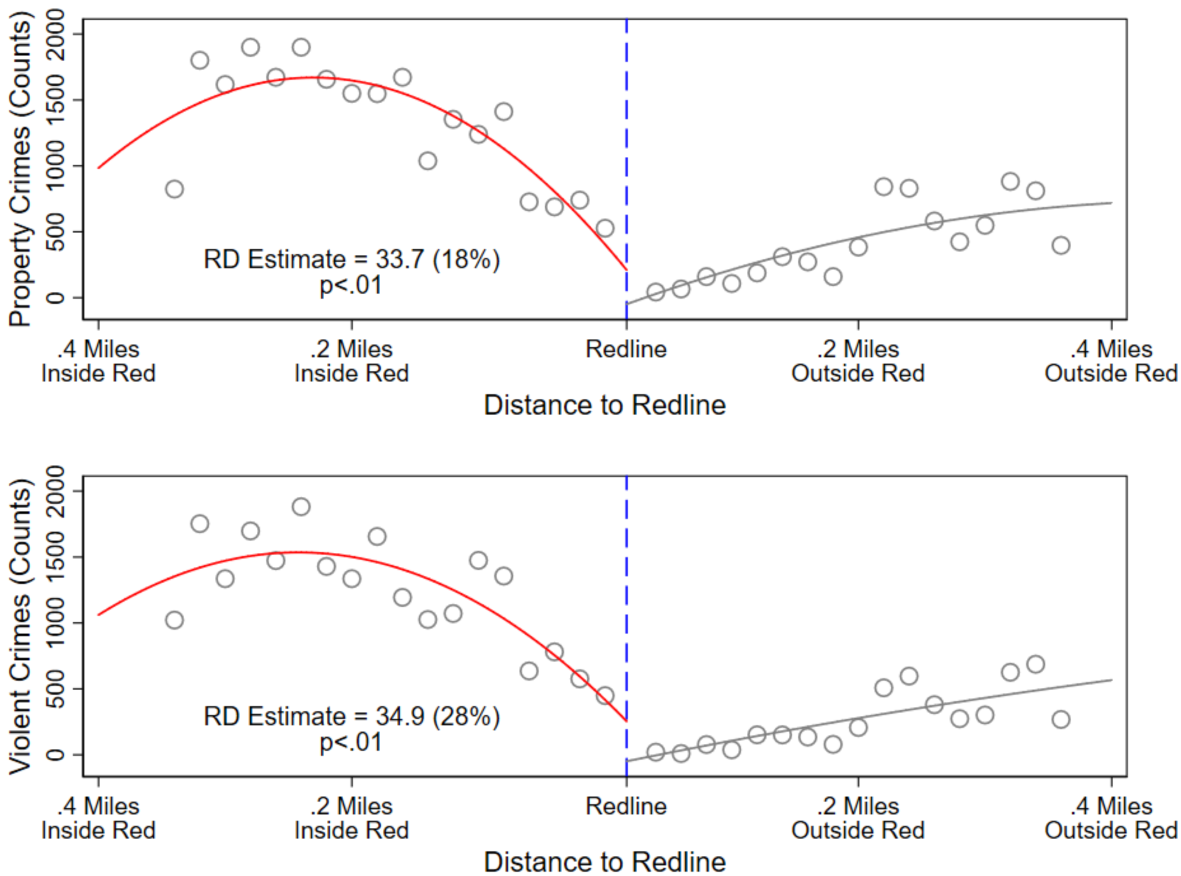


(a) Pre-Period Credit Access

(b) Post Mapping Credit Access

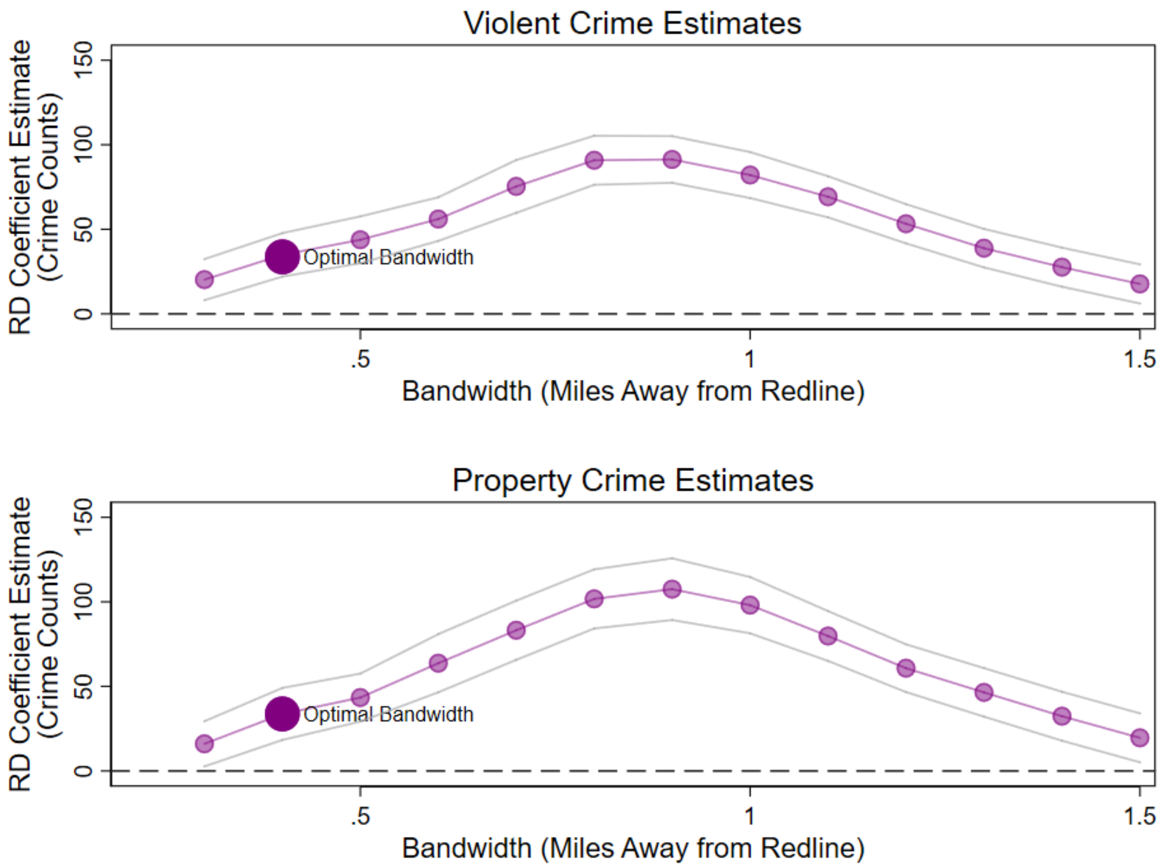
Note: Each figure shows a theoretical regression discontinuity diagram. In both panels, the running variable is the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps, and the threshold is the redline itself. In the left panel, I depict credit access as a continuous and linear function of distance away the redline; this is the situation we expect to hold prior to the creation of a redline border. In the right panel, I depict credit access having been differentially affected by the creation of a HOLC border.

Figure 1.14: Impact of Redlining on Crime: Within-City Estimates, By Crime-Type



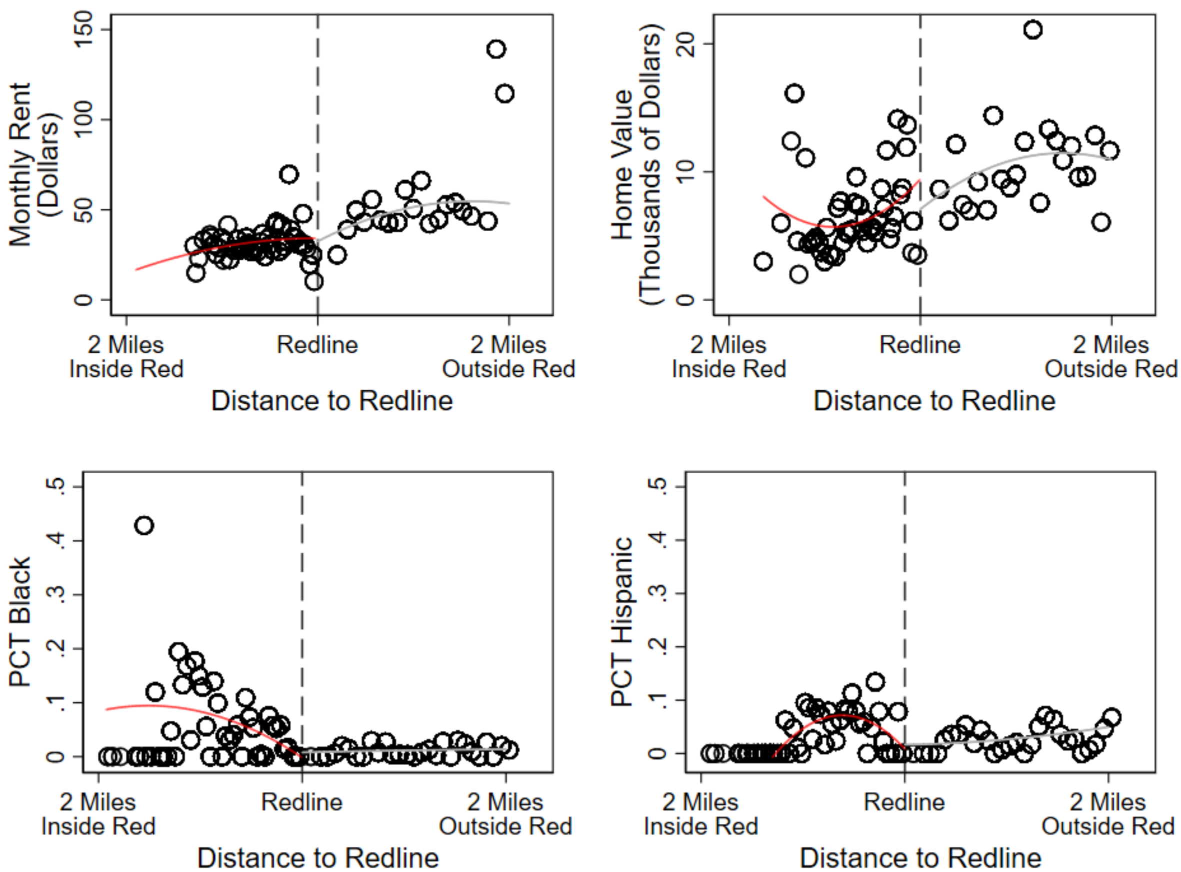
Note: Each figure shows a spatial regression discontinuity diagram for crimes in Los Angeles in 2010. The top panel is restricted to property crimes, while the bottom panel is restricted to violent crimes. Property crimes are defined as those crimes the description of which contains words such as "burglary" and "larceny"; violent crimes are defined as those crimes the description of which contains words such as "murder" and "robbery". Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are both chosen optimally following (14). The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps, and the threshold is the redline itself. I dough-nut out a small region around the threshold to eliminate the small number of crimes committed inside the streets that divide neighborhoods. In all specifications, the sample is restricted to areas which received some HOLC color assignment in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.

Figure 1.15: Impact of Redlining on Crime: Within-City Estimates, Bandwidth Sensitivity



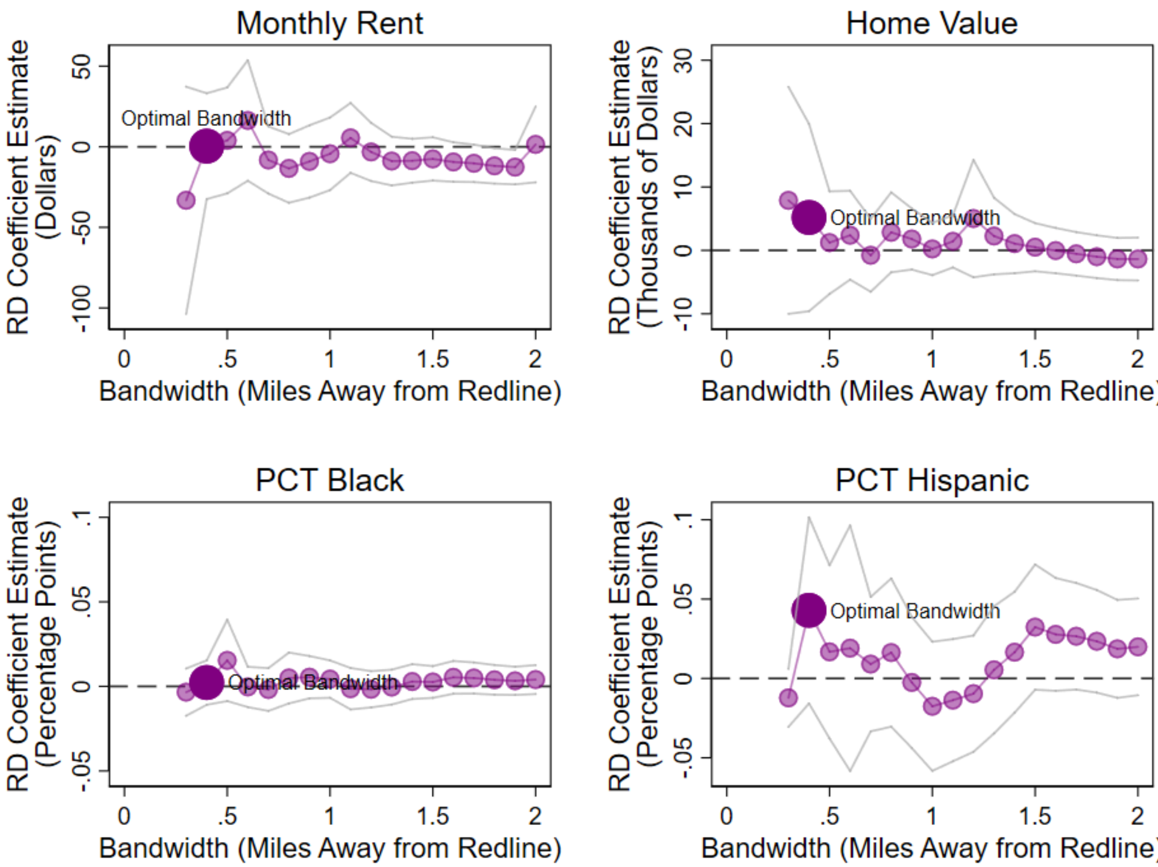
Note: Each figure shows a profile of regression discontinuity coefficient estimates across a range of bandwidth selections. The outcome is always crimes in Los Angeles in 2010. The top panel is restricted to property crimes, while the bottom panel is restricted to violent crimes. Property crimes are defined as those crimes the description of which contains words such as “burglary” and “larceny”; violent crimes are defined as those crimes the description of which contained words such as “murder” and “robbery”. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth, the bandwidth displayed in Figure 1.14 and reported in Table 1.8. In all specifications, the sample is restricted to areas which received some HOLC color designation in 1939. Data sources are city of Los Angeles crime data and HOLC archival records.

Figure 1.16: Balancing Tests: Within-City 1920-1930 Covariates



Note: Each figure shows a spatial regression discontinuity diagram where the dependent variable is a given pre-period covariate, measured in Los Angeles from 1920-1930. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a neighborhood that is Black and the percent that is Hispanic, respectively. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size is fixed at 2 miles to ease comparison. The running variable always is the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. The sample is restricted to areas which received some HOLC color designation in 1939. Data sources are 1920 and 1930 Census data and HOLC archival records.

Figure 1.17: Balancing Tests: Within-City 1920-1930 Covariate Estimates, By Bandwidth



Note: Each figure shows a profile of spatial regression discontinuity coefficient estimates across a range of bandwidth selections. The top panels show results for self-reported monthly rent and home value, respectively, while the bottom panels show results for the percent of a neighborhood that is Black and the percent that is Hispanic, respectively. Circles represent estimates, with the large circle representing the estimate for the optimal bandwidth, In all specifications, the sample is restricted to areas which received some HOLC color designation in 1939. Data sources are 1920 and 1930 Census data and HOLC archival records.

Figure 1.18: Home Owner's Loan Corporations Survey Report

AREA DESCRIPTION

Security Map of LOS ANGELES COUNTY

1. POPULATION: a. Increasing Slowly Decreasing - Static -
   b. Class and Occupation Artisans, oil well, service & white collar workers, Petty Naval officers, etc. Income $1200-2500
   c. Foreign Families 20% Nationalities Mexicans, Japanese & Italians d. Negro 5 %
   e. Shifting or Infiltration Slow increase of subversive racial elements.

2. BUILDINGS:

	PREDOMINATING 80%		OTHER TYPE %	
a. Type and Size	4 and 5 room		Large old dwellings 10%	
b. Construction	Frame (few stucco)		Apts.& Multi-family 10%	
c. Average Age	17 years			
d. Repair	Poor to fair			
e. Occupancy	98%			
f. Owner-occupied	25%			
g. 1935 Price Bracket	$1750-2500	% change	$	% change
h. 1937 Price Bracket	$2000-2750	%	$	%
i. 1939 Price Bracket	$2000-2750	%	$	%
j. Sales Demand	Fair			
k. Predicted Price Trend (next 6-12 months)	Static			
l. 1935 Rent Bracket	$15.00-27.50	% change	$	% change
m. 1937 Rent Bracket	$17.50-30.00	%	$	%
n. 1939 Rent Bracket	$17.50-30.00	%	$	%
o. Rental Demand	Good			
p. Predicted Rent Trend (next 6-12 months)	Static			

3. NEW CONSTRUCTION (past yr.) No. 50 Type & Price 5 rooms $3500-$3750 How Selling Moderately
4. OVERHANG OF HOME PROPERTIES: a. HOLC 3 b. Institutions Few
5. SALE OF HOME PROPERTIES (3 yr.) a. HOLC 38 b. Institutions Few
6. MORTGAGE FUNDS: Limited and Selective 7. TOTAL TAX RATE PER $1000 (193 1937-38) $ 53.40 County $37.80 - City $15.60
8. DESCRIPTION AND CHARACTERISTICS OF AREA:

Terrain: Level to rolling with noticeable slope from north to south. No construction hazards. Land improved 80%. Zoning is mixed, ranging from single to light industrial. However, area is overwhelmingly single family residential. Conveniences are all readily available. This area is very old and has slowly developed into a laboring man's district, with a highly heterogeneous population. A majority of the Mexican, Japanese and Negro residents of Long Beach are domiciled in this area. During the past five years residential building has been moderately active. Construction is generally of substandard quality and maintenance is spotted but usually of poor character. Improvements include many shacky dwellings and a number of low grade apartment houses and other multi-family structures. Land values are low, generally ranging from $8 to $10 per front foot. The Negro population is more or less concentrated along California Ave., but Mexicans and Japanese are scattered throughout. Proximity to the down town business section and industrial employment is a favorable factor. It is a good cheap rental district. The subversive influence of the Signal Hill oil field, which is adjacent on the north, is reflected throughout the area, which is accorded a "medial red" grade.

9. LOCATION Long Beach SECURITY GRADE 4th AREA NO. D-63 DATE 5-4-39

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Note: Figure shows a survey report produced for a neighborhood in Los Angeles by the Home Owner's Loan Corporation (HOLC) in May of 1939. This neighborhood is in the South of Los Angeles, in the Long Beach area; it was graded "4th" or "Red" and hence is said to be have been "redlined"; the "red" grade indicates that this neighborhood is considered to be among the riskiest neighborhoods for lenders. Surveyor expectations about neighborhood level racial demography can in found in item 1.e, "Shifting or Infiltration", which is boxed above.

Figure 1.19: Credit-Restrictions Randomly Assigned Within One City and Between Two Cities

$y_{H,11}$	$y_{L,21}$
$y_{L,31}$	$y_{L,41}$

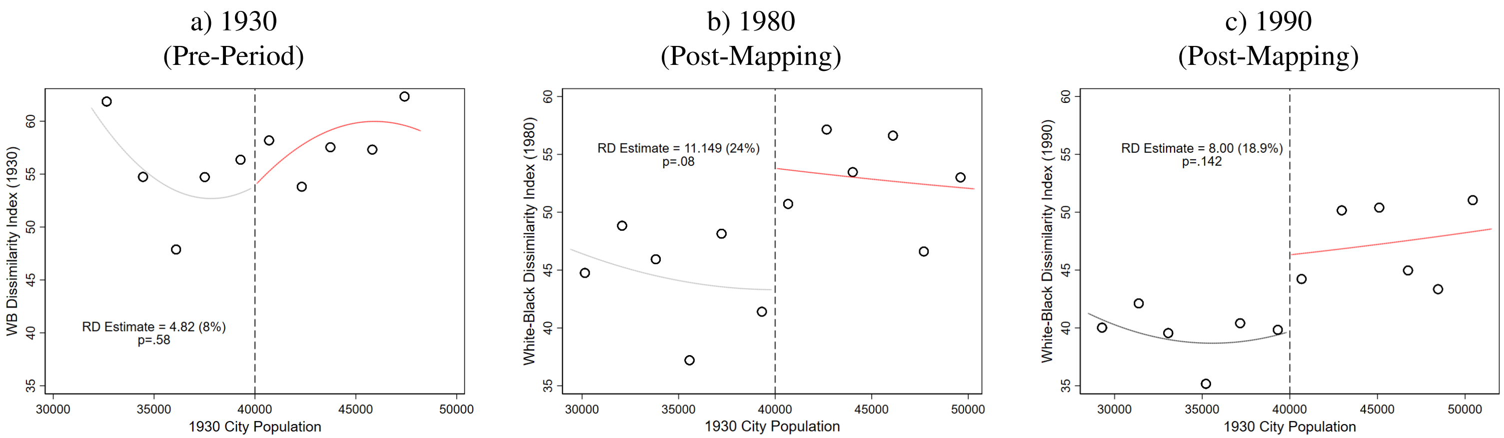
(a) Mapped City ($j = 1$)

$y_{0,12}$	$y_{0,22}$
$y_{0,32}$	$y_{0,42}$

(b) Non-Mapped City ($j = 2$)

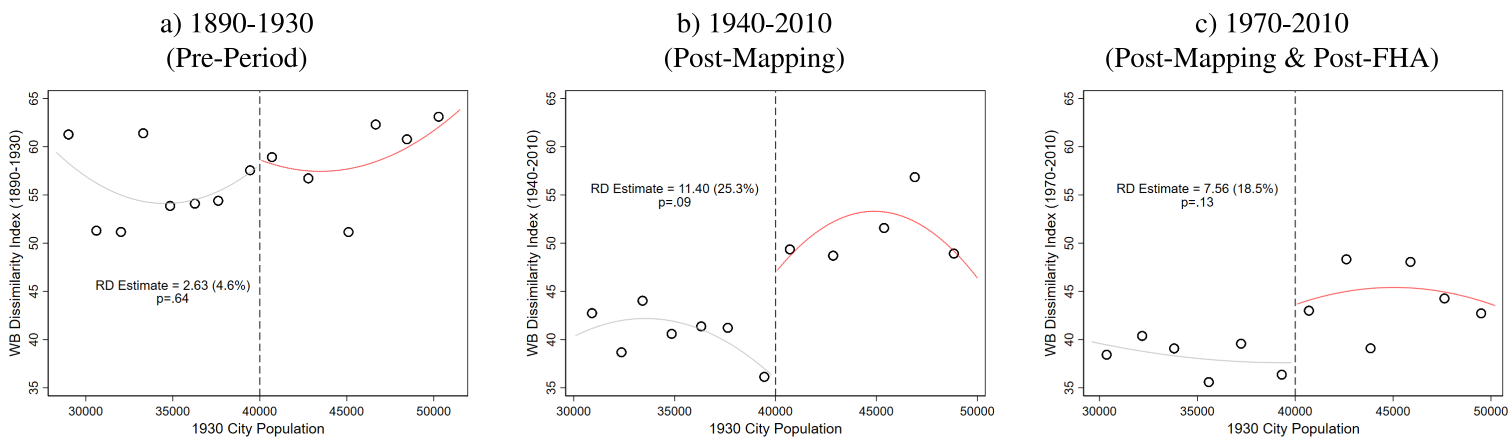
Note: Diagrammatic representation of two cities each with four neighborhoods. Neighborhood cell i in city j experiences a crime outcome y_{ij} based on the credit-restrictions it was assigned. In the mapped city ($j = 1$), only neighborhood cell 1 was randomly assigned a credit-restriction ("redlining"), and experienced a crime outcome $y_{H,11}$. In the non-mapped city ($j = 2$) none of the neighborhoods were assigned a credit-restriction, and each experienced a separate crime outcome $y_{0,i2}$.

Figure 1.20: Impact of redline-mapping on Racial Segregation: Between-City Estimates over Decades



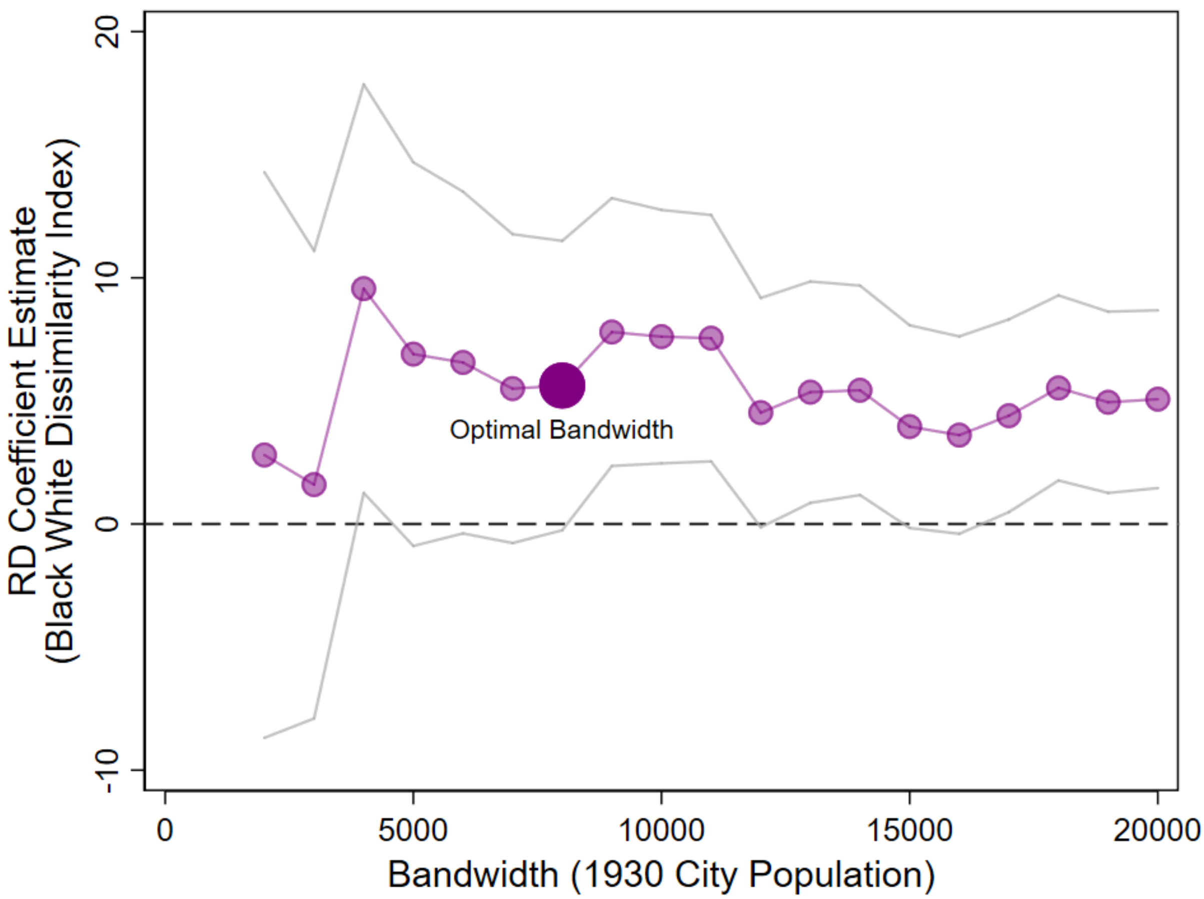
Note: Figure shows a regression discontinuity diagram where the outcome variable is White-Black Dissimilarity Index for a given city in a given year The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). Subfigure (a) shows a placebo test for White-Black segregation in the pre-period. Subfigures (b)-(c) show the impacts of redline-mapping on White-Black segregation in 1980 and 1990, respectively. Data sources are Logan (2011) and Home Owner Loan Corporation (HOLC) Archival records.

Figure 1.21: Impact of redline-mapping on Racial Segregation: Pooled Between-City Estimates



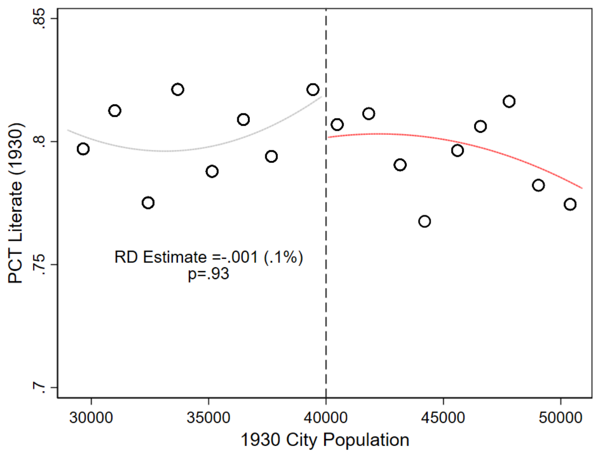
Note: Figure shows a regression discontinuity diagram where the outcome variable is White-Black Dissimilarity Index for a given city in a given year. The running variable is always 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). Subfigure (a) shows a placebo test for White-Black segregation in the period prior to redline-mapping, pooling data from 1890 to 1930. Subfigure (b) shows the impact of redline-mapping on White-Black segregation over the entire modern period, pooling data from 1940 to 2010. Subfigure (c) shows the impact of redline-mapping on White-Black segregation on the period after the Fair Housing Act (FHA) which first outlawed *de jure* discrimination in the credit market. Data sources are Logan (2011) and Home Owner Loan Corporation (HOLC) Archival records.

Figure 1.22: Impact of Redline-Mapping on Segregation: Bandwidth Sensitivity

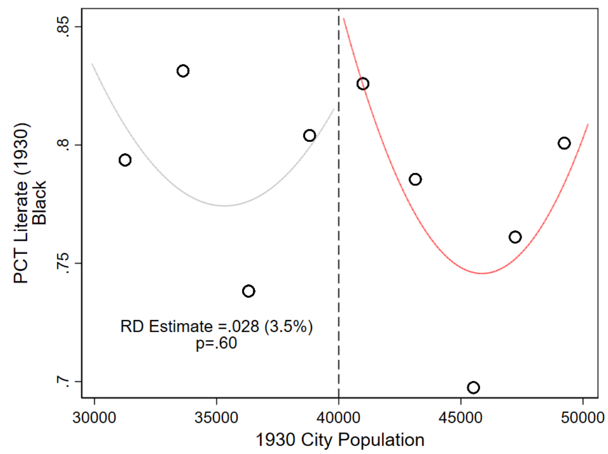


Note: The figure shows a profile of regression discontinuity estimates and 95% confidence intervals obtained by estimating Equation 1.1 on decadal segregation measures. These estimates constitute a bandwidth sensitivity test for panel (b) of Figure 1.21. Data sources are Logan (2011) and Home Owner Loan Corporation (HOLC) Archival records.

Figure 1.23: Impact of Redline-Mapping on Educational Attainment: Placebo Tests with Literacy



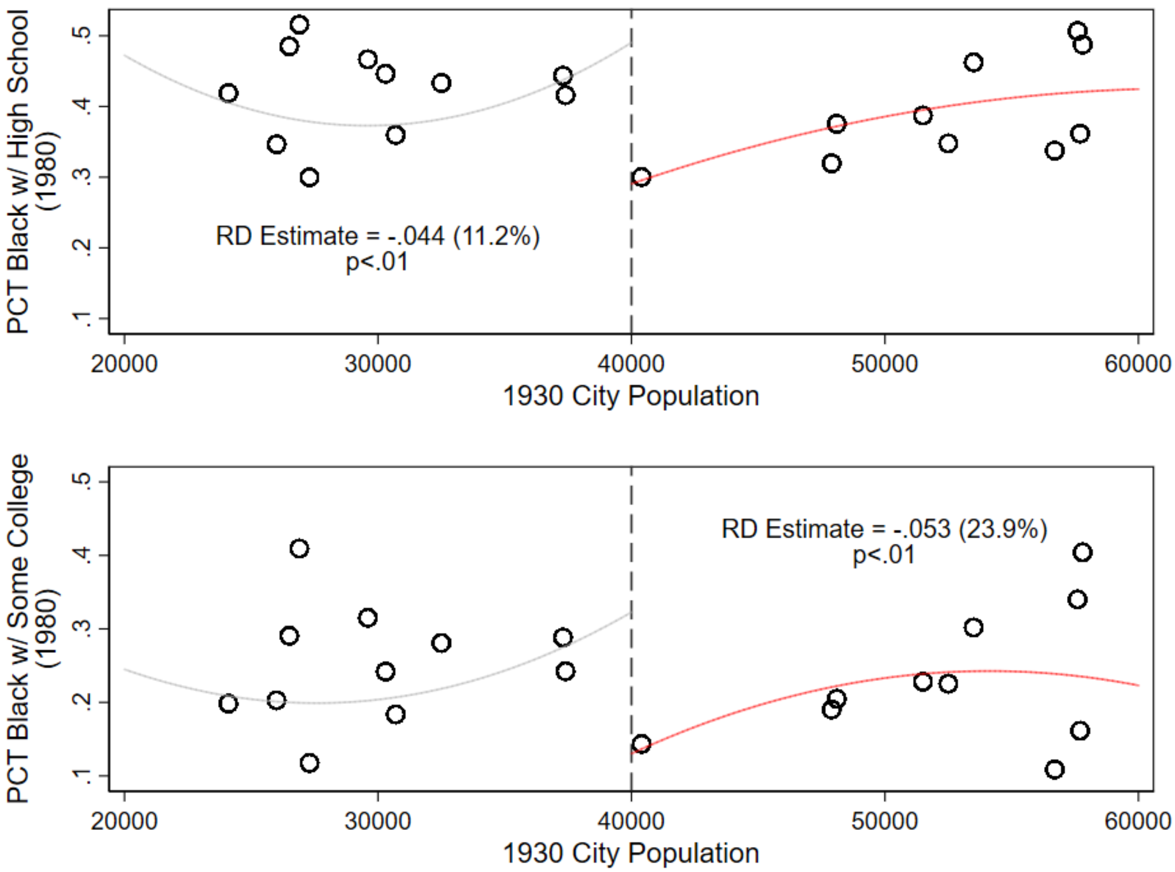
(a) Pre-Period Education



(b) Pre-Period Education: Black

Note: Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the share of individuals who report being literate in a given city in 1930, while in the bottom panel the outcome variable is the share of black individuals who report being literate in a given city in 1930. In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth and bin numbers are chosen optimally following (14). Data sources are the 1930 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.

Figure 1.24: Impact of Redline-Mapping on Educational Attainment: High School, Some College



Note: Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the share of black individuals who report having graduated high school in a given city in 1980, while in the bottom panel the outcome variable is the share of black individuals who report having attended at least some college in a given city in 1980. In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bin numbers are chosen optimally following (14), but bandwidth was set at 20,000 population to ease visual comparison (the optimal bandwidths being slightly over 22,000 and 28,000 population respectively). Data sources are the 1980 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.

Table 1.1: Summary Statistics, Los Angeles

Panel A: Neighborhood Level (Los Angeles)	Red	Non-Red	Total
2010 Crime Victimizations			
Distance to Redline (Miles)	-0.51	1.70	1.36
	(0.30)	(1.09)	(1.29)
Property Crimes	1991.00	1021.06	1170.83
	(2718.07)	(2102.43)	(2230.74)
Violent Crimes	1739.69	683.29	846.41
	(2853.51)	(2535.18)	(2609.44)
All Crimes	5756.83	2531.23	3029.30
	(8582.86)	(6936.69)	(7291.70)
1930 Demography			
PCT White	0.94	0.98	0.97
	(0.16)	(0.08)	(0.10)
PCT Black	0.02	0.01	0.01
	(0.05)	(0.02)	(0.03)
PCT Japanese	0.01	0.01	0.01
	(0.02)	(0.07)	(0.07)
PCT Non-Hispanic	0.92	0.98	0.97
	(0.12)	(0.06)	(0.08)
PCT Mexican	0.07	0.02	0.03
	(0.11)	(0.06)	(0.07)
PCT Native Born	0.53	0.62	0.60
	(0.19)	(0.21)	(0.21)
PCT Married (Spouse Present)	0.42	0.46	0.45
	(0.12)	(0.17)	(0.16)
PCT Have Children in HH	0.15	0.17	0.16
	(0.09)	(0.11)	(0.11)
PCT Have Children $\leq$5 in HH	0.03	0.02	0.02
	(0.04)	(0.05)	(0.05)
PCT Have a Radio	0.58	0.77	0.73
	(0.23)	(0.24)	(0.24)
PCT In School	0.20	0.21	0.20
	(0.10)	(0.13)	(0.12)
PCT Literate	0.85	0.85	0.85
	(0.08)	(0.12)	(0.11)
PCT In Labor Force	0.46	0.44	0.44
	(0.15)	(0.17)	(0.17)
PCT Self-Employed	0.06	0.11	0.10
	(0.04)	(0.15)	(0.14)
PCT Works for Wages	0.40	0.34	0.35
	(0.15)	(0.14)	(0.15)
House Value	8089.84	14539.23	13218.89
	(5519.34)	(22786.69)	(20614.56)
Rent Value	31.83	55.75	50.09
	(12.43)	(56.83)	(50.97)
PCT No Mortgage - Own Free and Clear	0.04	0.03	0.03
	(0.05)	(0.10)	(0.09)
PCT Have a Mortgage	0.05	0.03	0.03
	(0.05)	(0.05)	(0.05)
Observations	42	230	272

Note: Means reported with standard errors in parentheses. All observations are at the neighborhood level, neighborhoods being delineated accordingly to boundaries drawn by HOLC surveyors. Distributions are reported separately for neighborhoods assigned red ("redlined") and those assigned any other color. Panel A (2010 Crime Victimizations): Source is City of Los Angeles geocoded crime data (2010). The distance to redline variable measures the distance from a crime to the nearest redline (distances inside a red neighborhood out to its border being coded with negative values). Crimes are limited to UCR Type 1 crimes and broken down by property and violent crimes, respectively. Panel A (1930 Demography): Source is geocoded 1920-1930 decennial Census.

Table 1.2: Summary Statistics, Between-Cities

Panel B: City Level	Mapped	Non-Mapped	Total
Crime Victimizations			
All (White)	765.61	477.11	569.15
	(1385.38)	(1060.01)	(1179.46)
Violent (White)	101.03	57.19	71.17
	(179.96)	(129.90)	(148.89)
Property (White)	664.58	419.93	497.98
	(1223.03)	(936.79)	(1041.33)
All (Black)	262.74	95.59	148.92
	(729.44)	(323.91)	(496.21)
Violent (Black)	70.61	22.20	37.65
	(194.54)	(75.94)	(128.21)
Property (Black)	192.13	73.39	111.27
	(543.87)	(249.91)	(373.33)
All (Hispanic)	90.97	47.74	61.54
	(309.95)	(175.13)	(227.46)
Violent (Hispanic)	18.48	9.12	12.11
	(52.81)	(29.42)	(38.63)
Property (Hispanic)	72.50	38.62	49.43
	(260.30)	(149.61)	(192.26)
Observations	119	254	373
1930 Demography			
City Population	48,640.00	28,954.92	33,343.31
	(6,933.42)	(7,893.42)	(11,238.84)
PCT White	0.91	0.94	0.93
	(0.12)	(0.11)	(0.11)
PCT Black	0.09	0.06	0.07
	(0.12)	(0.11)	(0.11)
PCT Naturalized Citizens	0.07	0.08	0.08
	(0.06)	(0.06)	(0.06)
PCT Married (Spouse Present)	0.42	0.42	0.42
	(0.04)	(0.05)	(0.05)
PCT HH Having a Radio	0.45	0.48	0.47
	(0.16)	(0.18)	(0.18)
PCT in School	0.22	0.22	0.22
	(0.03)	(0.04)	(0.04)
PCT Literate	0.80	0.79	0.80
	(0.04)	(0.05)	(0.05)
PCT in Labor Force	0.42	0.41	0.41
	(0.04)	(0.05)	(0.05)
PCT Wage Workers	0.39	0.37	0.37
	(0.04)	(0.05)	(0.05)
Average Home Value ($)	6,976.20	6,636.16	6,711.97
	(2,955.04)	(4,276.88)	(4,018.21)
Average Rental Amount ($)	31.19	31.04	31.07
	(11.51)	(13.67)	(13.20)
Observations	70	244	314

Note: Means reported with standard errors in parentheses. Sample is restricted to observations for cities with a 1930 population between 20,000 and 60,000 people. Distributions are reported separately for cities which were redline-mapped and for those not mapped. Panel B Crime Victimization: Source is NIBRS 2015 Crime Victimization Data. Observations are at the agency-level. Panel B 1930 Demography: Source is address-level 1920-1930 Census Data. Observations are at the city level.

Table 1.3: Summary Statistics, Between-Cities

Panel C: City Level Continued			
	Mapped	Non-Mapped	Total
Criminal Arrests			
All (White)	438.25	300.83	347.68
	(911.49)	(547.62)	(695.45)
Violent (White)	114.87	70.57	85.67
	(271.90)	(163.89)	(207.89)
Property (White)	323.38	230.26	262.00
	(683.40)	(415.58)	(523.61)
All (Black)	271.60	116.70	169.50
	(511.23)	(231.70)	(359.80)
Violent (Black)	95.70	36.40	56.62
	(200.39)	(71.26)	(133.28)
Property (Black)	175.90	80.30	112.89
	(337.32)	(167.95)	(243.40)
Observations	150	290	440

Note: Means reported with standard errors in parentheses. Sample is restricted to observations for cities with a 1930 population between 20,000 and 60,000 people. Distributions are reported separately for cities which were Redline-Mapped and for those not mapped. Panel C Criminal Arrests: Source is UCR 2015 Arrest Data. Observations are at the agency-level.

Table 1.4: Selected List of Redline-Mapped and Not Mapped Cities

Not Mapped	**Mapped**
Tucson, AZ	Phoenix, AZ
Santa Barbara, CA	Stockton, CA
Bakersfield, CA	Fresno, CA
San Bernardino, CA	San Jose, CA
Colorado Springs, CO	Pueblo, CO
Orlando, FL	St. Petersburg, FL
Champaign, IL	Joliet, IL
Bloomington, IL	Aurora, IL
Ashland, KY	Lexington, KY
Melrose, MA	Pittsfield, MA
Gloucester, MA	Holyoke, MA
Ann Arbor, MI	Kalamazoo, MI
St. Cloud, MN	Rochester, MN
Vicksburg, MS	Jackson, MS
Ithaca, NY	Poughkeepsie, NY
Middletown, NY	Jamestown, NY
Lubbock, TX	Amarillo, TX
Brownsville, TX	Wichita Falls, TX
Abilene, TX	Port Arthur, TX
San Angelo, TX	Waco, TX
Corpus Christi, TX	Galveston, TX
Laredo, TX	Austin, TX
Bristol, VA	Lynchburg, VA
Green Bay, WI	Madison, WI

Note: Reported cities all have a 1930 population between 20,000 and 60,000, the redline-mapping cutoff being 40,000. Data sources are 1930 Census and HOLC archival records.

Table 1.5: Impact of Redline-Mapping on Crime: Between-City Estimates, By Crime-Type, Race

	(1) Black	(2) Hispanic	(3) White
Panel A: All Crimes			
RD Estimate	2.03**	1.18	2.55*
	(0.92)	(0.82)	(1.31)
Mean (Bandwidth)	128.19	78.66	687.92
Mean (Non-Mapped)	60.07	23.93	318.17
Bandwidth (1930 Population)	10,855	12,447	10,760
Panel B: Property Crimes			
RD Estimate	2.06**	1.14	2.52**
	(0.87)	(0.79)	(1.28)
Mean (Bandwidth)	119.36	63.79	606.93
Mean (Non-Mapped)	47.17	19.77	282.84
Bandwidth (1930 Population)	11,348	12,468	10,709
Panel C: Violent Crimes			
RD Estimate	1.45**	0.95*	1.87**
	(0.65)	(0.53)	(0.89)
Mean (Bandwidth)	31.71	14.31	88.23
Mean (Non-Mapped)	12.9	4.16	35.33
Bandwidth (1930 Population)	11,265	13,109	11,447
Observations	966	966	966
Kernel	Uniform	Uniform	Uniform
Polynomial	Local linear	Local linear	Local linear

Note: Table shows regression discontinuity estimates of the impact of redline-mapping on crime with standard errors reported in parentheses. Observations are at the city-level. The outcome variable is the log of crime victimizations in a given city in 2015. Reported means are city-level counts of crime victimizations in 2015. The running variable is always 1930 city population. Bandwidth size is chosen optimally following (14). Data sources are NIBRS Crime Victimization Data (2015) and HOLC archival documents.

Table 1.6: Impact of Redline-Mapping on Educational Attainment: Between City Estimates

	(1) PCT Black with High School	(2) PCT Black with College
RD Estimate	-0.044***	-0.053***
	(0.009)	(0.008)
Observations	573,683	573,683
Mean	.390	.221

Note: Table shows regression discontinuity estimates of the impact of redline-mapping on educational attainment with standard errors reported in parentheses. Observations are at the individual level. The outcome variable is the percent of black individuals having graduated high school and having attended at least some college, in columns (1) and (2) respectively. The running variable is 1930 city population. Bandwidth size is chosen optimally following (14). Source: 1980 Census and HOLC archival documents. The reported mean is for non-mapped cities within the optimal population bandwidth. Significance levels indicated by: * $(p < 0.10)$, ** $(p < 0.05)$, ***$(p < 0.01)$

Table 1.7: Correlation Between Demography, HOLC Mapping Assignments and Contemporary Crime

	(1)	(2)
2010 Violent Crimes		
1939 Hispanic Population	382.3**	-35.1
	(193.4)	(261.7)
Blue		111.6***
		(36.9)
Yellow		698.1***
		(219.6)
Red		963.5**
		(412.3)
Observations	416	416
Mean	530.3	530.3
Pseudo R^2	.094	.168

Note: The table reports average marginal effects from Poisson regressions with heteroskedasticity-robust errors reported in parentheses. The outcome variable is the count of violent crime in a neighborhood in 2010. Violent crimes are defined as those crimes the description of which contains words such as "murder" and "robbery". "1939 Mexican Population" is an indicator variable which is 1 when a HOLC surveyor reported a significant Mexican population and 0 otherwise; Census data from 1930 show that the percentage of Hispanic residents is 3 times larger when the indicator is 1 rather than 0. Estimates of color designations are relative to Green, the lowest risk category. (See Figure 1.9 for the map with neighborhood color assignments indicated.) The regressions control for population using 1920-1930 Census data. In all specifications, the sample is restricted to areas which received some HOLC color designation in 1939. Data sources are city of Los Angeles crime data and HOLC archival records. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), ***($p < 0.01$)

Table 1.8: Impact of Redlining on Crime: Within-City Estimates, By Crime-Type

	(1) All	(2) Property	(3) Violent
RD Estimate	100.85***	33.72***	34.93***
	(19.68)	(8.02)	(6.63)
Observations	3423	3423	3423
Mean (Bandwidth)	171.56	60.10	51.58
Mean (Non-Red)	460.26	190.13	123.10
Kernel	Uniform	Uniform	Uniform
Polynomial	Local linear	Local linear	Local linear
Bandwidth	.4 miles	.4 miles	.4 miles

Note: The table reports spatial regression discontinuity estimates of the number of crime increases attributable to redlining by crime type. Standard errors are computed using a heteroskedasticity robust nearest neighbor variance estimator following (14) and reported in parentheses. The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. The outcome variable is crime in in Los Angeles in 2010. Property crimes are defined as those crimes the description of which contains words such as "burglary" and "larceny". Violent crimes are defined as those crimes the description of which contained words such as "murder" and "robbery". Bandwidth size and bin numbers are chosen optimally following (14). The threshold is at the redline where the distance to the redlined neighborhood is zero. I dough-nut out a small region around the threshold to eliminate the small number of crimes committed inside the streets that divide neighborhoods. Two means are reported: means within the bandwidth and means across all neighborhoods, regardless of bandwidth, assigned a color grade other than red. The sample is restricted to areas which received some HOLC color assignment in 1939. Data sources are city of Los Angeles crime data and 1939 HOLC maps. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

Table 1.9: Further Balancing Tests: Within-City 1920-1930 Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household Variables								
	Have Radio	Nmb Families	Nmb Subfamilies	Nmb Married	Nmb Mothers	Nmb Fathers	Own Home	Rent Home
RD Estimate	0.16	0.68	0.25	-0.15	0.90	1.13	-0.61	1.15
	(0.900)	(0.335)	(0.440)	(0.804)	(0.361)	(0.243)	(0.458)	(0.182)
	[0.974]	[0.748]	[0.748]	[0.945]	[0.748]	[0.748]	[0.748]	[0.748]
Observations	7449	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.54	1.6	.09	.76	.70	.55	.33	.58
Mean (Non-Red)	.61	1.54	.09	.84	.78	.64	.44	.51
Panel B: Family Formation Variables								
	Nmb Family Members	Nmb Children	Have Children <5	Female	Spouse Present	Spouse Absent	Divorced	Single
RD Estimate	1.19	0.35	0.05	0.36	-0.46	-0.10	0.01	0.14
	(0.751)	(0.722)	(0.254)	(0.377)	(0.321)	(0.461)	(0.865)	(0.751)
	[0.929]	[0.929]	[0.748]	[0.748]	[0.748]	[0.748]	[0.974]	[0.929]
Observations	10140		10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	3.41	.54	.02	.51	.41	.04	.03	.43
Mean (Non-Red)	3.64	.63	.03	.5	.44	.04	.02	.42
Panel C: Race and Class Variables								
	White	Chinese	Japanese	Asian/Pacific Islander	Cuban	Native Born	Mother Foreign Born	Foreign Born
RD Estimate	-0.00	-0.00	0.18	-0.13	-0.01	-0.07	0.02	-0.01
	(0.988)	(0.322)	(0.342)	(0.271)	(0.368)	(0.911)	(0.872)	(0.984)
	[0.988]	[0.748]	[0.748]	[0.748]	[0.748]	[0.974]	[0.974]	[0.988]
Observations	10140	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.96	0	.01	.01	0	.52	.03	.2
Mean (Non-Red)	.96	0	.02	0	.01	.6	.03	.17
Panel D: Education and Labor Force								
	Not In School	In School	Illiterate	Literate	Not in Labor Force	In Labor Force	Self-Employed	Works for Wages
RD Estimate	-0.34	0.34	-0.03	-0.36	0.20	-0.59	-0.82*	0.25
	(0.413)	(0.413)	(0.739)	(0.127)	(0.586)	(0.247)	(0.076)	(0.551)
	[0.748]	[0.748]	[0.929]	[0.748]	[0.835]	[0.748]	[0.715]	[0.810]
Observations	10140	10140	10140	10140	10140	10140	10140	10140
Mean (Bandwidth)	.82	.18	.02	.86	.31	.49	.08	.42
Mean (Non-Red)	.79	.21	.01	.84	.34	.43	.07	.36
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear	Local Linear
Bandwith	0.40	0.40	0.40	0.40	0.40	0.40	0.40	0.40

Note: The reported coefficients are spatial regression discontinuity estimates of whether there is an "effect" of redlining on the given pre-period variable, with p-values reported underneath. The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps and the threshold is the redline itself. Two p-values are reported: first, the standard p-value for a two-tailed hypothesis test is reported in parentheses, and, second, multiple-inference corrected p-values are reported in brackets. Bandwidth size is fixed at .4 miles to ease comparison. The sample is restricted to areas which received some HOLC color grade in 1939. Sample size is smaller for "Have Radio" because some households in the sample did not answer this survey question. Two means are reported: means within the bandwidth and means across all neighborhoods, regardless of bandwidth, given a color grade other than red. Data sources are 1920-1930 Census and HOLC archival records. Significance levels for standard p-values indicated by: * $(p < 0.10)$, ** $(p < 0.05)$, *** $(p < 0.01)$

Table 1.10: HOLC's Stated Preferences about Racial Composition

"Shifting or Infiltration": Sample Text Responses
A threat of subversive racial infiltration from nearby areas.
Area is hopelessly gone and cannot go much further
Being a beach resort, there is always danger of infiltration of lower racial elements.
Continued infiltration of Mexicans and Negroes
Deed restrictions protect against racial hazards.
Definite threat of further infiltration of subversive racial elements
Few Mexicans moving in along Filmore Place - Currier and along Holt. Ave. west of Filmore
Infiltration of Japanese and Negroes is a threat
Infiltration of goats, rabbits, and dark skinned babies indicated.
Infiltration of inharmonious Jewish element predicted. Thought remote.
Mexicans living on border agricultural lands a threat.
Mexicans said to be diminishing
Negroes are moving out but slowly
No further increase of subversive racial groups is anticipated
Possible future infiltration because of lack of restrictions
Said to be slight infiltration of well-to-do immigrant Jews into apartment houses
Serbs and Italians of better class
Said to be considerable infiltration of Jewish families

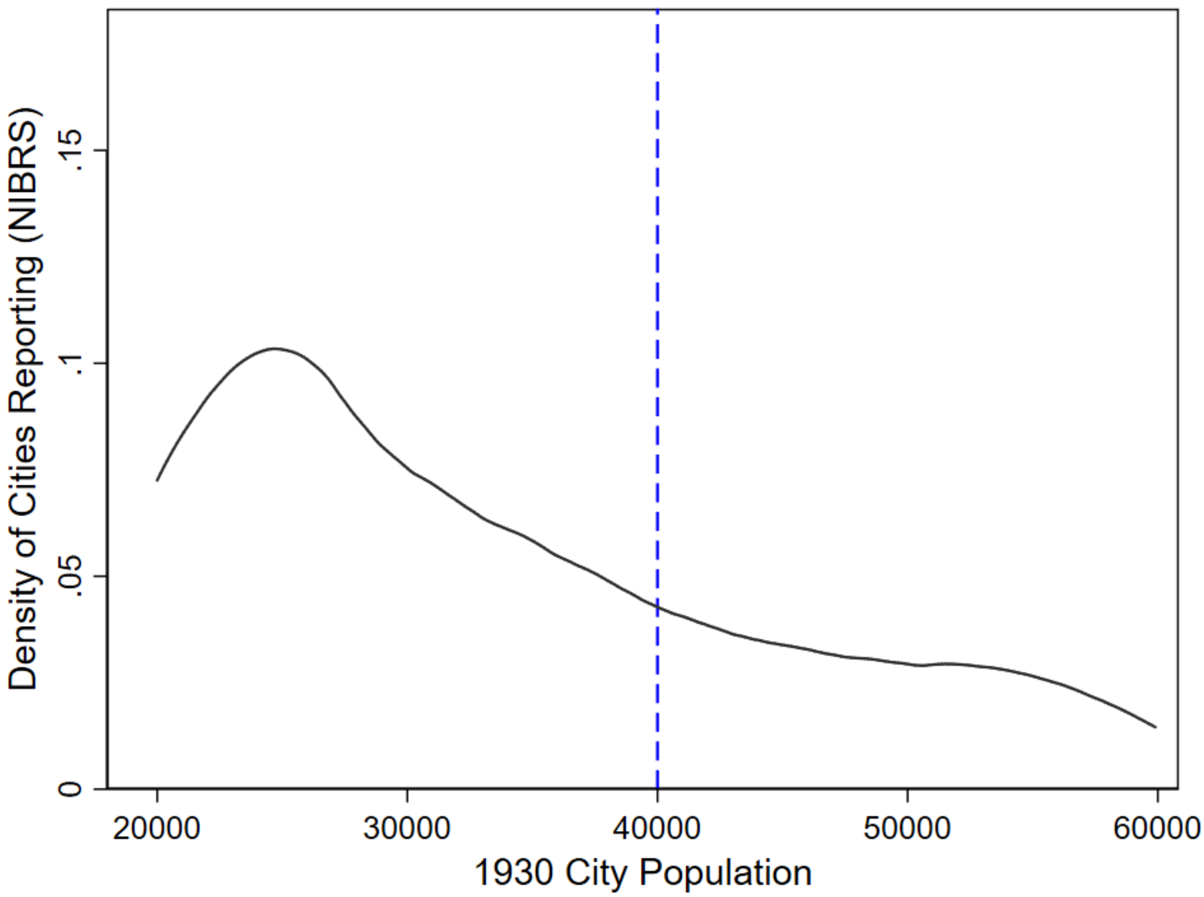
Note: HOLC surveyors were asked to detail their expectations about future racial composition of a neighborhood on survey forms on the field "1.e. Shifting or Infiltration" (For an example, see Figure 1.18). A sample of text responses made in field "Shifting or Infiltration" on survey sheets for Los Angeles, California are listed in the table. The data sources are HOLC archival records.

Table 1.11: HOLC's Revealed Preferences about Racial Composition

	Ordered Logit
Pr(Redlined)	
Increasing Black	0.051*
	(0.030)
Increasing Hispanic	0.019*
	(0.011)
Increasing Jewish	-0.000
	(0.008)
Increasing Japanese	0.027
	(0.023)
Increasing Subversive	0.016**
	(0.008)
No Inc Subversive	-0.020***
	(0.007)
Restrictive Covenant	-0.038**
	(0.016)
Test of Joint Significance	χ^2 = 98.21 p<.001
Observations	416
Mean	.24
Pseudo R^2	.169

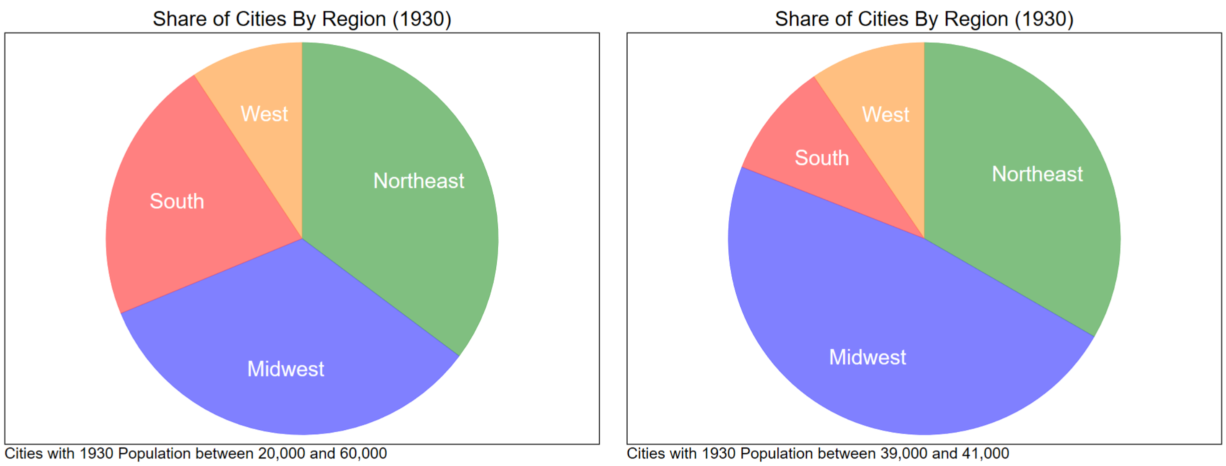
Note: Table reports average marginal effects from an ordered logistic regression with standard errors clustered at the neighborhood and reported in parentheses. The outcome variable is the ordinal rank assignment HOLC gave to each neighborhood ("red","yellow", "blue", "green"). The variables of interest are indicator variables constructed by performing text searches through the field on the HOLC Survey form entitled "Shifting or Infiltration" (See Figure 1.18 for an example of a HOLC Survey Form, and Table 1.10 for examples of text responses in the "Shifting or Infiltration" field, and Appendix A for a detailed explanation of the text searches performed.) Results are conditional on expectations about population increases and future wealth levels. The regressions control for population using 1920-1930 Census data. Data sources are HOLC archival records. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

Figure A1: Density of Agencies Reporting to NIBRS: Between-City Crime Data



Note: The figure shows the density of agencies reporting to the National Incident Based Reporting System (NIBRS) in 2015 across the 1930 city population in which the agency operates. Data sources are individual level NIBRS data from 2015 and Home Owner Loan Corporation (HOLC) archival records.

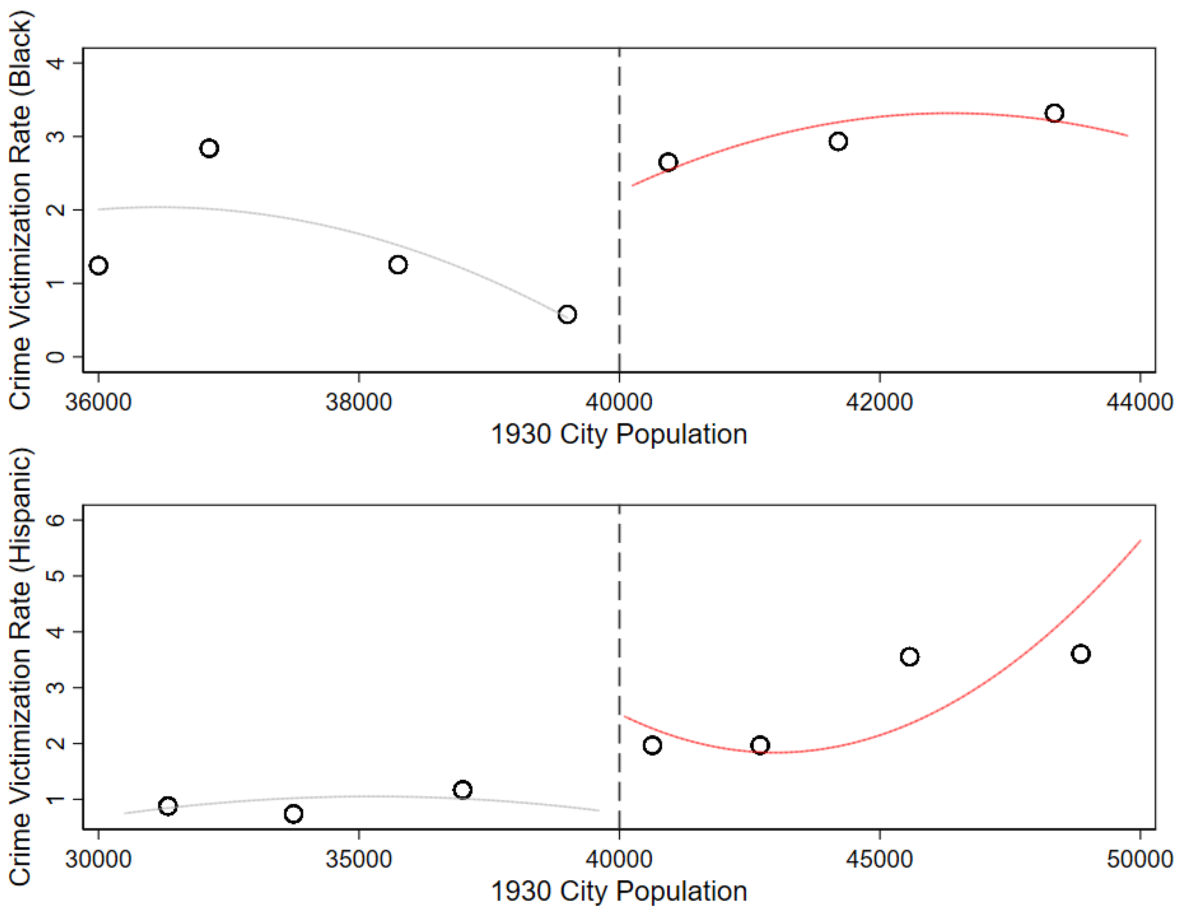
Figure A2: Regional Breakdown of Cities with Redline-Mapping Bandwidth: Between-City Regional Breakdowns



1930 City Pop $\in [20,000, 60,000]$ 1930 City Pop $\in [39,000, 41,000]$

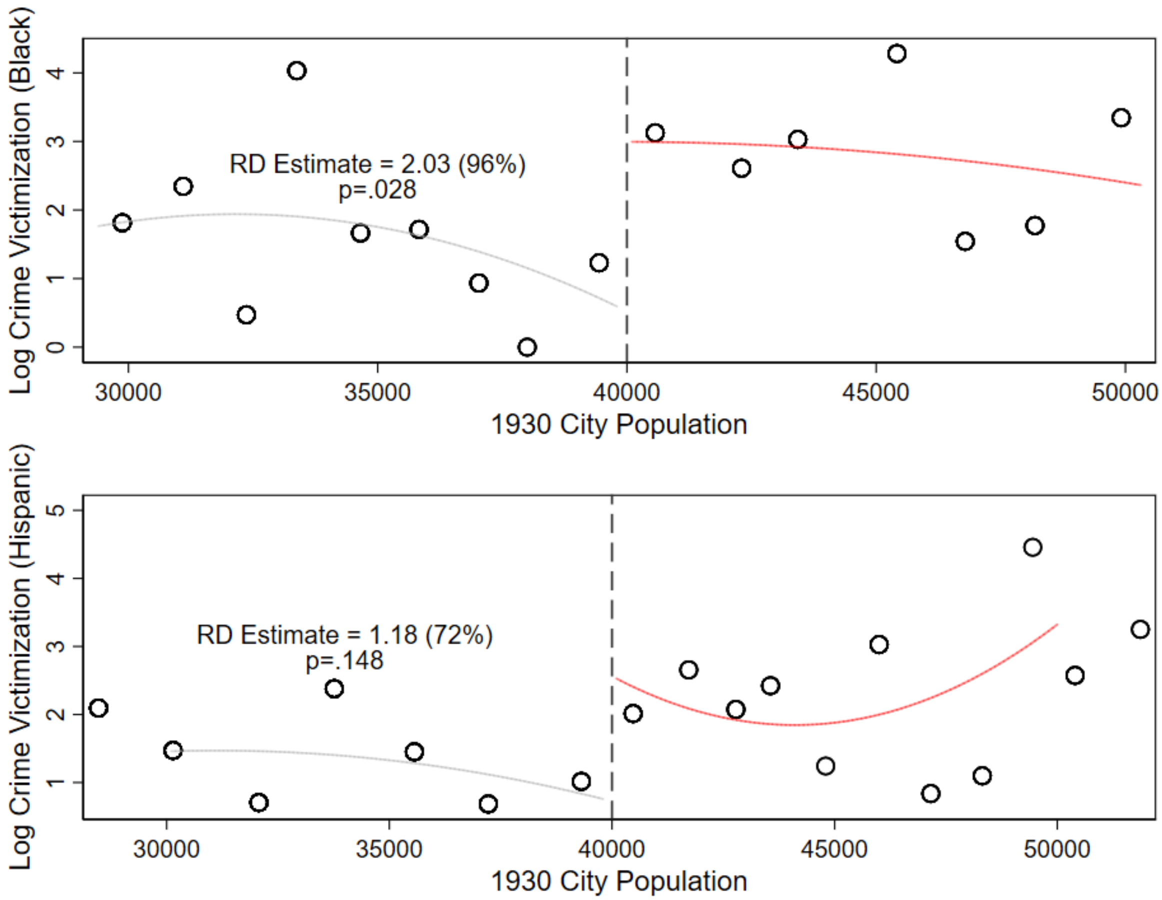
Note: The figure shows the regional share of cities that lie in two small bandwidths around the redling-mapping population threshold: in the left panel the regional shares for cities with 1930 population between 20,000 and 60,000 are shown, while in the right panel the regional shares for cities with 1930 population between 39,000 and 41,000 are shown. (The redline-mapping threshold was 40,000 people.) Data sources are the 1930 Census adn Home Owner Loan Corporation (HOLC) archival records.

Figure A3: Impact of Redline-Mapping on Crime: Between-City Estimates



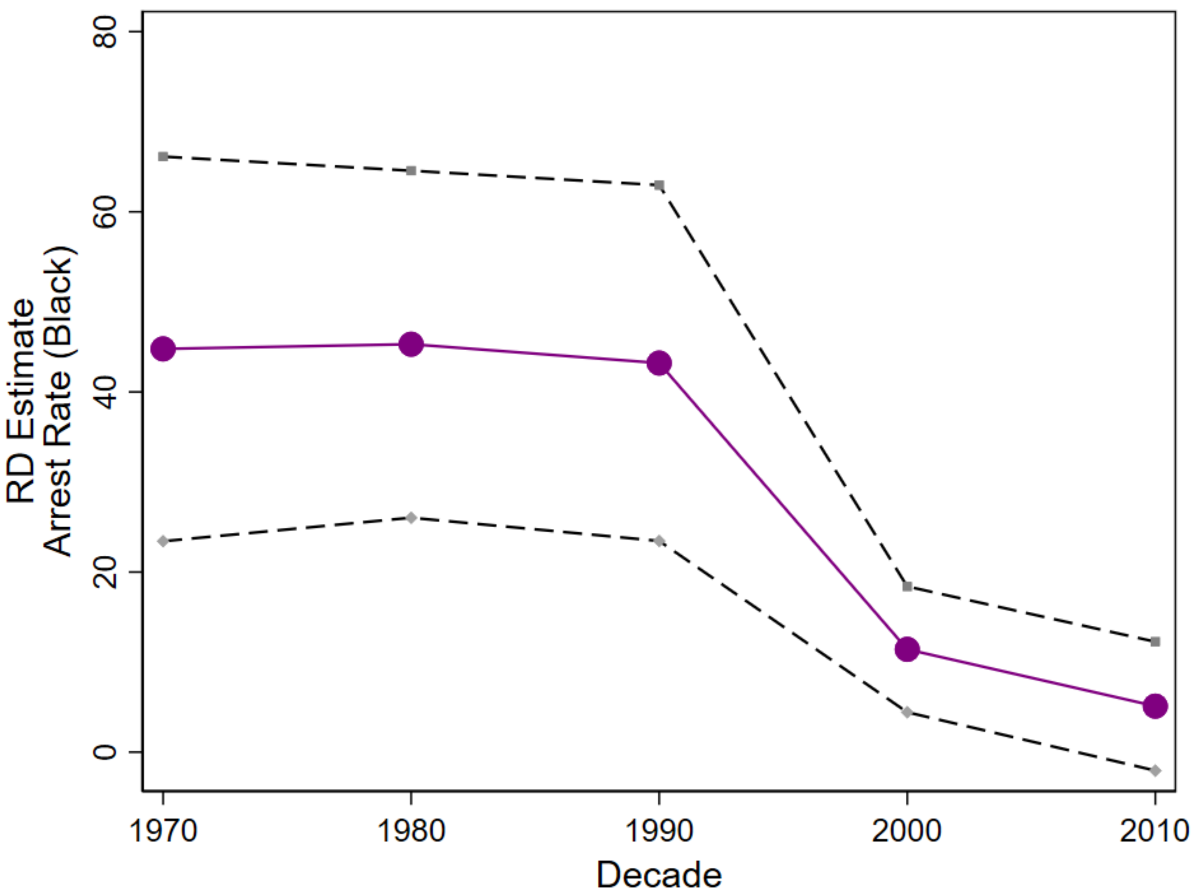
Note: Each figure shows a regression discontinuity diagram where the outcome variable is rate of crime victimization per 1,000 people in a given city in 2015. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). There are 133 agencies included in NIBRS 2015 data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. Data sources are individual-level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records.

Figure A4: Impact of Redline-Mapping on Crime: Between-City Estimates (Non-Optimal Bin Number)



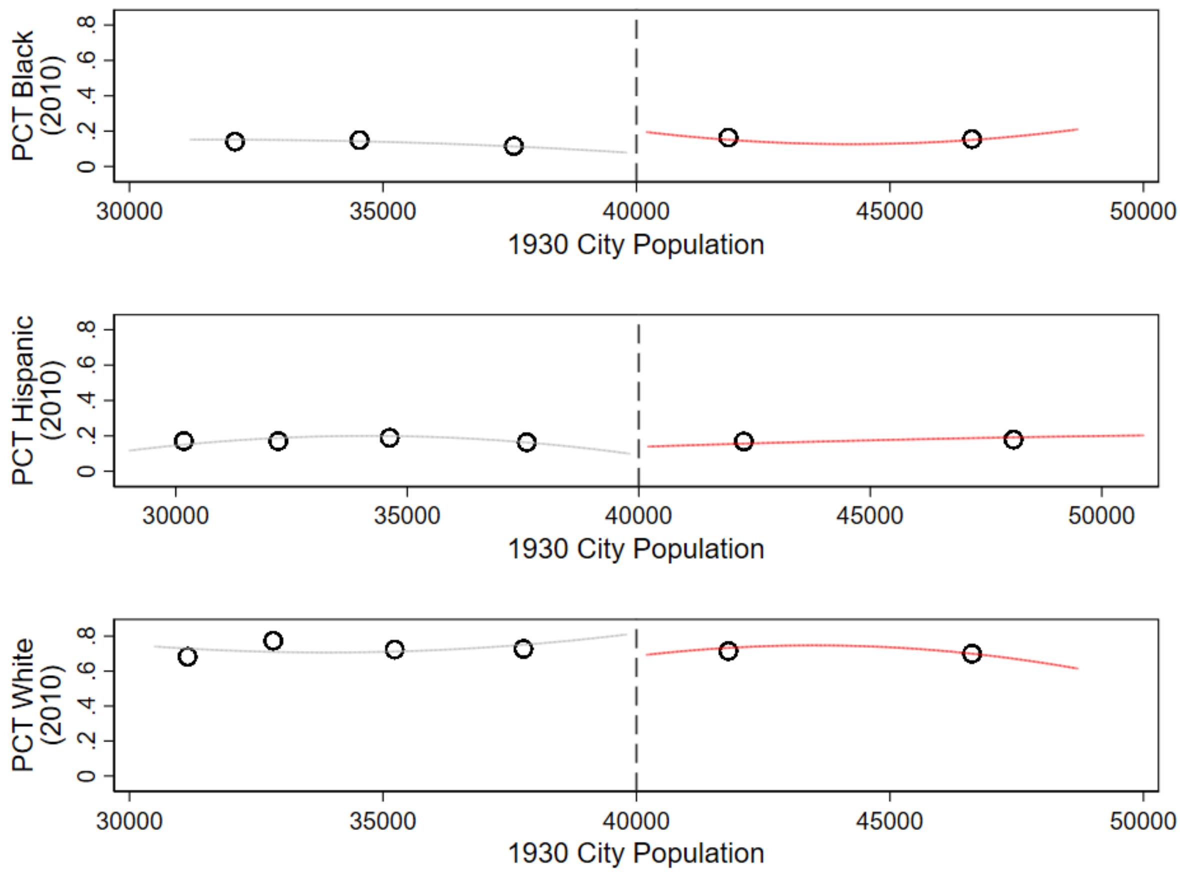
Note: Each figure shows a regression discontinuity diagram where the outcome variable is the log of crime victimizations in a given city in 2015. The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are chosen optimally following (14). There are 133 agencies included in NIBRS 2015 data who report crime outcomes for cities whose 1930 population places them within the optimal bandwidth; there are 84 reporting agencies on the left-hand side and 49 on the right-hand side. The estimates imply that 176 Black and 65 Hispanic crime victimizations per city in 2015 are attributable to redline-mapping. Data sources are individual-level NIBRS crime victimization data and Home Owner Loan Corporation (HOLC) archival records. These figures differ from those in Figure 1.3 in only one way: in Figure 1.3 techniques from (14) are used to select the number of bins on each side of the cutoff optimally, whereas in these figures the bin size is manually selected to show more of the variation in the outcome variable across the running variable.

Figure A5: Impact of Redline-Mapping on Arrests: Between-City Estimates Over Decades



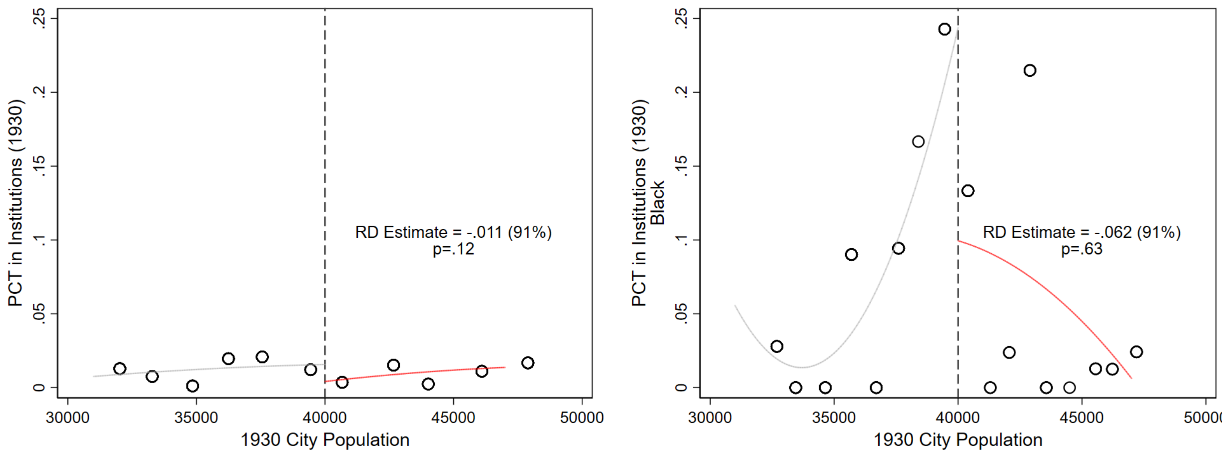
Note: The figure shows a profile of regression discontinuity estimates and 95% confidence intervals obtained by estimating Equation 1.1 on decadal UCR data. (Decadal UCR data is obtained by pooling monthly UCR data across decades.) In each estimate the the outcome variable is black arrest rate per 1,000 people in a given city in a given decade. Data sources are UCR arrest data (1974-2016) and Home Owner Loan Corporation (HOLC) archival records.

Figure A6: Impact of Redline-Mapping on Demography: Between-City Estimates of Compositional Migration



Note: The figure shows a set of regression discontinuity diagrams depicting the possible impact of redline-mapping on present day racial demography. Coefficient estimates are reported in Table A4. Data sources are the 2010 Census and Home Owner Loan Corporation (HOLC) archival records.

Figure A7: Impact of Redline-Mapping on Incarcerated Population: Placebo Tests with Institutional Group Quarters

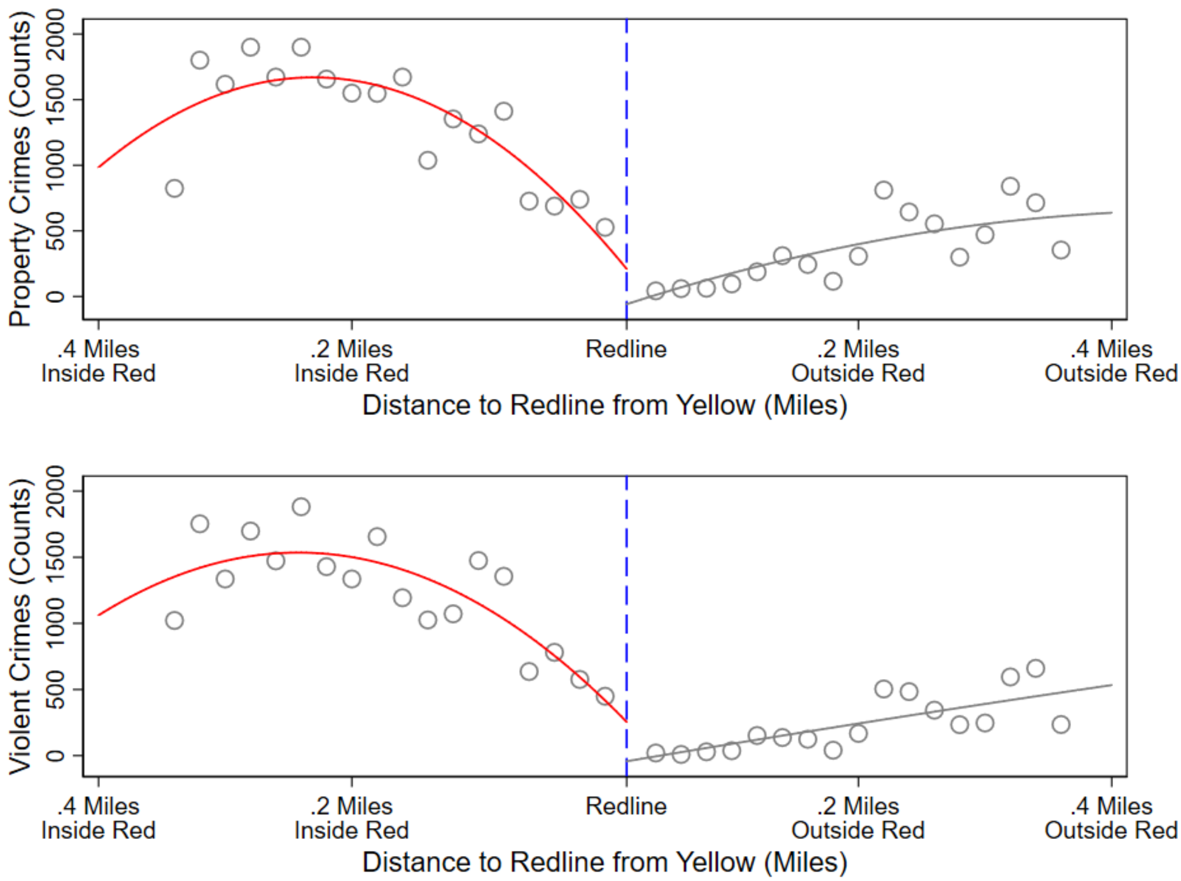


(a) Pre-Period Incarceration

(b) Pre-Period Incarceration: Black

Note: Each figure shows a regression discontinuity diagram. In the top panel, the outcome variable is the share of individuals who report living in an institutional group quarter in a given city in 1930, while in the bottom panel the outcome variable is the share of black individuals who report living in an institutional group quarter in a given city in 1930. Institutional group quarters include correctional facilities, nursing homes and mental hospitals. Starting in 1980, institutional group quarters excludes persons living in non-institutional group quarters such as college dormitories, miliary barracks, group homes, mission and shelters. However, in the 1930 Census, institutionalized group quarters includes "non-inmates" who would have been classified as living in non-institutional group quarters after 1980 (See the IPUMS documentation for the variable "GQ".) In both panels, The running variable is 1930 city population. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth and bin numbers are chosen optimally following (14). Data sources are the 1930 Census, as well as and Home Owner Loan Corporation (HOLC) archival records.

Figure A8: Impact of Redlining on Crime: Within-City Estimates, By Crime-Type



Note: Each figure shows a spatial regression discontinuity diagram for crimes in Los Angeles in 2010. The top panel is restricted to property crimes, while the bottom panel is restricted to violent crimes. Property crimes are defined as those crimes the description of which contains words such as “burglary” and “larceny”; violent crimes are defined as those crimes the description of which contains words such as “murder” and “robbery”. Circles represent bin means, while lines represent fitted quadratic curves. Bandwidth size and bin numbers are both chosen optimally following (14). The running variable is always the distance away from the redline on Home Owners Loan Corporation (HOLC) security maps, and the threshold is the redline itself. I dough-nut out a small region around the threshold to eliminate the small number of crimes committed inside the streets that divide neighborhoods. In all specifications, the sample is restricted to areas which received some HOLC color assignment in 1939. Data sources are city of Los Angeles crime data and HOLC archival records. These figures are the same as those in Figure 1.14, except that these in these figures the running variable is distance away from a redlined area towards an area designated as yellow, while in the figures in Figure ‘1.14 the running variable is the distance away from a redlined area towards an area which received any HOLC color designation.

Table A1: Impact of Redline-Mapping on Present Day Housing Market: Between City Estimates

	(1) PCT Vacant	(2) PCT Mortgaged	(3) AVG Rent
RD Estimate	0.050***	-0.070***	-121.21***
	(0.009)	(0.009)	(26.61)
Observations	3203	3202	3184
Mean	.125	.691	$792.35

Note: Table shows regression discontinuity estimates of the impact of redline-mapping on measures of housing market strength with standard errors reported in parentheses. Observations are at the city level. The outcome variable is the percent of vacant homes, the percent of homes under mortgage and average reported monthly rent in 2010 dollars in columns (1), (2) and (3) respectively. The running variable is always 1930 city population. Bandwidth size is chosen optimally in each column following (14). Slight differences in the number of observations arise from there being different optimal bandwidths for each outcome variable. The reported mean is for non-mapped cities within the optimal population bandwidth. Source: 2010 Census and HOLC archival documents. Significance levels indicated by: * $(p < 0.10)$, ** $(p < 0.05)$, ***$(p < 0.01)$

Table A2: Impact of Redline-Mapping on Housing Stock: Between City Estimates

	(1) PCT Detached Single Family Homes	(2) PCT Attached Single Family Homes	(3) PCT 2-4 Family Housing Units	(4) PCT 5+ Family Housing Units	(5) PCT Mobile Home Units	(6) PCT Other Housing Stock
Panel A1: 1960 Housing Stock						
RD Estimate	0.1203	0.0146	-0.0507	-0.0633	-0.0140	-0.0000
	(0.0734)	(0.0272)	(0.0474)	(0.0404)	(0.0136)	(0.0003)
Mean	.4704	.0615	.1612	.0816	.02053	.0004
Panel A2: 1960 Housing Stock With Black Residents						
RD Estimate	0.2519	0.0621	-0.0974	-0.1668**	-0.0042	-0.0009
	(0.1954)	(0.0930)	(0.0778)	(0.0808)	(0.0139)	(0.0011)
Mean	.3568	.0864	.1571	.1039	.0245	.0006
Panel B1: 1980 Housing Stock						
RD Estimate	0.0367	-0.0089	0.0122	-0.0304	-0.0068	-0.0000
	(0.0398)	(0.0202)	(0.0165)	(0.0243)	(0.0098)	(0.0000)
Mean	.6493	.0457	.0993	.1619	.0140	.00005
Panel B2: 1980 Housing Stock With Black Residents						
RD Estimate	-0.0487	-0.0050	0.0315	0.0246	0.0011	
	(0.0687)	(0.0335)	(0.0291)	(0.0765)	(0.0045)	
Mean	.5487	.0610	.1246	.2256	.0034	
Panel C1: 2000 Housing Stock						
RD Estimate	0.0423	-0.0149	-0.0227	-0.0040	-0.0016	-0.0031
	(0.0647)	(0.0261)	(0.0217)	(0.0467)	(0.0112)	(0.0033)
Mean	.6044	.0668	.1046	.1954	.01988	.0092
Panel C2: 2000 Housing Stock With Black Residents						
RD Estimate	-0.0335	-0.0333	0.0005	0.0479	0.0019	0.0001
	(0.0993)	(0.0380)	(0.0610)	(0.0818)	(0.0112)	(0.0071)
Mean	.4761	.0633	.1432	.3168	.00537	.0102
Observations	143	143	143	143	126	126

Note: The table shows regression discontinuity estimates of the impact of redline-mapping on city-level housing stock and city-level black housing occupancy. The outcome variables are aggregated tabulations of the Census variable UNITSSTR. In panels A1, B1 and C1 the outcome variables measure available housing stock at the city year level; in panels A2, B2 and C2 the outcome variables measure housing stock with black residents at the city year level. The running variable is always 1930 city population. Bandwidth size is chosen optimally following (14). The reported mean is for cities within the optimal population bandwidth. There is a small amount of variation in the number of cities reporting non-missing UNITSSTR values across decades; reported observations are for the 2000 sample. In 1980, the estimate for "other" housing stock with black residents is missing because there is not enough support in the outcome variable over the bandwidth to perform the estimation. The sources are the Decennial Census and HOLC archival documents. Significance levels indicated by: * ($p < 0.10$), ** ($p < 0.05$), ***($p < 0.01$)

Table A3: Impact of Redline-Mapping On Short Run Migration (1940): Between-City Estimates

	(1) Same House	(2) Same Community	(3) Same City	(4) Moved Within County	(5) Moved Wthn St	(6) Btw St (Contig)
City Was HOLC Mapped	0.0670***	0.0617***	0.0617***	-0.00815*	0.00314	0.00114
	(0.00981)	(0.0123)	(0.0123)	(0.00408)	(0.00214)	(0.00270)
Observations	266	266	266	266	266	266
Mean	.2227	.6407	.6407	.4524	.0199	.0394

Note: Table reports estimates of the impact of redline-mapping on various measures of short-run migration. Observations are at the city-level. The estimates are obtained by regressing a given short-run migration measure against an indicator variable for whether a city were mapped. The sample is restricted to cities with a 1930 population between 20,000 and 60,000. Each measure is obtained from respondent's answer on the 1940 Census to questions about residency on April 1, 1935. In column (1) the outcome variable is an indicator for whether or not the respondent reports living in the same house at the time of survey as in 1935. Columns (2)-(4) use similar measures at the community, city and county level. Column (5) uses a measure of moving within the state of residence, and column (6) uses a measures of moving between contiguous states as the outcome variable. The reported mean is for non-mapped cities within this population bandwidth. Source: 1940 Census and HOLC archival documents Significance levels indicated by: * $(p < 0.10)$, ** $(p < 0.05)$, ***$(p < 0.01)$

Table A4: Impact of Redline-Mapping on Demography: Between-City Estimates of Compositional Migration

	(1) PCT Black	(2) PCT Hispanic	(3) PCT White
RD Estimate	0.06	-0.03	-0.01
	(0.07)	(0.08)	(0.07)
Observations	559	559	559
Mean (Bandwidth)	.14	.17	.72
Kernel	Uniform	Uniform	Uniform
Polynomial	Local Linear	Local Linear	Local Linear

Note: Table shows regression discontinuity estimates of the possible impact of redline-mapping on present day racial demography. Corresponding regression discontinuity diagrams are displayed in Figure A6. Observations are at the city level. Data sources are the 2010 Census and Home Owner Loan Corporation (HOLC) archival records.

2. MEDICAID, MARIJUANA AND OPIOIDS: IDENTIFYING SHOCKS TO OPIOID MORTALITY

2.1 Introduction

The United States is in the midst of a drug overdose epidemic driven by a rise in opioid abuse. In 2016, approximately 30,000 deaths were attributable to opioid abuse.[1] This death toll - more than 80 people per day - makes opioid abuse as deadly as car accidents and gun shots ((24)). Because of its magnitude, the opioid epidemic has drawn concern from the public and policymakers alike, prompting many to ask what policies might be able to combat this epidemic.

To what extent has the epidemic been driven by changes in the accessibility of opioids and opioid substitutes? There is anecdotal evidence that in recent years physicians (perhaps with financial motivation from pharmaceutical companies) have been writing a large volume of opioid prescriptions for pain treatment, and have thereby brought about new addictions. Indeed, there are anecdotes in which patients receive standard medical treatments such as surgery and report having developed an addiction to opioids after having been prescribed a powerful, synthetic opioid to treat short-term pain ((25)). There account finds further justification from studies that document associations between the remunerations received from physicians and the volume of opioids those physicians prescribed ((26), (27), (28), (29)). There is also a body of evidence showing that access to an opioid substitute such as Marijuana lowers opioid mortality ((30), (31), (32), (33), (34)).

To disentangle factors pertaining to opioid accessibility from factors pertaining to opioid substitute accessibility, I utilize policy variation both in opioid access and in opioid substitute access. In particular, I exploit the staggered state-level expansion of the Medicaid program (as allowed under the Affordable Care Act) as a natural experiment to ascertain whether increased access to medical services, including prescription drugs, increased opioid-related deaths. I also exploit the

[1] Author's calculations from CDC Mortality Files.

staggered stage-level legalization of Marijuana to see whether the increased availability of an opioid substitute decreased opioid-related deaths.

Utilizing variation in both Medicaid expansion and Marijuana legalization allows me to address two interconnected research questions. First, using variation in Medicaid expansion allows me to answer the question, (1) "has the opioid epidemic been partly caused by increases in opioid accessibility?". Given that I find evidence that opioid-accessibility shocks are responsible for approximately a third of the death-toll, it is natural to ask whether policy ought to aim at restricting access to opioids. On the one hand, my results suggest that restricting access to opioid prescriptions (e.g. encouraging doctors to write fewer prescriptions) could save lives. But, on the other hand, these restrictions might harm those who rely upon, but do not abuse, opioid prescriptions to manage chronic pain ((35)). Furthermore, there is evidence that an existing attempt at restricting access for would-be opioid abusers (implemented by reformulating a popular opioid to be abuse-deterrent) caused substitution to heroin, rather than a reduction of deaths ((36)). Accordingly, using variation in Marijuana legalization allows me to answer the question, (2) "can a policy effectively mitigate opioid-related mortality without restricting access to opioids?". In short, my research design allows me to study the full interaction of the impacts of opioid access and opioid-substitute access on opioid mortality.

Using a panel fixed-effects estimation strategy, I produce separate difference-in-difference estimates of the impact of Medicaid expansion and Marijuana legalization, respectively, on opioid-related deaths. Because this estimation strategy controls for location-specific and time-specific factors, it is able to isolate the causal impact of a given policy shock on opioid mortality. I find no evidence of pre-existing differences in opioid-related mortality trends prior to each policy shock, which suggests that the policy shock is responsible for the changes in opioid mortality that follow it. Furthermore, with respect to Medicaid expansion, I implement a triple difference estimation strategy and find that areas with larger pools of individuals who are both uninsured and Medicaid eligible experienced larger increases in opioid-related mortality than areas with smaller pools of uninsured individuals. I also perform several robustness checks such as including county-year economic covariates and including state-specific time trends, and find that the results are robust.

There are several mechanisms through which the increased physician access (occasioned by Medicaid expansions) could increase opioid-related deaths: new found health insurance gives greater access to prescription drugs including opioid prescriptions, but also reduces the burden of the health care costs borne by the newly insured of engaging in risky behaviors such as using heroin. To try to isolate the mechanism, I use Medicaid claims data to show that opioid prescriptions reimbursed by Medicaid increase differentially based on Medicaid expansion. I also use aggregated data on opioid prescriptions rates for the general population to show that overall prescription rates do not respond differentially as clearly or as strongly as the Medicaid reimbursements do.

This paper makes three contributions to the literatures on the opioid-epidemic, the effects of Medicaid expansion, and the determinants of substance abuse: (1) This paper provides the first evidence that recent increases in health insurance access (Medicaid expansions) and the increases in medical access they occasioned have directly contributed to the opioid crisis; my estimates suggest that recent expansions of Medicaid account for roughly one third of the rise in opioid-related deaths from 2012 to 2016. (2) This paper provides the first evidence of how these shocks to opioid accessibility (Medicaid expansions) interact with shocks to opioid-substitute accessibility (Marijuana legalization). I show that legal access to recreational Marijuana mitigates the effect of Medicaid expansions on opioid-related deaths; my estimates suggest that Marijuana legalization is responsible for reductions in opioid-related deaths that are as large as the increases generated by Medicaid expansions. (3) This paper is also (to the best of my knowledge) the first to estimate, for a given drug about which abuse is a concern, the effects *both* of drug-accessibility *and* drug-substitute accessibility on drug-related mortality.[2] Accordingly, I provide the first policy analysis

[2]For a given drug about which abuse is a concern (e.g., alcohol, marijuana), one finds many examples of separate, stand-alone estimates of either drug-accessibility effects or drug-substitute effects, but not both together.

that compares the relative effectiveness of drug access policies and drug-substitute access policies a policy-maker could use to combat the opioid epidemic.

2.2 Background: Drug Accessibility Policies and the Opioid Epidemic

2.2.1 Opioid Access: Is Opioid Prescription Accessibility to Blame?

Popular discussions of the opioid epidemic in the media are replete with anecdotes that feature patients who are prescribed powerful opioids to manage pain and later find themselves suffering from addiction. Some patients feel that their doctors prescribed these powerful drugs unnecessarily and for their own financial benefit ((25)). These anecdotes motive us to question whether doctors are overprescribing opioids (at least in part because they are motivated by remunerations from pharmaceutical companies) . Indeed there is a large literature on the extent to which physicians generally respond to financial incentives ((37), (38), (39), (40), (41), (29), (28), (27)). In the case of opioids in particular, there is a small literature documenting various correlations between physician reimbursements and prescription volumes ((26), (27), (28), (29)).

These pieces of evidence together paint a picture according to which physician behavior increased the accessibility of opioids and thereby contributed to opioid abuse. Furthermore, utilizing variation in the implementation of Medicare Part D, (42) find that increased opioid access for the Medicare-eligible population had a spillover effect on the Medicare-ineligible population, increasing the opioid-death rates for the non-elderly population who did not gain direct access to opioids. However, there is not yet in the literature a causal estimate of the *direct* impact of the accessibility of opioid prescriptions on opioid mortality.

2.2.2 Opioid-Substitute Access: Marijuana as an Opioid Substitute

There is a body of evidence showing that access to an opioid substitute such as Marijuana lowers opioid mortality ((30), (31), (32), (33), (34)). Because Marijuana is a painkiller it can be used to manage chronic pain and hence is a candidate substitute for opioids. Because Marijuana is much less likely to cause overdoses than opioids, it is also a safe alternative to powerful, synthetic opioids ((43)). However, existing estimates of the impact of Marijuana legalization on opioid mortality have not yet been produced alongside of and compared to comparable supply-side estimates; we do not yet understand the size of the effects of opioid substitute access relative to the size of the effects of opioid access and, accordingly, we don't yet understand whether policy would be better served by addressing the opioid-access or the opioid-substitute access.

2.2.3 Separate Shocks: Medicaid Expansion and Marijuana Legalization

There is evidence that shocks to opioid access brought about by Medicaid expansions are orthogonal to shocks to opioid-substitute acesss brought about by Marijuana legalization Figure A15 shows the cumulative percent of the population exposed to the interaction of these policy shocks.[3] While all states that legalized recreational Marijuana expanded Medicaid, many expander states did not legalize Marijuana. Moreover, many non-expander states did not legalize Marijuana.[4] Table A2 shows estimates of the extent to which Marijuana legalization can successfully predict Medicaid expansion.[5] We see that the success Marijuana legalization has in predicting Medicaid expansion is largely soaked up when other endogenous state-level variables are taken into account (See columns (1) and (2) of Table A2). Furthermore, we see that Marijuana legalization is not successful in predicting the timing of Medicaid expansion (See columns (3) and (4) of Table A2). I conclude that, at least for the purposes of understanding opioid-related mortality, these two policy shocks are separate from each other and can be used as independent sources of policy variation.

2.3 Data

2.3.1 SAHIE Health Insurance Estimates

Data measuring health insurance coverage are taken from Census Small Area Health Insurance Estimates (SAHIE), which measure the number of uninsured at the county-year level. Because

[3] Appendix Table A16 shows the summary statistics of the variables containing the policy variation.

[4] Figure A2 and Figure A14 show the timing of the variation in Medicaid expansion and Marijuana legalization, respectively. Figure A15 shows the interaction.

[5] We already know that Medicaid expansion cannot predict Marijuana legalization since the latter preceded the former.

the Medicaid expansions focused on childless adults with income at or below 138 percent of the Federal Poverty Line, my outcome of interest is the number of uninsured between the ages of 18 to 65 whose reported income falls at or below 138 percent of the Federal Poverty Line. These data are used to obtain a first stage estimate of the extent to which Medicaid expansion increased the number of insured individuals in a given county-year.

Table A1, Panel A contains the summary statistics of these variables, while Figure A6 shows the calendar year trends in these variables by whether or not a state expanded Medicaid between 2014 and 2016. While expander states enjoyed a lower rate of uninsured individuals (about 9 percentage points or 26% off the overall mean (Table A1, Panel A, Row 2), the groups tracked each other well prior to January of 2014 when the Medicaid expansions began (Figure A6).

2.3.2 CDC Individual-Level Mortality Files

Data measuring opioid-related deaths are taken from restricted CDC Mortality files. These individual level data include demographic information about the deceased (race, ethnicity, gender, educational attainment) as well as ICD-10 codes which classify the imputed "underlying cause of death".[6] I count a death as "opioid-related" if one of its cause of death codes involves an opioid abuse.[7]. My key outcome of interest is the count of opioid-related deaths at the county-month level, which I obtain by collapsing the individual level data. I use this measure to obtain a reduced form estimate of the extent to which Medicaid expansion and Marijuana legalization affect opioid-related mortality.

While an average county saw less than one opioid-related death per month, some counties experienced more than 100 such deaths in a month (Figure A18). As the summary statistics contained in Panel D of Table A1 show, expander states average .6 additional opioid-related deaths per month (or approximately 2 additional deaths per quarter). Figure A3 shows trends in opioid-related deaths by expansion status; Figure A12 further breaks down these trends, showing them separately

[6]Each death can be assigned up to 20 such cause of death codes.

[7]This includes the following ICD-10 codes: T40.1 (Heroin), T40.2 (Other opioids), T40.4 (Other synthetic narcotics)

for white men without any college attainment and white women with college attainment. We can see that each of these groups track each other well in the period prior to Medicaid expansion, despite the fact the fact that expanders have a higher overall volume of deaths.

2.3.3 Opioid Prescriptions

There are several mechanisms through which increased medical access could increase opioid-related mortality: new found health insurance gives greater access to prescription drugs including opioid prescriptions, but also reduces the burden of the health care costs borne by the insured of engaging in risky behavior such as using heroin. To isolate the mechanism, I use Medicaid claims data to explore whether opioid prescriptions reimbursed by Medicaid increase differentially based on Medicaid expansion. I also use CDC data on opioid prescriptions rates for the general population to explore whether overall prescription rates respond as strongly as those in the Medicaid claims (Section 2.4.2.3). I detail both data sets just below.

2.3.3.1 CMS State-Quarter Opioid Prescriptions

The Centers for Medicare and Medicaid Services (CMS) claims data give counts of prescription drug products at the state-quarter level for the universe of claims billed to Medicaid. I use these data to measure the extent to which state-level Medicaid expansion affected the number of units of opioids prescribed, the number of opioid prescriptions, as well as the amount reimbursed for opioids.[8].

Table A1, Panel B contains summary statistics for variables measuring opioid drug claims filed through Medicaid. These variables are at the state-quarter level and measure prescriptions for the universe of claims filed through Medicaid. We can see that across several measures, expander status have Medicaid programs which provided more opioids on average than non-expander states.[9] Furthermore, the trends in Figure A8 show that at the beginning of 2014, when Medicaid expan-

[8]I classify prescription drug products as opioids following (44). For details, see Data Appendix.

[9]Figure A22 shows the state-quarter level distribution of opioid prescriptions filed through Medicaid as well as the distribution of the amounts reimbursed by Medicaid.

sion first occurred, there was a significant upward jump in the number and dollar value of opioid prescriptions reimbursed by Medicaid, suggesting that Medicaid expansion is associated with an increase in the volume of opioids accessed.

2.3.3.2 CDC County-Year Opioid Prescriptions

Because CMS state-drug utilization data are derived from the universe of Medicaid claims, I additionally test whether a measure of opioid prescriptions that includes prescriptions outside the universe of Medicaid prescriptions is also responsive to Medicaid expansion. The CDC provides a county-year measure of "retail opioid prescriptions dispensed per 100 persons" ((45)), which includes prescriptions filed through Medicaid and those reimbursed by any other payer, including out of pocket payments. We can see that expander states had slightly lower overall opioid prescriptions rates (about 6 fewer prescriptions per 100 people or about 7% fewer off the overall mean), but that the distributions are not significantly different by expansion status (Panel C of Table A1).

2.4 Medicaid Expansions: Increased Access to Opioids Increased Deaths

In this section, I provide evidence that exogenous increases to health insurance and the medical access it occasioned (including access to prescription drugs) increased opioid-related deaths. I utilize the staggered state-level Medicaid expansion licensed by the Affordable Care Act to provide quasi-random variation in access to health insurance and medical care (including access to prescription drugs). These estimates show the extent to which the opioid epidemic has been driven by increases in opioid accessibility. I also use the state-level decision to legalize recreational Marijuana to show how these opioid access increases interact with opioid-substitute access.

2.4.1 Main Results

2.4.1.1 Estimation

I use a panel fixed effects model to estimate the impact of Medicaid expansion on opioid-related deaths, health insurance, and opioid prescriptions. My main specification is a simple (nondynamic) difference in difference equation:

$$y_{cst} = \alpha_c + \alpha_t + \beta Expansion_{st} + \epsilon_{cst}, \tag{2.1}$$

where y_{cst} is the either the number of opioid-related deaths, the number of uninsured persons, or the number of opioid prescriptions in county c, state s and time period t. I am primarily interested in the coefficient on $Expansion_{st}$, which indicates that the county-month observation is from an expander state after it expanded Medicaid.

To understand the dynamics of the effects, I also use a dynamic panel fixed effects model to estimate the impact of expansion on opioid-related deaths, health insurance, and opioid prescriptions. In the dynamic difference in difference estimator, I include a set of variables which indicate time periods away from Medicaid expansion:.

$$y_{cst} = \alpha_c + \alpha_t + \sum_{\tau=-n}^{n+} \beta_\tau 1(t = T_s + \tau) + \epsilon_{cst}, \tag{2.2}$$

where y_{cst} is the either the number of opioid-related deaths, the number of uninsured persons, or the number of opioid prescriptions in county c, state s and time period t. I am primarily interested in the coefficients on the indicators, $1(t = T_s + \tau)$, each of which indicates how many time periods t in state s a given observation is removed from the first time period in which state s expanded Medicaid, T_s.

2.4.1.2 Reduced Form Estimates: Opioid Death Increases

Ex ante, we expect an expansion in access to health insurance to result in increased access to medical services, which we expect to improve health outcomes and decrease mortality. If, however, physicians were prescribing opioids for pain treatment in ways that, on average, harm patients we might find that Medicaid expansion results in increased opioid mortality. To empirically test this, I estimate equations of the form of Equation 3.4 and Equation 2.2, where the dependent variable is the count of opioid-related deaths.

As Figure A4 shows, prior to expansion, expander and non-expander states were not trending differentially, but that after expansion, states that expanded began to experience an increase in opioid-related mortalities.[10] Table A4 displays the coefficient estimates depicted in Figure A4. The coefficients suggest that in the quarters immediately after expansion, counties in expander states experienced an increase of approximately .3 opioid-related deaths per county-quarter; a year after expansion the coefficients are statistically significant and suggest that Medicaid expansion is responsible for approximately 4 additional deaths per county-year.[11]

The static difference in difference estimators give the same account. Table A3 displays the static difference in difference estimation of Equation 3.4 under different specifications. The estimate in column (1) of Table A3 indicates that Medicaid expansions are associated with approximately .3 more opioid-deaths per county-month or 4 more opioid-related deaths per county-year. Column (2) of Table A3 shows that this result is consistent with OLS estimates using the log of opioid-related deaths. Finally, columns (3) and (4) show that standard non-linear specifications for count variables (Poisson and Negative Binominal regressions) both suggest that Medicaid expansion is associated with a $.05$ increase in opioid-related deaths per county-month or a $.6$ increase in opioid-related deaths per county-year. Though these non-linear estimates are statistically significant and economically meaningful, they attribute an increase ($.6$ more opioid-related deaths per county-year) that is approximately 6 times lower than the increase we found in the OLS estimates (4 more opiod-related deaths per county-year).[12] Each of these estimates is consistent with

[10] My main reduced form estimates use data disaggregated to the county-month level; however for visual ease I display the results in figures based on estimates using the data aggregated to the quarter level. For robustness I also show the Figure with estimates by county-month (Appendix Figure A20).

[11] These results are robust to using rates of opioid-related deaths as the dependent variable instead of counts (See Appendix Figure A21).

[12] This difference is partially attributable to the choice of including counties with no opioid-related deaths in the sample. Table A15 shows that approximately 10 percent of counties never record an opioid-death in my sample. These zero opioid-death counties are included in the the OLS estimates in columns (1) and (2) of Table A3, but dropped in the non-linear specifications reported in columns (3) and (4). The choice to drop the counties that never record an opioid-death is driven by technical details involved in the maximum likelihood estimation techniques standardly used in these non-linear models. An OLS estimate performed on a sample that drops zero opioid-death counties also gives

my preferred estimate (column (1) of Table A3) and tells qualitatively the same story: Medicaid expansion is associated with a statistically significant and economically meaningful increase in opioid-related deaths.

2.4.1.3 Identification: Threats to Interval Validity

In order to interpret the estimates from Equation 3.4 and Equation 2.2 as causal estimates, we must believe that (conditional on county and month fixed effects) the state level decision to expand Medicaid is not correlated with other factors that might increase opioid mortality.[13] We might reject the causal interpretation if, for example, states that expanded insurance access for the poor were, during this same time period, also implementing policies that in some way encouraged substance abuse. Ex ante, however, it would be reasonable to expect that states which expanded Medicaid were also actively pursuing various means of *improving* health outcomes and *decreasing* substance abuse and such an endogeneity concern would suggest that my estimates should be taken as lower bound estimates of the true effect. Nevertheless, to ensure against any possible threats to interval validity, I provide several pieces of empirical evidence which justify a causal interpretation of the estimates.

2.4.1.3.1 Identification: Parallel Trends in Deaths, Prescriptions

The main evidence to justify a causal interpretation comes from the fact that there are no pre-period differences either in calendar-year trends or in dynamic difference in difference estimates across expander and non-expander states, with respect to opioid-related death measures, opioid prescription measures, and uninsured individuals. Figure A3 shows that, prior to all Medicaid expansions, expander states track non-expander states in counts of opioid-related deaths[14]; Figure A4 shows that the dynamic difference in difference estimates in the pre-period hover around zero, con-

the same qualitative story as the non-linear estimates.

[13]Mathematically, we must assume: $E(\epsilon_{cst}|\alpha_c, \alpha_t, Expansion_{st}) = 0$.

[14]Appendix Figure A19 shows that these groups also track each other in terms of log opioid deaths.

firming that expander and non-expander states were on common trends prior to expansion.[15]

Secondly, as Figures A6 and A7 show, prior to expansion, expander and non-expanders states share common trends in the number of uninsured individuals residing in their counties. Whether we measure in counts or in logs, we see that expanders and non-expanders track each other well before 2014, but that expansion states see a sharp drop in their uninsured population beginning in 2014, when the expansions first occurred (Figure A6). The difference in difference estimates in the pre-period also hover around zero, confirming that the groups share a common trend (Figure A7).

Lastly, as Figure A8 shows, prior to all Medicaid expansions, expander states track non-expander states in both the number of opioid prescriptions reimbursed by Medicaid and the amounts reimbursed for those opioid prescriptions. Furthermore, as Figure A9 shows, the difference in difference estimates in the pre-period hover around zero both for opioid units reimbursed by Medicaid and amounts reimbursed. I conclude that in terms of the outcome variable and other immediately relevant variables, expander and non-expander states shared a common trend before expansion, and hence that the difference-in-difference estimates are to be interpreted causally.

2.4.1.3.2 Identification: Determinants of Expansion

To further ensure that my identification strategy is valid, I consider a rich set of state level covariates including variables which measure the political environment of the state, the expenditure portfolio of the state, as well as demographic and economic conditions. I then estimate an equation of the form:

$$Pr(Expansion_s) = \sum \beta_{s,pol} Z1_{s,pol} + \sum \beta_{s,exp} Z2_{s,exp} + \sum \beta_{s,demog} Z3_{s,demog} + \sum \beta_{s,econ} Z4_{s,econ} + \epsilon_s \tag{2.3}$$

where $Z1$ is a vector of variables measuring the state's political environment, $Z2$ is a vector of variables measuring state level expenditures, $Z3$ is a vector of variables measuring demography,

[15]The dynamic difference in difference estimates in the pre-period also hover around zero when the dependent variable is opioid mortality measured as a rate (See Appendix Figure A21).

and $Z4$ is a vector of variables measures economic conditions. Column 1 of Table A5 shows the results of estimating Equation 2.3. We can see that one of the strongest predictors of expansion is the percent of the state's lower chamber which is Republican. This along with the coefficient on education expenditures, welfare expenditures, and percent white, suggests that the state level decision to expand was strongly associated with the state-level political environment in the time-period prior to expansion.[16] I conclude from these results that expansion was not driven by state-level public health conditions relevant to substance abuse, and in particular opioid abuse; expansion was driven by the pre-period political composition of the state legislature.

2.4.2 Robustness, Mechanism and Heterogeneity

2.4.2.1 Robustness Checks: DDD, Controls and State-Specific Trends

If Medicaid expansion were responsible for increasing opioid-related deaths, we would expect to find larger increases of deaths in those counties where Medicaid expansion was likely to occasion a very large health insurance increase, than in those counties where expansion was likely to occasion a very small health insurance increase. I exploit county-level pre-period differences in the volume of uninsured individuals to test whether opioid-related deaths responded more strongly to Medicaid expansion in counties with a higher volume of uninsured individuals prior to expansion than in counties with a lower volume of uninsured individuals prior to expansion. First, I take the distribution of individuals in a county who are both uninsured and have an income at or below 138% of Federal Poverty line and I assign counties to various quantiles of this distribution. For example, in the lowest quintile of this distribution, the average county had 240 uninsured people at or below 138% of the FPL (which is 37% of the population at or below 138%), while in the highest quintile of this distribution, the average county had 27,000 uninsured at or below 138% of the FPL (which is 39% of the population at or below 138%)[17]. I then run my preferred difference

[16] Column 2 of Table A5 shows that these and other associated factors were also predicative of the timing of the state-level decision to expand (since some states expanded at a time other than January 2014).

[17] I consider the volume of individuals aged 18-65 whose income falls at or below 138 percent of the federal poverty line (FPL) because adults without children who fell into this subpopulation became newly eligible for Medicaid under

in difference estimator separately upon samples that include only counties in a given quantile. As Figure A5 shows, the difference in difference estimates are small and nearly zero for counties who had a small volume of uninsured individuals prior to expansion, but these estimates grow larger as the sample features counties with larger volumes of uninsured individuals prior to expansion.[18] Because these difference in difference estimates suggest that my overall results are driven by counties with above median uninsured volumes, I also consider a triple difference specification where I include all pairwise interactions between an indicator for being in an expander state after expansion and an indicator for having an above median uninsured volume in the period before expansion. The coefficients reported in Table A6 show that, across a range of specifications, counties with above median uninsured populations prior to expansion saw larger increases in opioid-related deaths than those with below median uninsured populations. These results are what we would expect to find if opioid-related deaths were increased by the new found health-insurance access which Medicaid expansion occasioned.

Furthermore, the baseline estimates in Table A3 are robust to the inclusion of county-year economic covariates and state-specific time trends. Table A7 reports OLS estimates of the impact of Medicaid expansion on opioid-related deaths with observations aggregated to the county-quarter level. Column (1) of Table A7 is comparable to column (1) of Table A3 (which was estimated at the county-month level). Column (2) of Table A7 shows that these estimates are robust to the inclusion of the contemporaneous county-year unemployment rate as well as its lag. Column (3) of Table A7 shows that these estimates are also robust to the inclusion of state-specific trends by calendar year. Finally, column (4) shows that the estimates are robust to the inclusion of both the state-specific trends and the county-level unemployment covariates.

Lastly, Table A8 shows that the baseline estimates in Table A3 are robust to the inclusion not

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the expansions in question

[18]The variance of these estimates also grows larger as the sample features counties with larger and larger volumes of uninsured individuals. This is not due to the different samples being of different size. This growing variance reflects various sorts of heterogeneities that are more strongly present in the highest portions of the quantiles: including controls for pre-period county demography attenuates some of this increased variance.

only of state specific linear trends, but also to the inclusion of six separate state-level controlled substance prescribing policies enacted from 2012-2016. Because these policies were designed in part to reduce opioid abuse and opioid related deaths, they are candidate policy confounders. For example, prescription drug monitoring programs (PDMP), added by 19 states over this period, collect data on controlled-substance dispensing to flag potentially excessive prescribing behavior (See (46) for a full description of each policy). Following (47), I test for whether these potential confounders are responsible for the variation in opioid mortality I would otherwise attribute to Medicaid expansion. In columns (3)-(6) of Table A8, I include various combinations of these potential confounders as regressors (alongside state specific linear trends). The main results are robust to the inclusion of these policies both individually and collectively.[19]

2.4.2.2 First Stage Estimates: Health Insurance Increases

It is well known that the Medicaid Expansion occasioned by the Affordable Care Act increased the number of those with health insurance (See (48)). Nevertheless, in order to produce first stage estimates to aid the interpretation of my main reduced form estimates, I estimate equations of the form of Equation 3.4 and Equation 2.2 where the dependent variable is the count of uninsured people aged 18-65 whose income falls at or below 138 percent of the federal poverty line. This age and income restriction is chosen because adults without children who fell into this subpopulation became newly eligible for Medicaid under the expansions in question.

Figure A7 shows the dynamic difference in difference estimates for the uninsured population, obtained by estimating an equation of the form of Equation 2.2. Table A9 displays estimates of Equation 3.4 under four different specifications: OLS estimates using counts, OLS estimates using logs, Poisson and Negative Binomial. All four specifications give strongly consistent estimates.[20] In my preferred specification I find that, for an average county, Medicaid expansion led approxi-

[19] An extended version of the state-year policy variation in (46) was generously shared by Jennifer Doleac and Anita Mukherjee, who utilize these data in similar robustness checks in (47).

[20] Point estimates of the newly insured (with an income at or below 138 percent of the federal poverty line) attributable to Medicaid are: $2,884$, $2,668$, $2,554$ and $3,254$ for OLS with counts, OLS with logs, Poisson and Negative Binomial, respectively.

mately 2,800 more individuals to be insured per year.

2.4.2.3 Mechanism: Opioid Prescription Increases

An exogenous increase in health insurance access could affect drug abuse through at least two channels:

1. *Medical Access:* increased access to health insurance increases access to physicians, which, in turn, increases access to prescription drugs that can be abused

2. *Moral Hazard:* increased access to health insurance lowers the health care cost burden borne by the insured of the adverse effects from drug abuse, and consequently may increase the abuse of prescription opioids or illegal opioids such as heroin.

While it is difficult to rule out the moral hazard channel completely, if it were the case that Medicaid expansion significantly increased the volume of opioid prescriptions, this result would provide evidence that access to prescriptions was at least a significant channel through which increased health insurance access increased opioid-related mortality. By contrast, if it were the case the Medicaid expansion was *not* associated with significant increases in opioid prescriptions, we would conclude that access to prescriptions was *not* a channel through which increased health insurance affected mortality, and alternative channels such as the moral hazard account would appear more likely.

Figures A9 and A10 show estimates of Equation 2.2 where some measure of opioid-drug reimbursement by Medicaid is the dependent variable. For example, in Panel (a) of Figure A9, we see the dynamic estimates of the counts of opioid drug units reimbursed by Medicaid and in Panel (b) the dynamic estimates of the amounts reimbursed by Medicaid for those drug units. While the difference in difference estimates hover around zero prior to expansion, after expansion we find a large and rather pronounced relative increase in the expander states. Table A10 shows the static difference in difference estimates of Equation 3.4 with the same dependent variables as Figures A9 and A10. Column (1) of Table A10 reports that Medicaid expansion led 2,800,000 more opioid units to be prescribed per state-quarter or 175,000 more opioid units to be prescribed per county-year.[21]

This estimate should be treated with caution: it suggests that Medicaid was able to reimburse claims for 175,000 more opioid drug-units in an average county-year in states which expanded. However, the number of prescribed opioid units attributable to expansion still could have remained the same (i.e. there could be no differential increase) for at least two reasons:

1. Medicaid claims were reimbursed at higher rates in expander states; in other words, the same volume of opioids was prescribed and given out in all counties, but the prescriptions were financed independently of Medicaid more frequently in non-expander states.

2. Opioid prescriptions for patients not filing with Medicaid increased differentially by Medicaid expansion; in other words, the same volume of opioids was prescribed and given out in all counties, but more prescriptions were given to non-Medicaid patients in non-expander states.

To test (1), I exploit breakdowns within the CMS state-drug utilization data which allows me to observe the amount reimbursed by Medicaid for all Medicaid claims and the amount reimbursed by some other payer for all Medicaid claims. If the first story were true we would expect to find that the volume of opioid prescriptions reimbursed by a payer other than Medicaid responds to Medicaid expansion. But Figure A10 does not provide evidence of this. Medicaid reimbursement patterns are not affected by the expansion; the volume increase in opioids seems to reflect a real increase in drug units given out, not a shift in the composition of how they were reimbursed.

To test the second story, (2), I use CDC data measuring the overall opioid prescription rate for a given county, which includes both Medicaid claims and non-Medicaid claims aggregated together. If the second story were true, we would expect to find that the overall opioid prescription rate would respond differentially to expansion, but Figure A11 shows that the overall opioid rate

[21] Calculated as (2.8 million * 4 quarters)/ 63 counties per state. The average state in the US has 63 counties.

does not to respond to expansion.[22] This suggests that the increases through Medicaid were not differentially offset by prescriptions financed through other insurance payers (or with out of pocket payments).

2.4.2.4 Heterogeneity: White Men Without College Hit Hardest

The results of Sections 2.4.1.1- 2.4.2.3 suggest that approximately a third of the death-toll involved in the opioid epidemic is attributable to increases in opioid access. Descriptive evidence, however, suggests that the rise in "deaths of despair" fell largely on white non-Hispanic men without college degrees ((49)). If these effects are partly coming from increases in opioid accessibility, we would still expect to find a similar sort of heterogeneity by demographic breakdown. Figure A12 shows trends in opioid-related deaths restricting to white men without college degrees, and then again restricting to white women with college degrees; Figure A13 shows estimates of Equation 2.2 restricting to white men without college degrees, and then again restricting to white women with college degrees. These estimates suggest that the increases associated with Medicaid expansion are considerably larger for white men without college degrees; indeed they are more than 7 times larger than the largest estimates for college educated white women (See column (1) of Table A11).[23]

2.4.3 Marijuana Legalization: Interaction of Opioid Access with Opioid-Substitute Access

In this section I provide evidence that exogenous increases in the availability of an opioid-substitute decreased opioid-related deaths even conditional on increases in opioid access. I utilize state level variation in the legality of Marijuana to provide quasi-random variation in the availability of an opioid substitute. I interact Marijuana legalization with Medicaid expansion to identify

[22] Appendix Figure A11 shows that the derivative of the overall opioid rate seems to increase following expansion. In the "overlapping sample", we see an increase in the overall opioid prescription rate (Figure A5). However, neither of these increases are enough to fully account for the estimated increases in units prescribed.

[23] As a percent of the mean, the increases are approximately twice as large for white men without college degrees as they are for white women with some college (42% increase vs a 19% increase). For the reader interested in further exploration of heterogeneity, Appendix Figure A25 shows estimates separately by race and educational attainment cells.

the interactive effects of access to opioid and opioid-substitutes.

There are at least two candidate measures of Marijuana legality - medical Marijuana and recreational Marijuana. I focus on the latter since this is mostly likely to impact a white non-college male who is a candidate for a “death of despair” ((49)): without steady labor market opportunities or reliable health care access, medical Marijuana would not provide such an individual with an accessible substitute to an opioid, but recreational Marijuana legalization would lower the effective cost of using Marijuana.[24] I always measure legalization of Marijuana based on the date legalization was enacted rather than the date dispensaries officially became operational.

Whenever something is first legalized, it is difficult to measure the first stage impact of that legalization, since it is unlikely for the first-stage to be measured well during the pre-period when the activity in question was illegal. Accordingly, surveys concerning Marijuana use conducted prior to legalization should be treated with caution because respondents have an incentive not to answer truthfully in the period when Marijuana use is illegal. Thus, I do not attempt to identify a first-stage effect of Marijuana legalization on Marijuana use. The reader can consult (30) and (31) and their references for studies that use survey data and find that legalization is associated with increases in self-reported use.

There already exist studies that measure the impact of Marijuana legalization on opioid-abuse and opioid mortality (See (50), (51), (32), (34)). My study complements their studies by using more exact individual level data as well as interacting the the impact of Marijuana legalization with the impact of Medicaid expansion. I will show that the mortality increases attributable to Medicaid expansion can be almost entirely mitigated by increasing opioid-substitute accessibility.

2.4.3.1 Reduced Form Estimates: Opioid Death Decreases

Ex ante, we expect an increase in access to a less dangerous substitute painkiller to decrease opioid use, decrease opioid addictions, and thereby decrease opioid-related deaths. Indeed, there

[24] Since a significant portion of the Marijuana legalization occurred before Medicaid expansion, an individual in the Medicaid eligible pool could not likely use Medicaid to access medical Marijuana products.

are several studies that find effects of Marijuana legalization on opioid mortality that point in this direction (See (50), (51), (32), and (34) for an overview). To produce estimates of this opioid-substitute accessibility shock, I estimate equations of the form of Equation 3.4 and Equation 2.2, where the left hand side variable is the count of opioid-related deaths.

As Figure A17 shows, I find that opioid-related mortality decreased in states that legalized Marijuana by slightly over 1 death per county-quarter or 4 deaths per county-year. Table A12 displays the static difference in difference estimation of Equation 3.4 under different specifications. The point estimates in columns (1)-(4) are strongly consistent with each other, and each suggests that Marijuana legalization is associated with a decrease of approximately .332 opioid-related deaths per county-month (or a decrease of approximately 4 opioid-related deaths per county-year).

My estimates are consistent with those in the literature. (32) finds that the legalization of recreational marijuana is associated with a 6% reduction in the opioid-prescription rate for Medicaid-covered individuals (which amounts to a 3,000 fewer total prescriptions per state-quarter).[25] (50) finds that certain Marijuana legalization policies were responsible for lowering pain reliever treatment admissions by 18.5% and (together with the presence of active dispensaries) were responsible for reducing opioid-related deaths by 18%. These estimates are similar in magnitude to those in (51), which estimates that legalization is responsible for a 25% reduction in opioid-related deaths.[26] My preferred estimates suggest that Marijuana legalization was responsible for .3 fewer opioid-related deaths per county-month from 2012 to 2016, which is a reduction of 37% off the mean. My estimates are slightly higher than other estimates since I am studying outcomes for a slightly later time period, chosen to facilitate the comparison to Medicaid expansion.[27]

[25]The average number of opioid-prescriptions per 1,000 enrollees during 2010-2016 was 162.04. Approximately 65 million individuals were enrolled in Medicaid per year over this period, which suggests that for an average state approximately 320,000 individuals were enrolled in a quarter.

[26](52) studies time trends in Colorado and find that legalization is associated with .7 fewer deaths per month, a reduction of 6.5%

[27](51) uses a sample from 1999 to 2010 while (50) adds 2011-2013 to the sample. My sample is from 2012 to 2016.

2.4.3.2 Back of the Envelope Calculations

In Section 4, I have been discussing estimates which suggest that, for an average county, experiencing Medicaid expansion led to approximately 2,800 more people having health insurance a year (Section 2.4.2.2), approximately 175,000 additional opioid units being prescribed in a year (Section 2.4.2.3), and approximately 4 additional opioid-related deaths per county year (Section 2.4.1.2). Thus, for every opioid-related death, my estimates associate 700 additional insured residents and 44,000 additional prescribed opioid-units; this further suggests that, on average, a newly insured Medicaid enrollee was prescribed 63 opioid units.

Lastly, if we assume that the access to medical services occasioned by Medicaid expansion causes opioid-deaths *only* through its causing an increase in opioid-prescriptions written to the newly insured, then a back of the envelope calculation suggests that .14% of the newly insured pool experienced an opioid-related death. This final statistic should, however, be treated with caution both because there could be significant peer-effects and spillovers at work if the newly insured share prescribed drugs with their peers and because the access to medical services that Medicaid expansion occasioned could be associated with other factors which could influence risky-behavior such as using illicit opioids.

An analogous back of the envelope calculation suggests that the effect of national-level legalization of recreational Marijuana would save approximately 12,000 opioid-related deaths per year. These results suggest that a significant part of the opioid epidemic is attributable to factors pertaining to opioid-substitute accessibility.

The full welfare implications of recreational Marijuana legalization, however, remain less clear. Although it is clear that Marijuana is safer than opioids in that it is far less addictive and carries a much lower risk of overdose ((53), (50)), there could still be substantial social welfare costs to the legalization of recreational Marijuana if it acts as a complement to encourage other risky behavior. Nevertheless, the evidence on the legalization of medical Marijuana is encouraging in that it suggests that increased access to Marijuana is *not* associated with other increases in risky behavior. For example, there is evidence that medical Marijuana legalization does not increase either alcohol use or Marijuana use among minors ((54), (55)). Furthermore, among adults, medical Marijuana legalization is associated with increased Marijuana use, and this increased use is at least in part due to adults substituting Marijuana for alcohol, a substitution which may be responsible for the fact that Marijuana legalization is associated with a 9 percent decrease in traffic fatalities ((56)).[28] Though these results suggest that increased Marijuana access is not necessarily associated with increases in alcohol consumption and traffic fatalities, more work is needed to understand how these outcomes respond to recreational Marijuana legalization, as opposed to medical Marijuana legalization.

2.4.3.3 Interactive Estimates: Opioid Access and Opioid Substitute Access

In Section 2.4.1.2 we saw that increasing in opioid access increased opioid deaths. In Section 2.4.3.1 we saw that increases in opioid-substitute access decreased opioid deaths. In Section 2.4.3.2, we saw that the magnitude of the death increase caused by increased opioid access was roughly equal to that of the death decrease caused by opioid-substitute access. In this Section, I explore the interaction between opioid access and opioid-substitute access to confirm that the death increases associated with increased access to opioids can be mitigated by increased access to opioid-substitutes.

Table A13 reports results from estimating Equation 3.4 separately for states which, at some point before 2016, legalized recreational Marijuana and for states which did not. Comparing column (1) to column (2) of Table A13 shows that Marijuana legalizing states saw opioid related deaths increases that were 73% smaller than the increases in states that never legalized Marijuana. Table A14 reports the results from estimating a specification of Equation 3.4 in which both Medicaid expansion and Marijuana legalization as well as their interaction are included as regressors.

[28]These results should be contrasted with (57) who find that in Colorado the trend in the proportion of drivers in a fatal motor vehicle crash who were Marijuana-positive increased slightly after medical Marijuana legalization in 2009. (57) do not compare these trends to contemporaneous trends in other states.

Coefficient estimates suggest that a state which *both* expanded Medicaid *and* legalized recreational Marijuana would expect to see a modest net reduction in opioid-related deaths.[29] The estimated opioid death reduction would be .153 deaths per county per month[30], or about 2 fewer deaths per county per year.

2.5 Conclusion: Policy Can Mitigate Deaths Without Restricting Access

In order for policy to be effective at reducing opioid-related deaths, it must be aimed at the underlying cause of the epidemic. To isolate factors pertaining to drug-accessibility, I utilize policy variation both in opioid access and in opioid-substitute access. First, I exploit the staggered state-level expansion of the Medicaid program (as allowed under the Affordable Care Act) as a natural experiment to ascertain whether increased access to medical services, including prescription drugs, increased opioid-related mortality. Next, I exploit the staggered stage-level legalization of recreational Marijuana to see whether the increased availability of an opioid-substitute decreased opioid-related mortality.

I find that states that expanded Medicaid under the Affordable Care Act saw substantial increases both in opioid prescriptions and in opioid-related deaths. These results vary strongly by demography, being driven largely by deaths of white men without college degrees. A back of the envelope calculation suggests that for an average county, Medicaid expansion caused approximately 2,800 more people to be insured per year, 175,000 more opioid units to be prescribed per year, and 4 additional opioid related deaths per year. Overall, these opioid accessibility shocks explain about 12,000 opioid deaths per year, or nearly a third of the overall death toll. I also find that, for an average county, recreational Marijuana legalization (i.e. legalization of a substitute painkiller) led to 4 fewer opioid related deaths. Overall, these opioid-substitute accessibility shocks also explain about 12,000 opioid deaths per year.

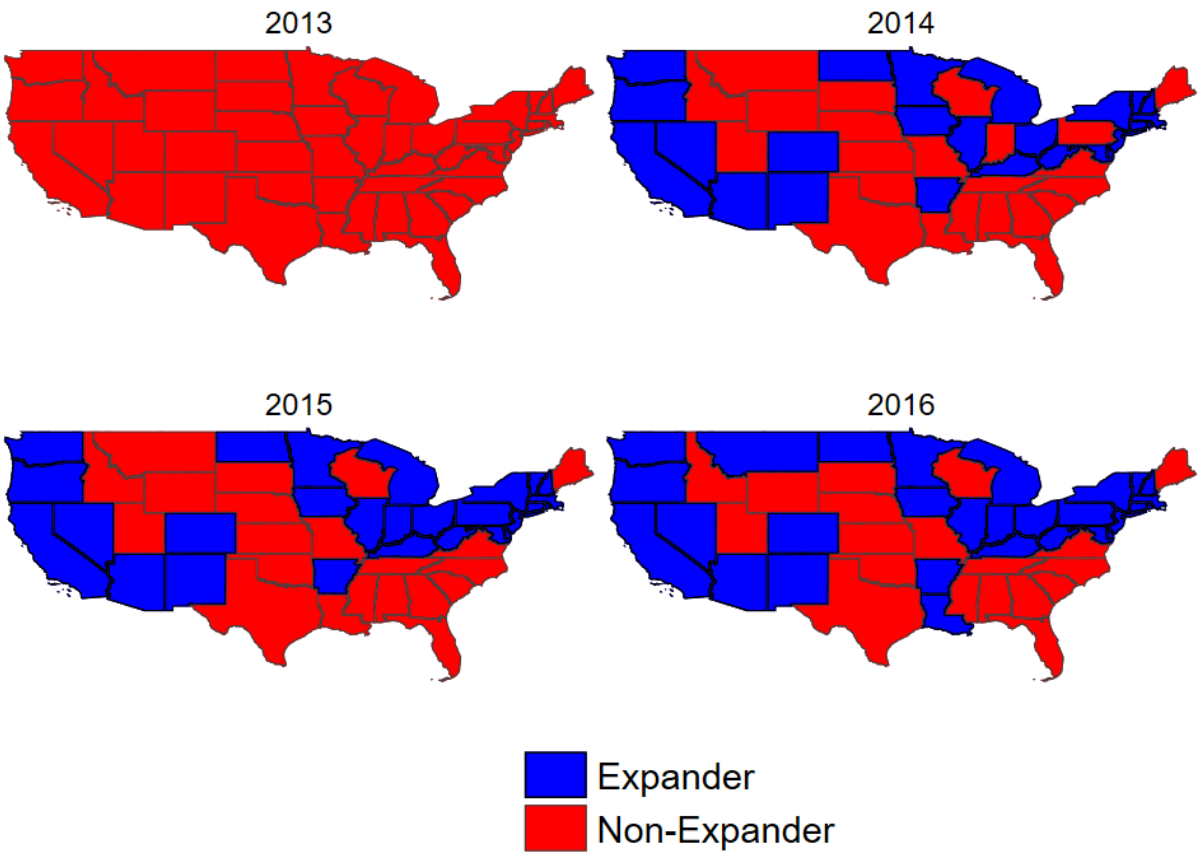
[29] It is also worth noting that the opioid death estimate associated with Medicaid expansion holds even conditional on Marijuana legalization; the unconditional estimate is .319 additional deaths/county-month, while the conditional estimate is .367 additional deaths/county-month.

[30] Obtained by simply summing the estimates reported in Table A14

My research design allows me to study the full interaction of the impacts of opioid access and opioid-substitute access on opioid mortality. Given that I find evidence that opioid-accessibility shocks are responsible for approximately a third of the death-toll, it is natural to ask whether policy ought to aim at restricting access to opioids. On the one hand, my results suggest that restricting access to opioid prescriptions (e.g. encouraging doctors to write fewer prescriptions) could save lives. But, on the other hand, these restrictions might harm those who rely upon, but do not abuse, opioid prescriptions to manage chronic pain. Furthermore, there is evidence that an existing attempt at restricting access for would-be opioid abusers (implemented by reformulating a popular opioid to be abuse-deterrent) caused substitution to heroin, rather than a reduction of deaths ((36)). Accordingly, using variation in Marijuana legalization and interacting these policy changes with Medicaid expansion allows me to show that a policy maker can effectively mitigate opioid-related mortality without restricting access to opioids. In particular, my results suggest that a policy-maker could increase access to prescription opioids *without* increasing opioid-related mortality so long as the policy maker also increases access to safe and reliable substitutes such as Marijuana.

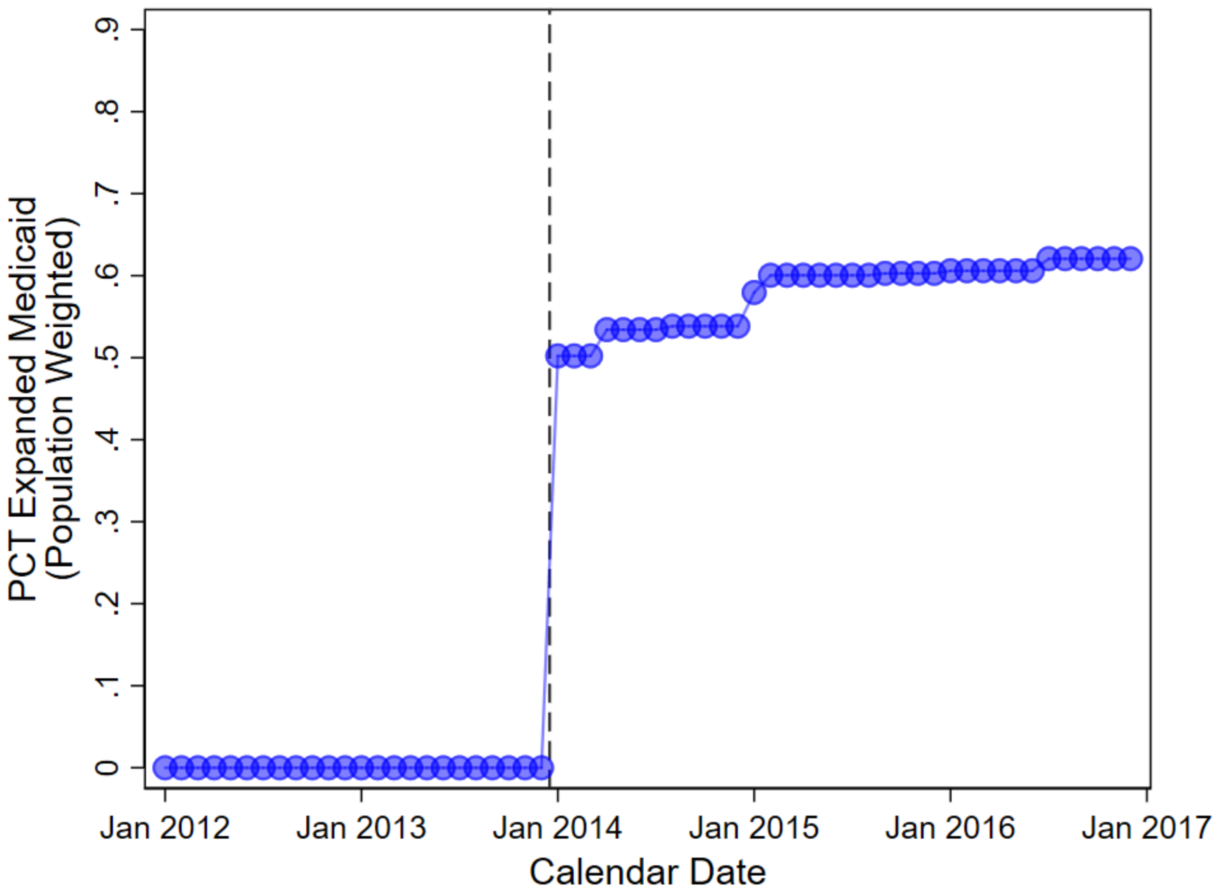
Figures and Tables

Figure A1: Medicaid Expansion Map



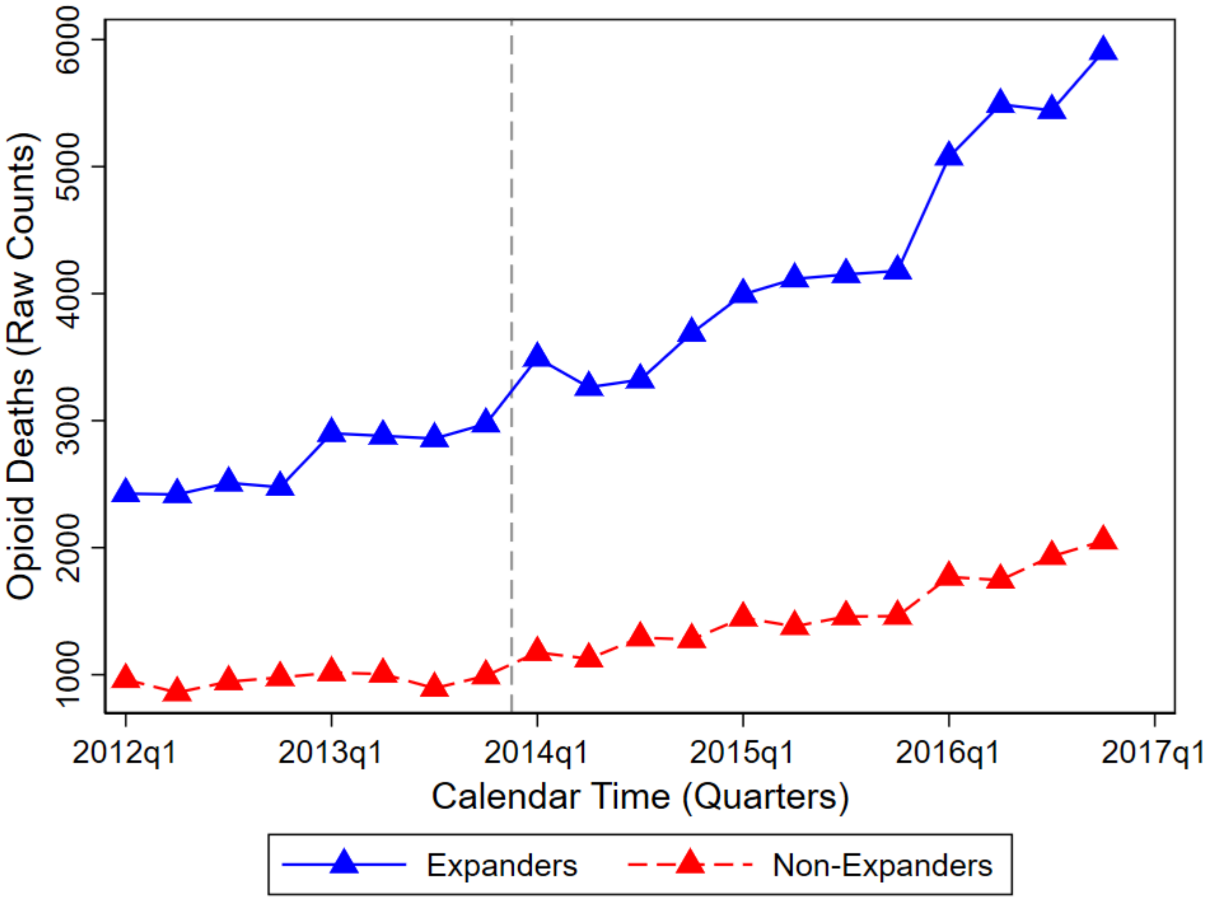
Note: Figure shows which states expanded Medicaid (under the Affordable Care Act) by the end of a given calendar year. Expansions began in January of 2014.

Figure A2: Medicaid Expansion under the ACA (2010-2016)



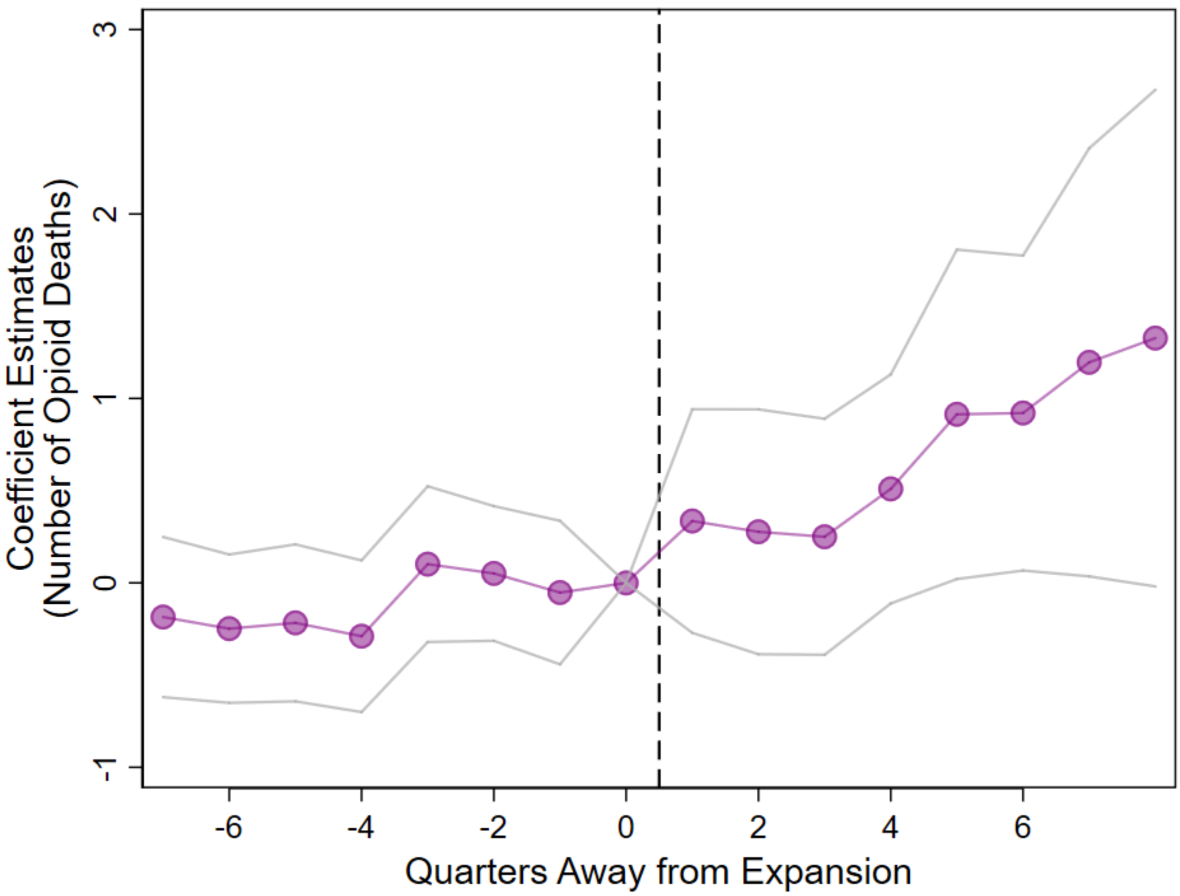
Note: Figure shows the share of the population exposed to Medicaid expansion under the Affordable Care Act. Observations are at the state-month level. Expansion began in January of 2014. 2010 state populations are used as population weights

Figure A3: Medicaid Expansion and Opioid-Related Deaths



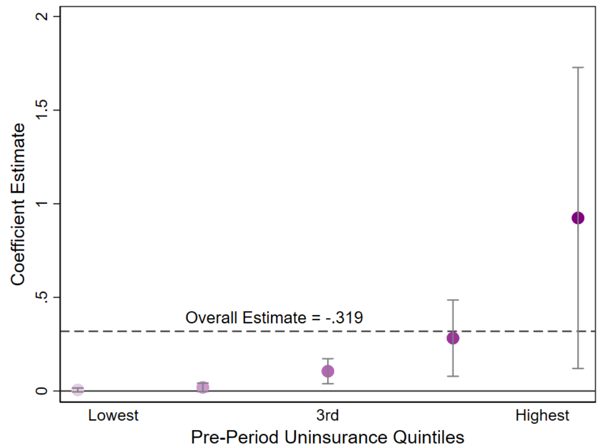
Note: Figure shows trends in opioid-related deaths. Observations are at the county-month level and collapsed to the county-quarter level for visual ease. Trends are shown separately for counties in states that expanded Medicaid and those that did not. The sample includes all states and all demographics. Expansions began in January of 2014. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A4: Impact of Medicaid Expansion on Opioid-Related Deaths

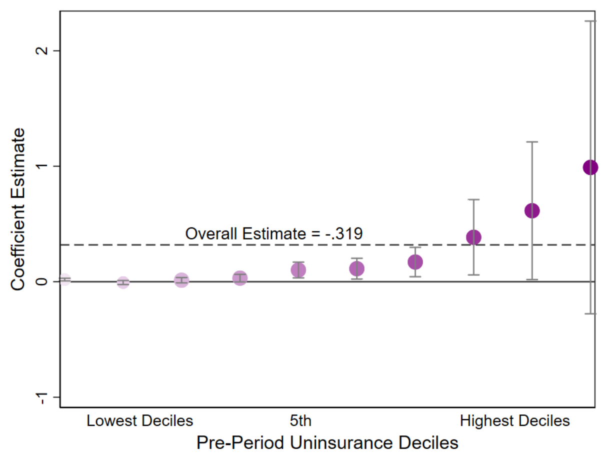


Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with the count of opioid-related deaths as the dependent variable. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A5: Impact of Medicaid Expansion on Opioid-Related Deaths: by Pre-Period Uninsurance



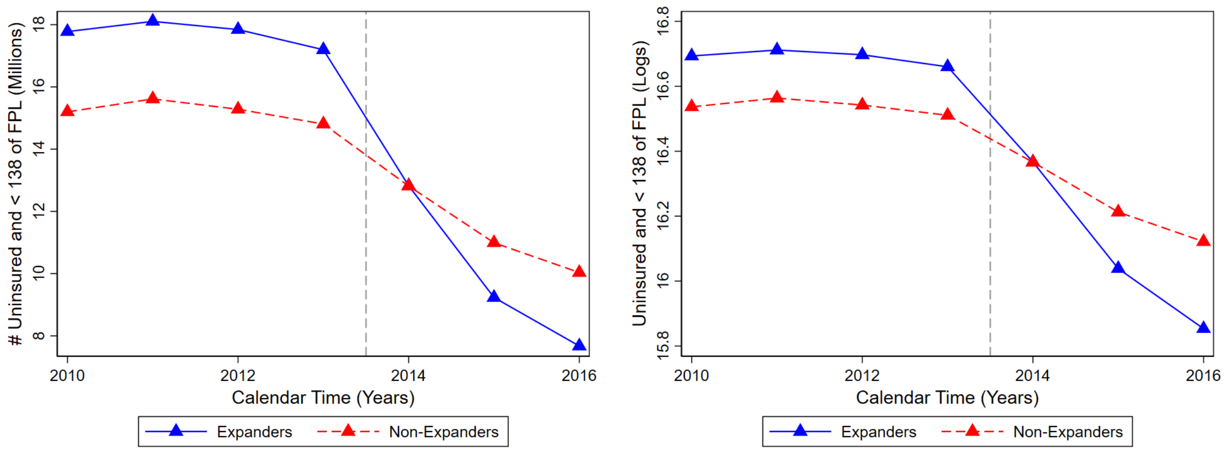
(a) By Quintiles



(b) By Deciles

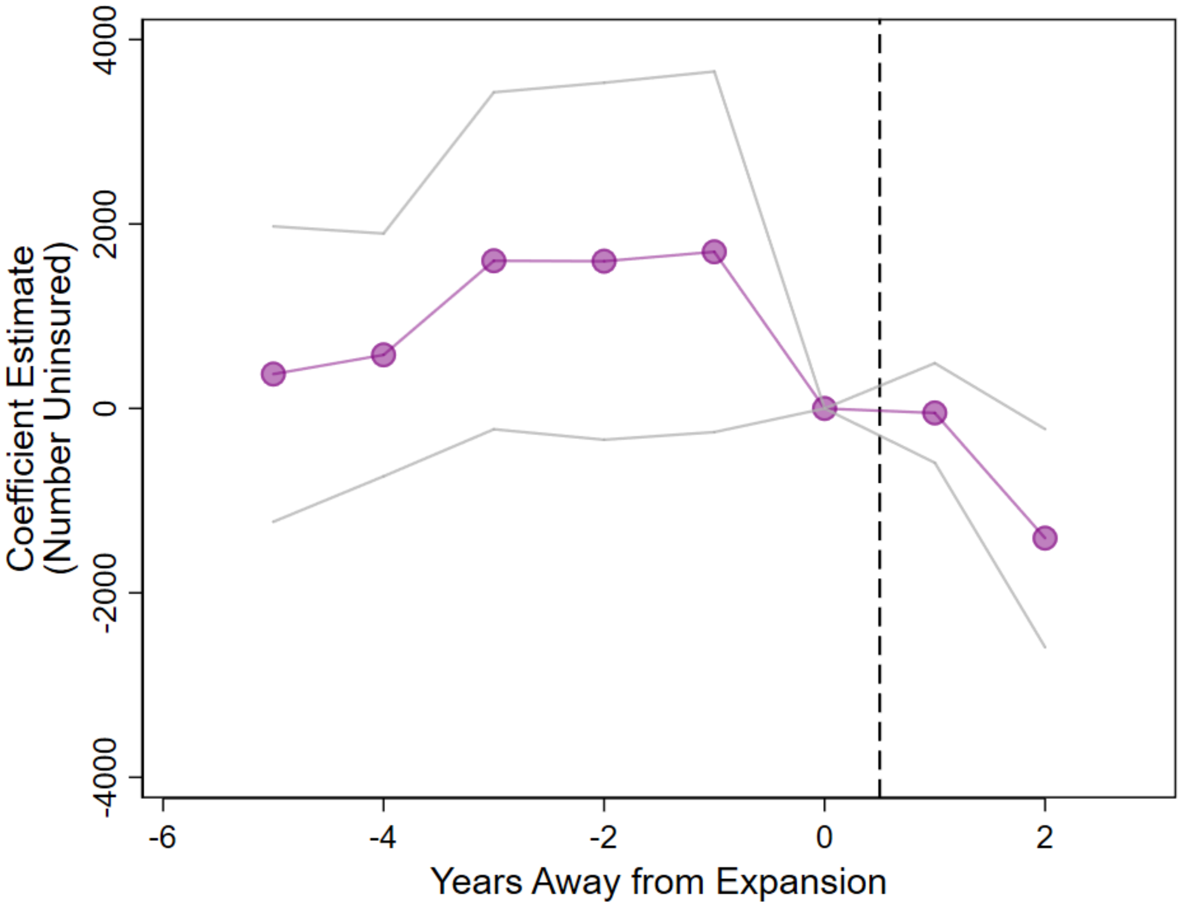
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with counts of opioid-related deaths as the dependent variable. Estimates are produced separately for various quantiles of the county-level distribution of those without health insurance whose income is at or below 138% of the Federal Poverty Line in the year prior to expansion. For example, in the lowest quintile of this distribution, the average county had 240 uninsured people at or below 138% (which is 37% of the population at or below 138%), while in the highest quantile of this distribution, the average county had 27,000 uninsured at or below 138% of the Federal Poverty Line (which is 39% of the population at or below 138%). Observations are at county-month level. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and all demographics. The source is CDC Individual-Level Mortality Files (2012-2016), as well as SAHIE estimates of the volume of uninsured (2010-2016).

Figure A6: Medicaid Expansion and Health Insurance



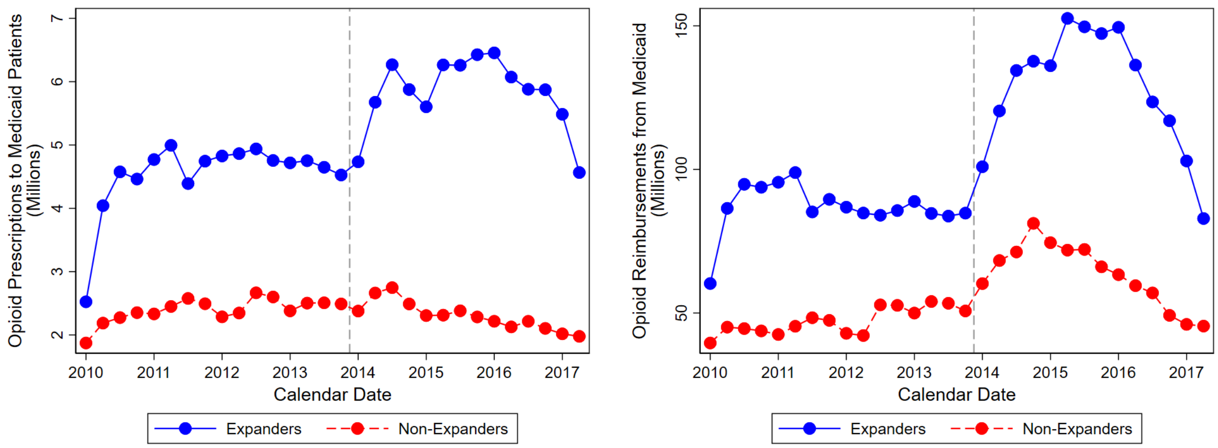
Note: Figure shows annual trends in the counts of uninsured individuals and the log of uninsured individuals, respectively. Observations are at the county-year level, but aggregated by whether or not the county is in a state which expanded Medicaid. The sample is restricted to individuals between the ages of 18-65 with income at or below 138 percent of the Federal Poverty Line but includes all races and both sexes. (This demographic became newly eligible under the Medicaid expansion licensed by the Affordable Care Act.) Expansions begin in January of 2014. The source is SAHIE (2010-2016)

Figure A7: Impact of Medicaid Expansion on Health Insurance



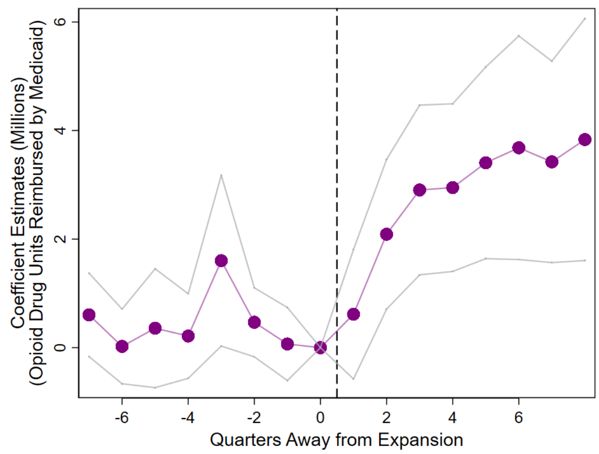
Note: Figure shows estimates of the impact of Medicaid expansion on health insurance. Estimates are obtained by estimating Equation 2.2 using counts of uninsured individuals as the dependent variable. Observations are at the county-year level. The specification includes county fixed effects, as well as calendar year fixed effects. Sample is restricted to individuals aged 18 to 65 whose income is at or below 138 percent of the Federal Poverty Line. The source is SAHIE (2010-2016).

Figure A8: Medicaid Expansion and Opioid Prescriptions (CMS State Drug Utilization)

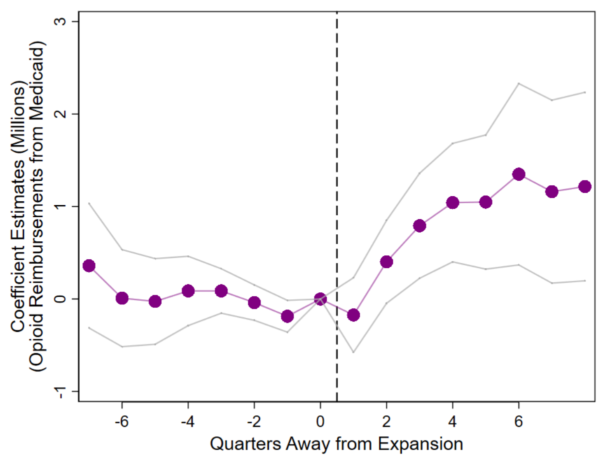


Note: Figure shows quarterly trends in opioid prescription counts filed through Medicaid and Medicaid reimbursement amounts, respectively. Observations are at the state-quarter level. Trends are shown separately for state which expanded Medicaid and those that did not. The sample is the universe of claims filed to Medicaid. Sample includes all states. Expansions began in the first quarter of 2014. The source is CMS state drug utilization (2010-2017).

Figure A9: Impact of Medicaid Expansion on Opioid Units Prescribed (CMS State Drug Utilization)



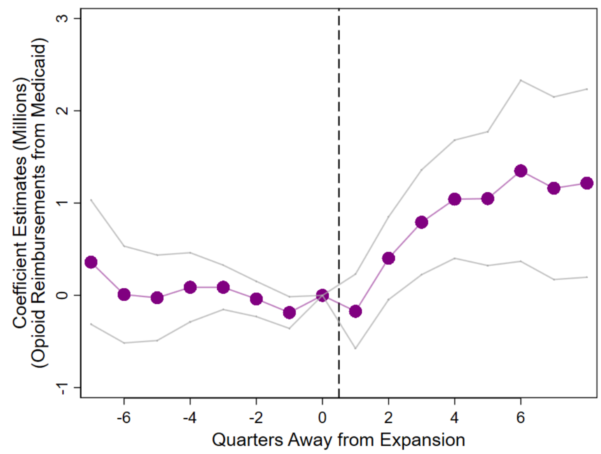
(a) Opioid Drug Units Reimbursed by Medicaid



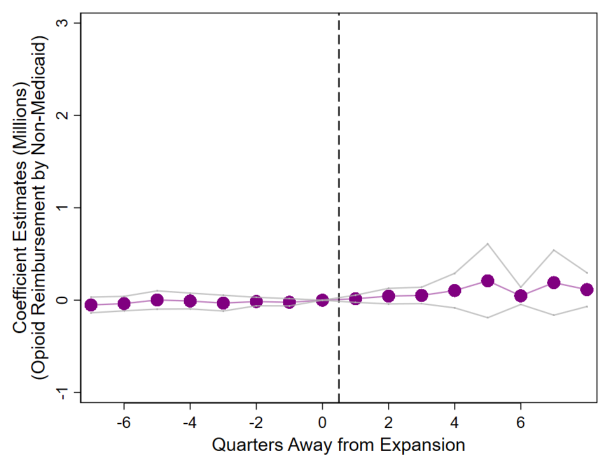
(b) Amount Reimbursed for Opioids by Medicaid

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with counts of opioid drug units prescribed and amounts reimbursed as the dependent variable, respectively. Observations are at state-quarter level. Both specifications include state fixed effects, as well as calendar quarter and year fixed effects. The sample contains the universe of claims filed through Medicaid. The sample includes all states and all demographics. The source is CMS state drug utilization data (2010-2017).

Figure A10: Impact of Medicaid Expansion on Opioid Reimbursement (CMS State Drug Utilization)



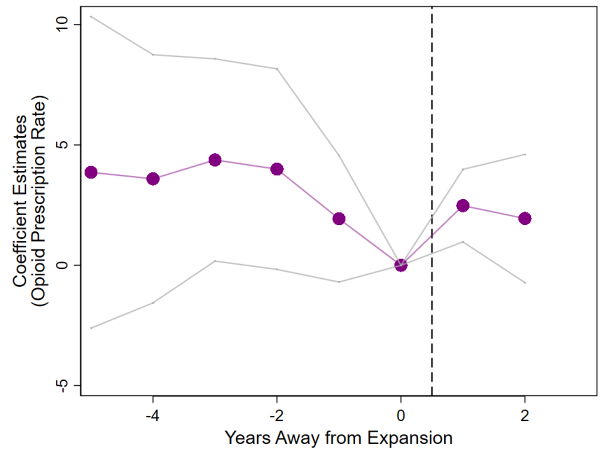
(a) Amount Reimbursed for Opioids by Medicaid



(b) Amount Reimbursed for Opioids by Non-Medicaid Payer

Note: Figures shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with a measure of the amount reimbursed for opioid prescriptions as the dependent variable. The top panel shows these estimates for reimbursements made by Medicaid; the bottom panel shows these estimates for reimbursements made by providers other than Medicaid. The sample contains the universe of claims filed through Medicaid, so the amounts reimbursed by providers other than Medicaid reflect charges filed through Medicaid but which Medicaid declined to reimburse. Observations are at the state-quarter level. Both specifications include state fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and all demographics. The source is CMS state drug utilization data (2010-2017).

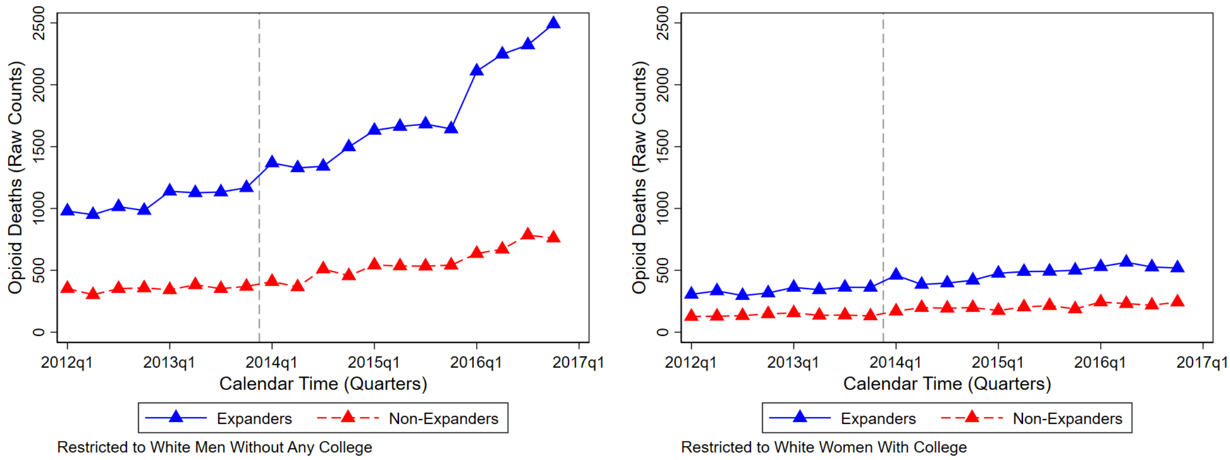
Figure A11: Impact of Medicaid Expansion on Overall Opioid Prescriptions (CDC Data)



(a) Opioid Prescription Rate (per 100 people)

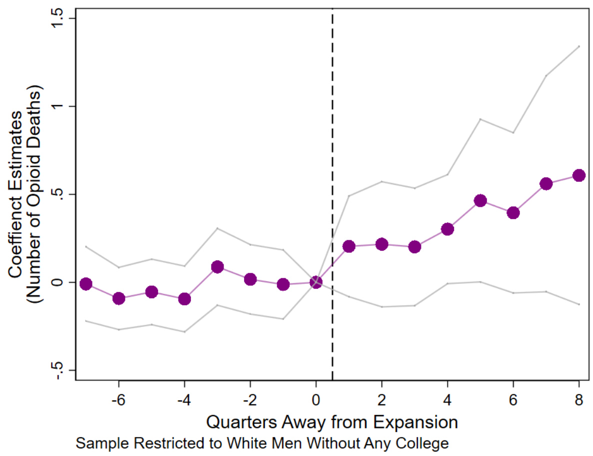
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with opioid prescriptions per 100 people as the dependent variable. Both specifications include county fixed effects, as well as calendar year fixed effects. Sample includes all states. The source is CDC opioid prescription rates reported at the county-year level (2010-2016).

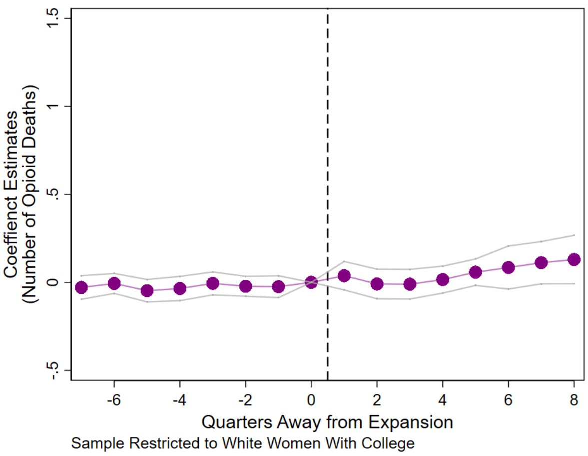
Figure A12: Medicaid Expansion and Opioid-Related Deaths: Heterogeneity



Note: Figure shows trends in opioid-related deaths. Observations are at the county-month level and collapsed to the county-quarter level for visual ease. Trends are shown separately for counties in states that expanded Medicaid and those that did not. The sample includes all states but restricts to white men without any college attendance and white women with college attendance, respectively. Expansions began in January of 2014. The source is CDC Individual-Level Mortality Files (2012-2016).

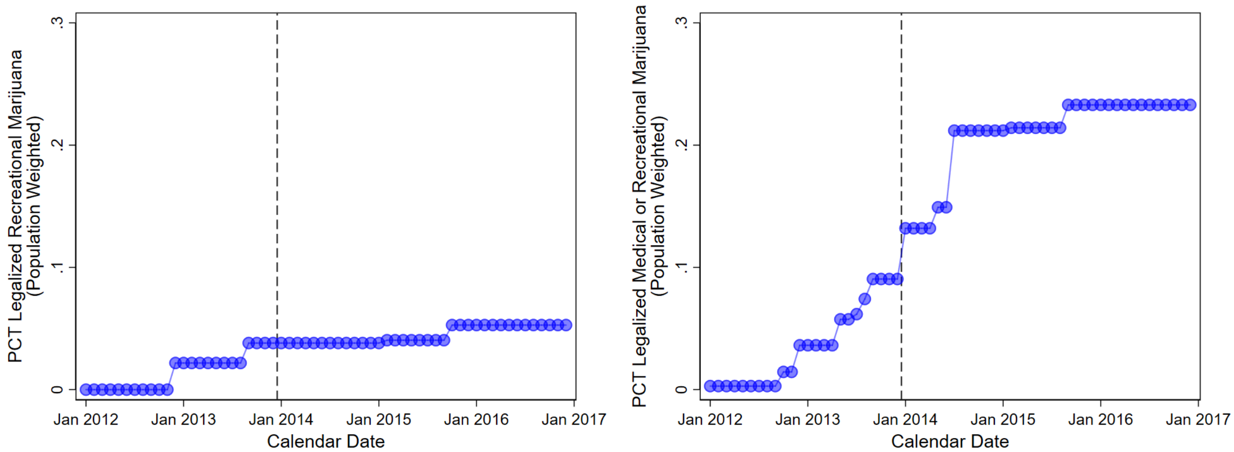
Figure A13: Impact of Medicaid Expansion on Opioid-Related Deaths: Heterogeneity





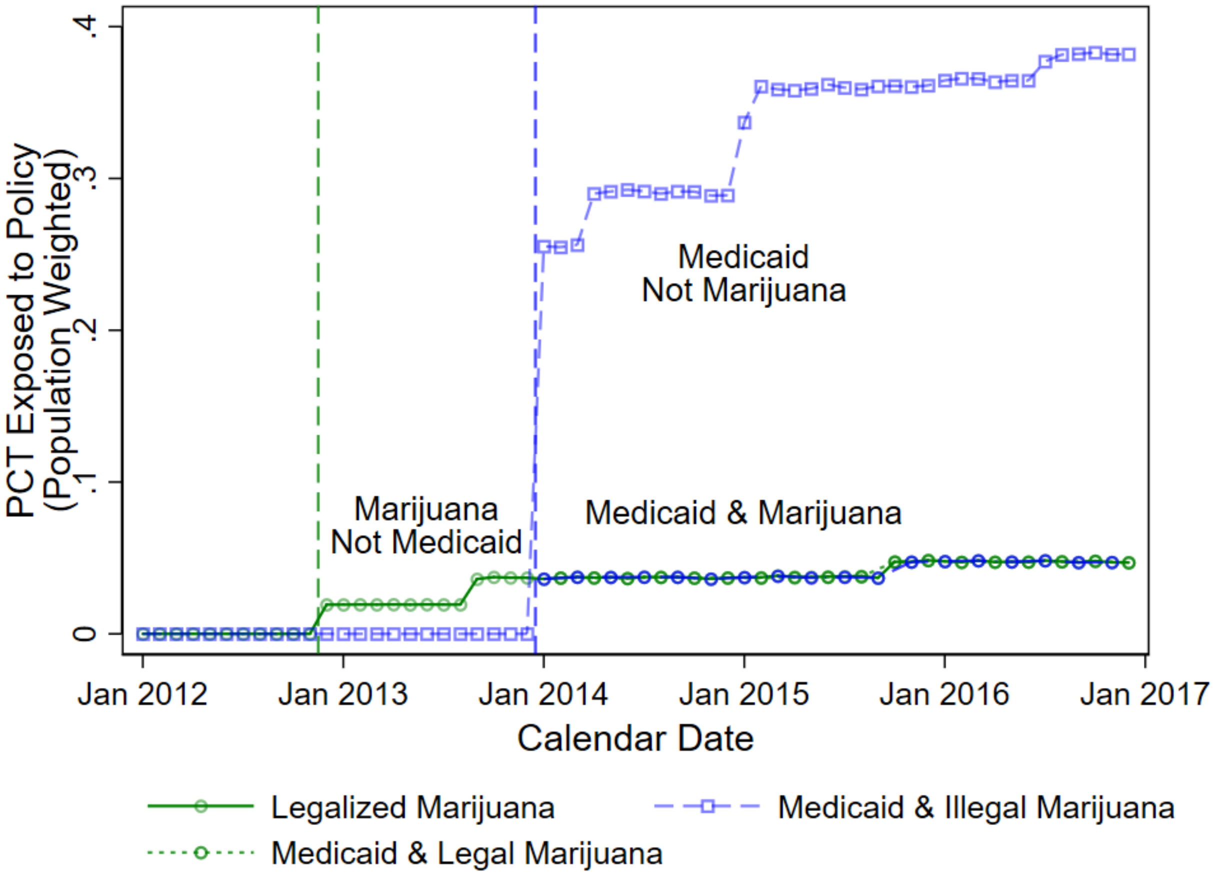
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with counts of opioid-related deaths as the dependent variable. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. Both specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states but restricts to white men without college attendance and white women with college attendance, respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A14: Marijuana Legalization (2010-2016)



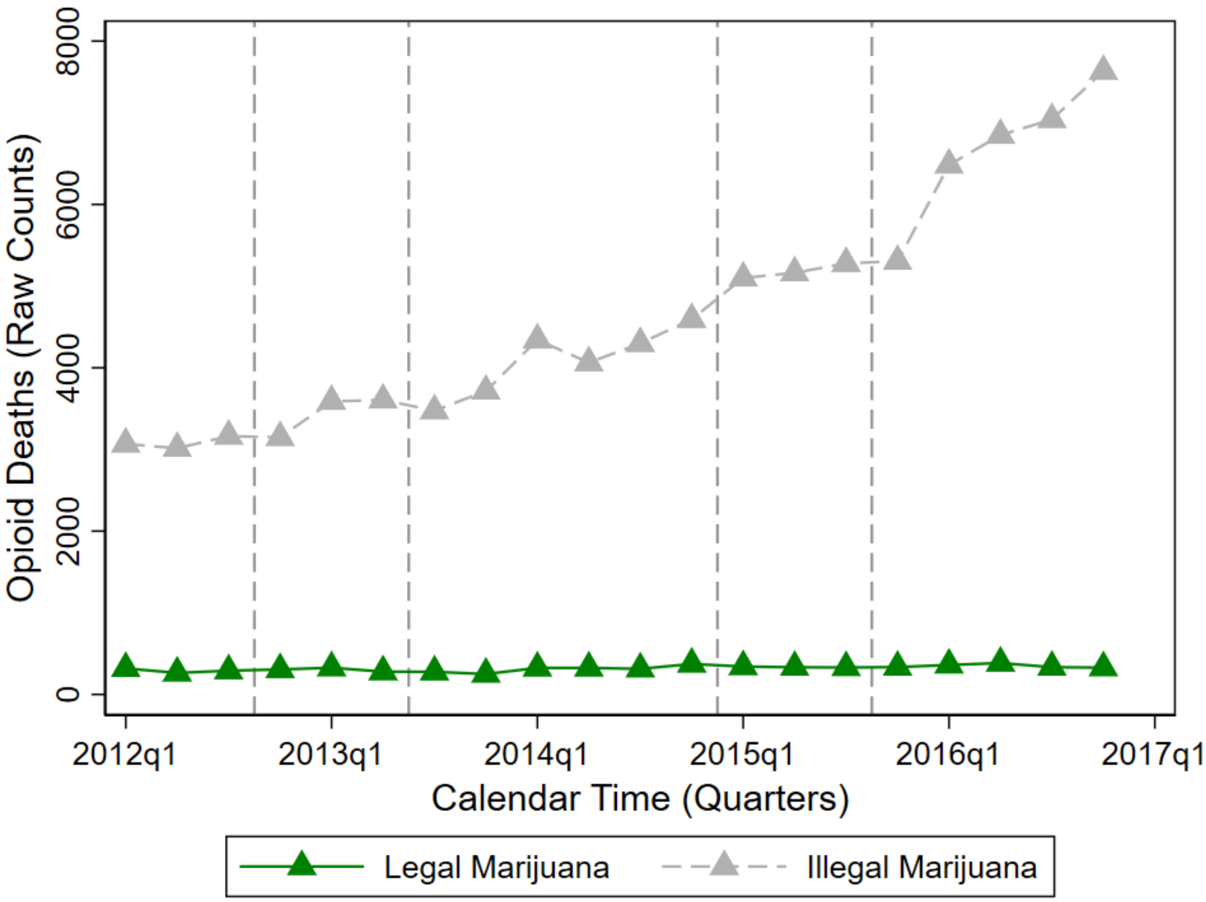
Note: Figure shows the share of the population exposed to a legal form of Marijuana. The left panel shows the share of the population living in states with legalized recreational Marijuana, while the right panel shows the share of the population living in states with either legalized recreational or legalized medical Marijuana. In both panels, I measure exposure to legal Marijuana based on the date legalization was enacted rather than the date dispensaries officially became operational. Observations are at the state-month level. Recreational Marijuana legalization began in December of 2012. As of 2017 four states legalized recreational Marijuana (Alaska, Colorado, Oregon, and Washington). 2010 state populations are used as population weights

Figure A15: Medicaid Expansion Interacting with Marijuana Legalization (2010-2016)



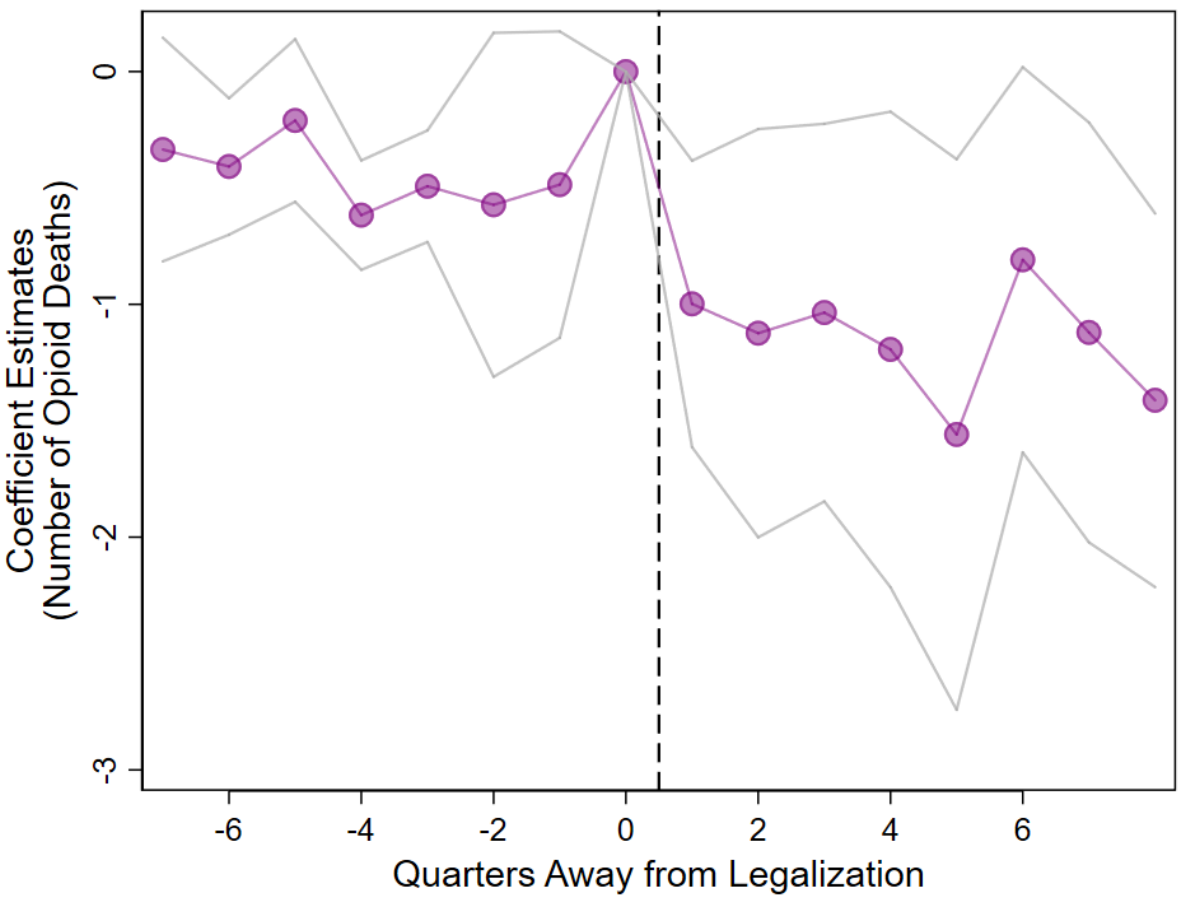
Note: Figure shows the share of the population exposed to Medicaid expansion, legalized recreational Marijuana and the interaction of these policies. In particular, the figure separately shows the share of the population living in states with legalized recreational Marijuana, as well as the share of the population living in states with both expansions of Medicaid and legalized recreational Marijuana, as distinct from the share of the population living in states with Medicaid expansion, but not legalized recreational Marijuana. Every state which legalized recreational Marijuana at some time expanded Medicaid, but many states which expanded Medicaid never legalized Marijuana. I measure exposure to legal Marijuana based on the date legalization was enacted rather than the date dispensaries officially became operational. Medicaid expansion began in January of 2014, while Marijuana legalization began in December of 2012. Observations are at the state-month level. 2010 state populations are used as population weights

Figure A16: Recreational Marijuana Legalization and Opioid-Related Deaths



Note: Figure shows trends in opioid-related deaths. Observations are at the county-month level and collapsed to the county-quarter level for visual ease. Trends are shown separately for counties in states that legalized recreational Marijuana at some time and those that never legalized Marijuana in the sample. The sample includes all states and all demographics. Legalizations began in December of 2012. (See Figure A14 for the timing of the state-level legalizations.) The source for opioid-related deaths is CDC Individual-Level Mortality Files (2012-2016).

Figure A17: Impact of Recreational Marijuana Legalization on Opioid-Related Deaths



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with counts of opioid deaths as the dependent variable and quarters away from Marijuana Legalization as the regressors of interest. Observations are at the county-month level but collapsed to county-quarter for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and all demographics. The source is CDC Individual-Level Mortality files (2012-2016).

Table A1: Summary Statistics

	Non-Expander	Expander	Total
A: Health Insurance Variables			
Number Uninsured, ≤ 138	8147.3	9402.4	8748.9
	(63622.6)	(62569.8)	(63121.9)
PCT Uninsured, ≤ 138	38.70	29.55	34.31
	(9.696)	(11.61)	(11.60)
Observations	11629	10705	22334
B: Drug Prescription Variables (Medicaid)			
Opioid Drug Units Reimbursed by Medicaid	7781969	10073230	9219623
	(6516709)	(10550068)	(9318929)
Opioid Prescriptions Reimbursed by Medicaid	123738	160393	146737
	(98433)	(168579)	(147460)
Amount Reimbursed for Opioids by Medicaid	2880355	3311954	3151162
	(2737623)	(3562359)	(3285135)
Amount Reimbursed for Opioids by Non-Medicaid	75393	141129	116639
	(138876)	(660021)	(530487)
Observations	570	960	1530
C: Drug Prescription Variables (Overall)			
Opioid Prescription Rate per 100 Persons	90.26	84.38	87.37
	(52.84)	(42.59)	(48.16)
Observations	10088	9772	19860
D: Opioid Death Variables			
Opioid Related Deaths (Raw Counts)	0.51	1.18	0.88
	(1.75)	(3.50)	(2.87)
PCT Counties with Zero Opioid Deaths	0.14	0.09	0.11
	(0.34)	(0.28)	(0.31)
Observations	50549	62204	112753

Note: Means reported with standard errors in parentheses. Distributions are reported separately based upon the state-level decision to expand Medicaid. Panel A: Source is SAHIE (2010-2016). Observations are at the county-year level. (Uninsured adults 18-65 with an income at or below 138 percent of the poverty line became newly eligible for Medicaid under the expansion.) Panel B: Source is CMS State-Drug Utilization Data (2010-2017). Observations are at the state-quarter level and include Washington D.C. Data contain the universe of claims filed to Medicaid. Panel C: Source is CDC reported rates of prescriptions per 100 people. Observations are at the county-year level. Panel D: Source is CDC Individual-Level Mortality Files (2010-2016). Observations are at the county-month level.

Table A2: Correlations between Supply-side and Demand-side shocks

	(1) Expansion	(2) Expansion	(3) Exp Date	(4) Exp Date
Legalized Marijuana	0.435^{***}	0.064	-2.751^{*}	2.725
	(0.117)	(0.087)	(1.576)	(6.400)
Observations	112753	109962	62204	62144
Mean	0.586	0.589	March 2014	March 2014
Covariates		X		X

Note: Each column reports a separate OLS regression of a measure of a given state's decision concerning Medicaid expansion against an indicator for whether the state legalized recreational Marijuana. Standard errors are clustered at the state level and reported in parentheses. In columns (1) and (2) the dependent variable is an indicator for whether the state expanded Medicaid, and in columns (3) and (4) the dependent variable is the date of Medicaid expansion (conditional upon expansion). All estimates measure the extent to which the state-level decision to legalize recreational Marijuana predicts Medicaid expansion. Coefficients in columns (1) and (2) measure predicted impact on the likelihood of expansion; coefficients in columns (3) and (4) measure the number of months before or after the average expansion date an average state expanded as predicted by the state-level decision to legalize Marijuana. Observations are always at the county-month level ranging from 2010 to 2016 and all regressions are weighted by 2010 population. Specifications in columns (2) and (4) include county level covariates from 2010, which are listed in Table A5 (some of these covariates were not available in 2016, which shrank the sample). The data sources are state legal databases. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A3: Impact of Medicaid Expansion on Opioid-Related Deaths

	(1) Count	(2) Log	(3) Poisson	(4) NB
Medicaid Expansion	0.319**	0.050**	0.052	0.048***
	(0.127)	(0.023)	(0.039)	(0.015)
Observations	112753	112753	95085	95085
Mean	.881	.881	.989	.989

Note: Each column reports estimates from a separate regression with standard errors reported in parentheses. Observations are at the county-month level. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. In column (1) the count of opioid-related deaths is the dependent variable in an OLS estimation of equation 3.4; column (2) is also an OLS estimation of equation 3.4 but with the log of opioid-related deaths as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of equation 3.4, respectively, both using the count of opioid-related deaths as the dependent variable. All specifications include county fixed effects as well as calendar month and year fixed effects. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). The coefficient estimate in column (2) implies a county-month increase in the count of opioid-related deaths of $.045$, while the estimates in columns (3) and (4) suggest increases of $.052$ and $.047$, respectively. The sample includes deaths of all US Residents. In the poisson and negative binomial specifications, I drop counties with all zero counts resulting in a reduction in sample size. (Table A15 shows the distribution of counties with all zero counts.) The source is CDC Individual-Level Mortality Files (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A4: Impact of Medicaid Expansion on Opioid-Related Deaths: Dynamic Estimates

	(1)
-8	-0.18
	(0.22)
-7	-0.25
	(0.21)
-6	-0.22
	(0.22)
-5	-0.29
	(0.21)
-4	0.10
	(0.22)
-3	0.05
	(0.19)
-2	-0.05
	(0.20)
Expansion Quarter	0.34
	(0.31)
+2	0.28
	(0.34)
+3	0.25
	(0.33)
+4	0.51
	(0.32)
+5	0.91^{*}
	(0.46)
+6	0.92^{**}
	(0.44)
+7	1.20^{**}
	(0.59)
+8	1.33^{*}
	(0.69)
Observations	38058
Mean	2.61

Note: The Table reports estimates from an OLS regression of Equation 2.2 with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-quarter level. The count of opioid-related deaths is the dependent variable. The specification includes county fixed effects as well as calendar quarter and year fixed effects. The sample includes deaths of all US Residents. The source is CDC Individual-Level Mortality Data (2012-2016). $^{*}\ p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$

Table A5: Covariate Predications of State Level Medicaid Expansion

	Expanded	Expanded Late
Political Environment		
PCT R (Lower Chamber)	-0.020**	-0.031**
PCT R (Upper Chamber)	0.005	0.047***
R Governor	0.000	-0.002**
Expenditures		
PCT Education Expenditure	0.007***	0.010*
PCT Welfare Expenditure	-0.009***	-0.008
PCT Hospital Expenditure	-0.007	-0.014***
PCT Health Expenditure	0.010	-0.002
PCT Police Expenditure	-0.025	0.007
PCT Unemp Insurance Expenditure	0.007**	0.006
Demography		
Log Population	0.090	-0.202***
PCT Male	0.010	0.111***
PCT White	0.016*	0.017
PCT Black	-0.005	-0.011***
PCT Hispanic	0.001	-0.005
Economic Covariates		
PCT Unemployed	-0.032	-0.064*
PCT Rural	-0.002	-0.019*
PCT Uninsured	-0.059	-0.266**
Per Capita GDP	-0.117	-1.430***
Per Capita Personal Income	0.006	0.234***
Per Capita Medicaid Beneficiaries	0.036	-0.036**
Poverty Rate	0.041	0.206***
Observations	49	31

Note: Each column reports a separate OLS regression with standard errors clustered at the state level reported in parentheses. Observations are at the state level; Nebraska is omitted because its state government is unicameral and non-partisan. In the left column, the dependent variable is an indicator for whether or not the state ever expanded Medicaid. In the right column, the dependent variable is an indicator for whether the state expanded Medicaid sometime after January 2014. (Approximately 70% of states who expanded did so in January 2014.) Also included but not reported are demographic variables that measure the percent of the population aged 0-9, aged 10-19, . . ., aged 80+. Significance levels indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Impact of Medicaid Expansion on Opioid-Related Deaths: DDD Estimates

	(1) Count	(2) Log	(3) Poisson	(4) NB
Expansion $\times$ High Uninsured	0.997***	0.169***	0.069	0.035
	(0.192)	(0.027)	(0.061)	(0.040)
Observations	112753	112753	100398	100398
Mean	0.881	.881	.881	.881

Note: Each column reports a separate OLS regression with standard errors reported in parentheses. In column (1) the count of opioid-related deaths is the dependent variable in an OLS estimation of Equation 3.4; column (2) is also an OLS estimation of Equation 3.4 but with the log of opioid-related deaths as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of equation 3.4, respectively, both using the count of opioid-related deaths as the dependent variable. All specifications include county fixed effects as well as calendar month and year fixed effects. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). The sample includes deaths of all US Residents. In the poisson and negative binomial specifications, I drop counties with all zero counts resulting in a reduction in sample size. (Table A15 shows the distribution of counties with all zero counts.) These specifications differ from those in Table A3 in that these regressions include all the pairwise interactions between the indicator for being in a state after Medicaid expansion and the indicator for being in a "High Uninsurance" county . I define a "High Uninsurance" county as a county which has an above median volume of uninsured individuals with income at or above 138% of the Federal Poverty Line in the year prior to Medicaid expansion. Counties above the median averaged 12,500 uninsured individuals (which is 39% of the county population who are at or below 138% in the year prior to expansion) while counties below the median averaged 650 uninsured individuals (which is 36%" of the county population at or below 138% in the year prior to expansion). The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid multiplied by an indicator for whether the county is a "High Uninsurance" county. The source is CDC Individual-Level Mortality Files (2012-2016), as well as SAHIE estimates of the uninsured (2010-2016). $^{*}\ p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$

Table A7: Impact of Medicaid Expansion on Opioid-Related Deaths: Robustness

	(1)	(2)	(3)	(4)
Medicaid Expansion	0.942**	0.850**	0.939**	0.848**
	(0.378)	(0.357)	(0.378)	(0.356)
Observations	38058	36077	38058	36077
Mean	2.610	2.659	2.610	2.610
Unemployment		X		X
State-Year Trends			X	X

Note: Each column reports a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-month level, but collapsed to county-quarter level. The dependent variable is the count of opioid-related deaths. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Sample includes deaths of all US Residents. All specifications include county fixed effects as well as calendar quarter and year fixed effects. In columns (2) and (4) county-year level measures of unemployment are included. Unemployment controls include contemporaneous county-level unemployment rate as well as its lag. (Including the lag of unemployment drops the sample size slightly.) In columns (3) and (4) the specifications include state-specific year trends. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A8: Impact of Medicaid Expansion on Opioid-Related Deaths: Confounders

	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid Expansion	0.319**	0.289**	0.288**	0.276**	0.301**	0.290**
	(0.127)	(0.119)	(0.118)	(0.115)	(0.122)	(0.119)
Observations	112753	112753	112753	112753	112753	112753
Mean	.881	.881	.881	.881	.881	.881
State-Year Trends		X	X	X	X	X
Policy Confounders:						
PDMP			X	X		X
Doctor Shopping, Pain clinic regs				X		X
Physician exam, Pharm verification, Require ID					X	X

Note: Each column reports a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-month level. The dependent variable is the count of opioid-related deaths. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. Sample includes deaths of all US Residents. All specifications include county fixed effects as well as calendar month and year fixed effects. In columns (2)-(6) state specific linear trends are included. Following (47), in columns (3)-(6) various combinations of state level health care policies aimed at reducing opioid abuse are included as regressors. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A9: Impact of Medicaid Expansion on Health Insurance

	(1) Counts	(2) Logs	(3) Poisson	(4) NB
Medicaid Expansion	-2884**	-0.362***	-0.292***	-0.372***
	(1437)	(0.040)	(0.018)	(0.003)
Observations	22334	22334	22334	22334
Mean	8749	7.150	8749	8749

Note: Each column reports estimates from a separate regression with standard errors reported in parentheses. Observations are at the county-year level. The dependent variable is the count of uninsured persons (or the log thereof) with an income at or below 138 percent of the federal poverty line. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. In column (1) the count of uninsured individuals is the dependent variable in an OLS estimation of Equation 3.4; column (2) is also an OLS estimation of Equation 3.4 but with the log of uninsured individuals as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of Equation 3.4, respectively, both using the count of uninsured individuals as the dependent variable. All specifications include county fixed effects as well as calendar year fixed effects. Sample includes all US counties. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). The coefficient estimate in column (2) implies a county-year level reduction in the count of uninsured persons (with an income at or below 138 percent of the federal poverty line) of $2,668$, while the estimates in columns (3) and (4) suggest reductions of $2,554$ and $3,254$, respectively. The source is 2010-2016 SAHIE Data. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A10: Impact of Medicaid Expansion on Opioid Prescriptions (CMS State Drug Utilization)

	(1) Opioid Units	(2) Opioid Prescriptions	(3) Amount Reimbursed
Medicaid Expansion	2754809***	48819***	851250**
	(750284)	(13073)	(416405)
Observations	1530	1530	1530
Mean	9219623	146737	3267801

Note: Each column reports estimates from a separate OLS regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the state-quarter level. The dependent variable is the count of prescribed opioid drug units, the count of opioid prescriptions and the amount reimbursed for opioid prescriptions by Medicaid, respectively. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. All specifications include state fixed effects as well as calendar quarter and year fixed effects. The source is CMS State-Drug Utilization Data (2010-2017). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A11: Impact of Medicaid Expansion on Opioid-Related Deaths: Heterogeneity

	(1) Count	(2) Log	(3) Poisson	(4) NB
Panel A: White Men Without Any College				
Medicaid Expansion	0.156**	0.037**	0.083*	0.083***
	(0.064)	(0.016)	(0.045)	(0.023)
Observations	106814	106814	84503	84503
Mean	.369	.369	.466	.466
Panel B: White Women With Some College				
Medicaid Expansion	0.025**	0.005	0.005	0.001
	(0.013)	(0.003)	(0.050)	(0.006)
Observations	93197	93197	64069	64069
Mean	.129	.129	.188	.188

Note: Each column reports estimates from a separate regression with standard errors reported in parentheses. Observations are at the county-month level. In column (1) the count of opioid-related deaths is the dependent variable in an OLS estimation of equation 3.4; column (2) is also an OLS estimation of equation 3.4 but with the log of opioid-related deaths as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of equation 3.4, respectively, both using the count of opioid-related deaths as the dependent variable. All specifications include county fixed effects as well as calendar month and year fixed effects. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). The coefficient estimates in columns (2)-(4) of Panel A implies a county-month increase in the count of opioid-related deaths for white men without any college of .01, .04, and .04, respectively; the estimates in columns (3) and (4) of Panel B suggest increases for white women with some college of .0006, .0009, and .0002, respectively. The sample includes deaths of all US Residents. In the poisson and negative binomial specifications, I drop counties with all zero counts resulting in a reduction in sample size. (Table A15 shows the distribution of counties with all zero counts.) Sample size will also differ by demographic since some county-month cells are missing a specific demographic breakdown associated with opioid deaths (for example, either sex or educational attainment is not reported for a specific county-month cell). The source is CDC Individual-Level Mortality Data (2012-2016). $^{*}\ p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$

Table A12: Impact of Recreational Marijuana Legalization on Opioid-Related Deaths

	(1) Count	(2) Log	(3) Poisson	(4) NB
Marijuana Legalization	-0.332***	-0.078***	-0.376***	-0.367***
	(0.104)	(0.021)	(0.060)	(0.034)
Observations	112753	112753	100398	100398
Mean	0.881	.881	.989	.989

Note: Each column reports estimates from a separate regression with standard errors reported in parentheses. Observations are at the county-month level. The reported coefficient of interest is an indicator for whether the county is in a state which legalized recreational Marijuana. In column (1) the count of opioid-related deaths is the dependent variable in an OLS estimation of Equation 3.4; column (2) is also an OLS estimation of Equation 3.4 but with the log of opioid-related deaths as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of Equation 3.4, respectively, both using the count of opioid-related deaths as the dependent variable. All specifications include county fixed effects as well as calendar month and year fixed effects. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). The coefficient estimate in column (2) implies a county-month decrease in the count of opioid-related deaths of .071, while the estimates in columns (3) and (4) suggest decreases of .371 and .362, respectively. The sample includes deaths of all US Residents. In the poisson and negative binomial specifications, I drop counties with all zero counts resulting in a reduction in sample size. (Table A15 shows the distribution of counties with all zero counts.) The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

Table A13: Impact of Medicaid Expansion on Opioid-Related Deaths: Conditional on Recreational Marijuana Legalization

	(1) Marijuana	(2) No Marijuana
Medicaid Expansion	0.102**	0.376**
	(0.022)	(0.142)
Observations	7781	104972
Mean	0.822	0.885

Note: Each column reports estimates from a separate regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-month level. The reported coefficient of interest is an indicator for whether the county is in a state which legalized recreational Marijuana. All specifications include county fixed effects as well as calendar month and year fixed effects. In column (1), the sample is restricted to states which at some time from 2010-2016 legalized adult-use recreational marijuana. In column (2), the sample is restricted to states which did not legalize adult-use recreational marijuana between 2010 and 2016. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

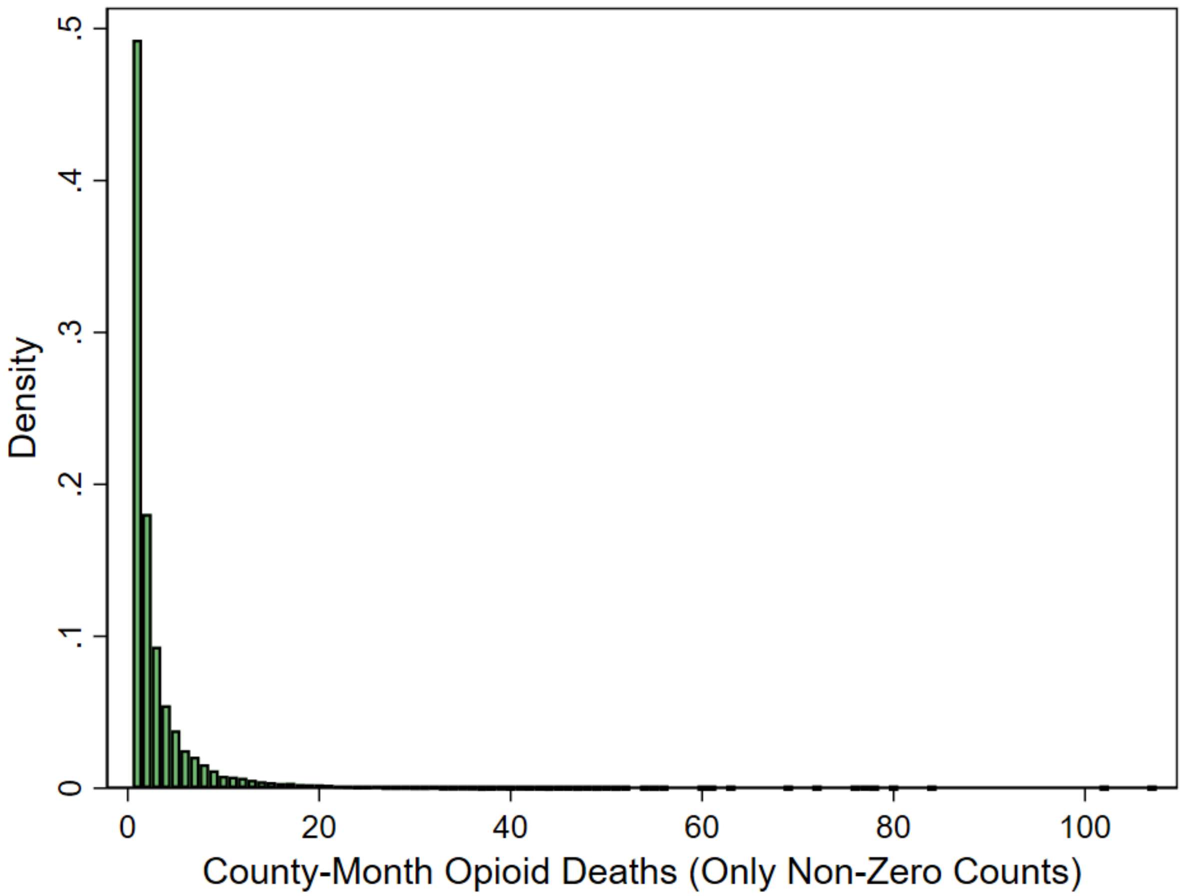
Table A14: Interactive Impact of Medicaid Expansion and Marijuana Legalization on Opioid-Related Deaths

	(1)
Marijuana Legalization	-0.159***
	(0.036)
Medicaid Expansion	0.367**
	(0.137)
Medicaid and Marijuana	-0.361***
	(0.126)
Observations	112753
Mean	0.881

Note: Each column reports estimates from a separate regression with standard errors clustered at the state-level and reported in parentheses. Observations are at the county-month level. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid, legalized recreational Marijuana or did both. All specifications include county fixed effects as well as calendar month and year fixed effects. The source is CDC Individual-Level Mortality Data (2012-2016). * $p < .1$, ** $p < .05$, *** $p < .01$

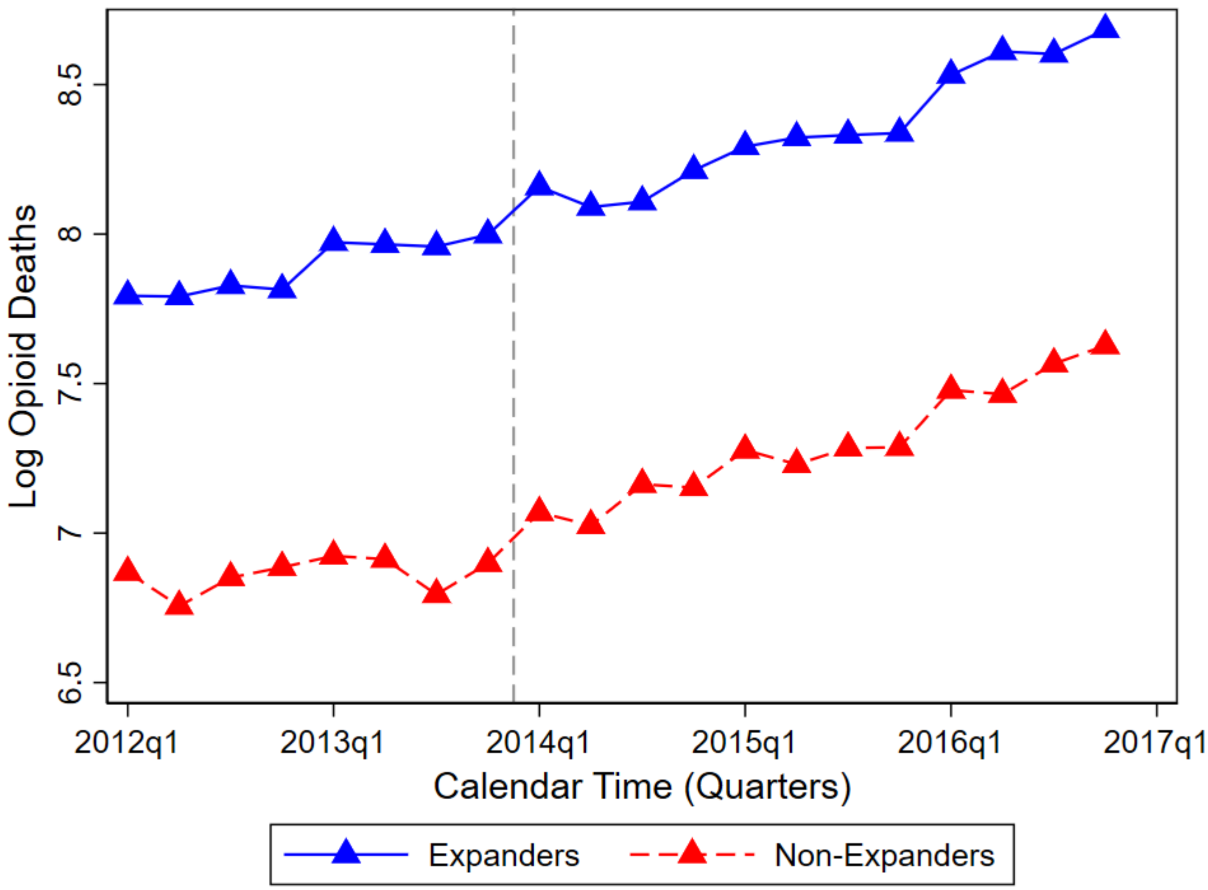
Appendix Figures

Figure A18: Distribution of Opioid-Related Deaths



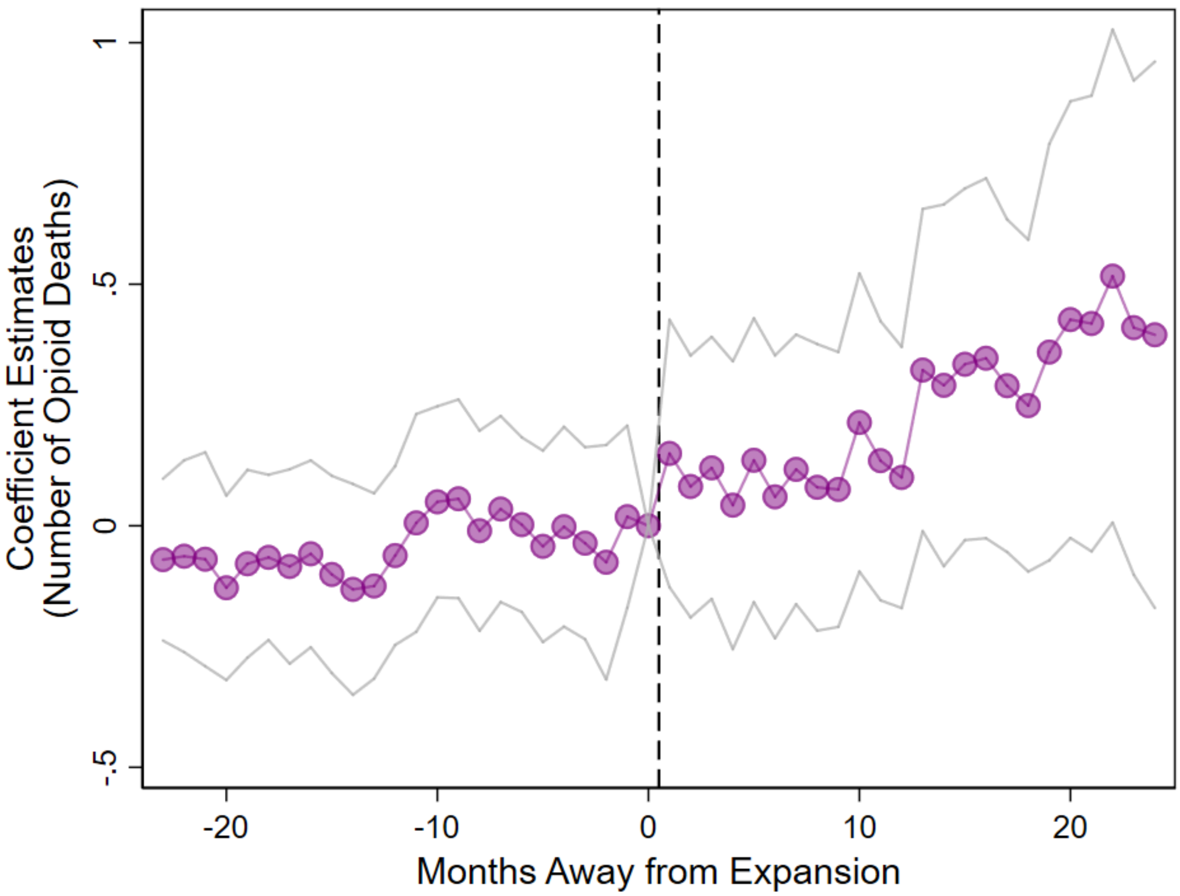
Note: The Figure shows the county-month level distribution of opioid-related deaths. The sample is restricted to county-month cells with at least one opioid related death. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A19: Medicaid Expansion and Log Opioid-Related Deaths



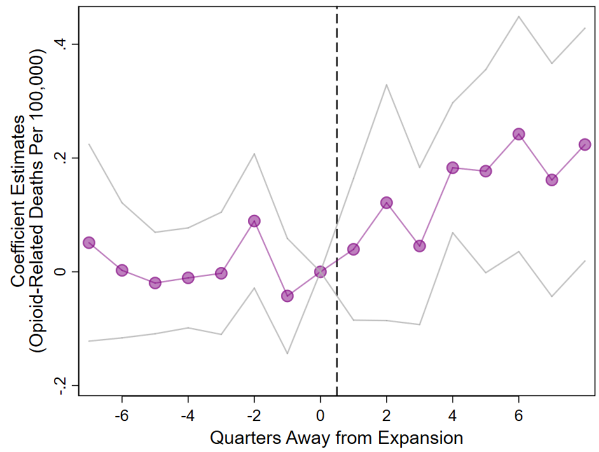
Note: Figure shows trends in opioid-related deaths. Trends are shown separately for states that expanded Medicaid and those that failed to expand. The sample includes all states and all demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A20: Impact of Medicaid Expansion on Opioid-Related Deaths: County-Month Level

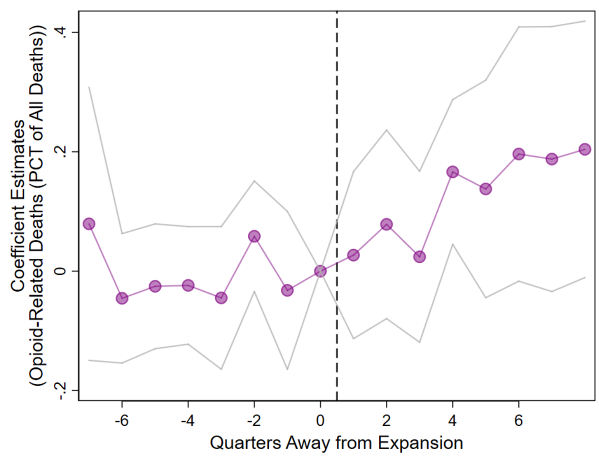


Note: Figure shows the coefficient estimates and 95% confidence interval from an OLS regression of Equation'2.2 where the dependent variable is the count of opioid related-deaths. Observations are at the county-month level. In Figure A4, comparable estimates are reported at the county-quarter level. All specifications include county fixed effects, as well as calendar month and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2010-2016).

Figure A21: Impact of Medicaid Expansion on Opioid-Related Deaths



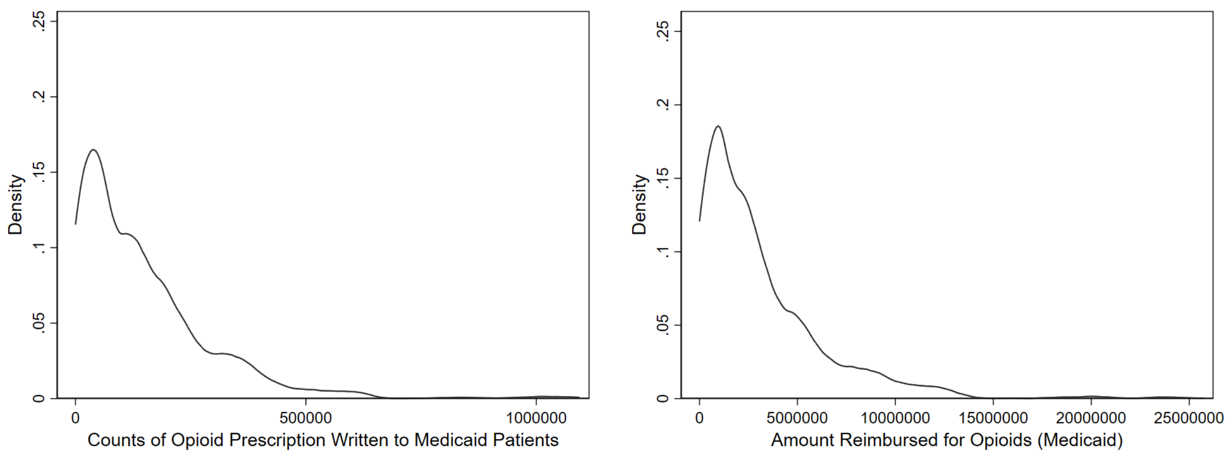
(a) Rate Per 100,000 Population



(b) Rate Per 100 Deaths

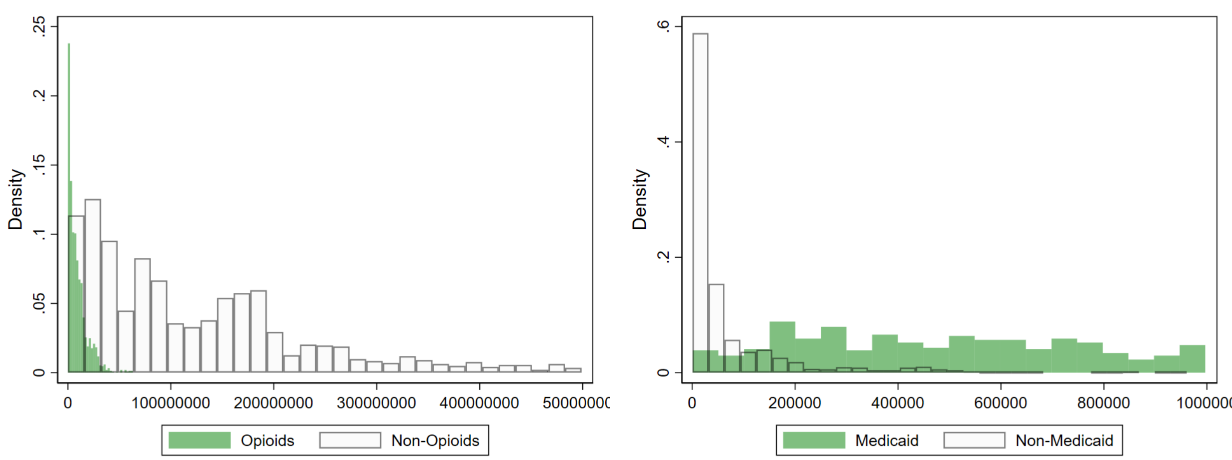
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with the rate of opioid-related deaths as the dependent variable. In the upper panel, the rate is the ratio of opioid-related deaths in a county-month to annual county population (measured per 100,000 people); in the lower panel, the rate is the ratio of opioid-related deaths in a county-month to the total deaths in a county-month (measured per 100 deaths). Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The specification includes county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states and demographics. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A22: Distribution of Opioid Prescriptions



Note: The Figure shows the state-quarter level distribution of opioid prescription counts and reimbursement amounts, respectively. All states are included in the sample. The source is CMS state drug utilization data (2010-2017).

Figure A23: Distribution of Opioid Reimbursements

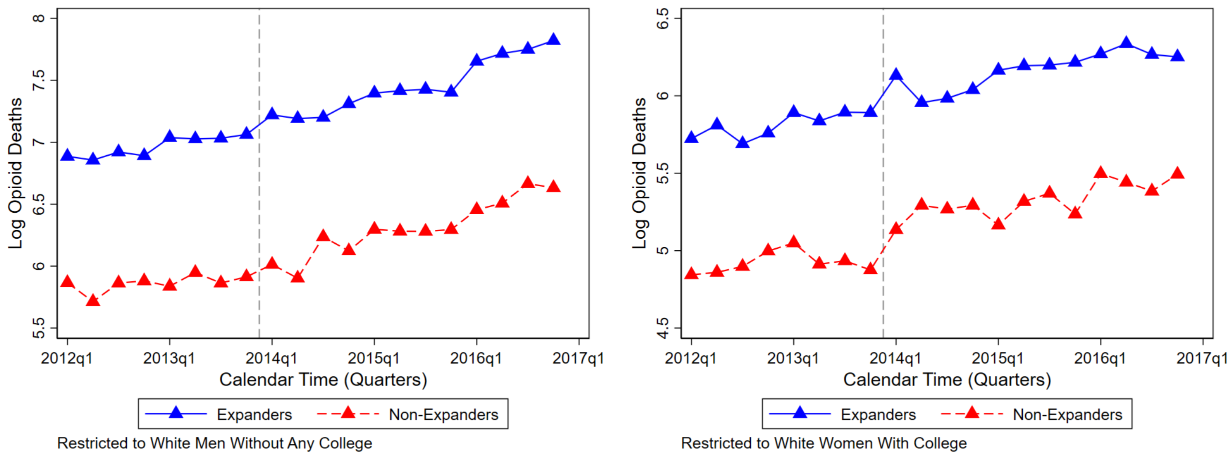


(a) Prescriptions Reimbursed by Medicaid

(b) Opioid Reimbursements

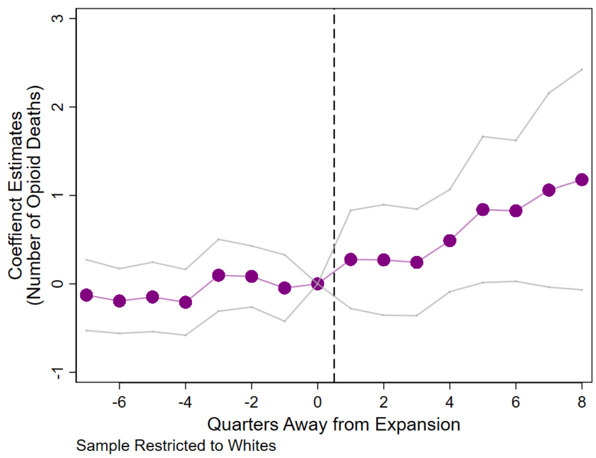
Note: The Figure shows the state-quarter level distribution of opioid prescriptions and reimbursements. The left panel contrasts prescriptions written to Medicaid patients for opioids with prescriptions written to Medicaid patients for all other drugs. The right panel contrasts reimbursements for opioid prescriptions made by Medicaid with reimbursements made by all other providers. All states are included in the sample. The source is CMS state drug utilization data (2010-2017).

Figure A24: Medicaid Expansion and Log Opioid-Related Deaths: Heterogeneity

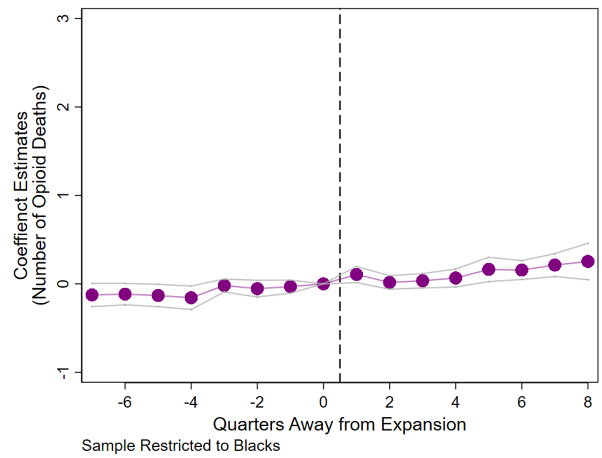


Note: Figure shows trends in the log of opioid-related deaths. Trends are shown separately for states that expanded Medicaid and those that failed to expand. The sample includes all states but restricts to white men without any college attendance and white women with college attendance, respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

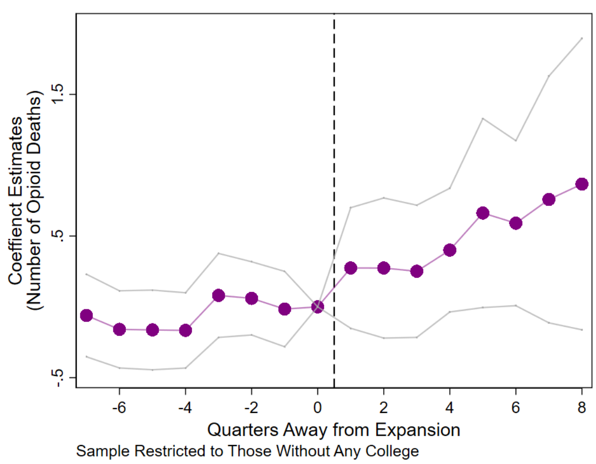
Figure A25: Impact of Medicaid Expansion on Opioid-Related Deaths: Heterogeneity



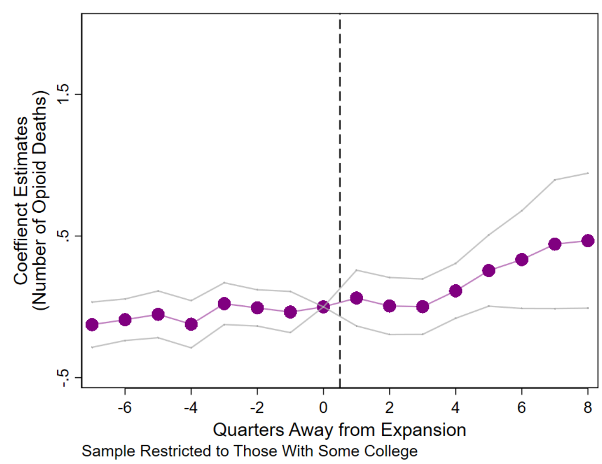
(a) White



(b) Black



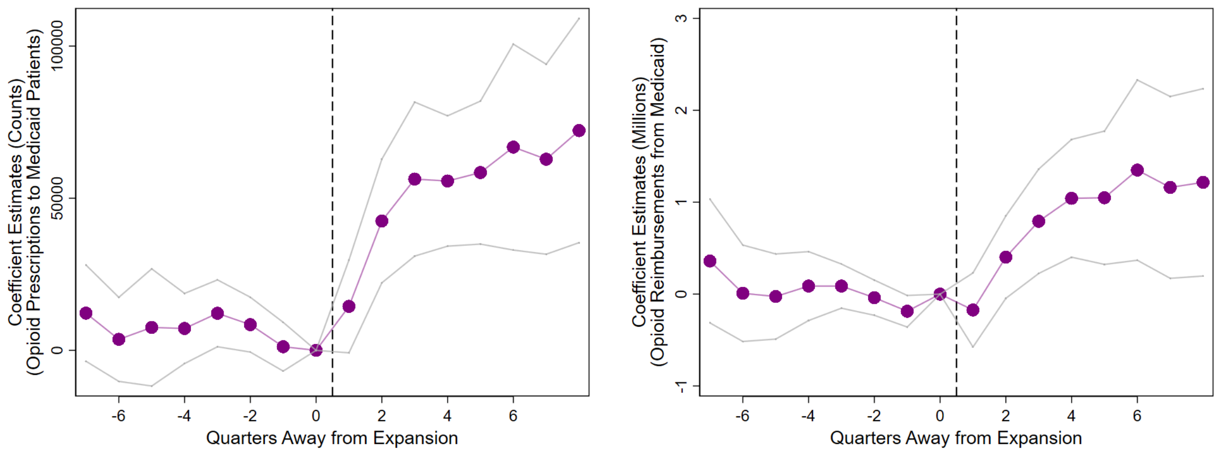
(c) No College



(d) College

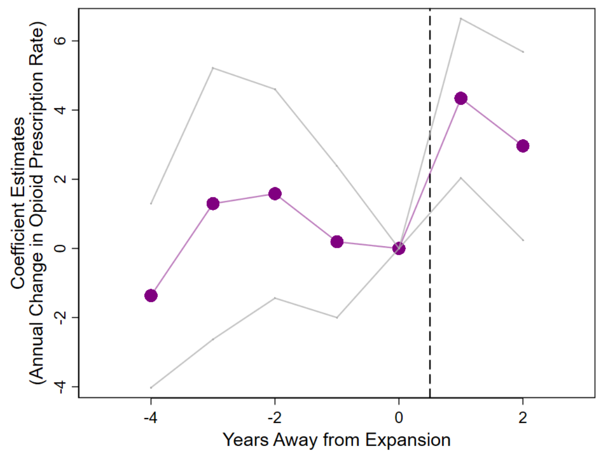
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with opioid-related deaths as the outcome variable. Observations are at the county-month level but collapsed to the county-quarter level for visual ease. The dependent variable is the count of opioid related deaths. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states but restricts to white individuals, black individuals, those without college attendance and those with college attendance, respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A26: Impact of Medicaid Expansion on Opioid Prescriptions (CMS State Drug Utilization)



Note: Figures show estimates of the impact of Medicaid expansion on opioid prescriptions and reimbursements by Medicaid, respectively. Estimates are obtained from an OLS regression of Equation 2.2 with the count of the number opioid prescriptions and the amount reimbursed for opioid, respectively as the outcome variable. Observations are at the state-quarter level. This Figure differs from Figure A10 in that it features prescription counts in the left panel rather than drug unit counts. Sample includes all states. Source is CMS state drug utilization (2010-2017).

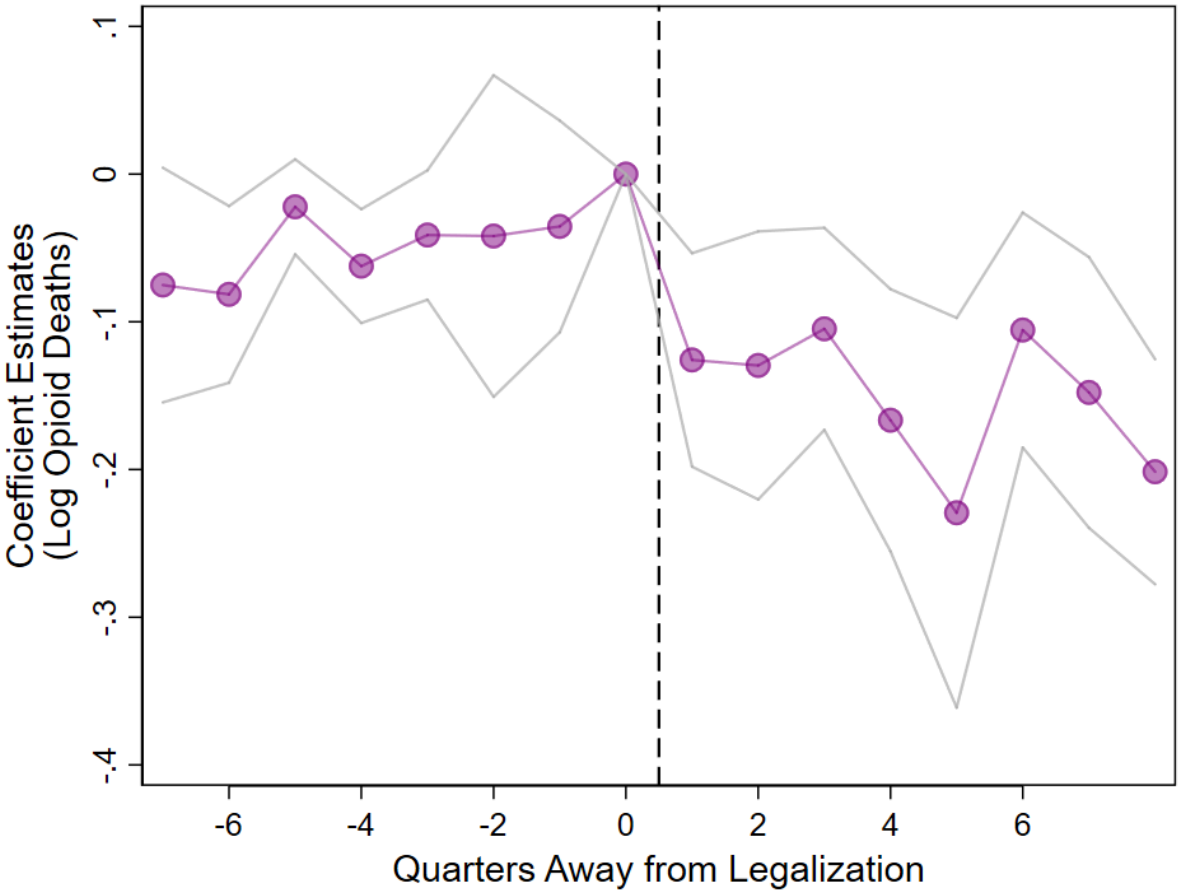
Figure A27: Impact of Medicaid Expansion on Overall Opioid Prescriptions (CDC Data)



(a) Annual Change in Opioid Prescription Rate

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with the annual change in opioid prescription per 100 people as the dependent variable. Both specifications include county fixed effects, as well as calendar year fixed effects. Sample includes all states. The source is CDC opioid prescription rates reported at the county-year level (2010-2016).

Figure A28: Impact of Marijuana Legalization on Log Opioid-Related Deaths



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2. Observations are at the county-month level but collapsed to county-quarter for visual ease. The dependent variable is the log of opioid-related deaths with quarters away from Marijuana legalization as the regressors of interest. All specifications include county fixed effects, as well as calendar quarter and year fixed effects. The sample includes all states. The source is CDC Individual-Level Mortality Files (2012-2016)

Appendix Tables

Table A15: Summary Statistics: Further Breakdowns

	Marijuana Legalization	No Marijuana Legalization		Total
		Expander	Non-Expander	
Opioid Death Variables				
Opioid Related Deaths	0.82	1.23	0.51	0.88
	(2.06)	(3.66)	(1.75)	(2.87)
Log Opioid Related Deaths	0.33	0.41	0.22	0.32
	(0.61)	(0.70)	(0.49)	(0.62)
PCT Counties w/ Zero Opioid Deaths	0.11	0.08	0.14	0.11
	(0.31)	(0.28)	(0.34)	(0.31)
Observations	7781	54423	50549	112753

Note: Means reported with standard errors in parentheses. Observations are at county-month level. Distributions are reported separately based both upon the state-level decision to expand Medicaid, upon the state-level decision to legalize recreational Marijuana, and the interaction of these policies. All states that legalized recreational Marijuana expanded Medicaid at some time. Source: CDC Individual-Level Mortality Files (2010-2016) and state legal databases.

Table A16: Summary Statistics: Treatment Variables

Medicaid Expansion	0.621 (0.490)
Date of Medicaid Expansion	March 2014 (6.122)
Recreational Marijuana	0.0528 (0.226)
Date of Recreational Legalization	November 2013 (15.68)
Recreational or Medical Marijuana	0.233 (0.427)
Earliest Date of any Legalization	January 2014 (10.01)
Observations	51

Note: Means reported with standard errors in parentheses. Source: State legal databases. Observations are at the state-level. 2010 state-level populations are used as population weights. Expansion and legalization means (rows 1,3 and 5) are to be interpreted as the average percent of the population exposed to those policies across all states (from 2010-2016). Date variables are in units of months and standard errors are to be interpreted as months away from the reported mean date. By the end of 2016, 32 states in the sample expanded Medicaid. By the end of 2016, 4 states had legalized recreational Marijuana and 12 had legalized either recreational or medical Marijuana. All states that legalized recreational Marijuana expanded Medicaid at some point.

3. A HEAD START ON FIGHTING CRIME? THE EFFECT OF ACCESS TO EARLY CHILDHOOD EDUCATION

3.1 Introduction

Are criminals made or born? Not only does the answer to this fundamental question have important implications for our understanding of criminality, but it is central to efforts aimed at reducing the large costs that crime imposes on society ($2 trillion annually).[1] Policies that address these costs primarily through the justice system implicitly assume that the development of criminals cannot be prevented cost-effectively and instead focus on incapacitating and rehabilitating those who have already become criminals. However, relatively little is known about the factors that influence an individual's likelihood of becoming a criminal and their malleability. Furthermore, the concentration of crime among a small number of perpetrators (less than 6 % of the population commit the majority of crime) provides an opportunity for policy interventions to have outsized effects if they can prevent the development of criminals.[2] In fact, some estimates suggest that preventing the development of a single career criminal could result in as many as 600 fewer victims of crime each year.[3]

We explore the role of one popular policy intervention in influencing later criminal behavior: early childhood education. The importance of understanding this relationship is heightened by recent expansions in the share of children attending public preschools, driven in large part by a belief among policymakers that early childhood education interventions have large impacts later in life. We bring new evidence to this question by investigating the effect of childhood Head Start

[1] For context, $2 trillion dollars is 17% of annual GDP. (United States. Senate Committee on the Judiciary. Hearing on The Costs of Crime. September 19, 2006 (statement of Jens Ludwig))

[2] Farrington et al. (2006) generate this statistic by tracking the criminal behavior of a set of boys in London. Given the higher propensity to commit crime among males, this 6 % is likely a substantial overestimate of the share of the population that commits the majority of crime.

[3] Across major crime categories, estimates suggest that a relatively small proportion of individuals (consistently less than 10%) account for the majority of crime. These "career criminals" commit hundreds of crimes each year (authors' calculations from Chaiken and Chaiken (1982)).

availability on later criminal behavior.

Recent attention has focused on early childhood as a critical developmental period, but the limited evidence on the effect of early childhood education on later criminal behavior is mixed and inconclusive. The most compelling evidence comes from a single evaluation of a small-scale high-intensity intervention, Perry Preschool, where effects on crime account for 40-65% of the estimated benefits of the program (Heckman et al. 2010). However, a randomized evaluation of a similar program, the Abecedarian Project, indicates no effect of the program on crime (Campbell et al. 2012). Furthermore, while these studies provide rigorous evidence driven by random assignment, both rely on very small and attrition-plagued samples to support their conclusions.[4] Evidence on the effects of Head Start on criminal behavior is also mixed and relies on small samples, self-reported crime data, and sibling comparison approaches that raise questions about the validity of the estimates (Deming 2009; Garces et al. 2002).

We make three primary contributions to this literature. First, we provide the only large-scale evidence that early childhood education reduces later criminal behavior. Second, we provide the first estimates that rely on administrative crime data to determine the effects of Head Start availability on later criminal behavior. Third, we estimate that, in high poverty counties, the discounted benefits generated by Head Start's later crime reduction were greater than the costs of the program itself.

To investigate the link between early childhood education and later criminal behavior, we take advantage of the staggered introduction of the Head Start program during the 1960s. The Head Start program, funded and administered through the U.S. Department of Health and Human Services, has been an integral part of U.S. early childhood education for the 50 years of its existence. Easily the largest early childhood education program in the United States, annual Head Start enroll-

[4] At adult follow up, the Perry experiment had 123 members and the Abecedarian experiment had 101, both somewhat reduced from initial samples. The Abecedarian follow up relied on self-reported crime data, while the Perry follow up relied on a combination of self-reports and county and state-level arrest data. There were also issues with the Perry randomization protocol; while subsequently carefully addressed by researchers, ex-post correction of compromised randomization is never ideal (Heckman et al. 2011).

ment has grown from 400,000 during the early years of the program to nearly a million participants today. We leverage county-level variation in the timing of the Head Start program rollout to identify the effect of Head Start availability on later criminal behavior in adulthood. Given the focus of the Head Start program on poor children and the resulting concentration of funding among high-poverty counties, we focus much of our analysis on this set of more heavily treated counties.[5]

We estimate the effect of Head Start on criminal behavior using individual-level administrative data for the universe of convicted criminals in North Carolina between 1972 and 2015. These administrative data are particularly well suited to our estimation strategy as they contain each criminal's county of birth, allowing us to overcome a variety of measurement and endogeneity concerns that likely inhibited earlier attempts to investigate the effects of the early childhood environment on later criminal behavior.[6] We combine these data with counts of births to construct county of birth by birth cohort conviction rates, which we link with information on the availability of Head Start in each county and year.

We find that Head Start availability reduces the likelihood of a serious conviction by age 35 by 1.3 percentage points, but only in high-poverty counties. These estimates imply treatment effects of Head Start participation of roughly 6-9 percentage points; while substantial, these estimates are half to two-thirds of the size of effects reported in an evaluation of the Perry Preschool program (Heckman et al. 2010).[7]

The estimates are robust to the inclusion of time-varying county-level controls for the avail-

[5] Head Start funding per capita is between three and four times larger in high- versus low-poverty counties.

[6] Most administrative crime datasets do not contain county of birth, forcing researchers interested in the early childhood environment to make relatively strong assumptions about the relationship between location of arrest and earlier residence (for example, Reyes (2007)), or to link multiple datasets together to obtain better measures of both childhood environment (or treatment status) and later criminal behavior. This latter strategy has been used in several small-scale experimental evaluations (for example, Heckman et al. 2010; Campbell et al. 2012). While some survey datasets contain measures of criminal behavior and early childhood environment, small sample sizes, high rates of attrition, and well known issues with underreporting of criminal behavior present their own difficulties (Hindelang et al., 1981).

[7] While our measures are not directly comparable, our point estimates suggest somewhat smaller effects on criminal behavior (than those estimated in evaluations of Perry Preschool) across a range of measures. We discuss this further in Section 4.

ability of other War on Poverty Programs as well as birth county trends.[8] The legitimacy of the identification strategy is further bolstered by event study estimates showing no significant "impact" of Head Start in the years prior to its rollout in a given county and a sharp jump immediately following the program's introduction. While our crime data only cover crimes committed in North Carolina, we find no evidence of differential migration out of one's state of birth as a result of Head Start availability.[9]

Among high-poverty counties, the effects of Head Start availability on later crime are somewhat larger for cohorts exposed to a Head Start program after its first year in operation, perhaps as a result of a ramp up period for the program. The effects also appear to be larger for serious property crimes than violent crimes, suggesting that the effect of Head Start may operate by changing the opportunity cost of crime rather than improving impulse control. Finally, back of the envelope calculations indicate that, in high poverty counties, the discounted benefits generated by Head Start's later crime reduction were likely larger than the costs of the program itself.

3.2 Evidence on the Origins of Criminal Behavior

Research on the developmental factors that influence the likelihood that an individual will become a criminal is limited, with many studies focusing on the period of adolescence. A number of evaluations of the Moving to Opportunity project provide mixed evidence on the effect of neighborhood environment on criminal behavior, while studies of assignment to foster care suggest that family environment has an important role in affecting both contemporaneous and later criminal behavior (Sanbonmatsu et al. 2011; Doyle 2007; Doyle 2008).[10] Several studies have focused

[8]Furthermore, Head Start availability is unrelated to other policy changes shown to affect crime (e.g., removal of lead from gasoline, changes to compulsory schooling law ages in North Carolina, or the legalization of abortion), which occurred at the state level and generally affected different cohorts of individuals.

[9]Across a variety of approaches and subsamples our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. Assuming similar patterns of criminality among North Carolina leavers and stayers, our upper bound estimate of additional migration can explain at most 5% of our estimated effect. We return to this below.

[10]While early evaluations of the program found mixed evidence of effects on involvement with the criminal justice system at different ages (Katz, Kling, and Liebman 2001; Kling, Ludwig, and Katz 2005; Ludwig and Kling 2007), Sanbonmatsu et al. (2011) indicates no clear pattern of significant effects on arrests or delinquent behavior. Any effects that exist appear to be a result of current neighborhood conditions rather than the neighborhood that one grew

on the relationship between secondary education and crime, suggesting that additional years of schooling, increases in school quality, and changes in the composition of school peers can affect the likelihood of criminal behavior several years later (Lochner and Moretti 2004; Deming 2011). Because these adolescent treatments occur at an age when individuals typically first decide to engage in crime, they may directly impact the costs or benefits of crime (e.g. through direct exposure to crime or criminal peers) rather than impacting the individual's development.[11]

Research focusing on earlier periods of development is somewhat less common, with mixed evidence of effects. Emerging evidence suggests an important role for early childhood health and nutrition. Evaluations of the Nurse-Family Partnership Program, the CDC's recommended treatment protocol for lead-poisoned children, and the Food Stamp program, all suggest significant effects of early health interventions on adolescent or adult criminal behavior (Olds et al. 1998, 2007; Billings and Schnepel 2017; Barr and Smith 2018).[12]

There are fewer studies that examine the role of early childhood education. Evaluations of somewhat resource intensive early childhood education programs provide mixed evidence. Heckman et al. (2010) suggests that HighScope Perry preschool participation led to large reductions in criminal behavior, but Campbell et al.'s (2012) evaluation of the Abecedarian program indicates limited effects of the program on crime. Furthermore, while these studies provide rigorous evidence driven by random assignment, both rely on small sample sizes from single sites to support their conclusions.[13] Even if one takes these effects as given, it is unclear whether these types of programs will continue to be effective at a larger scale.

up in. Doyle (2008) finds that those on the margin of placement are two to three times more likely to enter the criminal justice system as adults if they are placed in foster care.

[11] Deming (2011) suggests peer effects as one explanation for the effect of school quality on criminal behavior. Bayer, Hjalmarsson and Pozen (2009) estimate criminal peer effects more directly, showing that juvenile offenders assigned to the same facility affect each other's subsequent criminal behavior.

[12] Related to Billings and Schnepel (2017), there is also a growing literature on the effects of lead exposure on criminal behavior (Aizer and Currie 2017; Feigenbaum and Muller 2016).

[13] Recent evidence that adjusts for multiple hypothesis testing suggests that neither program had statistically significant effects on crime and suggests there may not have been statistically significant benefits for boy participants in either program (Anderson 2008).

3.2.1 The Evidence on Head Start

The Head Start program was an early piece of President Lyndon B. Johnson's War on Poverty, commencing as a summer program in 1965, serving 560,000 children (Vinovskis 2005). It quickly expanded to a year-round program in the following year. Head Start's mission was to "[provide] the children of the poor with an equal opportunity to develop their full potential" (Office of Child Development 1970). To that end, it was designed to focus on the "whole child" by providing a number of wrap-around services alongside education (Ludwig and Miller 2007). These additional services included providing nutritious meals and snacks and access to social workers, mental health and dental treatment, immunizations, and health screenings.

Head Start served a decidedly disadvantaged population in the early years of the program. The median family income of children enrolled in Head Start was less than half that of all families in the U.S. (Office of Child Development 1968). In the early years of the program, between nine and 17 percent of families reported having no running water inside the home and 65 to 70 percent of participants' mothers did not finish high school. Approximately 25 percent lived in female-headed households and between 65 and 70 percent of participating children's mothers were unemployed (Office of Child Development 1968).

While there is some debate about the pattern of short-run Head Start effects, prior quasi-experimental studies suggest Head Start has had important long-term effects for cohorts of children who participated from the late 1960s through the 1980s.[14] Leveraging sibling comparisons and discontinuities in grant-writing assistance and program eligibility, studies have documented increased educational attainment, better health, and higher earnings (65; 66; 67; 68), even in the presence of short-term test-score fadeout (66).

[14]While the Head Start Impact Study (HSIS) found initial positive effects on cognitive skill for participants in the mid-2000s, there were no persistent effects at first and third grade follow-ups (Puma et al. 2005, Puma et al. 2010, Puma et al. 2012). Re-analyses of the HSIS data suggest a more nuanced picture (61). These analyses revealed that there is considerable variation in impact by center (62), that effects are most pronounced among children who would otherwise be in parental or relative care (63), and that Hispanic children and children with low skills at program entry experience the greatest benefit (64).

Two prior studies have included criminal behavior in their investigations of the long-run effects of Head Start, providing conflicting evidence. Garces et al. (2002) find that Head Start participation reduces later criminality among blacks, but Deming (2009) finds no effect. While effects on crime are not the focus of either paper, these estimates should be interpreted cautiously given well known issues with underreporting in self-reported measures of criminal behavior (Hindelang et al. 1981). Moreover, as both of these studies use family fixed effects designs, we might worry that even within families certain types of siblings select into treatment, which could lead to biased estimates of effects.

To overcome these measurement and endogeneity concerns, we leverage (1) unique administrative crime data from North Carolina containing offender county of birth, and (2) the plausibly exogenous rollout of the Head Start program over space and time (see Figure A1). The Head Start program was rolled out quickly and grant funds were distributed directly to local grantees as a means to circumvent governors, state legislatures, and agencies that may have prevented the funds from reaching black children (Gibbs et al. 2011; Vinovskis 2005). In the early years of the program, approximately 40 percent of counties in the U.S. received Head Start funding. As a result of the local distribution of funding, programs became available in different counties at different times. We leverage this variation to identify the effects of Head Start availability on adult criminal behavior.

Three concurrent papers use the early introduction of Head Start over geography and time to explore impacts on other outcomes using survey data.[15] Using the NLSY 79, (69) demonstrates that individuals born in counties with greater levels of Head Start funding attain more education and have better health and earnings in adulthood. Using the PSID, (70) focus on the interaction between Head Start funding levels and subsequent schooling investments, suggesting the presence of dynamic complementarities for these two inputs. Finally, Barr and Gibbs (2017) explore the

[15] Ludwig and Miller (2007) also rely on county-level Head Start availability, leveraging a county poverty rate discontinuity in Head Start grant-writing assistance, to demonstrate effects of the program on mortality.

intergenerational effects of Head Start availability.

3.3 Data

Our primary data source is administrative conviction data from the state of North Carolina. We use these data, combined with information on the number of births within counties over time, to calculate rates of conviction for cohorts across counties. We use Head Start funding by county and year to construct a binary measure of Head Start availability by birth county and cohort. We link this to county by cohort conviction rates to estimate the effect of Head Start availability on crime.

3.3.1 North Carolina Data

We obtained data containing public information on all individuals convicted of a crime in North Carolina between 1972 and 2015 from the North Carolina Department of Public Safety. The administrative data contain information on the type of crime, including the statute of the offense and whether it was a felony, as well as the name, dates of birth, gender, and race of the perpetrator. An important advantage of the North Carolina data over other state criminal databases is the inclusion of county of birth for each individual. Combining information on criminals' years and counties of birth with birth counts obtained from the North Carolina Department of Health and Human Services allows us to construct conviction rates for birth cohorts of individuals born in North Carolina. For example, to generate the cohort conviction rate for children born in county c in 1961, we divide the number of convicted individuals born in county c in 1961 by the total number of individuals born in county c in 1961.

We restrict the sample to individuals born between 1955 and 1968, allowing us to leverage the variation in Head Start availability that occurred up to and including 1972 (as Head Start availability is measured four years after birth). Summary statistics are contained in Table A1. Roughly 5 percent of individuals born between 1955 and 1968 were convicted of a crime by age 35. Looking by type of crime, 2.2 percent were convicted of violent crime and 2.6 percent were convicted of a property crime by age 35.[16] While the data contain the universe of individuals convicted of a crime in North Carolina during this time period and allow us to link these individuals to their counties of birth, they are limited in that they do not allow us to observe convictions for individuals who are born in North Carolina and then leave the state. While most likely criminals remain in their state of birth (and the rate of criminal behavior is actually lower for those who leave), this may be a concern for interpretation of our estimates if Head Start availability generates additional migration of individuals out of the state, but they still commit crime elsewhere.[17] We return to this concern below, providing evidence that program availability does not appear to influence migration rates.[18,19]

3.3.2 Head Start Data

We follow Barr and Gibb's (2017) construction of Head Start availability measures, which rely on county-by-year data from the Community Action Programs (CAP) and Federal Outlay System (FOS) files obtained from the National Archives and Records Administration (NARA).[20] We aggregate funding data by county and year and construct an availability measure as an indicator equal to one if a county had Head Start expenditures per four-year old above the tenth percentile.[21]

[16] We largely follow the convention of FBI's Uniform Crime Reporting Statistics for Part I offenses. Violent crimes are defined as offenses containing the words "murder", "assault", or "robbery". Property crimes are defined as offenses containing the words "burglary" or "larceny".

[17] Roughly 70% of individuals born in North Carolina during this period reside there between the ages of 18 and 35. This share is even higher (roughly 80%) for those with the highest rates of criminal behavior (between ages 18 and 24, non-white, or with less than a high-school degree).

[18] Specifically, we explore the relationship between measures of childhood Head Start availability (at the state of birth by birth cohort level) and the likelihood of living in one's state of birth. Across a variety of approaches and subsamples our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. We address this concern further in Section 3.4.3

[19] We may also be missing individuals with one-time nonviolent convictions (at any age) or one-time drug convictions (under age 22) that hired a lawyer and had the record expunged.

[20] See Barr and Gibb's (2017) Data Appendix for details.

[21] We use this threshold for consistency with Barr and Gibbs (2017), who find that using this threshold better predicts Head Start take-up, but neither the values of the availability indicator (in North Carolina) nor the main results are sensitive to moving this threshold.

3.4 Estimation of Program Availability Effects

To estimate the effect of Head Start availability during childhood on adult crime, we leverage within county variation in the availability of Head Start generated by the initial roll-out of the program in the 1960s. For example, we utilize the fact that eligible four-year-olds in 55 out of North Carolina's 100 counties had access to Head Start in 1968 while no four-year-olds had access to Head Start prior to 1965 (See Figure A1).

The Head Start program targeted children from families at or below the federal poverty line.[22] Therefore, Head Start availability likely had a more dramatic impact in counties with high poverty rates. Indeed, funding per four-year old is three to four times as high in high-poverty counties (Figure A2). Accordingly, we conduct much of our analyses separately for high and low poverty counties, splitting counties at the median poverty level for all counties in North Carolina in 1960. For both poverty groups we estimate a difference-in-differences specification:

$$C_{ct} = \alpha_c + \alpha_t + \beta HS_{ct} + \gamma(X_{c,60} \times t) + \epsilon_{ct},$$

where C_{ct} is the conviction rate for those born in county c in year t, HS_{ct} is indicator for whether a county-cohort was exposed to Head Start, α_c and α_t are birth county and birth cohort fixed effects, and $X_{c,60} \times t$ are controls for birth county characteristics in 1960 interacted with a time trend. Including these trends allows, for example, counties which are more rural or have an older age demographic to trend differently than more urban and younger counties.[23] Standard errors are clustered at the county of birth level.

[22] At least 90 percent of Head Start participants at each site had to be from families below the poverty line.

[23] The county characteristics include the percent of people living in families with less than $3,000 (1960 dollars), the percent living in urban areas, the percent black, the percent under 5 years old, the percent over 65 years old, the percent of land in farming, and the percent of employment in agriculture.

3.4.1 Main Results

Our primary interest is in the coefficient β, which represents the effect of Head Start availability on adult crime. Given the focus of the Head Start program on poor children and the resulting concentration of funding among high-poverty counties, we present estimates separately for high and low poverty counties. Our main estimates suggest a 1.3 percentage point reduction in high poverty counties, but offer no evidence of effects in low poverty counties (Table A2). The 1.3 percentage point reduction in high poverty counties is 28% off the mean conviction rate of 4.7%.[24] While the point estimates in low poverty counties are small, positive, and insignificantly different from zero, we cannot reject the equivalence of the effects of Head Start *participation* given the substantially higher participation rates in high-poverty counties. In other words, the differences in estimates across the high and low poverty counties can potentially be explained as capturing the differential Head Start "dosage" by poverty level.[25] We have also estimated specifications that interact the continuous poverty rate with an indicator for Head Start availability (Appendix Table A2). Using this approach, we estimate that the reduction in crime rate due to Head Start availability is 0.2 percentage points larger for each 10 percentage point increase in the poverty rate. Consistent with our prior estimates these estimates suggest that the effect of Head Start ranged from 0.000 in the county with the lowest poverty rate (23 percent) to roughly 1 percentage point in the county with the highest (74 percent).

The estimates are robust to the inclusion of pretreatment (1960) county characteristics interacted with time trends as well as the inclusion of covariates indicating availability of other War on Poverty programs, such as Food Stamps, Medicaid, Community Health Centers, etc. (Tables A3 and A4). While our baseline inference relies on standard errors clustered at the county of birth level, we have also explored the robustness of our p-values to an even more conservative approach:

[24] While our main estimates are identified off of the set of counties that ever received Head Start between 1965 and 1976, the results are robust to the inclusion of matched control counties that did not receive Head Start during this period (Table A1).

[25] Figure A3 presents coefficient estimates for the same specification by poverty quintiles. While there is evidence of a Head Start effect in the fourth quintile, the most dramatic effect occurs in counties in the highest poverty quintile.

randomization inference. Under this procedure, we randomly assign the rollout year of Head Start in each county and estimate our baseline specification. The distribution of these estimates over 1,000 iterations is contained in Appendix Figure A1. As can be seen in the figures, the estimates we observe in our baseline results are quite unlikely under random assignment. The two-tailed p-values we obtain from this randomization inference approach are similar to those obtained using our baseline approach.[26]

To understand the dynamics of how the program may have affected adult criminal outcomes and to test for pre-trends that may confound our baseline specification, we also present estimates from an event study specification. We center counties around the first year that Head Start is available, and estimate the following specification separately for counties above and below the median poverty rate:

$$C_{ct} = \sum_{\tau=-6}^{7} \beta_\tau 1(t = T_c + \tau) + \alpha_c + \alpha_t + \gamma(X_{c,60} \times t) + \epsilon_{ct},$$

We are primarily interested in the coefficients on the indicators, $1(t = T_c + \tau)$, each of which indicates how many years cohort t in county c is removed from the first cohort in county c exposed to Head Start, T_c. The first cohort with Head Start available four years after birth was born in 1961, allowing us to identify up to 7 years of post-availability effects.

Our baseline dynamic estimates (Table A3 and Figure A4) indicate a flat trend in cohort conviction rates before Head Start rollout for both high and low poverty counties. This provides evidence that our difference-in-differences estimates are not capturing differential pre-existing trends in the years prior to county's rollout of Head Start. For cohorts exposed to Head Start, we see significant decreases in the conviction rate for the high poverty counties but continue to see no evidence of changes in the low poverty counties. In the high poverty counties, the estimates of crime reduction

[26]P-values presented are the two-tailed statistics calculated as the share of coefficient estimates obtained under random assignment of Head Start timing that are larger in absolute magnitude than the estimate produced using the true timing of assignment.

appear to grow somewhat as the program persists in a county. In particular, the impact of Head Start availability in the first year of the program is substantially smaller than in subsequent years. This may be due to centers improving (or increasing the size of) their Head Start programs during the first years of operation or as a result of peer effects.[27] Indeed, funding does appear to increase somewhat during the early years of program operation (Figure 2).

3.4.2 Magnitude of Effect on Criminal Behavior

Our estimated effects of the availability of Head Start on criminal behavior are substantial. Our preferred estimates indicate reductions in the likelihood of any serious conviction of 1.3 percentage points (among cohorts in high poverty counties). To put our results in the context of recent literature with similar outcome measures, these estimates imply treatment-on-the-treated (TOT) effects of 6 to 9 percentage points.[28]

While it isn't straightforward to construct comparable measures of criminal behavior across studies, our implied TOT effects are between half and two-thirds of the size of effects on somewhat similar measures reported in evaluations of the Perry Preschool program (11 to 12 percentage points on any arrest (or any charges) by age 40).[29,30] As in the Perry evaluation, we find larger effects on property crimes; Head Start access reduces the likelihood of a serious property conviction by 0.9 percentage points, a TOT effect of roughly 5 percentage points in high-poverty counties

[27] If peer effects are an important factor in criminal behavior, we would expect smaller effects of the program in the first year as compared to subsequent years when older peers would have also experienced the program.

[28] These TOT estimates are based on estimated Head Start participation rates in high poverty counties of 15 to 21 percent. The lower bound is based on OEO statistics on state-level North Carolina Head Start enrollment in 1966 and the upper bound is based on author's calculations assuming the national per participant funding level is fixed across North Carolina counties.

[29] The treatment effect of Perry Preschool on any felony arrest, the definition of which overlaps substantially with Part 1 crimes, is even larger (15 percentage points), but is reported only for males (Heckman et al. 2009).

[30] Our TOT estimates are less than half of the effects estimated for the Nurse-Family Partnership by age 19 (16 percentage points on likelihood of conviction or arrest) and the effects estimated for the full set of services provided by a more recent intervention targeted at children with high blood lead levels (17 percentage points on likelihood of arrest). The less intensive set of services, primarily information on how to reduce lead exposure and eat better, produced effects of a similar size to our implied TOT estimates (Olds et al. 1998, 2007; and Billings and Schnepel 2017). Our effect sizes are similar to recent estimates of the effects of early childhood Food Stamp access (Barr and Smith 2018). In contrast to all three of these health interventions, which found strong effects on violent criminal behavior, the effects of Head Start access are stronger on property crimes.

(Table A4). While there is no significant effect on serious violent convictions, the point estimate (0.0046) implies a TOT of approximately 3 percentage points.[31] In comparison, Schweinhart et al. (2005) find a 16 percentage point reduction in violent arrests by age 40 (32 versus 48 percent) and a 22 percentage point reduction in property arrests by age 40 (36 versus 58 percent) in their evaluation of Perry preschool, four to five times the size of our effects.[32] Perry Preschool enrolled a very particular type of student: extremely disadvantaged, black children in Ypsilanti, Michigan. If we split our property crime estimates by race, we find similar effects for whites and non-whites (Table A5).[33]

Of course it may not be reasonable to convert our estimates to TOT effects as there may be important spillover effects of program availability; indeed, it is not difficult to imagine that improving the behavioral trajectories of a significant share of a group results in improvements for the group as a whole that are substantially larger than what we might expect to see if an individual was treated in isolation. Unlike the Perry evaluation, in which fewer than 50 children were offered a spot in the treatment group, Head Start was attended by a substantial fraction of children, particularly in poor areas. As participants interacted with others in their cohort, effects of the program might have spilled over to the children of non-participants in a way that would have been unlikely with the smaller treatment and control groups in the Perry evaluation. It is easy to see how these spillovers might operate through peer effects. Given the potential for large spillovers, we focus our discussion on the estimated effects of Head Start availability rather than participation.

[31]The event studies indicate that Head Start availability likely reduced both types of crime (Figures A5 and A6). These p-values are also robust to randomization inference (Appendix Figures A2 and A3).

[32]Although we note that these are effects on *any* arrest and thus may not be directly comparable to convictions for a serious violent or property crime. Treatment estimates of Perry Preschool on the *number* of felony arrests indicates no significant difference in the number of serious violent crimes and a 90 % reduction in the number of felony property arrests (0.31 versus 2.91 per individual).

[33]The estimates for violent crime are in Appendix Table A5. During this period in North Carolina, blacks comprised more than 95% percent of the non-white population (1970 Census).

3.4.3 Threats to Internal Validity

To interpret these estimates as the causal effect of Head Start availability, it must be the case that the availability of a Head Start program is, conditional on county and year of birth fixed effects, unrelated to other factors that would affect the outcomes of children born to women who did and did not have the program available. While the evidence indicates large negative effects of Head Start availability on crime, here we address concerns related to the endogeneity of Head Start program adoption as well potential concerns related to the effect of Head Start availability on migration out of North Carolina.

3.4.3.1 Endogeneity of Head Start Availability

Whereas the initial policy implementation occurred at the federal level, variation in the rollout of the policy occurred at the county level. Because we are controlling for variation over time (with birth cohort fixed effects) and fixed differences between counties (with county fixed effects), the concern is that counties adopted the Head Start program when four year olds in those counties happened to be less likely to commit crimes as adults for some other reason. For example, counties that chose to adopt the Head Start program earlier may be those who were proactively improving medical or childcare for four year olds at the same time. If this were the case, we might observe reduced criminal behavior for these cohorts due to a comprehensive effort to help them, and not because of Head Start availability.

If this type of endogenous policy implementation were occurring, we would expect to see some strong association between county characteristics and the timing of adoption. In Tables A6 and A7 we explore the endogeneity of Head Start adoption within North Carolina, regressing county characteristics on Head Start timing. We find no statistically significant relationship between county characteristics in 1960 and the timing of Head Start availability, whereas more populous counties were more likely to get the program at all during this time period.[34] Consistent with this, the in-

[34]We present these relationships between county characteristics and the timing of Head Start adoption graphically in Appendix Figure A6. As with our regression estimates, there is little relationship between county characteristics

clusion of 1960 county characteristics interacted with a trend in birth year has little impact on our estimates. A related concern is that there are pre-existing trends in the likelihood of criminal behavior in counties that are related to the timing of Head Start adoption. Allowing for birth-county specific trends also has no effect on our point estimates

The event-study estimation depicted in Figure A4 further addresses concerns related to endogenous program adoption by demonstrating no "effect" of Head Start availability in the years prior to the program's initial rollout in a county and a sharp jump immediately following the program's introduction. This figure also addresses concerns that there were subsequent changes in a county that affected crime rates, such as changes to its criminal justice system, that are correlated with but not caused by the timing of a county's Head Start adoption. For such a correlation to produce our event study results, the policy change would have to precisely target only cohorts exposed to Head Start availability and have no effect on cohorts born just a couple years earlier.

If changes in availability of other War on Poverty programs occurred in a county at the same time as the rollout of Head Start, then our estimates could be capturing the effects of those programs rather than the effect Head Start. We address this concern by including controls for the availability of various War on Poverty Programs in Tables A3 and A4.[35] We find that our baseline estimates are robust to the inclusion of these controls.

We also test directly for relationships between these potential confounders and our measure of Head Start availability. Consistent with the limited effect of the War on Poverty controls on our estimates, we find no significant relationships between funding for various War on Poverty Programs and Head Start availability (Table A8). We also find no relationship between Head Start availability and measures of infant mortality. This suggests that the relationship between Head Start availability and later criminal behavior is not driven by broader improvements in infant health or medical treatment unrelated to Head Start.

and the timing of adoption, supporting the validity of our identification strategy.

[35]Following (71), we consider controls for the Food Stamp Program as well as per capita expenditures on Public Assistance Transfers, Medicaid expenditures, Community Health Centers and Community Action Agencies.

3.4.3.2 *Effects of Head Start Availability on Migration out of North Carolina*

Another potential threat to the validity of our estimates relates to our data. While the data contain the universe of individuals convicted of a crime in North Carolina during this time period and allow us to link these individuals to their counties of birth, they are limited in that they do not allow us to observe convictions for individuals who are born in North Carolina and then leave the state. Fortunately, most individuals born in North Carolina remain there during adulthood; roughly 70% born in North Carolina during this period reside there between the ages of 18 and 35. This share is even higher (roughly 80%) for those with the highest rates of criminal behavior (between ages 18 and 24, non-white, or with less than a high-school degree).

And yet we might still be concerned if Head Start availability has differential effects on migration out of the state. While this will not affect our estimates of convictions in North Carolina, it is a potential concern for interpreting the estimates as representing an overall reduction in criminal behavior. Specifically, we would be concerned if Head Start availability led individuals to be more likely to leave the state but no less likely to commit a crime, as we could confuse this for a reduction in crime. In Appendix Table A10, we explore the relationship between measures of childhood Head Start availability (at the state of birth by birth cohort level) and the likelihood of living in one's state of birth. Across a variety of approaches and subsamples our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. Assuming similar patterns of criminality among North Carolina leavers and stayers, our upper bound estimate of additional migration can explain at most 5% of our estimated effect.[36] Even this upper bound is likely an overestimate as the mean rate of criminal conviction for movers to North Carolina (i.e., the equivalent of state of birth leavers) is lower than the rate for those born in North Carolina in our data.

[36] Even assuming the largest estimated effect on migration, it would have to be the case that 65% of the marginal migrants were criminals to account for our estimates.

3.4.4 Quantifying the Benefits

How do Head Start's future benefits of crime reduction compare to the costs of the program? To enable this comparison, we present back-of-the-envelope estimates of the discounted future value of crime reduction in Table A6 by offense for various choices of discount rates (Columns 4-7). Column 2 shows the reduction in number of convictions per Head Start enrollee implied by our difference-in-differences estimate for high poverty counties.[37] Column 3 shows the reduction in the number of crimes associated with this reduction in convictions.[38] We apply McCollister et al (2010) estimates of the social cost (2015 dollars) of each type of crime (Column 1) to arrive at our estimates for the benefits generated by Head Start participation in high poverty counties (Columns 4-7).[39] Undiscounted, we estimate these benefits to be $9,835, at least three times the cost of the program per individual during this time period.[40] Under standard discount rates (3-5%), we estimate that the discounted benefits from property crime reduction exceed the costs of the program. With a discount rate of 7%, the estimated benefits fall to $2,335, somewhat less than the cost of the program. However, we view these figures as quite conservative as they focus exclusively on serious property crimes, despite the high likelihood of effects on other crime types, suggesting that in high-poverty counties the Head Start program passes a cost-benefit test based on its effects on crime alone.

[37] We convert the difference-in-differences coefficient estimate to the number of convictions per Head Start enrollee in two steps. First, we divide it by the Head Start participation rate to obtain the TOT effect. Second, we convert unique convicts per Head Start enrollee to number of convictions per Head Start enrollee by multiplying by the mean convictions by age for the sample of convicts. Column 2 shows the result across ages 18-35.

[38] North Carolina has roughly 5.4 burglary and larceny arrests per conviction and roughly 5.8 reported burglary and larceny offenses per arrest (authors' calculations using statistics from the NC State Bureau of Investigation's "Crime in North Carolina -1995" report).

[39] Benefits are calculated for each age from 18-35 and then discounted back to age 4 (for comparison with the program cost) at the given rate.

[40] We report the benefit cost ratios based on expenditures per pupil during the first few years of the program. The ratios are somewhat smaller using full-time expenditures per pupil estimates from the later years of our sample period.

3.5 Conclusion

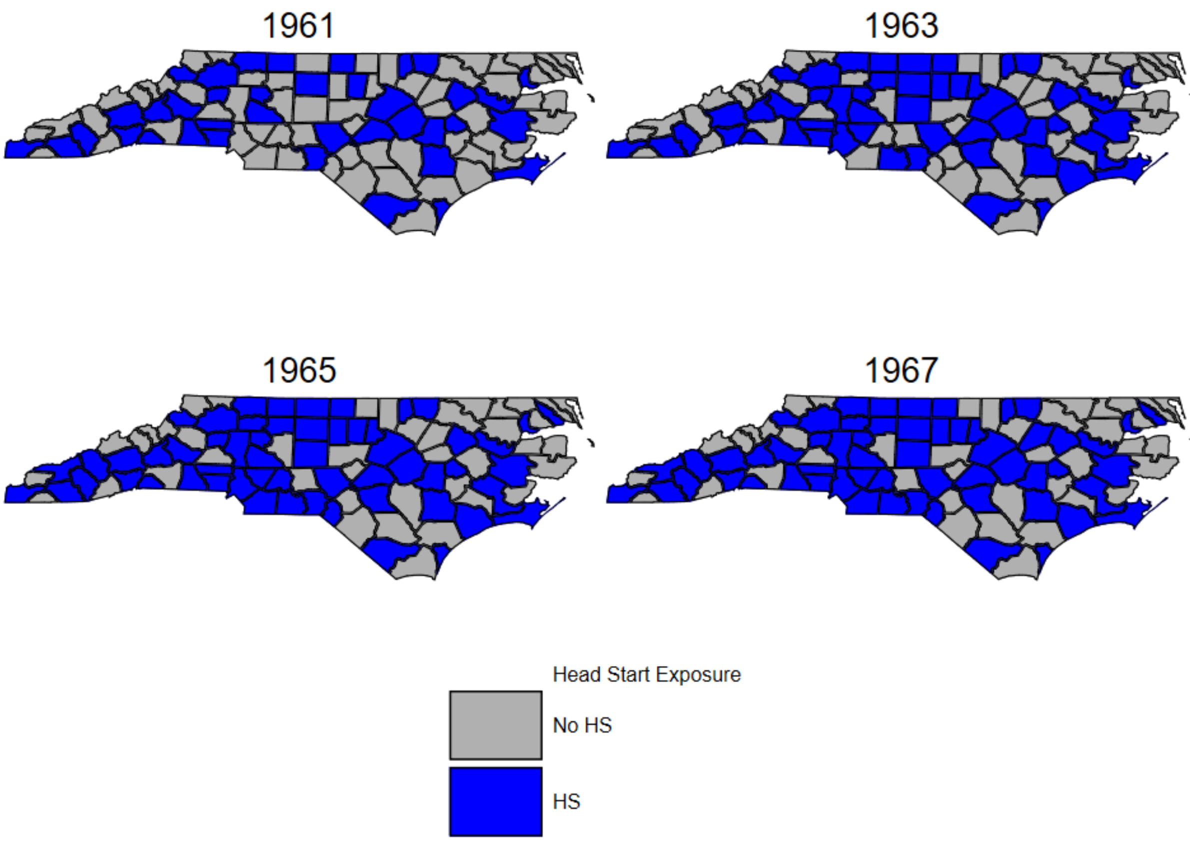
We contribute to the sparse literature on the developmental factors that influence an individual's likelihood of becoming a criminal by exploring the effect of early childhood education on criminal behavior. Understanding the role of early childhood education in later criminal behavior has become increasingly relevant given recent expansions in the share of children attending public preschools. These expansions have been driven in large part by a belief among policymakers that early childhood education interventions have large impacts later in life. Given the major contribution of crime reduction to cost-benefit analyses of similar programs used to motivate recent expansions (for example, crime reduction accounts for 40-65% of the benefits estimated in the context of Perry preschool), it is critical to better understand the relationship between early childhood education and later criminal behavior and the extent to which this relationship may hold at scale. We bring new evidence to this question by investigating the effect of Head Start availability on criminal behavior.

We use individual-level administrative data for the universe of convicted criminals in North Carolina between 1972 and 2015. These administrative data are particularly well suited to our estimation strategy as they contain each criminal's county of birth, allowing us to overcome a variety of measurement and endogeneity concerns that likely inhibited earlier attempts to investigate the effects of the early childhood environment on later criminal behavior. Using these data, we provide the first large-scale evidence that early childhood education reduces later criminal behavior and the first estimates of the effect of Head Start availability on crime using administrative data (and thus not subject to concerns about the reporting of crime in survey data). We find that Head Start availability reduces the likelihood of a serious conviction by age 35 by 1.3 percentage points, but only in high-poverty counties. These estimates imply treatment effects of Head Start participation of roughly 6-9 percentage points. Given the high costs of crime, back-of-the-envelope calculations using these estimates indicate that the size of the discounted external benefits generated by Head

Start's later crime reduction likely exceeded the costs of the program in high poverty counties. This is especially noteworthy considering that later crime reduction was not the stated objective of the program.

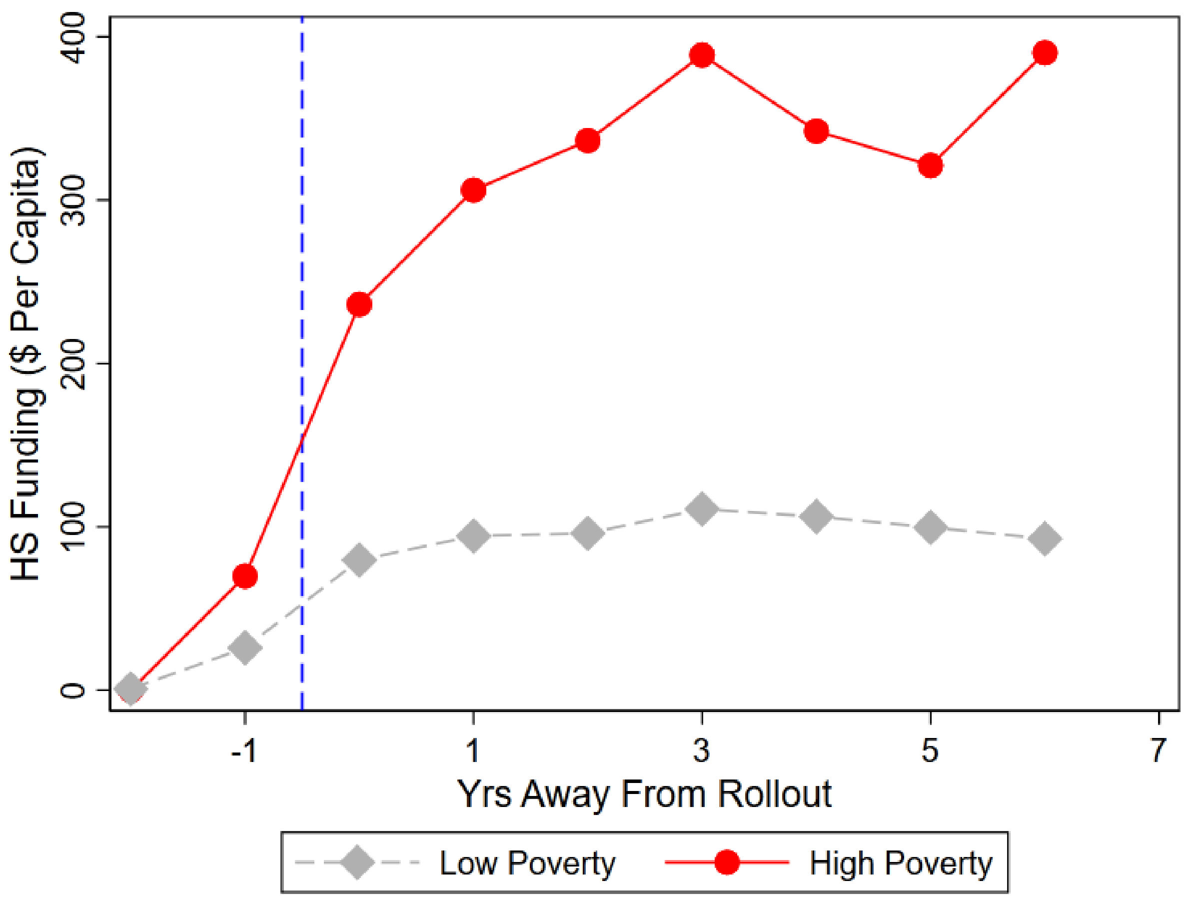
While additional analysis of other and more recent early childhood programs is warranted, our results indicate a meaningful connection between targeted, large scale early childhood education interventions and criminal behavior. These results provide evidence in support of recent state efforts to expand early childhood education, but point to large potential gains from targeting these efforts toward higher poverty areas. Additional work is needed to better understand the extent to which the effects of targeted early childhood education programs extrapolate to the increasingly open or universal access programs proposed and implemented in recent years.

Figure A1: County by Birth Cohort Head Start Rollout in North Carolina



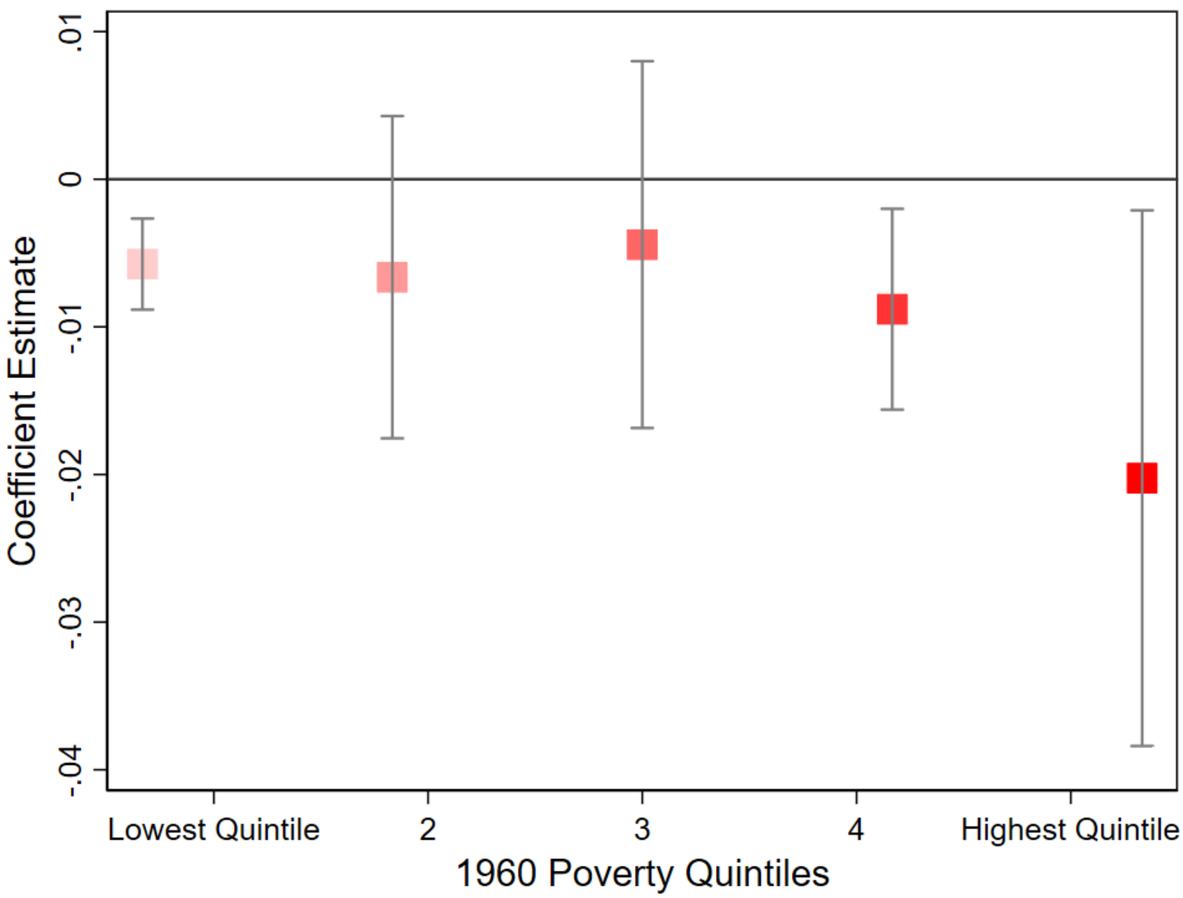
Note: Figure shows which birth-cohorts born to which counties had Head Start available to them in North Carolina from 1961 to 1967. Prior to the 1961 birth cohort no counties had Head Start available. Start availability is identified from county by year level Head Start funding data following (72). Head Start funding levels are obtained from Head Start Historical Records.

Figure A2: Head Start Funding By County Poverty Level



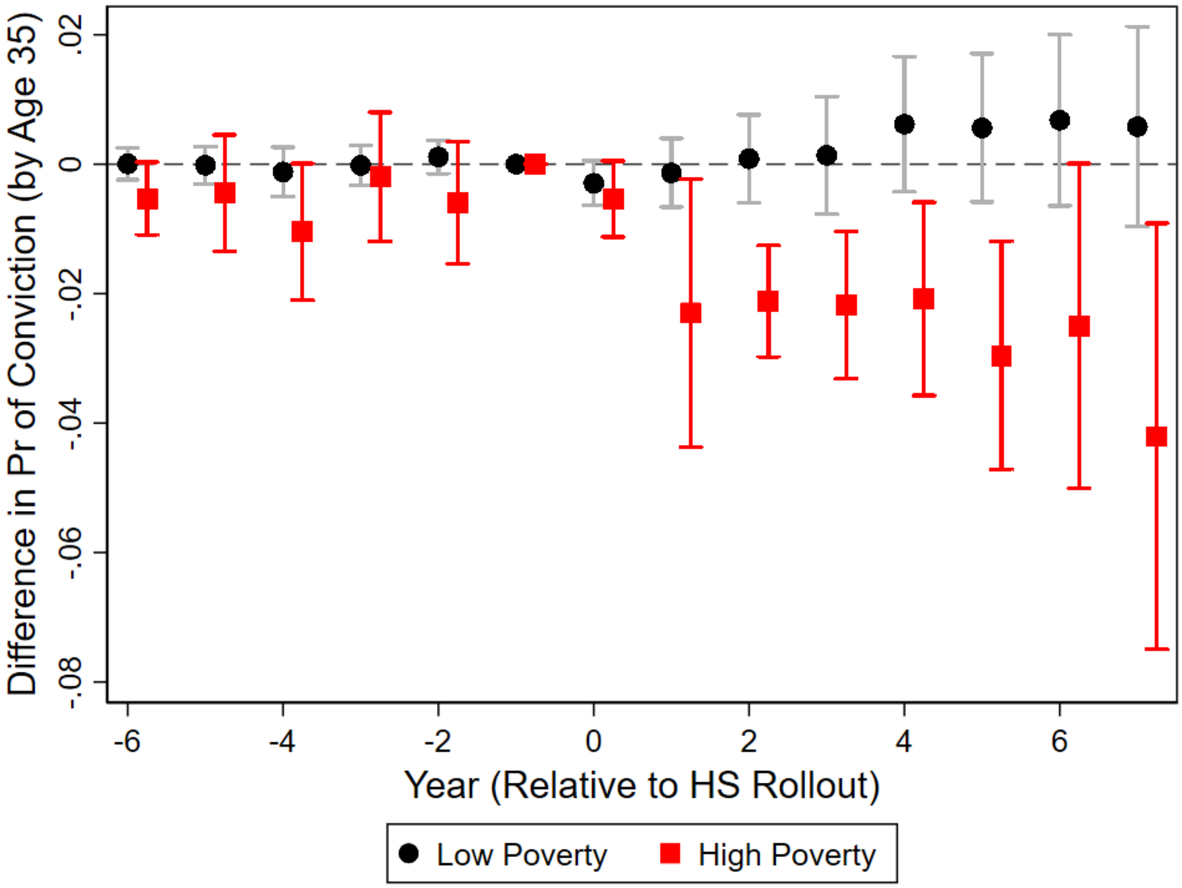
Note: Figure shows per capita county level Head Start funding (given in $ per 4 year olds) separately for high and low poverty counties. There exist non-zero funding levels in the year prior to Head Start rollout for two reasons: first, following (72), county birth cohorts with very low funding levels are treated as not having Head Start availability, and, second, we do not count 1965 as the first year of availability since the Head Start program was introduced only as a pilot program over the Summer in that year. High poverty counties are those counties with a 1960 poverty rate above the median in North Carolina (40.2% poverty), while low poverty are those with a below median 1960 poverty rate.

Figure A3: DD Estimates by Quintiles



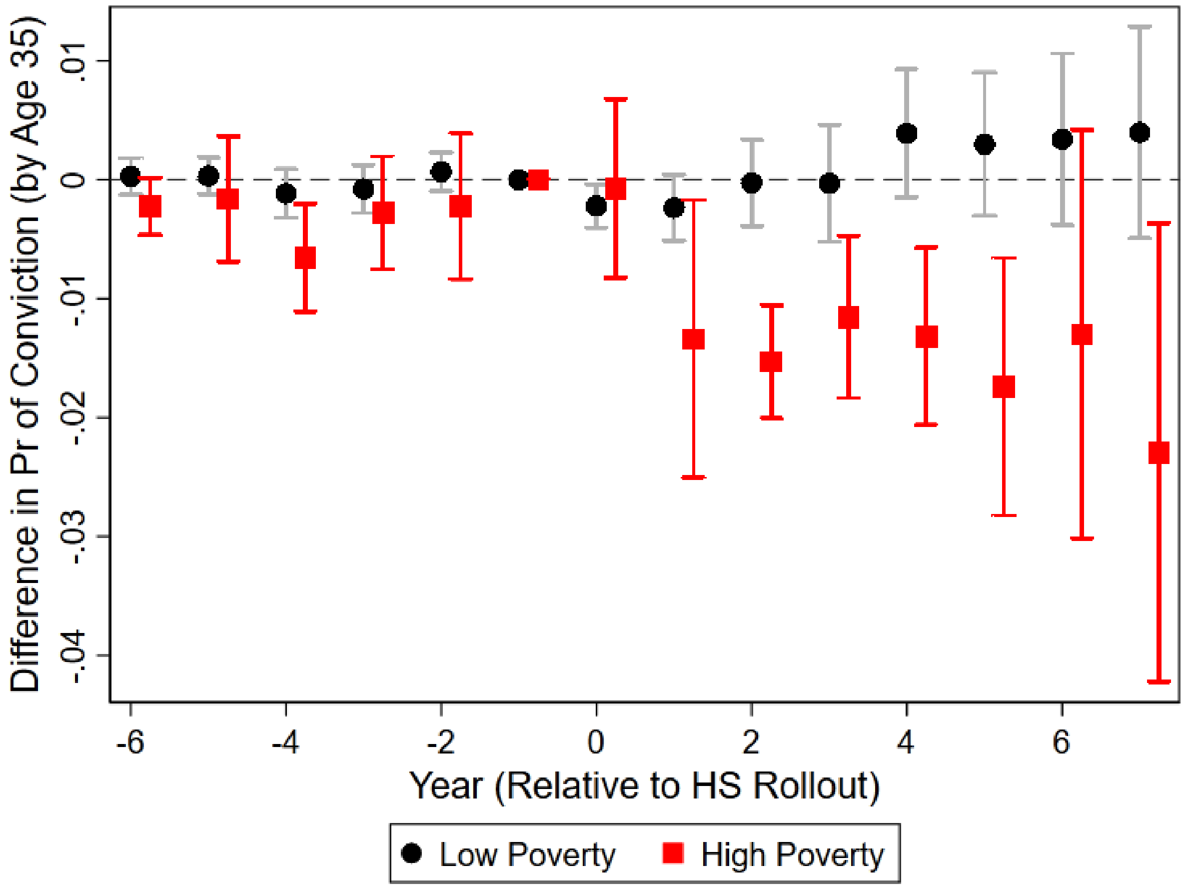
Note: Figure shows the coefficient estimates and 95% confidence intervals from estimating our basic difference-in-differences specification separately for counties in each quintile of the 1960 North Carolina poverty rate. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A4: Event Study



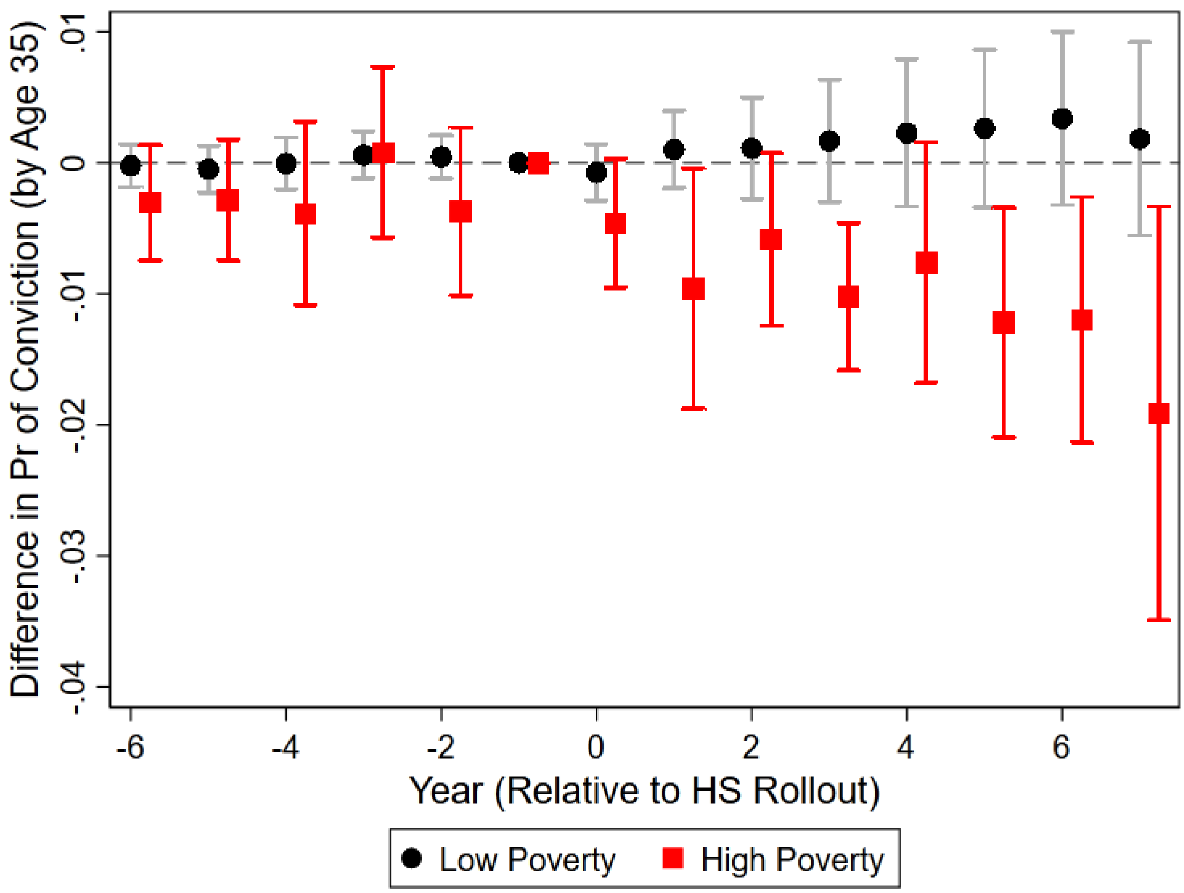
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 3.4.1 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955 (just as in Table A3). The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A5: Event Study, Part 1 Property Crimes



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 3.4.1 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 property crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A6: Event Study, Part 1 Violent Crimes



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 3.4.1 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955 (just as in Table A3). The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 violent crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2%) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Table A1: Descriptive Statistics

	All	High Poverty	Low Poverty
Panel A: Crime Outcome Variables			
Part 1 Conviction by Age 35	0.0476	0.0469	0.0478
	(0.0230)	(0.0265)	(0.0220)
Property	0.0256	0.0255	0.0257
	(0.0125)	(0.0146)	(0.0118)
Violent	0.0220	0.0214	0.0221
	(0.0114)	(0.0128)	(0.0109)
White	0.0135	0.0128	0.0137
	(0.00730)	(0.00851)	(0.00689)
Non-White	0.0513	0.0555	0.0555
	(0.0251)	(0.0214)	(0.0247)
Panel B: Head Start Availability Variables			
First Cohort with HS Availability	1962.3	1962.3	1962.3
	(2.480)	(2.741)	(2.403)
HS Funding ($ per 4 year old)	139.12	301.73	93.83
	(188.37)	(299.61)	(105.11)
Observations	882	308	574

Note: Panel A contains summary statistics of crime outcome variables for the sample of birth cohorts born from 1955 to 1968. Each observation is at the county birth-cohort level. The outcome variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). Panel B contains summary statistics for Head Start availability and funding. (Funding levels are given for exposed county-cohorts only, so that only non-zero values are included.) All variables are further broken down by county level poverty status. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. Standard deviations are given in parentheses. Data sources are, respectively, the NC Department of Corrections, and Head Start Historical Records.

Table A2: Effect of Head Start Availability on Rate of Serious Criminal Conviction by Age 35

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Head Start Availability	-0.0018	-0.0030	-0.0131**	-0.0131**	0.0026	0.0012
	(0.0031)	(0.0030)	(0.0057)	(0.0059)	(0.0032)	(0.0040)
Observations	882	882	308	308	574	574
Mean	0.0476	0.0476	0.0469	0.0469	0.0478	0.0478
Baseline Chars x Trend		X		X		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A3: Effect of Head Start Availability on Rate of Serious Criminal Conviction - Dynamics

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
-6	-0.0011	-0.0006	-0.0049**	-0.0053*	-0.0004	0.0001
	(0.0011)	(0.0011)	(0.0020)	(0.0029)	(0.0012)	(0.0013)
-5	-0.0016	-0.0011	-0.0043	-0.0045	-0.0007	-0.0002
	(0.0012)	(0.0013)	(0.0040)	(0.0046)	(0.0014)	(0.0015)
-4	-0.0029*	-0.0024	-0.0101*	-0.0105*	-0.0017	-0.0012
	(0.0016)	(0.0016)	(0.0050)	(0.0054)	(0.0019)	(0.0019)
-3	-0.0006	-0.0003	-0.0015	-0.0020	-0.0006	-0.0002
	(0.0015)	(0.0016)	(0.0056)	(0.0051)	(0.0015)	(0.0016)
-2	0.0006	0.0007	-0.0057	-0.0060	0.0009	0.0011
	(0.0013)	(0.0013)	(0.0048)	(0.0048)	(0.0013)	(0.0013)
First Year of Availability	-0.0031**	-0.0033**	-0.0053**	-0.0054*	-0.0027*	-0.0029
	(0.0013)	(0.0013)	(0.0026)	(0.0030)	(0.0016)	(0.0018)
1	-0.0041*	-0.0044*	-0.0230**	-0.0230**	-0.0012	-0.0013
	(0.0021)	(0.0023)	(0.0098)	(0.0106)	(0.0021)	(0.0027)
2	-0.0032	-0.0037	-0.0198***	-0.0212***	0.0012	0.0008
	(0.0027)	(0.0026)	(0.0034)	(0.0044)	(0.0028)	(0.0035)
3	-0.0038	-0.0046	-0.0203***	-0.0218***	0.0020	0.0014
	(0.0035)	(0.0036)	(0.0052)	(0.0058)	(0.0035)	(0.0046)
4	-0.0002	-0.0013	-0.0213**	-0.0208**	0.0072	0.0062
	(0.0047)	(0.0044)	(0.0078)	(0.0076)	(0.0044)	(0.0053)
5	-0.0025	-0.0041	-0.0300***	-0.0296***	0.0069	0.0056
	(0.0049)	(0.0049)	(0.0090)	(0.0090)	(0.0044)	(0.0058)
6	-0.0013	-0.0039	-0.0253*	-0.0250*	0.0093*	0.0068
	(0.0056)	(0.0058)	(0.0123)	(0.0128)	(0.0051)	(0.0067)
7+	-0.0023	-0.0071	-0.0423**	-0.0421**	0.0114*	0.0058
	(0.0069)	(0.0068)	(0.0169)	(0.0168)	(0.0065)	(0.0079)
Observations	882	882	308	308	574	574
Mean	0.0476	0.0476	0.0469	0.0469	0.0478	0.0478
Baseline Chars X Trend		X		X		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variables of interest are a set of indicators for how many years away from the first year of Head Start availability in their birth county a given birth cohort was. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$) .

Table A4: Effect of Head Start Availability on Rate of Serious Criminal Conviction - By Crime Type

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Type 1 Property Crimes						
Head Start Availability	-0.0024	-0.0028*	-0.0085***	-0.0086***	0.0000	-0.0005
	(0.0016)	(0.0015)	(0.0028)	(0.0028)	(0.0016)	(0.0021)
Observations	882	882	308	308	574	574
Mean	0.0256	0.0256	0.0255	0.0255	0.0257	0.0257
Panel B: Type 1 Violent Crimes						
Head Start Availability	0.0005	-0.0002	-0.0046	-0.0046	0.0026	0.0017
	(0.0017)	(0.0016)	(0.0031)	(0.0032)	(0.0018)	(0.0020)
Observations	882	882	308	308	574	574
Mean	0.0220	0.0220	0.0214	0.0214	0.0221	0.0221
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of either UCR Part 1 property crimes (Panel A) or Part 1 violent crimes (Panel B) in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A5: Effect of Head Start Availability on Rate of Serious Property Conviction- By Race

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: White						
Head Start Availability	-0.0027	-0.0031	-0.0076*	-0.0074	0.0001	0.0002
	(0.0023)	(0.0022)	(0.0042)	(0.0043)	(0.0021)	(0.0023)
Observations	667	667	252	252	415	415
Mean	0.0156	0.0156	0.0150	0.0150	0.0158	0.0158
Panel B: Non-White						
Head Start Availability	-0.0037	-0.0033	-0.0076**	-0.0085**	-0.0037	-0.0004
	(0.0037)	(0.0040)	(0.0031)	(0.0033)	(0.0058)	(0.0082)
Observations	667	667	252	252	415	415
Mean	0.0523	0.0523	0.0396	0.0396	0.0562	0.0562
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of white or non-white individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 property crime in North Carolina by age 35. UCR Part 1 property crimes are those in which the description of the offense contains the words “burglary” or “larceny”. Panel A presents these results for white cohorts, while Panel B reports them for non-white cohorts. Sample sizes are smaller for these specifications because the natality files for 25% of counties in North Carolina do not have race breakdowns before 1969, we do not know the race of approximately 13% of births in our sample. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called “High Poverty", while those below the median are called “Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

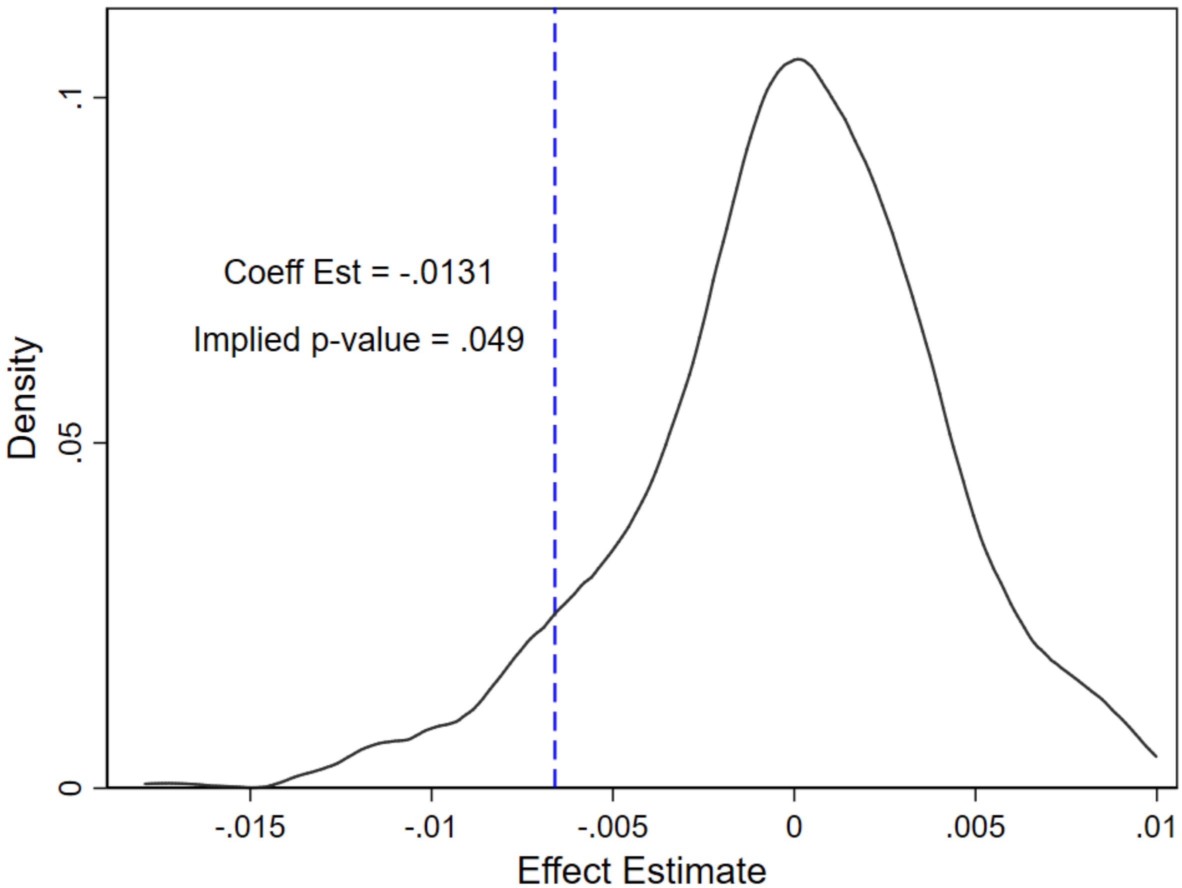
Table A6: Estimates of the Social Benefits of Crime Reduction from Head Start Participation

	Cost Estimate ($ 2015)	Est. Δ Convictions	Est. Δ Crimes	Discounted Social Benefits 0%	3%	5%	7%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
McCollister, French, and Fang (2010) Crime Cost Estimates:							
Larceny	3,911	-0.075	-2.375	9,289	4,884	3,255	2,207
Burglary	7,155	-0.002	-0.076	546	286	189	128
			CRIME BENEFITS:	**9,835**	**5,169**	**3,445**	**2,335**
			BENEFIT-COST RATIO:	**3.5**	**1.8**	**1.2**	**0.8**

Note: This table shows back-of-the-envelope calculations of the discounted social benefits of later crime reductions due to Head Start participation in high poverty counties. Social cost estimates for each crime type (Column 1) are adopted from McCollister et al. (2010). These estimates include victimization costs, criminal justice system costs, and the lost value of criminals' time, but do not include private expenditures on crime prevention. In Column 2, we report the estimated change in convictions by crime type, which we obtain by first dividing our property crime coefficient estimate by our estimated first stage and multiplying by mean number of property crimes of a particular type given any property conviction in North Carolina. In Column 3, we report the estimated change in criminal offenses associated with the given change in convictions. North Carolina has roughly 5.4 burglary and larceny arrests per conviction and roughly 5.8 reported burglary and larceny offenses per arrest (authors' calculations using statistics from the NC State Bureau of Investigation's "Crime in North Carolina -1995" report). Estimates of the discounted social benefit, contained in Columns 4-7, are produced by multiplying the dollar value of each offense's social cost by the change in offenses implied by our estimates (by age for ages 18-35) discounting back to age 4 (for comparison with the program cost) at the given rate. All monetary values are in 2015 dollars.

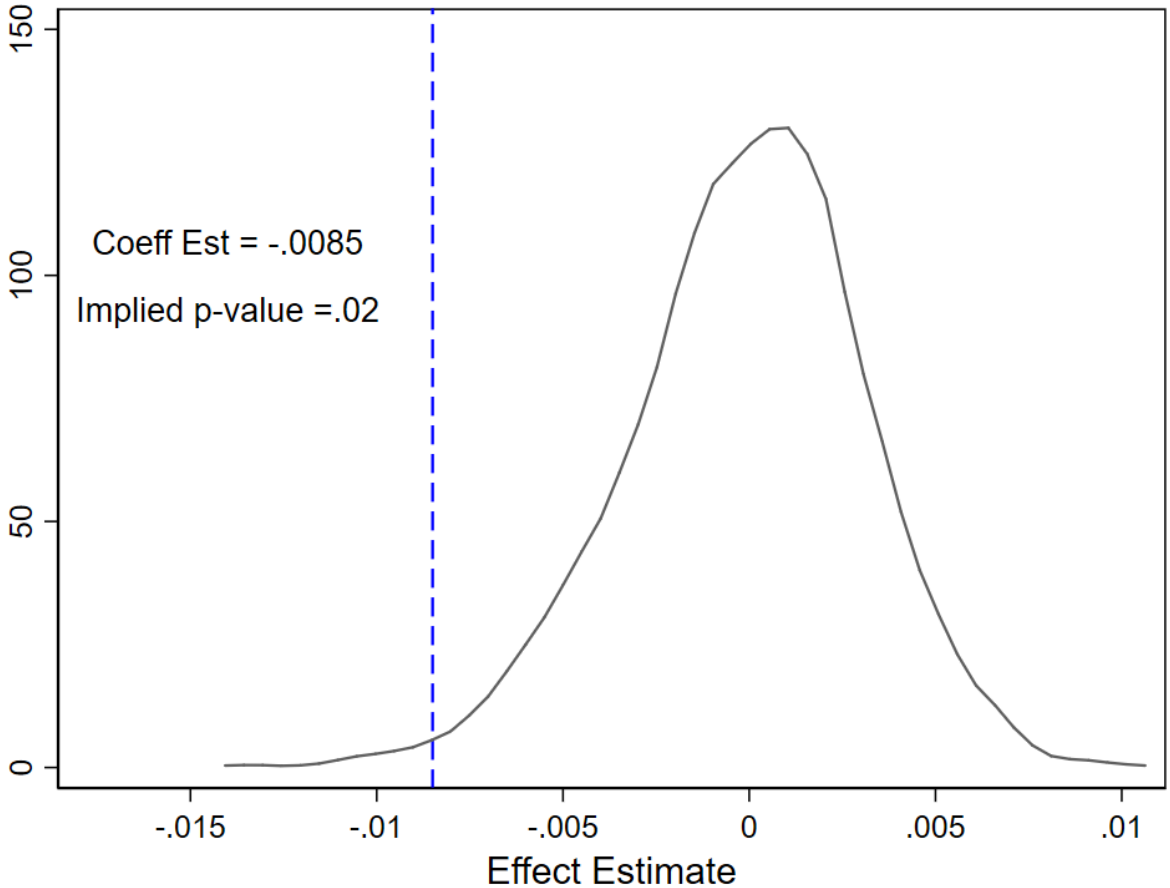
Appendix: Supplementary Figures

Figure A1: Randomization Inference, All Part 1 Crimes



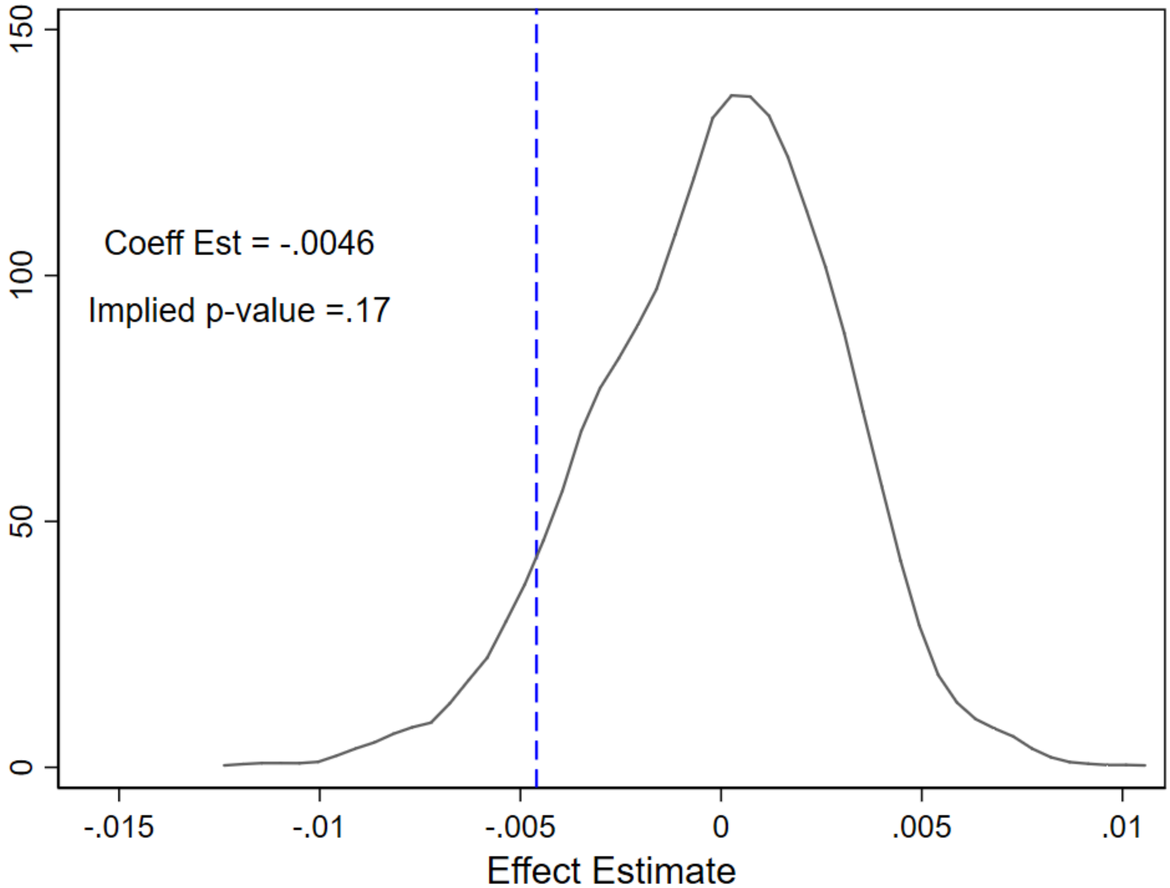
Note: Figure shows the kernel density of coefficient estimates under random assignment of Head Start availability to high poverty counties. 1000 repetitions were performed. The vertical line indicates the coefficient estimate obtained using the actual rollout of Head Start (See Table A2). A two-tailed test statistic is calculated as the share of estimates whose absolute value is greater than or equal to the estimate obtained using the actual rollout. Calculating this statistic gives an implied p-value of .049 as compared with the p-value of .038 given by the standard errors clustered at the county level.

Figure A2: Randomization Inference, Part 1 Property Crimes



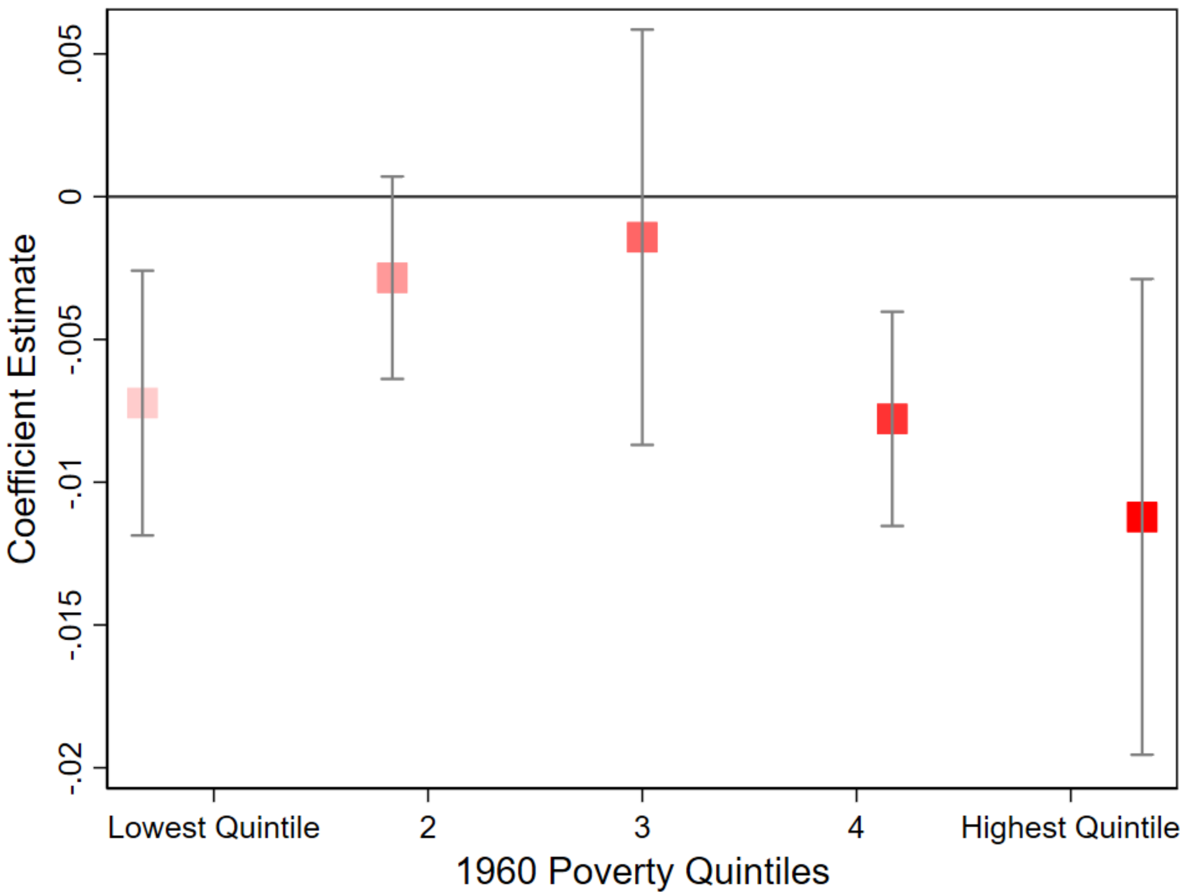
Note: Figure shows the kernel density of coefficient estimates under random assignment of Head Start exposure to high poverty counties. 1000 repetitions were performed. The vertical line indicates the coefficient estimate obtained using the actual rollout of Head Start (See Table A4). A two-tailed test statistic is calculated as the share of estimates whose absolute value is greater than or equal to the estimate obtained using the actual rollout. Calculating this statistic gives an implied p-value of .02 as compared with the p-value of .007 given by the standard errors clustered at the county level.

Figure A3: Randomization Inference, Part 1 Violent Crimes



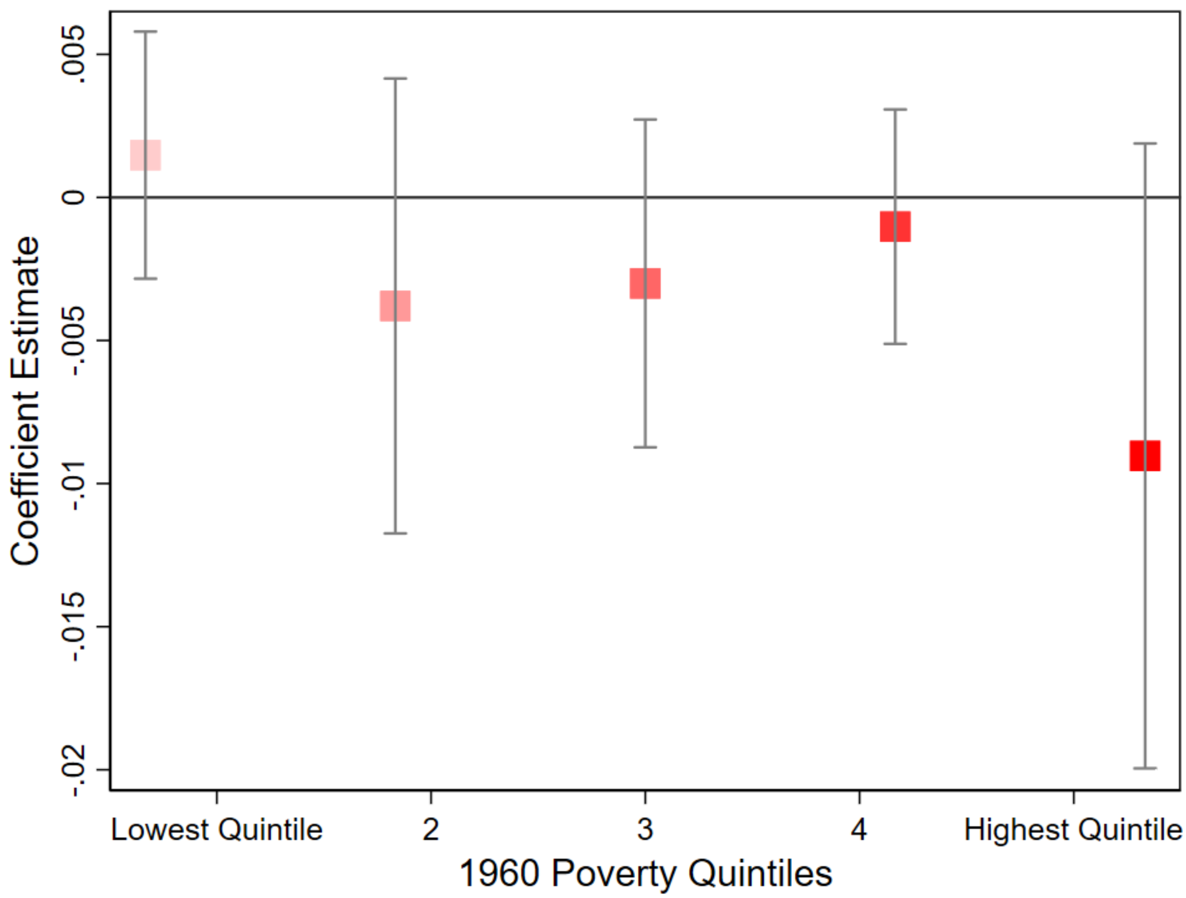
Note: Figure shows the kernel density of coefficient estimates under random assignment of Head Start exposure to high poverty counties. 1000 repetitions were performed. The vertical line indicates the coefficient estimate obtained using the actual rollout of Head Start (See Table A4). A two-tailed test statistic is calculated as the share of estimates whose absolute value is greater than or equal to the estimate obtained using the actual rollout. Calculating this statistic gives an implied p-value of .17 as compared with the p-value of .15 given by the standard errors clustered at the county level.

Figure A4: DD Estimates by Quintiles, Property Crimes



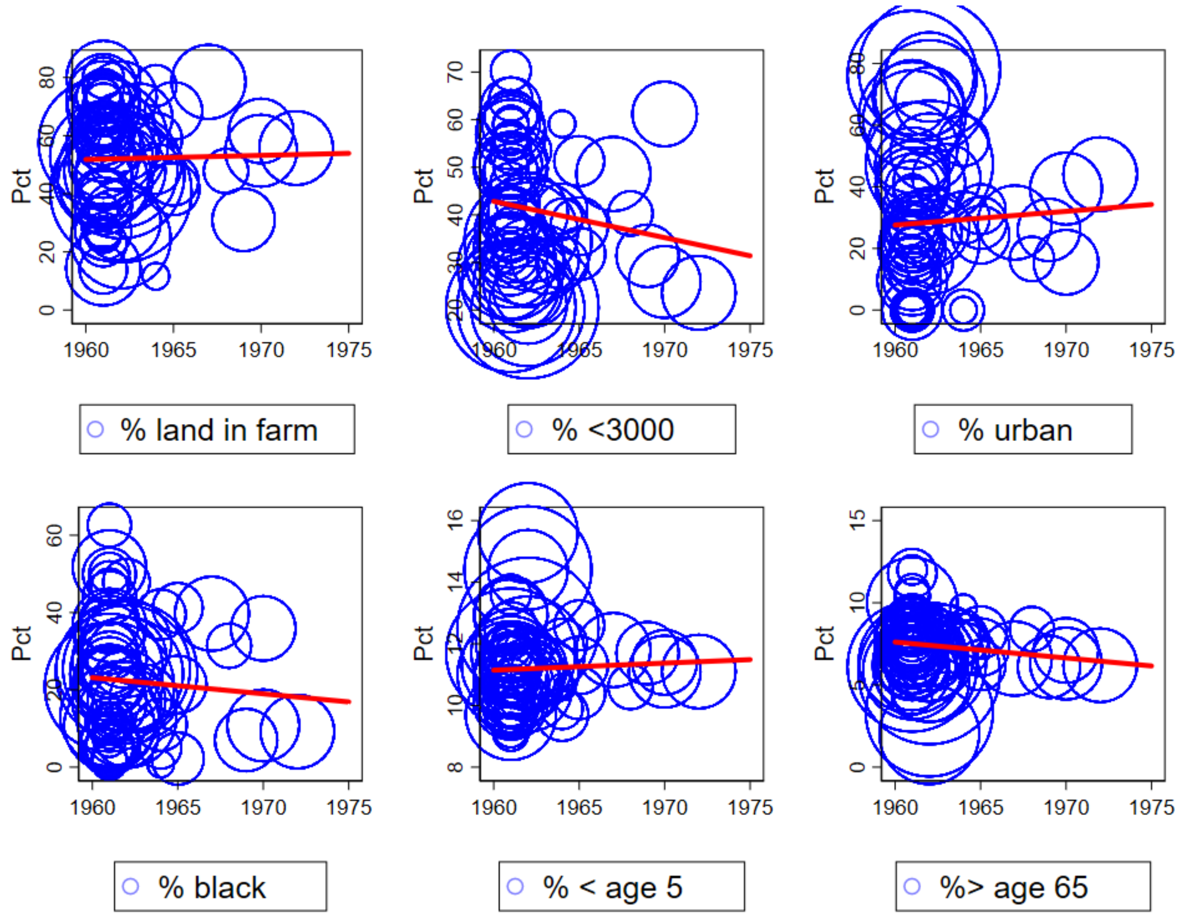
Note: Figure shows the coefficient estimates and 95% confidence intervals from estimating our basic difference-in-differences specification separately for counties in each quintile of the 1960 North Carolina poverty rate. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 property crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words “murder”, “assault”, or “robbery” (rape not being included), and property crimes (those in which the description of the offense contains the words “burglary” or “larceny”). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A5: DD Estimates by Quintiles, Violent Crimes



Note: Figure shows the coefficient estimates and 95% confidence intervals from estimating our basic difference-in-differences specification separately for counties in each quintile of the 1960 North Carolina poverty rate. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 violent crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words “murder”, “assault”, or “robbery” (rape not being included), and property crimes (those in which the description of the offense contains the words “burglary” or “larceny”). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A6: Exploring Endogeneity of Head Start Adoption



Note: Figure shows population weighted scatterplots of county characteristics against the year in which Head Start first became available in that county. Data are at the county level and weights are defined using 1955 births (represented by circle radius). A flat, horizontal fitted line suggests that the values of a given county characteristic are not systematically connected to the timing of Head Start availability.

Appendix: Supplementary Tables

Table A1: Effect of Head Start Availability on Rate of Criminal Conviction by Age 35 - Robustness of High Poverty Estimates to Inclusion of Counties that Did Not Receive Head Start

	Main		Nearest Neighbor Matched	
	(1)	(2)	(3)	(4)
Head Start Availability	-0.0131**	-0.0131**	-0.0096***	-0.0079***
	(0.0057)	(0.0059)	(0.0032)	(0.0040)
Observations	308	308	434	434
Mean	0.0469	0.0469	0.0433	0.0433
Baseline Chars x Trend		X		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words “murder”, “assault”, or “robbery” (rape not being included), and property crimes (those in which the description of the offense contains the words “burglary” or “larceny”). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty), the “High Poverty" counties. In the first two columns, the sample is restricted to counties that ever received Head Start between 1965 and 1976. In the second two columns, the sample includes counties that ever received Head Start between 1965 and 1976 as well as a nearest neighbor matched county for each of these counties, matched on the logit estimates of the propensity of receiving Head Start in this period based on the 1960 county characteristics mentioned above. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A2: Head Start Availability and Serious Criminal Conviction - Continuous Measure of Poverty Estimates

	All	
	(1)	(2)
HS Exposure	0.0059	0.0044
	(0.0058)	(0.0041)
HS Exposure X Poverty	-0.0202*	-0.0188**
	(0.0111)	(0.0078)
Observations	882	882
Mean	0.0476	0.0476
Baseline Chars X Trend		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort interacted with the county poverty rate in 1960. (The reported estimates are also scaled up by a factor of 100.). All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. These regressions do not restrict the sample based on the county poverty rate in 1960. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A3: Other War On Poverty Programs and Head Start - High Poverty Counties

	High Poverty				
	(1)	(2)	(3)	(4)	(5)
Head Start Availability	-0.0131**	-0.0126**	-0.0126**	-0.0158**	-0.0153**
	(0.0059)	(0.0058)	(0.0059)	(0.0068)	(0.0061)
Observations	308	308	308	308	308
Mean	0.0469	0.0469	0.0469	0.0469	0.0469
Baseline Chars X Trend	X		X		X
WOP Controls	None	FS	FS	FS + Other WOP	FS + Other WOP

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample includes only high poverty counties. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. In these specifications, controls for exposure to various War on Poverty programs, including the Food Stamp Program (FS) are also included. "Other War on Poverty Programs" are those recommended by (71) and include per capita expenditures on Public Assistance Transfers, Medicaid expenditures, Community Health Centers and Community Action Agencies. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A4: Other War On Poverty Programs and Head Start - Low Poverty Counties

	Low Poverty				
	(1)	(2)	(3)	(4)	(5)
Head Start Availability	0.0012	0.0026	0.0012	0.0013	0.0012
	(0.0040)	(0.0033)	(0.0040)	(0.0028)	(0.0033)
Observations	574	574	574	574	574
Mean	0.0478	0.0478	0.0478	0.0478	0.0478
Baseline Chars X Trend	X		X		X
WOP	None	FS	FS	FS + Other WOP	FS + Other WOP

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included)) and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample includes only low poverty counties. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. In these specifications, controls for exposure to various War on Poverty programs, including the Food Stamp Program (FS) are also included. "Other War on Poverty Programs" are those recommended by (71) and include per capita expenditures on Public Assistance Transfers, Medicaid expenditures, Community Health Centers and Community Action Agencies. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A5: Effect of Head Start Availability on Rate of Serious Violent Criminal Conviction - By Race

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: White						
Head Start Availability	-0.0014	-0.0016	-0.0029	-0.0029	-0.0003	-0.0000
	(0.0016)	(0.0017)	(0.0023)	(0.0024)	(0.0022)	(0.0023)
Observations	667	667	252	252	415	415
Mean	0.0114	0.0114	0.0107	0.0107	0.0116	0.0116
Panel B: Non-White						
Head Start Availability	0.0041	0.0028	-0.0047	-0.0052	0.0074	0.0086
	(0.0037)	(0.0041)	(0.0049)	(0.0052)	(0.0052)	(0.0070)
Observations	667	667	252	252	415	415
Mean	0.0503	0.0503	0.0354	0.0354	0.0548	0.0548
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of white or non-white individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 violent crime in North Carolina by age 35. UCR Part 1 violent crimes are those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included). Panel A presents these results for white cohorts, while Panel B reports them for non-white cohorts. Sample sizes are smaller for these specifications because the natality files for 25% of counties in North Carolina do not have race breakdowns before 1969, we do not know the race of approximately 13% of births in our sample. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2%) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A6: Exploring Endogeneity of Head Start Availability

	All	High Poverty	Low Poverty
	(1)	(2)	(3)
Head Start Ever Available In County			
1960 CCDB: % of land in farming	0.00353	0.00782	0.00110
	(0.0177)	(0.0290)	(0.0265)
1960 CCDB: % of people living in families with $\leq$ \$3000	-0.0503	0.0939	-0.381***
	(0.0369)	(0.0945)	(0.123)
1960 CCDB: % of population urban	-0.0297	-0.00259	-0.0712
	(0.0283)	(0.0427)	(0.0621)
1960 CCDB: % of people black	0.00244	0.0233	0.00175
	(0.0259)	(0.0272)	(0.0472)
1960 CCDB: % of people $\leq$ age 5	-0.364	-0.766	0.696
	(0.404)	(0.488)	(0.678)
1960 CCDB: % of people $\geq$ age 65	-0.112	-0.568	1.137
	(0.337)	(0.423)	(1.014)
1960 CCDB: % of employment in agriculture	-9.870	-25.45	16.11
	(14.23)	(20.82)	(20.80)
1960 CCBD: log population	1.720**	0.988	4.548*
	(0.717)	(0.791)	(2.478)
Observations	100	50	50
Mean	0.630	0.440	0.820

Note: Each column reports a separate logistic regression of an indicator for whether a county ever got Head Start by 1976 against the eight county level characteristics recommended in Hoynes and Schanzenbach (2009) and drawn from the 1960 City and County Data Books (CCDB). Observations are at the county level. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A7: Exploring Endogeneity of the Timing of Head Start Availability

	All	High Poverty	Low Poverty
	(1)	(2)	(3)
First Birth Cohort in County To Have Head Start			
1960 CCDB: % of land in farming	0.00735	-0.0396	0.00827
	(0.0234)	(0.0658)	(0.0330)
1960 CCDB: % of people living in families with $\leq$ \$3000	-0.0375	0.0158	-0.138
	(0.0473)	(0.168)	(0.105)
1960 CCDB: % of population urban	-0.0122	-0.00119	-0.000130
	(0.0235)	(0.0358)	(0.0607)
1960 CCDB: % of people black	0.00251	0.0215	-0.0117
	(0.0328)	(0.0756)	(0.111)
1960 CCDB: % of people $\leq$age 5	-0.198	-0.187	0.115
	(0.387)	(1.374)	(0.582)
1960 CCDB: % of people $\geq$age 65	-0.418	-0.358	-0.350
	(0.259)	(0.898)	(0.266)
1960 CCDB: % of employment in agriculture	-4.219	-7.168	1.668
	(13.35)	(34.97)	(19.64)
1960 CCBD: log population	-0.397	1.003	-1.444
	(0.749)	(1.483)	(1.224)
Observations	63	22	41
Mean	0.381	0.0455	0.561

Note: Each column reports a separate OLS regression of the birth year (normalized to 1962) of the first birth cohort in a given county to which Head Start was available against the eight county level characteristics recommended in Hoynes and Schanzenbach (2009) and drawn from the 1960 City and County Data Books (CCDB). Observations are at the county level. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Significance levels indicated by: *$(p < 0.10)$, **$(p < 0.05)$, ***$(p < 0.01)$

Table A8: Relationship between Head Start Availability and Possible Confounders

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: War on Povery Programs						
0-5 Food Stamp Exposure	-0.0429	-0.0429	0.0666	0.0666	-0.0639	-0.0639
	(0.0802)	(0.0802)	(0.1593)	(0.1593)	(0.0950)	(0.0950)
Public Assistance Transfers	6.9467	6.9467	0.7701	0.7701	11.5153**	11.5153**
	(5.7513)	(5.7513)	(6.7353)	(6.7353)	(4.5238)	(4.5238)
Medicaid	9.2430	9.2430	2.2806	2.2806	12.8272**	12.8272**
	(5.7438)	(5.7438)	(5.9320)	(5.9320)	(5.9322)	(5.9322)
Community Health Center Funds	681.9056	681.9056	-82.5776	-82.5776	916.6069	916.6069
	(521.5891)	(521.5891)	(502.3024)	(502.3024)	(720.6964)	(720.6964)
CAP Seniors Program Grant	0.0356	0.0356	0.0297	0.0297	0.0302	0.0302
	(0.0521)	(0.0521)	(0.0407)	(0.0407)	(0.0734)	(0.0734)
Legal Services Program Grant	0.0460	0.0460	-0.0013	-0.0013	0.0557	0.0557
	(0.0333)	(0.0333)	(0.0080)	(0.0080)	(0.0434)	(0.0434)
Panel B: Health						
Adjusted Mortality Rate, All Ages	0.8698	0.8698	19.5563	19.5563	-5.3087	-5.3087
	(9.0852)	(9.0852)	(17.5338)	(17.5338)	(9.3156)	(9.3156)
White, Infant Mortality Rate	0.4678	0.4678	0.2843	0.2843	0.1544	0.1544
	(0.8910)	(0.8910)	(1.3270)	(1.3270)	(1.4135)	(1.4135)
Nonwhite Infant Mortality Rate	-2.2424	-2.2424	-3.6512	-3.6512	-1.4567	-1.4567
	(2.3849)	(2.3849)	(3.8859)	(3.8859)	(3.2687)	(3.2687)
Infant Mortality Rate	-1.1731	-1.1731	-1.1761	-1.1761	-1.5512	-1.5512
	(0.9761)	(0.9761)	(1.1773)	(1.1773)	(1.0473)	(1.0473)
Neonatal Infant Mortality Rate	0.2524	0.2524	0.1171	0.1171	0.1792	0.1792
	(0.8686)	(0.8686)	(1.0237)	(1.0237)	(1.1750)	(1.1750)
Postneonatal Infant Mortality Rate	-1.4255*	-1.4255*	-1.2931	-1.2931	-1.7304**	-1.7304**
	(0.7816)	(0.7816)	(1.3091)	(1.3091)	(0.6643)	(0.6643)
Observations	882	882	308	308	574	574
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. In each row the dependent variable is a county-year measure of spending or infant health that could potentially confound our estimates of the impact of Head Start. All dependent variables are taken from Bailey et al (2017). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2%) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that received Head Start between 1965 and 1976. All samples are further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A9: Head Start Availability and Serious Criminal Conviction: Includes Birth-county by Birth-year Trends

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Head Start Availability	-0.0018	-0.0056*	-0.0131**	-0.0132*	0.0026	-0.0022
	(0.0031)	(0.0031)	(0.0057)	(0.0073)	(0.0032)	(0.0032)
Observations	882	882	308	308	574	574
Mean	0.0476	0.0476	0.0469	0.0469	0.0478	0.0478
Birth-county X Birth-year Trend		X		X		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words “murder”, “assault”, or “robbery” (rape not being included), and property crimes (those in which the description of the offense contains the words “burglary” or “larceny”). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 birth-county by birth-year trends. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2%) are called “High Poverty", while those below the median are called “Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A10: Head Start and Likelihood of Residing in One's State of Birth (Census)

	National				South			
	All		Men Only		All		Men Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction with HS Avail.	-0.021	0.004	-0.017	0.009	-0.018	-0.009	-0.013	-0.005
	(0.016)	(0.008)	(0.017)	(0.008)	(0.029)	(0.020)	(0.029)	(0.021)
Obs	3,150,292	3,150,292	1,546,355	1,546,355	1,002,875	1,002,875	487,059	487,059
Mean	0.66	0.66	0.66	0.66	0.68	0.68	0.68	0.68
State Linear Trend		X		X		X		X

Note: Each cell represents a separate OLS regression with standard errors clustered at the state of birth level (in parentheses). Observations are at the individual level from the 1990 and 2000 Census. The dependent variable is whether an individual is currently living in his or her state of birth. The key explanatory variables are measures of Head Start availability for a birth cohort in a particular state. This is the weighted average of the Head Start availability variable across counties in a state, where the weights are the number of births in each county in 1960. All specifications include birth state and birth year fixed effects as well as indicators for race, age, and sex. Sample restricted to ages 18-35. Significance levels indicated by: *$(p < 0.10)$, **$(p < 0.05)$, ***$(p < 0.01)$.

4. SUMMARY AND CONCLUSION

This dissertation spans several subfields of applied microeconomics, including public, health and urban economics. The dissertation is concerned with identifying the long-run effects of large, transformational Federal policies. In particular, it shows how increases in access to credit markets, early childhood education and medical care can influence the course of a person's life.

In each chapter, the dissertation identifies an effect of a transformational policy that was almost surely *not* intended by policy makers. There is no reason to believe that the Roosevelt administration was deliberately trying to shift the burden of crime onto Black and Hispanic residents for three quarters of a century; "redlining" was intended to bolster a failing housing market and incentivize lending. The Affordable Care Act was *inter alia* trying to *decrease* deaths from substance abuse, not increase opioid deaths; the legalization of recreational Marijuana was *not* designed to combat opioid deaths, but to turn back the "War on Drugs". Head Start was *not* intended to be a crime deterrent, but an attempt to bridge educational and developmental gaps already observable at the earliest stages of elementary school.

Redlining, the Affordable Care Act and Head Start were each bold policies, shaped by the vision of policy makers who strove to alleviate some pressing social ill: lack of access to homeownership, lack of access to medical care and lack of access to early childhood education, respectively. Indeed, it is clear that these policies have increased overall access to homeownership, medical care and early childhood education. These policies were also, as this dissertation shows, transformational in ways that were not intended. Perhaps these unintended transformations would have been difficult to anticipate for even the most thoughtful policy maker. While the existence of unforeseeable effects is not necessarily a justification for opposing the implementation of large, transformational policies, perhaps the magnitude of these unanticipated changes ought to give a policy maker pause. That policies can have such large and lasting impacts on individuals is something for policy makers and the public to wonder at:

ἁρμονίη ἀφανής φανερῆς κρείττων

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APPENDIX A

APPENDIX TO CHAPTER 1

Appendix: Redlining and Migration

Redline-mapping could cause both within-city migration across neighborhoods and between-city migration across cities. Understanding both types of migration is important for interpreting the reduced form impact of redlining both at the within-city and between-city level.

The estimates reported in Table A10 provide evidence that redline-mapping did not cause either significant within or between city migration in the short run.[1] If we saw evidence of differential within-city migration from the estimates in Table A10, this might suggest that the within-city effects of redlining on crime could be due largely to residents of a city sorting themselves between neighborhoods in response to the mapping. However, the estimates in columns (1)-(3) of Table A10 suggest that redline-mapping may have *decreased* within-city moves by about 6 percentage points (a 10% decrease off the mean) in the short run; columns (4)-(6) suggest redline-mapping did not affect between-cities moving rates in the short run.

It is still possible that redline-mapping is responsible for shaping long run migration patterns. For example, it could be that some of the well known "Great Migration" patterns of black residents moving away from the South were influenced by redline-mapping practices. Figure A6 and Table A4 shows regression discontinuity estimates of the possible impact of redline-mapping on present day city-level racial composition; they utilize the same identification strategy I describe in Section 2.4.1.1. The estimates show suggestive evidence that redline-mapping may have increased

[1] The short run in this case is the period of time between April 1, 1935 and the date of the 1940 Census survey. Because this is the first year the Census began to ask this migration question it is not possible to run a similar specification using the 1930 Census. Redline-mapping began in 1935 and continued through 1940. In Los Angeles, for example, city mapping occurred mainly in March of 1939 while the 1940 decennial Census surveys were given out so as to be reflective of conditions April 1, 1940. While there is variation in when cities were mapped, it is reasonable to think that the migration responses of a survey in April of 1940 could pick up migration patterns in the immediate aftermath of redline-mapping.

share black and decreased share white at the city level; these estimates are consistent with an account in which some, but not all,[2] of the reduced form effect of redline-mapping on crime is due to between-city migration and accompanying shifts in the racial composition of cities.

I am in the process of using restricted Census data which links individuals in various Census surveys to their place of birth to more definitely answer the question of whether between-city migration was affected by redline-mapping.

[2]Back of the envelope calculations show that the point estimates in Table A4 can at most explain a third of the city level crime effects

APPENDIX B

APPENDICES TO CHAPTER 2

Data Appendix

To determine which specific NDC drug product codes count as opioids in analyzing the CMS state-drug utilization data, I follow (44) in including the following products. Often the products involve combinations of opioids with other well known over the counter painkillers such as Acetaminophen or Aspirin.

List of Product Names of Opioid Products:

Acetaminophen, Caffeine, Cihydrocodeine

Buprenorphine HCl

Buprenorphine HCl and Naloxone HCl Dihydrate

Butalbital, Acetaminophen, Caffeine, Codeine

Butalbital, Aspirin, Caffeine, Codeine

Butorphanol Tatrtrate

Fentanyl

Hydrocodone with Acetaminophen

Hydrocodone with Ibuprofen

Hydromorphone HCl

Meperidine HCl

Methadone HCl

Morphine Sulfate

Morphine-Naltrexone

Nalbuphine HCl

Oxycodone HCl

Oxycodone with Acetaminophen

Oxycodone with Aspirin

Oxycodone with Ibuprofen

Oxymorphone HCl

Pentazocine with Naloxone

Pentazocine with Acetaminophen

Propoxyphene HCl

Propoxyphene HCl with Acetaminohen

Propoxyphene Napsylate with Acetaminophen

Tapentadol HCl

Tramadol HCl

Tramadol HCl with Acetaminophen

(44) find that the vast majority of prescriptions are for a small subset of the above products. The percentages gives are the percent of claims for opioids the authors find in medical claims taken from a 20% random sample of Medicare beneficiaries in 2010.

List of Often Prescribed Opioid Products:

Hydrocodone with acetaminophen (paracetamol) (42.9% of all claims)

Oxycodone with acetaminophen (11.6%)

Tramadol (11.9%)

Oxycodone (7.4%)

Morphine sulfate (4.5%)

Fentanyl (4.2%)

Appendix: (Supply Side Robustness) Results Restricted to States on Margin of Medicaid Expansion

To further address any endogeneity concerns not addressed by examining the pre-trends and pre-period “effect” estimates, I also consider a restricted sample I call the “overlapping sample”. This sample is obtained by first estimating Equation 2.3, capturing the predicted values by state and then plotting the distribution of these predicated values by expansion status. I then consider the distribution of these predicted values separately for expander and non-expander states. I use the separate distributions for expander and non-expander states to construct a sample of states where the two distributions overlap, and name this sample the “overlapping sample”. By construction, this sample includes only those states whose estimates of expansion likelihood was numerically comparable to the expansion likelihood of another state whose expansion status was different. In other words, the “overlapping sample” contains only expander states whose expansion likelihood was numerically equal to or less than the expansion likelihood of some state that failed to expand, and contains only non-expander states whose expansion likelihood was numerically equal to or greater than the expansion likelihood of some state that expanded. This sample of states ought to be free from any sort of state-level selection concerns such as the concern that less Republican, more wealthy states who expanded could have differed in unobservable ways from more Republican, less wealthy states who failed to expand and that these unobservable differences are somehow driving the difference in difference estimates.[1] I report key estimates using only the “overlapping sample” in Appendix Figures A5 and Appendix Table A2. These estimates are consistent with, and slightly larger in magnitude than, those that include all states; that the point estimates are consistent but slightly larger further corroborates the causal interpretation of the difference in difference estimations for the whole sample.

Figures A3 and A4 show calendar year trends in opioid-related deaths for the “overlapping

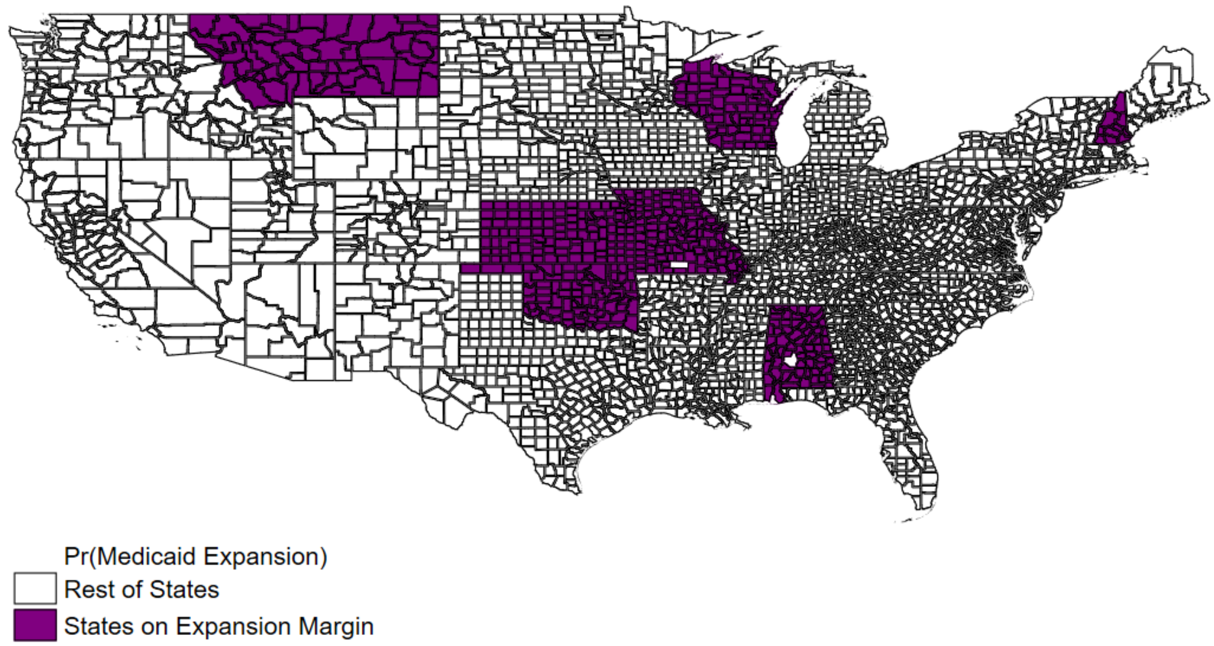
[1]Note that for such an endogeneity story to be successful the timing of the impact of these unobserved factors would also have to correlate with the timing of Medicaid expansion and its staggered rollout.

sample". We can see that prior to the expansion starting in January of 2014, expander and non-expander states in this sample tracked each other well (Figures A3); they also tracked each other well in the pre-period when restricted by key demographic breakdowns (Figure A4.)

Table A2 contains the results of the reduced form regressions (Equation 3.4 and Equation 2.2 restricted to the "overlapping sample", which includes only those states on the margin of Medicaid expansion. I find again that Medicaid expansion is associated with statistically significant increases in opioid-related mortality; the point estimates from the "overlapping sample" are similar in magnitude to those from the overall sample, but the "overlapping sample" estimates represent a much larger increase off the mean than the estimates from the overall sample. Indeed, the OLS estimates suggest that Medicaid expansion is associated with approximately .4 additional opioid-related deaths per county-month (columns (1) of Table A2) compared to the estimate of .3 additional deaths, obtained using the overall sample (columns (1) of Table A3). However, the estimates obtained using the "overlapping sample" represent a much larger increase off the mean than those obtained using the overall sample: non-linear estimates using the "overlapping sample" (columns (3)-(4) of Table A2) suggest that Medicaid expansion is associated with an increase in opioid-related deaths of approximately 50%, while the OLS estimate (column (1) of Table A2) suggests that Medicaid expansion is associated with nearly a doubling in the volume of opioid-related deaths. Estimates using the overall sample suggest an increase off the mean that ranges from 6% to 36% (Table A3). Thus, the estimates using the "overlapping sample" suggest that, for the states on the margin of expansion, there was a pronounced increase in opioid-related mortality comparable in size to that which occurred in any given state but considerably larger in relation to its mean.

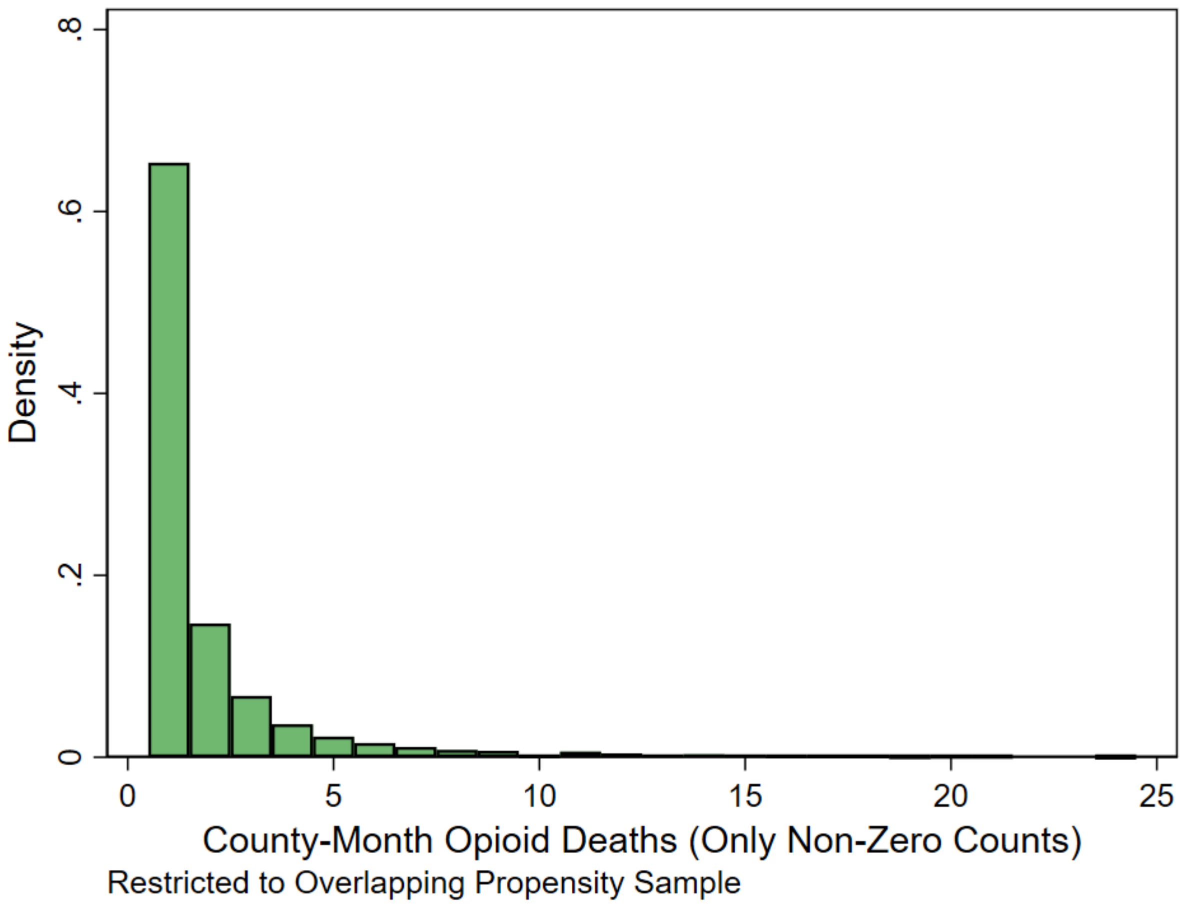
Appendix: Tables and Figures Restricted to States on Margin of Medicaid Expansion

Figure A1: Distribution of Propensity Estimates: Map



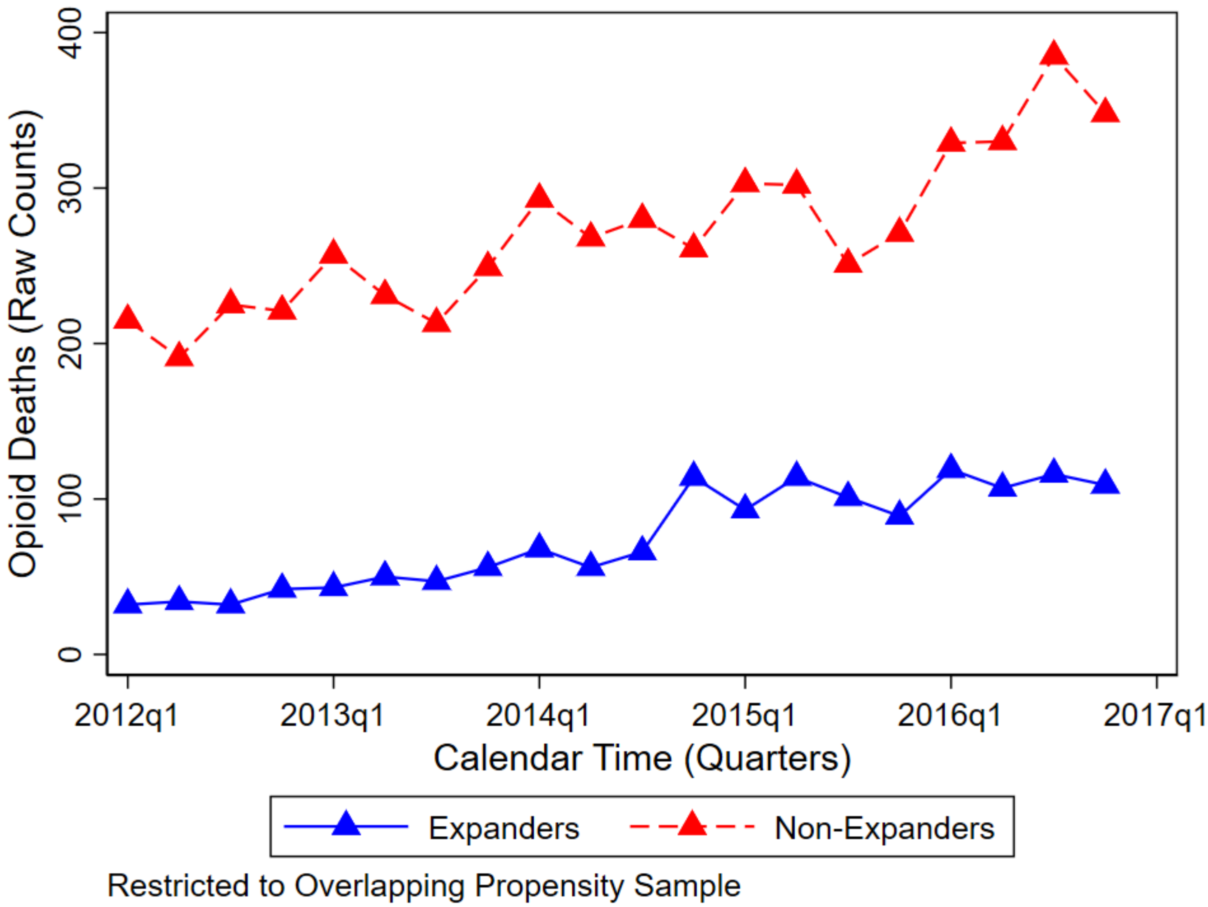
Note: Figure shows the "Overlapping" sample or States "on the margin of Expansion". The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state.

Figure A2: Distribution of Opioid-Related Deaths



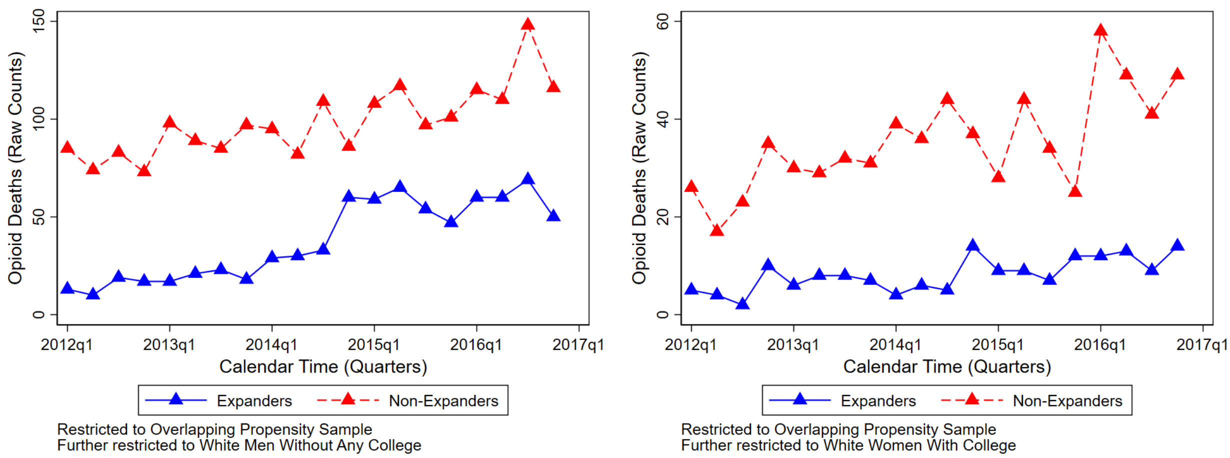
Note: The Figure shows the county-month level distribution of opioid-related deaths. The sample is restricted to county-month cells with at least one opioid related death. The sample is further restricted to states on the margin of expanding Medicaid (the "overlapping" sample). The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. The source is CDC Individual-Level Mortality Files from 2012-2016.

Figure A3: Medicaid Expansion and Opioid-Related Deaths: Overlapping Sample



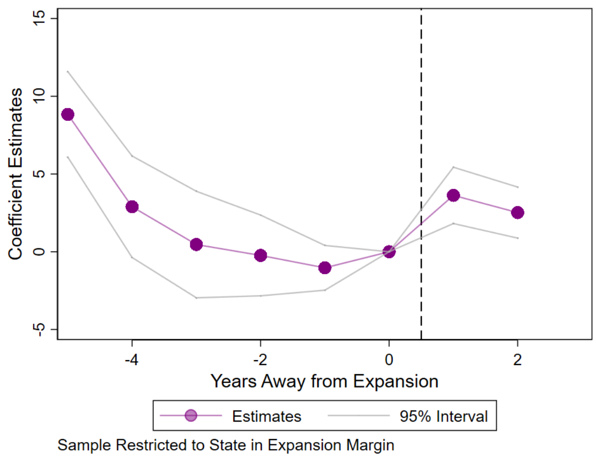
Note: Figure shows trends in opioid-related deaths. Trends are shown separately for states that expanded Medicaid and those that failed to expand. The sample includes all demographics. The sample is restricted to states on the margin of expanding Medicaid (the "overlapping" sample). The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A4: Medicaid Expansion and Opioid-Related Deaths: Overlapping Sample, Heterogeneity

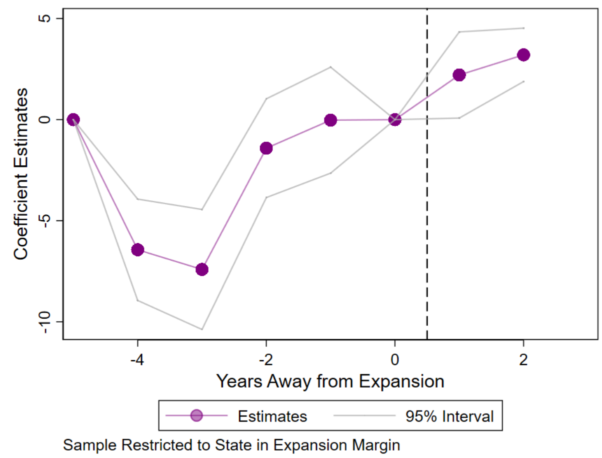


Note: Figure shows trends in opioid-related deaths. Trends are shown separately for states that expanded Medicaid and those that failed to expand. The sample is restricted to states on the margin of expanding Medicaid (the "overlapping" sample). The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. The sample is further restricted to white men without any college attendance and white women with college attendance respectively. The source is CDC Individual-Level Mortality Files (2012-2016).

Figure A5: Impact of Medicaid Expansion on Overall Opioid Prescriptions (CDC Data): Overlapping Sample



(a) Opioid Prescription Rate



(b) Annual Change in Opioid Prescription Rate

Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2.2 with opioid prescriptions per 100 people and the annual change in opioid prescription per 100 people, respectively, as the outcomes. The sample is restricted to states on the margin of expanding Medicaid (the "overlapping" sample). The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. Source is CDC opioid prescription rates reported at the county-year level (2010-2016).

Table A1: Predicted Propensities of State Level Expansion: Treatment Contrast Overlap

	Overall		
	Expander	Non-Expander	Δ
Pr(Expansion)	.623	.306	.317***
	Overlap Sample		
	Expander	Non-Expander	Δ
Pr(Expansion)	.433	.418	.014***

Note: Table shows differences (Δ) in predicted likelihood of expansion across all states in the top panel and restricted to the "overlapping" sample in the bottom panel. When we restrict to the "overlapping" sample, states which expanded have expansion probabilities comparable to those states who did not expand. The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the 'overlapping' sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state.

Table A2: Impact of Medicaid Expansion on Opioid-Related Deaths: Overlapping Sample

	(1) Count	(2) Log	(3) Poisson	(4) NB
Medicaid Expansion	0.396	0.092	0.554***	0.553***
	(0.580)	(0.143)	(0.133)	(0.063)
Observations	18356	18356	14774	14774
Mean	0.376	.376	.445	.445

Note: Each column reports estimates from a separate regression with standard errors reported in parentheses. Observations are at the county-month level. The reported coefficient of interest is an indicator for whether the county is in a state which expanded Medicaid. In column (1) the count of opioid-related deaths is the dependent variable in an OLS estimation of equation 3.4; column (2) is also an OLS estimation of equation 3.4 but with the log of opioid-related deaths as the dependent variable. Columns (3) and (4) are obtained from poisson and negative binomial estimates of equation 3.4, respectively, both using the count of opioid-related deaths as the dependent variable. All specifications include county fixed effects as well as calendar month and year fixed effects. Standard errors are clustered at the state level in the OLS estimates reported in columns (1) and (2), and heteroskedasticity robust in columns (3) and (4). In the poisson and negative binomial specifications, I drop counties with all zero counts resulting in a reduction in sample size. (Table A15 shows the distribution of counties with all zero counts) The sample is restricted to states on the margin of expanding Medicaid (the "overlapping" sample). The expansion margin is computed by capturing the predicted values from equation 2.3 and determining the distribution of resulting predicted values for expanders and non-expanders separately. States are in the "overlapping" sample just in case the state expanded Medicaid and its predicted likelihood of expansion is less than the predicted likelihood of some non-expanding state or the state failed to expand Medicaid and its predicted likelihood of expansion is greater than the predicted likelihood of some expanding state. The source is CDC Individual-Level Mortality Data (2012-2016). $^{*}\ p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$