Online Appendix to

Investing in Health and Public Safety: Childhood Medicaid Eligibility and Later Life Criminal Behavior

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I. LEVELS AND TRENDS IN CRIME RATES

Descriptive statistics for the crime rates for the states in our sample are reported in Table A1. Average annual rates of violent crime for the cohorts range from 16 per 10,000 for females to 84 per 10,000 for males, with the largest source of violent crime stemming from aggravated assault. Males in the sample also have substantially higher rates of property crime, drug crime, and DUI.

Figure A1 illustrates trends in the birth cohorts' crime rates across time. Arrests for violent and property crimes generally trended downward across the birth cohorts, while arrest rates for drug crimes and DUI were higher for cohorts born in the early 1980s than for their counterparts born in the late 1970s or later 1980s. Figure A2 separates the trends in the cohorts' crime rates by the size of their states' Medicaid expansions. Differences in trends in DUI rates are particularly stark in the quartile of states with the largest Medicaid expansions relative to the quartile with the smallest expansions, as the DUI rate across birth cohorts grew more and declined less over time for cohorts in small expansion states than for those in large expansion states. The differences in trends for the other crime rates are less obvious at this level of aggregation. However, violent crime rates fell more across birth cohorts in large versus small expansion states (from roughly 65 to 50 and from 50 to 40 per 10,000, respectively). Property crime rates also appear to have fallen more sharply across cohorts in large versus small expansion states.

II. SUMMARY STATISTICS AND COEFFICIENTS ON CONTROL VARIABLES

Tables A1-A3 present summary statistics for crime rates, eligibility variables and controls. Table A4 presents the estimated coefficients on the control variables included in the baseline regression. Among the maternal controls, only the estimated coefficient on the proportion of births to nonwhites is consistently significant. The estimates indicate positive relationships between births to nonwhites and violent and property crime, and negative relationships between births to nonwhites and DUI. Among the childhood controls, none of the estimated coefficients is consistently significant.

III. HETEROGENOUS TREATMENT EFFECTS BY RACE

The UCR reports counts of arrests separately by race, but not by race and age or race and sex. Instead, counts by race are only separately available for juveniles and for adults (of any age). Thus, although UCR data limitations preclude identifying separate effects by race for crime among 19-24 year olds, we are able to estimate separate impacts for blacks and whites by computing childhood Medicaid eligibility separately by race and assigning the adult crime rate to each birth cohort in the year they turn 19.[1] Note that this substantially limits the amount of variation across which we can identify effects because rather than linking each cohort to their age-specific crime rate in each year, we can use only one crime rate (the crime rate for all adults) for each birth cohort as the dependent variable. Thus, the estimated coefficients in Table A7 are based only on state-birth cohort variation, and the smaller sample of 420 observations represents 35 states*12 birth cohorts (rather than 35*12*6=2,520 observations in the baseline).

IV. ADDITIONAL ROBUSTNESS CHECKS

In addition to testing the robustness of our estimates to different assumptions made when computing the crime rates, we conduct an array of checks to examine the sensitivity of the estimates to including different sets of state-level trend variables, including additional controls, including fewer controls, using a different formulation of the Medicaid eligibility variable, and using alternative functional forms and levels of data aggregation. We also explore whether policies aimed at reducing lead exposure impact our estimates. Overall, we find that our results are robust to a wide array of alternatives.

A. Alternative Treatment of Time Trends. Table A8 presents results that illustrate the insensitivity of the estimates to alternative formulations of state-specific time trends. The baseline results (reported in Panel A for comparison purposes) include state, year, and age fixed effects as well as state-specific linear time trends that begin at each cohort's year of birth (e.g., 1976 for the 1976 cohort, 1977 for the 1977 birth cohort, and so on). Panel B presents estimates that exclude time trends. Panel C reports estimates that include quadratic state-specific time trends. Panel D presents estimates that include state-specific linear trends that begin with the first year of crime outcomes reported for each cohort (e.g., 1995 for the 1976 birth cohort, 1996 for the 1977 birth cohort, and so on), while the estimates in Panel E include both sets of state-specific linear time trends. The point estimates are generally smaller, but still statistically significant, when we exclude state-specific trends. Including state-specific birth year and outcome year trends generates estimates that are very similar to the baseline results. Finally, Panel F reports estimates from regressions that exclude time trends but include region-year fixed effects. Again, the estimates are qualitatively similar to the baseline.

B. Alternative Functional Forms, Controls, Standard Error Clustering, and Construction of the Simulated Eligibility Instrument. In Table A9, we present estimates that utilize alternate functional forms and levels of clustering for the standard errors, as well as an alternate construction of the simulated eligibility instrument. Given the relatively small number of clusters (states) in our models, we supplement our baseline estimates to include wild cluster bootstrapped t-statistics

(Cameron and Miller, 2015). These t-statistics, presented brackets in Panel A, do not generate differences in the statistical significance of our estimated coefficients. In Panel B, we test for sensitivity to clustering the standard errors at the state-birth cohort level to account for policy variation across cohorts, and find that it does not substantively affect the statistical significance of the estimates.

In Panel C, we present estimates while alternating the control variables included in the regressions. We first estimate equation (3) while including only a minimal set of control variables (state, year, age fixed effects and trends), and then estimate the regression while also alternately adding the maternal and childhood controls. We also add controls for state-level differences in the economic, social, and legal environments during each year of the cohorts' early adulthood (unemployment rates, poverty rates, rates of alcohol consumption, police officers per capita, whether the state had a HIFA waiver program, and controls for firearm and marijuana policies).[2] The estimated effects are consistent with the baseline estimates in all of these specifications.

In Panel D, we present estimates derived from an alternative construction of the simulated eligibility instrument. Rather than using a fixed-in-time (1996) random national CPS sample when constructing the eligibility measure, we instead construct the measure using random national samples generated in each year. The results are insensitive to this alternate construction.

Finally, in Panel E we report estimates using alternative functional forms to allow for potential non-linear relationships between eligibility and crime. The estimates are again consistent with those in the baseline in indicating a negative relationship between years of eligibility and later life crime. Although the point estimates for violent and property crime in the quadratic formulation are generally consistent with eligibility having a diminishing marginal effect, the estimated coefficients on the quadratic terms are statistically insignificant. The log-log formulation indicates that the elasticity of crime rates with respect to increases in Medicaid eligibility range from -0.04 for violent crime to -0.11 for drug crime.

C. Alternative Levels of Aggregation. Our baseline estimates measure the average impact of expanded Medicaid eligibility on crime rates across six years, one for each age 19-24 for each of the birth cohorts. For example, the 1980 birth cohort is linked to crime rates for 19-year-olds in 1999, to crime rates for 20-year-olds in 2000, and so on through 2004 (when they are 24). The 1981 birth cohort is linked to crime rates for 19-year-olds in 2000, 20-year-olds in 2001, and so on for each of the birth cohorts. This allows us to identify effects from crime rates that vary at the state-birth cohort-age level, and to capture variation in crime across each age of each cohorts' early adulthood.

An alternative would be to estimate impacts at the more aggregated state-birth cohort level. Rather than linking each cohort to their age-specific crime rate in each year, this would instead use age-aggregated crime rates that are averaged across ages 19-24 for each birth cohort as the dependent variable. Thus, each cohort would be linked to one crime rate (the average rate over ages 19-24) rather than to six crime rates (each corresponding to the year that the cohort turns age 19, 20, and so on). Table A10 presents the estimated coefficients based only on state-birth cohort variation (the sample of 420 represents 35 states*12 birth cohorts). Although estimated less precisely (which is not surprising given that at this level of aggregation there is less variation in the crime rates for each birth cohort), the estimated impacts are consistently negative and generally similar to the baseline.

Table A11 provides an illustration of the variation lost when aggregating crime rates across ages. In Table A10, the dependent variable is the average crime rate across ages 19-24 for each cohort. Each row of Table A11 presents estimates from a separate regression of Equation 3 in the text, replacing the dependent variable with the crime rate for each age. Note that this also reduces the sample size to 420 since we only observe one crime rate for each of the 12 birth cohorts in the 35 states in the sample, whereas in our baseline estimates, we observe six crime rates for each birth cohort in each state. The estimates indicate that Medicaid eligibility has different impacts on crime at different ages. For example, additional eligibility generates the largest and most significant drops in property crime among males ages 19-20 and has the most significant impact on DUI crime for 20-year-old males.

D. Lead Exposure Policy Controls. Exposure to lead has been linked to higher rates of crime, particularly violent crime and homicide (Reyes 2007; Feigenbaum and Muller 2016). The 1970 Clean Air Act generated dramatic reductions in the amount of lead in gasoline over 1975-1985, a period that overlaps with some of our birth cohorts' childhoods and implies that later birth cohorts in our sample were exposed to relatively lower rates of environmental lead. If states that were early or more aggressive adopters of lead-reducing policies were also states with greater expansions in childhood Medicaid eligibility, failure to control for lead exposure could bias our estimated impacts of Medicaid expansions away from zero.

We explore whether policies aimed at reducing lead exposure impact our estimates in the Table A12. As noted by Reyes (2007), leaded gasoline was one of the primary sources of lead exposure in the early 1970s (the other was leaded paint), and distribution patterns of gasoline across the U.S. generated differences in the amount of lead per gallon of gasoline across states. We combine EPA threshold standards for leaded gasoline across time with archived *Petroleum Marketing Annual* reports from the US Department of Energy (which provide sales volume data for various grades of gasoline by state and year) to estimate the amount of exposure to lead from gasoline by state for the years 1983-1987.[3]

In Panel A, we directly examine whether states with larger reductions in lead exposure were also states with greater levels of childhood Medicaid eligibility using a simple regression of eligibility on a measure of lead exposure from gasoline (plus state and year fixed effects). Our estimates indicate no significant relationship between lead exposure and Medicaid eligibility.

In Panel B, we present estimates using a restricted sample that includes only the 1983-1987 birth cohorts. In this heavily restricted "lead exposure sample," the estimated effects are no longer statistically significant for females, with the exception of a positive estimated impact on DUI.[4] For males, the estimated impacts on property crime and drug crime are negative but smaller in this subsample. When we include controls for lead exposure in the regression, the point estimates are similar in size and significance, leading us to conclude that while we cannot rule out that failure to control for lead exposure in our full sample is biasing the estimates, it is unlikely to be a primary driver of our results, particularly for the estimated impacts among males and on non-violent crimes.

V. QUASI-DIFFERENCE-IN-DIFFERENCE ANALYSIS

The incremental nature of the Medicaid eligibility expansions over time precludes performing a standard difference-in-difference (DD) analysis because there are no clear "pre-" and "post-" policy periods. However, as shown in Figure 1, there was a relatively large jump in eligibility resulting from the Deficit Reduction Act of 1984. For young children born after September 30, 1983, the Act eliminated the single-parent family structure requirement for Medicaid eligibility and extended eligibility to children in low-income families who were previously ineligible because of their family structure (e.g., two-parent households) even though their household income made them otherwise eligible.

We exploit this jump in eligibility to conduct a quasi-DD analysis that compares pre- and post-1984 birth cohort crime outcomes in the quartile of states with the largest increases in eligibility over 1976-1987 to similar changes across in the quartile of states with the smallest increases in eligibility over 1976-1987. Although this analysis generates attenuated estimates relative to the baseline (e.g., those in the small-expansion "comparison group" states receive some treatment), it allows for standard DD robustness checks, such as testing for pre-expansion trends.

The DD estimates are reported in Table A13, where we present results with and without including state-specific time trends.[5] The DD estimates' signs are consistent with those of the baseline specification (although they are generally smaller and less significant) regardless of whether state-year trends are included in the model. Although it is an imperfect comparison, we find the negative point estimates' consistency with the baseline results reassuring.

VI. EXPANDED DISCUSSION AND ANALYSIS OF UCR DATA

Our data on criminal outcomes come from the FBI's Uniform Crime Reporting (UCR) system for the years 1995 to 2011, available from the Inter-University Consortium for Political and Social Research.[6] The UCR collects self-reported arrest data from over 16,000 law enforcement agencies each year, including arrest counts by offense, sex, and individual age for those under 24, as well as a count of the total population covered by each agency. We aggregate the agency-level arrest counts to obtain the number of arrests at the state level for each offense, sex, and age. To compute arrest rates, we aggregate the agency-level population covered values to the state level to obtain the total state population covered by the UCR data. We then multiply the state UCR population counts by the proportion of the state's population in each age-sex group in each year, computed using the Surveillance, Epidemiology, and End Results (SEER) Program (2017).

Specifically, for each offense O we first compute the total number of arrests by age a, sex x, state s, and year t using Equation A1:

$$\text{(A1) } Total\ O\ Arrests_{a,x,s,t} = \sum_j O\ Arrests_{a,x,j,t}$$

Where j denotes each law enforcement agency reporting in the UCR in the state. Thus, for each offense O and for each state and year, Equation A1 computes the number of 19-year-old females arrested for O, the number of 19-year-old males arrested for O, the number of 20-year-old females arrested for O, number of 20-year-old males arrested for O, and so on through 24-year-old females and 24-year-old males.

To obtain arrest *rates* based on these arrest counts, we divide $Total\ O\ Arrests_{a,x,s,t}$ by $UCR\ Population_{a,x,s,t}$, the size of the state's population covered by the UCR that is sex x and age a, which we obtain using Equation A2:

$$\text{(A2) } UCR\ Population_{a,x,s,t} = \sum_j Population\ Covered_{j,t} * (Population_{a,x,s,t} / Total\ Population_{s,t})$$

Equation A2 computes the number of people age a and sex x covered by the UCR in each state s and year t by aggregating the population covered by each reporting law enforcement agency j to the state level to obtain the total state population (of any age or sex) covered by the UCR data. The variable $Population_{a,x,s,t}$ is the number of people of age a and sex x in each state and year, while $Total\ Population_{s,t}$ is the total population state s in year t. These population values are obtained from the Surveillance, Epidemiology, and End Results (SEER) Program at the National Cancer Institute.

Finally, we compute the arrest rate for each age, sex, state, and year using Equation A3:

$$\text{(A3)}\ \textit{Arrest Rate}_{a,x,s,t} = (\textit{Total Arrests}_{a,x,s,t} / \textit{UCR population}_{a,x,s,t})$$

One issue in working with UCR data is that law enforcement agencies may drop in and out of reporting over time (that is, not every agency reports in every year), and when they do report, agencies may not report for all crimes. Because not every agency reports in every year, the proportion of each state's population that is covered by the UCR data varies across states and time. To the extent that the drop-in-and-out and incomplete reporting is correlated with prior changes in Medicaid eligibility and crime, it could potentially impact our estimates.[7] We examine this possibility in several ways.

First, we regressed the crime rate and Medicaid eligibility variables on the proportion of each state's population covered by the UCR in each year. Figure A3 presents a histogram of the average UCR coverage rates over time for the states in our baseline sample. Coverage varies widely across states. The modal coverage level is just over 0.4 and no state reaches 100 percent coverage across time. The estimates from the regression of Medicaid eligibility and crime on these UCR coverage rates are presented in Table A14. The coefficients are all statistically insignificant when state and year fixed effects are included, indicating no consistent relationships among the UCR coverage rate and Medicaid eligibility or crime.

Second, we estimated a regression to assess whether the size of states' Medicaid expansions is correlated with whether a given agency is observed in our sample using Equation A4.

$$\text{(A4)}\ \Pr(\textit{In Sample}_{j,s}) = f(\beta_0 + \beta_1\ \%\Delta \textit{Eligibility}_{s,t} + \ + \delta_s + \gamma_y + e_{i,s,y})$$

The variable *In Sample*$_{j,s,y}$ is an indicator equal to one if agency *j* in state *s* reported to the UCR in all years (that is, the agency's data is in our baseline sample), and $\%\Delta Eligibility_{s,t}$ is the state-level annual growth rate in the percent of children eligible for Medicaid (averaged across all cohorts). The estimated coefficient on $\%\Delta Eligibility_{s,y}$ is small (0.0004) and statistically insignificant (p-value = 0.97), suggesting that the rate of growth in eligibility is not strongly related to whether an agency is in the baseline sample.[8]

Finally, we address this issue in several ways when we estimate Equations 2 and 3 in the main paper. First, we drop KY and MT, two states with very low average population coverage rates (0.04 and 0.19, respectively), from the analysis. Second, we weight our regressions by the proportion of each state's population covered by the UCR. Third, in our baseline estimates, we restrict the UCR data to include only agencies that report crime counts in all of our crime outcome years (1995-2011). Because 16 states had no law enforcement agencies that reported in every year, our sample includes 35 states.[9] For each of these states, we observe crime outcomes for 12 birth cohorts (1976-1987) over six years of age (one for each age 19-24), resulting in 2,520 state-cohort-age observations.

If an agency reported on some but not all crimes in a given year, we made the simplifying assumption that the agency did not report on crimes because they did not occur, so the crime count for unreported crimes was zero. Because it is possible that this assumption generated erroneously small crime rates for some types of crimes, in the baseline estimates we replaced outlier crime rates (i.e., those that were more than 1.5 times their interquartile range above the third or below the first quartile values) with their inverse distance weight predicted values.

In Table A15, we examine the impact of changes in these various assumptions. Panel A repeats the baseline estimates for comparison purposes, while Panel B presents unweighted estimates and Panel C includes the states' UCR coverage rates as a regressor. Both the unweighted estimates and those that include the UCR coverage rate in the model are very similar to the baseline.

Panel D presents estimates based on crime rates that are computed using agencies that reported in at least 75% of the outcome years (that is, in at least 14 of 18 years) and interpolating their crime counts for missing agency-years. Using the less restrictive reporting requirement expands the sample size from 2,520 to 3,312 because 46 states had agencies that reported in at least 75% of the years. The estimated effects using the larger set of reporting agencies and states are very similar to the baseline estimates.

In Panels E-G, we restrict the sample to include only states with at least 40%, 60%, and 80% of population coverage in the UCR data (that is, states incrementally further to the right in Figure A1). Although the sample sizes naturally get smaller as we impose these restrictions, the estimates are consistent with the baseline across all three sub-samples.

Finally, we examine the impact of replacing reporting agency-year-level missing crime counts with zeros and replacing outliers with their predicted values in two ways. First, in Panel H, we include the outlier values of crime rates rather than replacing them with predicted values. Second, in Panel I, we exclude outlier values of crime rates (which generates changes in sample size across the crime outcomes). In both instances, the estimates are very similar to the baseline.

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[1] We estimate childhood Medicaid eligibility by race using the same process described in Section IV of the main paper, but applied to race-specific CPS samples. For example, we estimate childhood Medicaid eligibility for blacks by restricting the CPS random sample to include black children only and then comparing each individual child's state, year of birth, family structure, etc.

to the relevant state-year income thresholds for Medicaid eligibility. We then repeat the exercise using a white only CPS sample to estimate Medicaid eligibility for whites.

[2] Summary statistics for these variables are reported in Table A3. We do not include the adulthood controls in our baseline specifications because they themselves are affected by Medicaid expansions and thus endogenous. Unemployment rate data are from Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics Series. Poverty rates are from the US Census Bureau. Alcohol consumption data are from National Institutes of Health (Haughwout and Slater, 2018). HIFA waiver data is from Atherly et al. (2012). Police officers per capita are from the UCR. Firearm data are from the Rand State Firearm Law Database (Cherney, Morral, and Schell 2018). Marijuana policy data are from the National Alliance for Model State Drug Laws (2017).

[3] Unfortunately, we have been unable to find consistent data on the amount of lead in gasoline across states and time for the full set of years of childhood for our 1976-1987 birth cohorts, making an ideal test of the impact of including controls for lead exposure infeasible.

[4] These estimates are available from the authors.

[5] We also estimated an event-study model, regressing crime rates on an indicator for "large expansion" states interacted with indicators for each birth year (excluding 1984), the childhood, maternal, and adulthood controls, and state, birth year, and age fixed effects. The estimates do not indicate systematic pre-1984 differences between "large expansion" and other states.

[6] Our identification strategy utilizes yearly variation in crime rates by age. The UCR reports arrest counts by *individual* age for those up to age 24, but only counts by *age groups* for those over 24. Thus, we utilize UCR data for 1995-2011 as it encompasses the year the 1976 birth cohort turned 19 through the year the 1987 birth cohort turned 24.

[7] As noted by Barr and Smith (2017) in their examination of the impact of the Food Stamp Program on later life crime, in order for measurement error in the UCR to explain our results, states that expanded Medicaid in the 1980s (when our cohorts were children) would need to underreport arrests 19-24 years later (when the cohorts were young adults). In addition, because our estimation strategy utilizes arrest rates for different birth cohorts in the same state and year, but at different ages, the underreporting would also have to impact reporting of arrests differently across individual age groups in the same year.

[8] When Equation A4 is estimated without including state and year fixed effects, the coefficient on $\%\Delta Eligibility_{s,t}$ is negative (-0.03) and significant (p-value = 0.00). Using the average annual growth rate in eligibility (0.03) implies that agencies in states that experience average annual eligibility growth are only 0.0009 (0.03*0.03) less likely to be in the baseline sample. Alternatively, agencies in states with the largest growth in eligibility (0.75) are only 0.02 less likely to be in the sample, supporting the assumption that agency reporting differences are not driving our results.

[9] The excluded states are AL, DE, DC, FL, GA, IL, KS, KY, LA, MS, MT, NH, NM, NY, VT, and WI.

Table A1: Crime Rate Summary Statistics

	Total		Males		Females	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Violent Crime	51.1	28.2	84.2	46.5	16.2	10.8
Murder	1.7	1.8	3.0	3.4	0.2	0.4
Rape	2.5	1.7	4.9	3.3	0.0	0.1
Robbery	11.5	9.4	20.4	17.2	2.1	1.9
Assault	35.1	19.8	55.3	30.7	13.7	9.6
Property Crime	146.8	66.9	199.7	99.9	90.6	39.1
Burglary	26.3	14.3	45.4	25.1	6.0	5.1
Larceny	106.8	51.3	131.8	70.5	80.2	36.7
Motor Vehicle Theft	12.5	11.1	20.6	18.5	3.7	4.4
Arson	0.8	0.8	1.3	1.4	0.2	0.3
Drug Crime	168.5	78.2	276.4	136.6	54.5	23.9
Drug Sale	33.9	28.6	57.3	52.4	9.4	5.9
Drug Possession	134.3	60.3	219.2	101.9	45.0	21.4
DUI	127.7	67.0	201.3	100.8	49.5	34.4

Number of observations is 2,520. Crime rates are computed as counts per 10,000 of the age-sex group population by state and year and are weighted by the percent of each state's population covered by the UCR data. Reported crime rates are averaged over ages 19-24 for each birth cohort born during 1976-1987.

Table A2: Medicaid Eligibility Summary Statistics

	Total		Male		Female	
Eligibility by Sex	**Mean**	**St. Dev.**	**Mean**	**St. Dev.**	**Mean**	**St. Dev.**
Actual Eligibility (ages 0-18)	3.5	1.7	3.4	1.7	3.6	1.7
Actual Eligibility (ages 0-5)	1.0	0.5	1.0	0.5	1.0	0.5
Actual Eligibility (ages 6-11)	1.0	0.6	0.9	0.6	1.0	0.6
Actual Eligibility (ages 12-18)	1.6	0.9	1.5	0.9	1.6	0.8
Simulated Eligibility (ages 0-18)	4.0	1.5	3.8	1.5	4.1	1.5
Simulated Eligibility (ages 0-5)	1.3	0.5	1.2	0.5	1.3	0.5
Simulated Eligibility (ages 6-11)	1.2	0.6	1.1	0.6	1.2	0.6
Simulated Eligibility (ages 12-18)	1.5	0.7	1.4	0.7	1.6	0.7
	Total		**White**		**Black**	
Eligibility by Race	**Mean**	**St. Dev.**	**Mean**	**St. Dev.**	**Mean**	**St. Dev.**
Actual Eligibility (ages 0-18)	3.5	1.7	3.0	1.7	6.3	3.4
Actual Eligibility (ages 0-5)	1.0	0.5	0.8	0.5	2.1	1.2
Actual Eligibility (ages 6-11)	1.0	0.6	0.8	0.6	1.8	1.1
Actual Eligibility (ages 12-18)	1.6	0.9	1.4	0.8	2.4	1.5
Simulated Eligibility (ages 0-18)	4.0	1.5	3.3	1.4	7.7	2.1
Simulated Eligibility (ages 0-5)	1.3	0.5	1.0	0.5	2.8	0.8
Simulated Eligibility (ages 6-11)	1.2	0.6	1.0	0.6	2.3	0.9
Simulated Eligibility (ages 12-18)	1.5	0.7	1.3	0.7	2.6	0.9

Number of observations is 2,520. Eligibility is measured in years. Medicaid eligibility by sex (race) is estimated using the process described in Section IV of the main paper, but applied to sex-specific (race-specific) CPS samples.

Table A3: Control Variable Summary Statistics

	Mean	St. Dev.
Maternal Controls		
Births to nonwhites (proportion)	0.2	0.1
Births to teenage mothers (proportion)	0.1	0.0
Births to single mothers (proportion)	0.2	0.1
No prenatal care in first trimester (proportion)	0.2	0.1
Childhood Controls		
State EITC credit (as % of Federal EITC)	0.1	0.7
Real welfare eligibility threshold ($)	548.5	182.4
Real maximum welfare payment ($)	726.5	184.9
Unemployment rate (%)	5.9	1.3
Adulthood Controls*		
Poverty rate (%)	11.4	2.7
Unemployment rate (%)	5.0	1.7
Per capita alcohol consumption (gallons of ethanol/year from all sources)	2.3	0.4
Police officers (#/10,000 population)	18.0	3.0
Concealed carry weapon law (0/1)	0.7	0.5
Medical marijuana allowed in state (0/1)	0.2	0.4
Marijuana decriminalized in state (0/1)	0.2	0.4
State implemented HIFA waiver (0/1)	0.1	0.3
State passed but did not implement HIFA waiver (0/1)	0.0	0.1

Number of observations is 2,520. Maternal controls are measured during each birth cohort's year of birth (1976-1987). Childhood controls are averaged across each cohort's years of childhood (1976-2005). Adulthood controls are measured at each age of each cohort's early adulthood (1995-2011).

* Regressions that include adulthood controls are reported in Table A9.

Table A4: Estimated Coefficients on Control Variables

	Violent	Property	Drug	DUI
Maternal Controls				
Births to nonwhites (proportion)	131.93***	311.62*	193.37	-143.48*
	(37.56)	(158.58)	(208.94)	(71.86)
Births to teenage mothers (proportion)	-30.14	25.94	183.47	-175.86
	(70.47)	(182.40)	(252.26)	(200.91)
Births to single mothers (proportion)	-3.42	-188.04	-256.18*	-4.34
	(40.16)	(122.23)	(128.75)	(136.27)
No prenatal care in first trimester (proportion)	19.20	-7.51	17.04	33.53
	(17.22)	(48.41)	(85.35)	(46.50)
Childhood Controls				
State EITC credit (as % of Federal EITC)	-0.58	-0.26	-2.63	-3.33
	(0.91)	(1.39)	(3.30)	(2.16)
Real welfare eligibility threshold ($)	-0.01	-0.15	0.09	0.05
	(0.04)	(0.12)	(0.11)	(0.09)
Real maximum welfare payment ($)	-0.03	0.08	-0.08	-0.04
	(0.02)	(0.06)	(0.08)	(0.08)
Unemployment rate (%)	2.49	-12.04*	-6.62	-6.36
	(2.11)	(6.18)	(11.95)	(4.66)

Number of observations is 2,520. Maternal controls are measured during each birth cohort's year of birth (1976-1987). Childhood controls are averaged across each cohort's years of childhood (1976-2005). Columns report estimates from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and also include state, year, and age fixed effects, linear state-birth year trends. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A5 - Treatment Effects on Disaggregated Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: Violent Crimes				Panel B: Property Crimes				Panel C: Drug Crimes	
Both Sexes	**Murder**	**Rape**	**Robbery**	**Assault**	**Burglary**	**Larceny**	**MV Theft**	**Arson**	**Sale**	**Possession**
Eligibility	0.04	0.37**	-0.27	-1.06*	-1.19**	-7.77***	-0.92*	-0.04	-3.52***	-5.77***
	(0.10)	(0.14)	(0.27)	(0.60)	(0.55)	(1.97)	(0.51)	(0.08)	(0.94)	(2.11)
% change	2.3	14.1	-2.3	-2.9	-4.4	-6.9	-7.3	-4.7	-10.7	-4.4
Dep. Var. Mean	1.7	2.6	12.0	36.6	27.0	112.5	12.5	0.8	32.8	132.3
Males										
Eligibility	0.05	0.66***	-0.81*	-2.01*	-2.60***	-13.99***	-1.68*	-0.09	-6.29***	-11.98***
	(0.18)	(0.24)	(0.43)	(1.06)	(0.86)	(2.91)	(0.84)	(0.14)	(1.71)	(3.72)
% change	1.6	13.0	-3.8	-3.5	-5.6	-10.1	-8.1	-6.4	-11.3	-5.5
Dep. Var. Mean	3.1	5.1	21.3	57.6	46.7	138.7	20.8	1.4	55.5	215.9
Females										
Eligibility	0.01	0.01	-0.05	-0.02	-0.12	-1.96	-0.16	0.04	-0.49	-0.26
	(0.03)	(0.01)	(0.13)	(0.27)	(0.44)	(1.53)	(0.27)	(0.03)	(0.40)	(0.94)
% change	4.7	114.9	-2.2	-0.1	-2.0	-2.3	-4.4	26.0	-5.3	-0.6
Dep. Var. Mean	0.2	0.0	2.2	14.4	6.1	84.7	3.6	0.2	9.2	44.5

Number of observations is 2,520. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, linear state-birth year trends, and the control variables listed in Table 1. The % change rows report the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A6 -Treatment Effects By Age of Eligibility

	(1) First Stage	(2) Violent	(3) Property	(4) Drug	(5) DUI
Females					
Eligibility ages 0-5	0.87***	-0.59	1.46	-0.58	0.05
	(0.01)	(0.78)	(4.06)	(3.25)	(2.65)
% change		-3.6	1.6	-1.1	0.1
Eligibility ages 6-11	0.83***	0.81	-5.94	-1.03	-0.72
	(0.02)	(0.67)	(3.66)	(2.32)	(1.73)
% change		5.0	-6.6	-1.9	-1.5
Eligibility ages 12-18	0.31***	-2.12	-10.94	-5.53	-2.42
	(0.02)	(1.50)	(6.51)	(4.54)	(4.46)
% change		-13.1	-12.1	-10.1	-4.9

Number of observations is 2,520. Column (1) reports estimates of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, linear state-birth year trends, and the control variables listed in Table 1. *Eligibility ages 0-5*, *Eligibility ages 6-11*, and *Eligibility ages 12-18* measure cumulative eligibility for each of these sub-periods of childhood. The % change rows report the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A7 - Treatment Effects by Race

	(1)	(2)	(3)	(4)	(5)
Panel A: Crime Rates and Eligibility Among Whites	**First Stage**	**Violent**	**Property**	**Drug**	**DUI**
Eligibility	0.58***	0.20	-0.58	-1.86	-1.14
	(0.07)	(0.41)	(1.23)	(1.41)	(1.58)
% change		1.4	-1.2	-4.4	-1.8
Dep. Var. Mean		14.11	47.68	42.38	62.97
Panel B: Crime Rates and Eligibility Among Blacks					
Eligibility	0.55***	-4.66	-14.51**	-8.75	3.31
	(0.14)	(4.17)	(6.51)	(7.35)	(3.79)
% change		-4.8	7.2	-4.2	3.6
Dep. Var. Mean		97.38	202.54	209.94	91.42

Number of observations is 420. Crime rates are averaged over adulthood for each race-birth cohort. Column (1) reports estimates of α_1 from Equation 2 in the text, but with eligibility measured separately for whites and blacks. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state and year fixed effects, linear state-birth year trends, and the control variables listed in Table 1. The % change rows report the (coefficient/dep. var. mean)*100. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A8 - Changes in Treatment of Time Trends

	(1) Violent	(2) Property	(3) Drug	(4) DUI
Panel A: Baseline (linear state-birth year trends)				
Eligibility	-1.12	-9.86***	-9.13***	-4.96*
	(0.75)	(2.52)	(2.37)	(2.64)
Panel B: No trend				
Eligibility	0.59	-3.29	-6.86*	-6.81**
	(1.30)	(2.67)	(3.43)	(2.79)
Panel C: Quadratic state-birth year trends				
Eligibility	-1.22	-3.52*	-1.86	-0.87
	(0.89)	(1.78)	(1.80)	(2.24)
Panel D: Linear state-outcome year trends				
Eligibility	-0.86	-1.53	-6.05	-2.43
	(1.13)	(4.86)	(6.56)	(3.06)
Panel E: Linear state-outcome and state-birth year trends				
Eligibility	-1.12	-9.86***	-9.13***	-4.96*
	(0.76)	(2.54)	(2.38)	(2.66)
Panel F: Region*year fixed effects				
Eligibility	0.42	-3.22	-8.04**	-6.64**
	(1.22)	(2.69)	(3.05)	(3.16)

Number of observations is 2,520. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, and the control variables listed in Table 1. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A9 - Other Robustness Checks

	(1) First Stage	(2) Violent	(3) Property	(4) Drug	(5) DUI
Panel A: Baseline					
Eligibility	0.74***	-1.12	-9.86***	-9.13***	-4.96*
	(0.02)	(0.75)	(2.52)	(2.37)	(2.64)
[wild cluster bootstrap t-stat.]		[-1.49]	[-3.07]	[-2.61]	[-2.12]
Panel B: Cluster st. err. at birth cohort-state level					
Eligibility		-1.12	-9.86***	-9.13***	-4.96**
		(0.73)	(1.92)	(2.56)	(1.92)
Panel C: Alternate sets of control variables					
state, year, age fixed effects, trends					
Eligibility	0.89***	-2.15***	-12.89***	-10.34***	-5.32***
	(0.02)	(0.59)	(1.46)	(2.21)	(1.75)
add childhood controls					
Eligibility	0.78***	-2.50***	-12.82***	-9.83***	-4.86***
	(0.02)	(0.62)	(1.45)	(2.40)	(1.67)
add maternal controls					
Eligibility	0.87***	-2.03***	-13.06***	-10.52***	-5.46***
	(0.02)	(0.57)	(1.45)	(2.19)	(1.81)
add adulthood controls to baseline					
Eligibility	0.74***	-1.03	-9.79***	-9.27***	-4.54*
	(0.02)	(0.70)	(2.50)	(2.39)	(2.66)
Panel D: Simulated eligibility from yearly national sample					
Eligibility	0.82***	-0.55	-9.54***	-7.73***	-5.06***
	(0.01)	(0.64)	(1.73)	(2.27)	(1.57)
Panel E: Alternative functional forms					
Quadratic eligibility					
Eligibility		-2.61	-11.13**	-7.52	-2.87
		(2.33)	(4.48)	(6.57)	(7.74)
Eligibility^2		0.20	0.17	-0.22	-0.28
		(0.28)	(0.38)	(0.76)	(0.76)
[F-stat]		[1.43]	[28.48]	[6.21]	[13.52]
Log-Log		-0.04	-0.05	-0.11*	-0.00
		(0.08)	(0.06)	(0.06)	(0.08)

Number of observations is 2,520. Column (1) reports the estimate of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors are reported in parentheses. Wild cluster bootstrapped t-statistics are presented in brackets in Panel A. With the exception of Panel B and the standard errors in brackets in Panel A, standard errors are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, linear state-birth year trends, and (with the exception of Panel C) the control variables listed in Table 1. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A10 - Aggregated Estimates

	(1)	(2)	(3)	(4)
	Violent	**Property**	**Drug**	**DUI**
Eligibility (RF)	-0.27	-3.43	-6.74*	-2.15
	(1.16)	(2.71)	(3.78)	(4.20)
Eligibility (IV)	-0.52	-6.65	-13.06*	-4.18
	(2.24)	(5.26)	(7.33)	(8.15)

Number of observations is 420. Crime rates are averaged over ages 19-24 for each birth cohort rather than assigned to different cohorts in a given state and year depending on their age in that year. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state and year fixed effects, linear state-birthyear trends, and the control variables listed in Table 1. Eligibility (RF) estimates are from crime rate regressed on simulated eligibility. Eligibility (IV) estimates are from crime rate regressed on actual years of Medicaid eligibility instrumented by simulated eligibility. The first stage estimate (standard error) of $\alpha 1$ from Equation 2 is 0.52 (0.05). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A11 -Treatment Effects on Age-Specific Crime Rates

	Both Sexes				Males				Females			
	Violent	**Property**	**Drug**	**DUI**	**Violent**	**Property**	**Drug**	**DUI**	**Violent**	**Property**	**Drug**	**DUI**
Crime Rate, 19-year-olds	2.83	-7.94	-16.37	7.54	7.16	-16.59	-35.86**	7.83	1.19	-6.11	-5.41	5.03
	(4.12)	(9.10)	(12.87)	(7.60)	(7.96)	(15.55)	(17.57)	(8.77)	(1.33)	(5.96)	(5.85)	(3.91)
Crime Rate, 20-year-olds	1.40	-9.95	-15.70	-7.49	4.23	-18.33	-20.05	-15.80*	0.16	-5.45	0.22	3.12
	(3.82)	(7.24)	(13.57)	(4.75)	(5.99)	(11.90)	(19.57)	(8.15)	(1.79)	(5.96)	(3.97)	(3.31)
Crime Rate, 21-year-olds	-2.74	-9.95	-14.00	-14.95	-6.44*	-9.09	-23.74	-15.54	-0.47	-9.27	-6.40	-6.23
	(2.58)	(6.75)	(9.84)	(12.22)	(3.80)	(9.04)	(15.83)	(16.05)	(1.51)	(5.54)	(4.18)	(4.48)
Crime Rate, 22-year-olds	-0.87	-7.49	-5.70	-8.38	-2.03	-14.17*	-11.77	-18.40	0.27	-2.59	-0.13	-2.08
	(3.22)	(6.17)	(7.51)	(14.48)	(6.20)	(7.31)	(13.65)	(21.50)	(1.47)	(5.89)	(2.55)	(6.16)
Crime Rate, 23-year-olds	0.96	-0.17	-0.45	-2.71	-0.44	-2.17	-3.38	-10.05	0.03	1.40	0.89	3.55
	(3.72)	(5.33)	(5.75)	(15.05)	(6.09)	(7.47)	(10.46)	(23.65)	(1.82)	(4.84)	(2.99)	(6.05)
Crime Rate, 24-year-olds	-3.77	7.58	-2.61	8.44	-5.73	9.52	-7.31	16.01	-1.41	6.17	4.66	10.52**
	(2.99)	(4.64)	(6.82)	(8.39)	(5.15)	(6.54)	(11.77)	(11.15)	(2.02)	(4.14)	(3.87)	(4.56)

Number of observations is 420. Each row represents a separate regression, with the age-specific crime rate as the dependent variable. Columns report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state and year fixed effects, state-specific linear time trends, and the control variables listed in Table 1. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A12 - Control for Lead Exposure

Panel A: Eligibility and Lead Exposure	**Years of Eligibility (ages 0-18)**	**Years of Eligibility (ages 0-5)**	**Years of Eligibility (ages 6-11)**	**Years of Eligibility (ages 12-18)**
Lead exposure	0.00	0.00	0.00	-0.00***
(grams per gallon * gallons sold (10 millions))	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Controls for Lead Exposure	**Violent**	**Property**	**Drug**	**DUI**
Lead exposure sample, exclude controls for lead				
Eligibility	-0.13	-1.45	4.35	1.77
	(2.52)	(3.00)	(3.91)	(2.78)
Lead exposure sample, include controls for lead				
Eligibility	-0.19	-1.67	4.71	2.22
	(2.58)	(3.13)	(3.85)	(2.77)
Dep. Var. Mean (males)	84.2	199.7	276.4	201.3

Number of observations is 1,020. Regressions are limited to the 1983-1987 birth cohorts and outcome years 2002-2011. Standard errors, reported in parentheses, are clustered at the state level. Panel A reports coefficients from regression of years of Medicaid eligibility on lead exposure and state and year fixed effects. Panel B reports coefficients from regressions of male crime rates on years of Medicaid eligibility for a restricted sample of cohorts and states for whom lead exposure data was available (results for females are available from the authors). All models in Panel B are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, and state-specific linear time trends. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A13 - Quasi-Difference-in-Differences Analysis

	(2) **Violent**	(3) **Property**	(4) **Drug**	(5) **DUI**
Baseline Estimates	-1.12	-9.86***	-9.13***	-4.96*
	(0.75)	(2.52)	(2.37)	(2.64)
Quasi-Difference-in-Difference Estimates				
No trends				
*post 1984*large expansion state*	-0.80	-0.50	-7.32	-11.40
	(2.20)	(5.57)	(6.13)	(7.04)
State-birth year trends				
*post 1984*large expansion state*	-0.26	-2.57	0.95	-5.27
	(1.72)	(5.49)	(4.15)	(5.77)
Dep. Var. Mean	49.2	145.1	140.3	130.8

Number of observations in baseline is 2,520. Number of observations in the DD models is 1,152. Standard errors (reported in parentheses) are clustered at the state level. All models are weighted by the proportion of each state's population covered by the UCR data and include state, year, and age fixed effects, state-specific linear time trends (unless specified otherwise), and the control variables listed in Table 1. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A14 - UCR Coverage Tests

	Eligibility	Violent	Property	Drug	DUI
UCR Coverage Rate (incl. state, and year fixed effects)	-0.02	1.12	0.97	-3.02	-0.05
	(0.06)	(0.98)	(2.21)	(2.47)	(3.30)
UCR Coverage Rate	0.01	-0.23	-0.72**	0.33	-0.61
	(0.01)	(0.20)	(0.28)	(0.49)	(0.43)

Number of observations is 2,520. Standard errors (reported in parentheses) are clustered at the state level. The model in the first row includes state and year fixed effects. The model in the second row includes no other regressors. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A15 - Control for Different Treatment of Crime Data

	First Stage	Violent	Property	Drug	DUI
Panel A: Baseline					
Eligibility	0.74***	-1.12	-9.86***	-9.13***	-4.96*
	(0.02)	(0.75)	(2.52)	(2.37)	(2.64)
Dep. Var. Mean		51.1	146.8	168.5	127.7
Number of observations		2520	2520	2520	2520
Panel B: Unweighted regressions					
Eligibility	0.76***	-1.02	-10.11***	-8.33***	-5.05*
	(0.02)	(0.88)	(3.35)	(2.80)	(2.74)
Panel C: Include state's UCR population coverage rate as a regressor					
Eligibility	0.74***	-2.94	-3.15	-16.14**	-8.99
	(0.02)	(4.12)	(5.03)	(6.49)	(5.99)
Panel D: Include agencies that reported in 75% of years					
Eligibility	0.84***	-0.60	-8.51***	-6.20**	-2.44
	(0.02)	(0.62)	(2.34)	(2.44)	(2.14)
Dep. Var. Mean		50.7	147.9	161.8	129.3
Number of observations		3312	3312	3312	3312
Panel E: Include only states with at least 40% population coverage in UCR data					
Eligibility	0.72***	-0.10	-8.60***	-9.72***	-4.34
	(0.02)	(0.80)	(2.75)	(2.42)	(3.67)
Dep. Var. Mean		48.3	150.4	168.3	131.7
Number of observations		1800	1800	1800	1800
Panel F: Include only states with at least 60% population coverage in UCR data					
Eligibility	0.73***	-1.19	-10.20***	-11.14***	-5.05**
	(0.04)	(1.12)	(1.79)	(3.54)	(2.22)
Dep. Var. Mean		52.2	135.43	181.0	106.0
Number of observations		864	864	864	864
Panel G: Include only states with at least 80% population coverage in UCR data					
Eligibility	0.63***	-2.19	-10.99**	-12.84**	-3.20
	(0.04)	(2.39)	(3.05)	(4.58)	(3.87)
Dep. Var. Mean		51.7	116.9	169.8	98.8
Number of observations		504	504	504	504
Panel H: Include outlier values					
Eligibility	0.74***	-0.82	-9.95***	-8.77***	-2.86
	(0.02)	(0.83)	(2.57)	(2.53)	(2.73)
Dep. Var. Mean		51.3	146.9	168.5	128.2
Number of observations		2520	2520	2520	2520
Panel I: Exclude outlier values					
Eligibility	0.82***	-0.47	-9.48***	-8.03***	-5.14***
	(0.01)	(0.66)	(1.72)	(2.27)	(1.60)
Dep. Var. Mean		51.2	146.9	168.8	127.3
Number of observations		2483	2507	2486	2483

Column (1) reports the estimate of α_1 from Equation 2 in the text. Columns (2)-(5) report estimates of β_1 from Equation 3 in the text. Standard errors, reported in parentheses, are clustered at the state level. All models include state, year, and age fixed effects, state-specific linear time trends, and the control variables listed in Table 1. Models in all panels except B are weighted by the proportion of each state's population covered by the UCR data. *p < 0.10, **p < 0.05, ***p < 0.01.

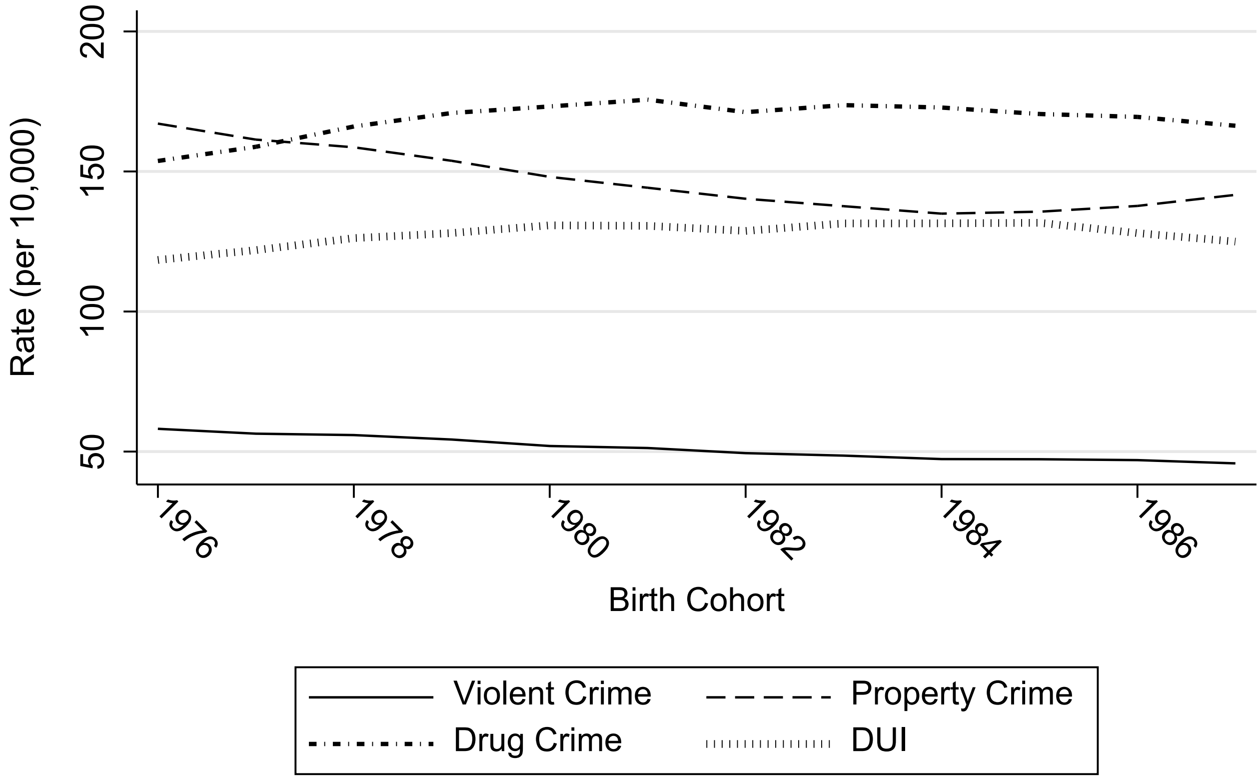

Figure A1
Crime Rates Across Birth Cohorts

Source: Authors' calculations based on weighted UCR data. See text for description of the 35 states included in the figure.

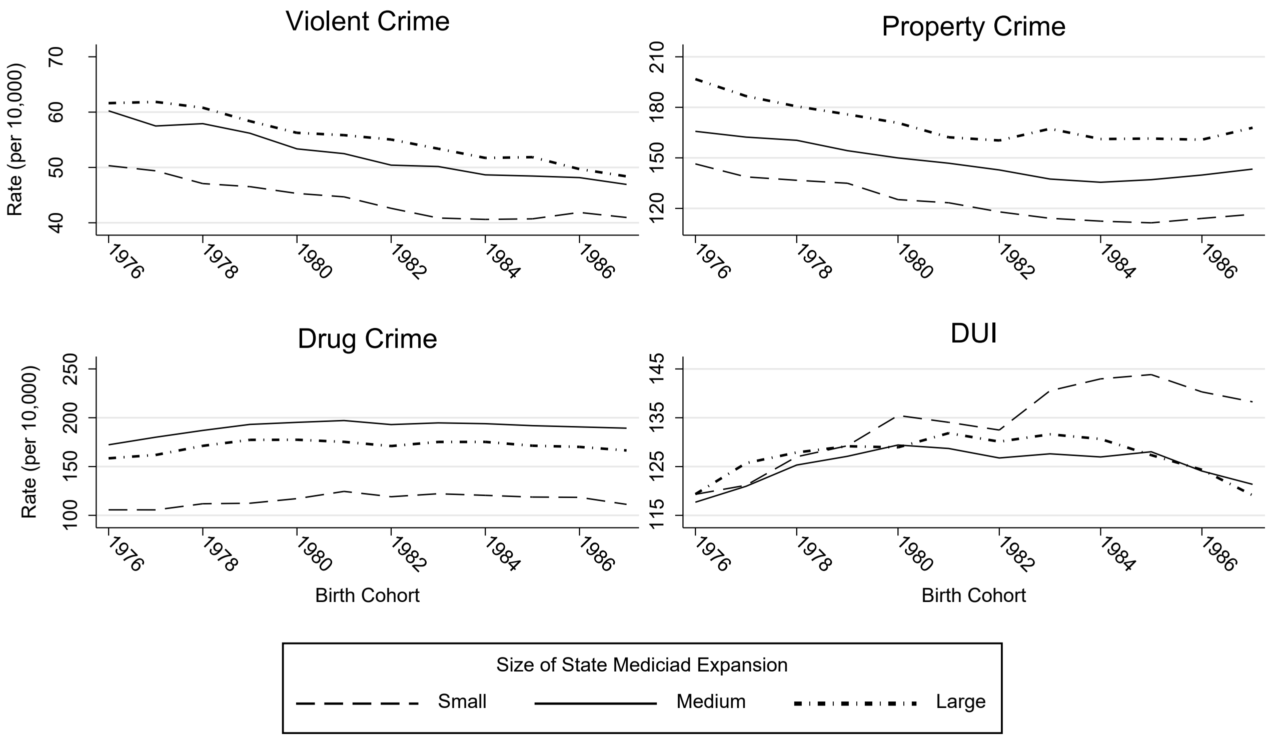

Figure A2
Crime Rates Across States

Source: Authors' calculations based on weighted UCR data. 'Small' and 'Large' refer to quartiles of states with the smallest and largest expansions in Medicaid eligibility between 1976 and 1987, respectively. See text for description of the 35 states included in the figure.

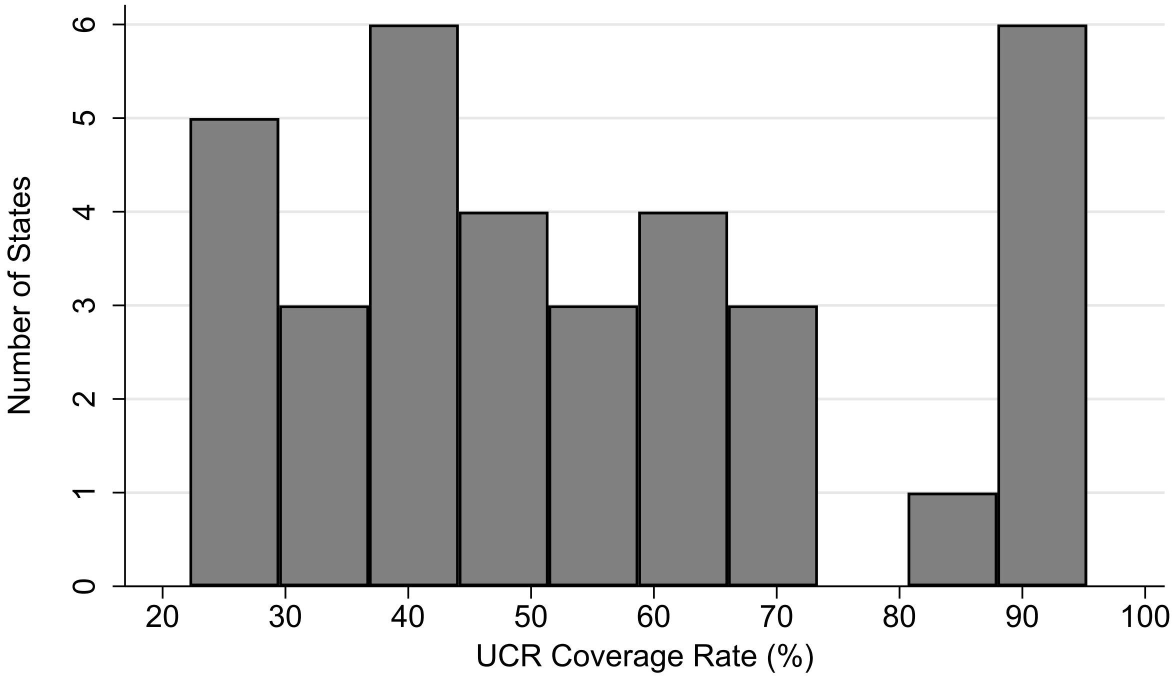

Figure A3
UCR Population Coverage Rates

Source: Authors' calculations based on UCR and SEER data. The UCR coverage rate is the percent of each state's population covered by the Uniform Crime Reporting system during 1995-2011.