Unintended Policy Effects and Youth Crime

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Boston College
The Graduate School of Arts and Sciences
Department of Economics

UNINTENDED POLICY EFFECTS AND YOUTH CRIME

A dissertation

by

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UNINTENDED POLICY EFFECTS AND YOUTH CRIME

ABSTRACT

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This dissertation examines how some policies, though not intended to, can influence youth crime.

The first chapter studies the minimum dropout age (MDA), a compulsory schooling policy. This paper exploits state-level policy variation to identify the immediate and long-run effects of the MDA on crime. I find that higher compulsory schooling ages decrease male property crime while individuals are forced to be in school, but this effect dissipates in early adulthood. Male drug crime, however, experiences a decrease in both the short and long-run. These results provide further evidence for the incapacitation effect of schooling. The inconsistent long-run effect, however, calls into question the size of compulsory schooling's human capital effect on crime. The evidence indicates that, rather than a human capital effect, long-run decreases in crime may be explained by a dynamic incapacitation effect that is stronger for certain crimes, e.g., drug vs. property crimes. These findings have policy implications for crime deterrence and our understanding of criminal career development.

The second chapter (co-authored with Drew Beauchamp) investigates how increases in the minimum wage impact the criminal behavior of affected workers. A growing body of empirical evidence indicates that increases in the minimum wage have a displacement effect on low-skilled workers. We use detailed panel data from the National Longitudinal Survey of Youth 1997 cohort to examine the effect of increases in the minimum wage on self-reported criminal activity and test the employment-crime substitution hypothesis. Exploiting changes

in state and federal minimum wage laws from 1997 to 2010, we find that workers who are affected by a change in the minimum wage are more likely to become idle and unemployed. Further, there is an increase of property theft among both the unemployed and employed, suggesting that substitution between employment and crime is stronger than the income effect. These findings have implications for policy regarding both the low-wage labor market and criminal activity.

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Chapter 1

Not Until You're Older: The Minimum Dropout Age and the Crime-Age Profile

> "We also know that when students aren't allowed to walk away from their education, more of them walk the stage to get their diploma. So tonight, I call on every State to require that all students stay in high school until they graduate or turn eighteen." - President Obama, State of the Union Address 2012

1.1 Introduction

Can forcing teenagers to remain in school reduce the crime they commit over their lifetime? The current debate over raising the compulsory schooling age for dropouts (henceforth the minimum dropout age, or MDA) to 18 largely centers around educational attainment, however, it may also have implications for crime prevention. Raising the MDA to 18 would mandate 16- and 17-year-olds to attend school, and crime statistics show that criminal activity increases rapidly at those ages. Economic theory gives us several reasons to believe that raising the MDA should diminish crime during both adolescence and later life. I use

recent changes in the MDA at the state-level as a quasi-experiment to estimate the effect of MDA policies on a cohort's crime rate at different points in the age profile. This approach allows me to compare the immediate and long-run effects of schooling in ways that other papers have not.

This paper contributes to the literature on crime and schooling in two main ways and also has several major policy implications. First, it establishes evidence for state dependent criminal behavior and a causal relationship between the age of criminal onset and length of criminal career. While the criminology literature has long accepted that early criminal careers tend to be longer and more serious, little evidence has been provided regarding the nature of that relationship due to the endogeneity of the age of onset. I find that the MDA can act as an exogenous delay of criminal careers and that, for certain types of crime, the age of criminal onset is causally related to criminal tenure. Second, this paper adds to our knowledge about the relationship between crime and the timing of education. Lochner and Moretti (2004) provide evidence that total years of schooling has a negative relationship with crime, while Heckman et al. (2010) show that schooling in early childhood also decreases criminal activity later in life. Both papers argue that the development of human capital lowers crime rates by increasing the opportunity cost of crime. This paper provides evidence that extra schooling in adolescence has negligible human capital effects on crime but instead acts to deter crime by keeping youth off the streets during a time that may be crucial for the development of a criminal career.

I exploit state variation in MDA policy over the period of 1972-2009 to identify the immediate and long-run effects of the MDA on crime. I use annual, county-level crime data to construct "synthetic cohorts"[1] that comprise the treatment and control groups in a quasi-experimental research design. I compare the crime rates of cohorts who were allowed to dropout of school at age 16 (MDA = 16) with cohorts forced to stay in school until age 17 or 18 (MDA = 17 or 18). I employ a differences-in-differences estimation strategy to measure the treatment effect of MDAs above 16 and allow the policy effect to vary across

[1]See, for example, Lleras-Muney (2005), who constructs cohorts from mortality data.

ages. This allows me to separate immediate "incapacitation" effects from longer run effects that persist past the compulsory schooling period.

I find that higher compulsory schooling ages decrease male property and drug crimes while individuals are forced to be in school, but the effect is inconsistent later in life across types of crime. The effect disappears in young adulthood for crimes that require little to no prior experience or criminal networks, such as property crime, with the least serious property crimes seeing the fastest dissipation of the effect. The effect persists for drug crimes, which require experience/networks.

My results add to the evidence for the incapacitation effect of schooling and show that the human capital effect of compulsory schooling on crime may be limited. Rather, the incapacitation effect of schooling is dynamic and has the strongest impact on crimes that have strong state-dependent behavior. As such, compulsory schooling decreases cohort crime, but mainly through these dynamic incapacitation effects.

My analysis is presented in the following structure. Section 2 provides a theoretical framework through which to consider the potential effects of the MDA on crime, as well as an overview of previous findings related to crime, schooling, and the timing of criminal behavior. Section 3 provides background on compulsory schooling law, and Section 4 describes my data. Section 5 presents my empirical approach and evidence that the policy variation is exogenous to youth crime, establishing the quasi-experimental structure of my analysis. In Section 6, I present my results, and I conclude in Section 7.

1.2 Theoretical Background

There are three main hypotheses of criminal activity that may explain how the MDA could affect crime in the short- and long-run: incapacitation, human capital, and state-dependent criminal behavior. The incapacitation effect results from the presence of a binding time constraint: an adolescent who is compelled to attend school five days per week will have less time to dedicate towards criminal activity. As the school day effectively incapacitates students from committing crime, the incapacitation effect is a short run effect of schooling.

In contrast, the human capital effect hypothesizes that students will develop skills in school that will improve their options outside of crime, increasing the opportunity cost to crime. As a result, the human capital effect should cause compulsory schooling to have a persistent, long-run effect on crime. Lastly, state-dependent behavior theory argues that prior criminal offending affects future offending through social learning, the development of networks, and habit formation. Under this hypothesis, the persistence of any policy effect on crime will depend on how strongly state-dependent the crime.

A simple model of criminal activity that incorporates these theories would look like

$$Crime_t = f(S_t, \sum_{y=0}^{y=t-1} S_y, Crime_{t-1})$$

where crime in period t is a function of time spent in school during that period (S_t), cumulative schooling from past periods (accumulated human capital) ($\sum_{y=0}^{y=t-1} S_y$), and crime performed in previous period $t-1$ ($Crime_{t-1}$). An incapacitation effect will be present if

$$\frac{\partial Crime_t}{\partial S_t} < 0.$$

The human capital effect, which works through human capital accumulated through school tenure depends on

$$\frac{\partial Crime_t}{\partial \sum_{y=0}^{y=t-1} S_y} < 0.$$

Finally, the state-dependence of criminal behavior will cause

$$\frac{\partial Crime_t}{\partial Crime_{t-1}} > 0.$$

The presence of state-dependence will make the incapacitation effect dynamic, since $Crime_{t-1}$ is a function of time spent in school in the previous period, S_{t-1}.

The full derivative of crime with respect to the MDA will show the effect of a change in

the MDA on contemporaneous crime:

$$\begin{aligned}\frac{dCrime_t}{dMDA_t} &= f_1\frac{dS_t}{dMDA_t} + f_2\frac{d\sum_{y=0}^{y=t-1} S_y}{dMDA_t} + f_3\frac{dCrime_{t-1}}{dMDA_t} \\ &= \frac{\partial Crime_t}{\partial S_t}\cdot\frac{dS_t}{dMDA_t} + 0 + 0 \\ &< 0.\end{aligned}$$

A change in the MDA will not have any effect on the accumulation of schooling previous to the change, nor will it affect criminal activity that has already occurred. The MDA can only increase the amount of schooling received during that period for the cohort bound by the policy. Consequently, the MDA should decrease criminal activity by cohorts forced to stay in school due to the incapacitation effect.

Previous studies show evidence of an incapacitation effect, with school days disrupting the criminal activity of school-aged youth. Jacob and Lefgren (2003) and Luallen (2006) find that school days appear to incapacitate property crime and may agitate violent crime. Anderson (2012) provides evidence that the incapacitation effect also results from increases in the MDA. Comparing 16-18 year-old crime with 13-15 year-old crime, he finds an incapacitation effect for both property and violent crimes.

The long-run effects of the MDA can be derived by taking the full derivative of future crime with respect to the MDA:

$$\begin{aligned}\frac{dCrime_{t+1}}{dMDA_t} &= f_1\frac{dS_{t+1}}{dMDA_t} + f_2\frac{d\sum_{y=0}^{y=t} S_y}{dMDA_t} + f_3\frac{dCrime_t}{dMDA_t} \\ &= 0 + \frac{\partial Crime_{t+1}}{\partial\sum_{y=0}^{y=t} S_y}\cdot\frac{d\sum_{y=0}^{y=t} S_y}{dMDA_t} + \frac{\partial Crime_{t+1}}{\partial Crime_t}\cdot\frac{dCrime_t}{dMDA_t} \\ &< 0.\end{aligned}$$

A change in the MDA this year does not compel students to remain in school next year, so the first term is zero. However, being forced to remain in school this year does impact human capital that will be accumulated by next year: the second term. Thus, the MDA can have a long-run effect through human capital. Additionally, as seen above, the MDA will

have an effect on this year's criminal activity, which will affect future crime through the state dependence of criminal behavior. While the human capital effect should be persistent, the dynamic incapacitation effect depends on how heavily state dependent crime is ($\frac{\partial Crime_{t+1}}{\partial Crimte_t}$).

The human capital effect is discussed at length in Lochner (2004), who develops a model of human capital and crime: education develops an individual's human capital, increasing the opportunity cost of crime through improved employment and wage prospects. Individuals who receive more schooling through MDA laws will therefore commit less crime as adults. Grogger (1998) finds that wages have a negative relationship with crime, though he does not find an effect through schooling attainment. Lochner and Moretti (2004) do, however, reveal the human capital effect for data on incarceration, arrest, and self-reported crime based on MDAs from 1914-1978. Hansen (2003) attempts to study the relationship between schooling and crime using cross-sectional British data, but does not control for the endogeneity of schooling.

The state-dependent crime theory is part of an ongoing debate in the criminology literature. It is widely accepted by criminologists that early criminal careers tend to last longer and be more deviant (Farrington et al., 2008). However, theories of the nature of the relationship can be categorized in two divisions: one of innate criminal propensity or endogenous career onset; and one of state dependent criminal behavior or habit formation. The literature is limited in its ability to test the two theories due to lack of exogenous variation in the age of criminal onset. Most papers that study the relationship are descriptive studies of longitudinal data where the age at which an individual first commits a criminal act remains potentially endogenous.[2] If, however, changes in the MDA can effectively delay teenagers from committing crime, then the MDA can provide the variation necessary to test the state-dependence of criminal career development.[3]

The MDA provides an exogenous source of schooling variation and has many consequences worth studying. This paper contributes to a recent flush of papers that examine modern

[2]See, for example, Bacon et al. (2009).

[3]Heckman and Masterov (2007) indicate that early investments in child development may be necessary to achieve variation in traits that determine criminal propensity, thus I am unable to test the propensity hypothesis with changes in the MDA.

MDA policy. Much of the existing MDA literature, however, examines MDA policies from the early and mid-1900s: a time when students faced a vastly different economic and educational environment than today.[4] There is, however, a small literature that analyzes recent policy. Oreopoulos (2009) reveals that a higher minimum dropout age increases employment and earnings in early adulthood. Gilpin and Pennig (2012) and Anderson (2012) consider the MDA and crime, finding a contemporaneous relationship between the policy and crimes committed by school-aged youth. In contrast, this paper examines the short and long run effects of the modern MDA on crime.

1.3 Policy Background

Compulsory schooling laws dictate the ages at which children must attend school, as well as associated penalties and exceptions. These laws were first established in the United States by Massachusetts in 1852 and were adopted by all states by 1918. The upper bound of the compulsory schooling ages is the youngest age at which students are legally allowed to dropout of school without a high school diploma: the minimum dropout age. Legislation is set at the state level, with states currently setting the MDA between ages 16 and 18. Figure 1.1 shows the variation in the MDA by state over the time period of interest: 1972-2009.

The recent push towards raising the MDA has been prompted by growing concerns about the rising dropout rate in the U.S.. From the early 1970s until recently, the high school graduation rate (measured as the share of 17 year-olds who graduate with their class) has steadily declined (Heckman and LaFontaine, 2010). Compulsory schooling has been spotlighted as a means to raise the graduation rate and mitigate the dropout trend.

Though there is considerable variation in how compulsory schooling laws are enforced across jurisdictions, there is evidence that the MDA is effective at reducing the dropout rate. While some states may have very little compulsory schooling law enforcement, others may

[4]For example, Angrist and Krueger (1991) and Acemoglu and Angrist (2001) show that an additional year of schooling raises adult earnings by 10 percent. Lleras-Muney (2005) find that educational attainment may reduce mortality in old age, while Black et al. (2008) show that compulsory schooling may also reduce female teenage childbearing. Lochner and Moretti (2004) demonstrate a negative relationship between years of educational attainment and adult crime.

have multiple programs in place to try to retain and engage potential dropouts. Despite this variation, the MDA appears to be effective on average. Anderson (2012) shows that the MDA reduces the dropout rate among both males and females, and Oreopoulos (2009) finds that students receive an average of 0.12 years of additional schooling as a result of the MDA.

1.4 Data

1.4.1 Reference Data

This analysis utilizes data from the FBI's Uniform Crime Reports (UCR), a national reporting system of crime incidence and arrests. Data for the UCR are provided by roughly 17,000 local, county, and state agencies in a given year, corresponding to 96.3% coverage of the national population as of 2009. The data provide annual police agency-level counts of arrests by age, gender, and offense for years 1980-2009. I aggregate the data by age, county, year, gender, and type of offense: property or drug. Property crimes are the sum of larceny, burglary, motor vehicle theft, and arson crimes. Drug crimes include all selling and possession crimes. I calculate crime rates using age-specific, county population data provided by the National Cancer Institute, Surveillance Epidemiology and End Results population data. These intercensal population estimates do not include incarcerated individuals, so the measured crime rate is for the non-institutionalized population.

Although crime incidence data would be ideal for this study, I use arrest data because it contains reliable offender age information.[5] This analysis is subject to, but not limited by, the main drawback of arrest data: arrests may underrepresent adolescent crime incidence.[6] Since my goal here is to compare differences in crime rates, rather than examine crime rate levels, the measurement error of arrest data is not a problem as long as it does not vary systematically with the MDA.[7] Because participation in the UCR program varies over

[5]Incidence data, for example, has unknown accuracy for crimes where the offender is not arrested.

[6]Arrests may represent a subset of actual crimes committed due to underreporting by victims, lack of criminal pursuit by police for young offenders or petty offenses, or because offenders go uncaught (Wolfgang et al., 1972).

[7]Lochner and Moretti (2004) show strong correlations between arrest and crime of 0.96 for rape and robbery, 0.94 for murder, assault, and burglary, and 0.93 for auto theft.

time, I calculate a UCR coverage index as the proportion of each county population that is reported as covered by a UCR-participating agency.

The National Center for Education Statistics's Digest of Education Statistics and state legislative records provide schooling laws for 1984-2009, with Acemoglu and Angrist (2001) providing laws from before 1984.[8] Additional covariates include state level policies on the minimum wage, minimum drinking age, and marijuana legalization, as well as county-level income per capita and demographic information: percent of the population that is male, black, and employed. Employment and income data are products of the Bureau of Economic Analysis. Income per capita and the minimum wage are both adjusted to constant dollars using the Current Price Index from the Bureau of Labor Statistics.

1.4.2 Constructed Data

The ideal data for this study would be longitudinal, with data on each criminal offender and his/her offenses. The crime data, however, does not track criminal behavior at the individual level. Consequently, using arrest data for 15-24 year-olds, I construct "synthetic cohorts" and attribute crimes to birth cohorts.[9] I then assign compulsory schooling legislation by cohort. Using the age and year of arrest, I back-out the offender's birth cohort. This creates an unbalanced panel of property and drug crime arrests for male and female cohorts born in 1956-1993.[10]

Each cohort is assigned MDA treatment according to the MDA that the cohort faced as it ascended through school. Because I cannot observe the state where offenders attended school, I rely on the state of arrest. Although it is not a guarantee that individuals are arrested in the same state where they attended school, as long as patterns of geographic mobility are uncorrelated with the MDA, they will result in classical measurement error and attenuate the results.[11] Given that less educated individuals in the United States are less

[8] I impute missing data by using earlier values. For example, if the MDA for 1980 is missing, I use the MDA from 1979.

[9] It should be noted that the data only provide the annual crime rate and not individual criminal sentencing or arrest history, I cannot draw conclusions about recidivism or individual levels of criminal activity.

[10] Not all cohorts can be observed at all ages due to the limited time period of the UCR crime data: for example, the 1956 cohort is observed at age 24 in 1980 and cannot be observed at earlier ages.

[11] This would be an issue if, for example, offenders who were treated by an MDA of 16 are systematically

geographically mobile,[12] it may be that students who are likely affected by the MDA are also less likely to move to a new state between the ages of 16 and 24.[13]

The approximation of the birth cohorts is another potential source of measurement error. When attributing crimes to cohorts, the method assumes that all offenders are born on January 1, making all crimes observed at any time through the year attributable to one birth cohort. Of course, not all offenders are born on January 1, so some crimes are misattributed across MDAs. This assignment error is again random relative to variation in the MDA and should only attenuate the results. To diminish this problem, I drop cohorts who would have experienced MDA policy changes in their respective states during the ages of 16-18 because I cannot be certain which MDA treatment applies to such cohorts.[14]

The final data set is an unbalanced panel of 101,534 county cohorts observed at ages from 16 to 24. Table 1.1 illustrates the breakdown of cohorts according to MDA treatment. The majority of cohorts in the data experienced an MDA of 16, with higher MDAs affecting cohorts born later in the sample. Figure 1.2 shows the unconditional averages of age-specific property and drug crimes in the data. Both crimes peak as cohorts enter the late teens, with property crime declining faster than drug crime as cohorts age. This pattern seems to indicate that drug crimes are more state-dependent than property crime. These graphs do not account for factors in the county, cohort, or time dimension that may affect crime rates.

1.5 Estimation Strategy

1.5.1 Estimating Equations

I compare the crime rates of cohorts who were allowed to dropout of school at age 16 (MDA = 16) with cohorts forced to stay in school until age 17 or 18 (MDA = 17 or 18).

arrested in a state where their birth cohort was treated by an MDA of 17 or 18, or vice versa. In the event that cross-state crimes are most likely to occur along state borders, I run my estimates on data that exclude all counties on state borders. The results do not change.

[12]Malamud and Wozniak (2010) show that college graduation have a mobility advantage over non-graduates of the same cohort.

[13]In the event that individuals may be more likely to move or commit crimes in states other than their home state if they reside along a state border, I also estimate the empirical model on data where counties on state borders have been removed. The results are unaffected by the change.

[14]Including these cohorts in the sample indeed causes attenuation bias.

I employ differences-in-differences estimation to measure the treatment effect of MDAs above 16 and allow the policy effect to vary across ages. Cohorts bound by an MDA of 16 comprise the control group, while cohorts subject to MDAs of 17 or 18 comprise the treatment group(s): first I use a treatment group of cohorts bound by any MDA above 16, then I allow MDAs of 17 and 18 to have separate treatment groups.

This empirical strategy results in two estimating equations:

$$Crime_{cjst} = \beta_0 + \sum_{a=15}^{24} \beta_1^a \mathbb{I}\{MDA_{cjs} = 17 or 18\}\mathbb{I}\{Age_{cjst} = a\} \tag{1.1}$$
$$+ X_{cjst}\beta_3 + \delta_{js} + \delta_t + \delta_s trend + \epsilon_{cjst}$$

$$Crime_{cjst} = \beta_0 + \sum_{a=15}^{24} \beta_1^a \mathbb{I}\{MDA_{cjs} = 17\}\mathbb{I}\{Age_{cjst} = a\} \tag{1.2}$$
$$+ \sum_{a=15}^{24} \beta_2^a \mathbb{I}\{MDA_{cjs} = 18\}\mathbb{I}\{Age_{cjst} = a\}$$
$$+ X_{cjst}\beta_3 + \delta_{js} + \delta_t + \delta_s trend + \epsilon_{cjst}$$

where c denotes cohort, t denotes year, and j a county located in state s. Age a at time t is defined as $a = t - c$. The indicator for a binding MDA of 17 or 18 in Equation (1.1) combines these policies to test for any effect over counties in states with a 16 year-old MDA, while separate indicators for binding MDAs of 17 and 18 in Equation (1.2) allow for the two policies to have different effects. The policy indicators are also interacted with a set of age indicators, which allows the policies to affect a cohort's crime rates differently at different ages. X_{cjst} captures time-varying county demographic and economic factors. Vectors of county indicators (δ_{js}) and year effects (δ_t) absorb time-invariant differences in arrests across counties and national crime trends. For example, county fixed effects will capture any persistent underreporting of crime at the county level, and year fixed effects will account for any national trends that might obscure the measured effect of MDAs of age 18 that were predominantly introduced later in the time window. I also correct for time-varying reporting coverage in the data by controlling for the share of each county's

population covered by UCR reporting agencies in each year. I weight observations according to average county population across the time period studied.[15] Because the policy varies by state-year but observations are taken at the county level, I could cluster the standard errors at the state-year level as discussed by Moulton (1986). I instead cluster at the state level to also account for potential serial correlation.

The age-specific policy effects are measured as β_1^a in Equation (1.1) and β_1^a and β_2^a in Equation (1.2). In (1.1), β_1^a represents the effect of experiencing an MDA above 16 on crime (relative to when the cohort MDA was 16) for age group a. In (1.2), β_1^a represents the effect of an MDA of 17 relative to an MDA of 16, and β_2^a represents the effect of an MDA of 18. Age-specific policy effects allow me to test the incapacitation and human capital effects of compulsory schooling since these effects should occur at different ages. The incapacitation effect of the policy "MDA = 17" will cause the policy effect for 16 year-olds (β_1^{16}) to be negative age. The incapacitation effect for "MDA = 18" will cause a negative effect at ages 16 and 17 (β_2^{16} and β_2^{17}, respectively. Human capital or habit formation/criminal experience effects, however, will be visible at 18 and later. Because all effects should decrease cohort crime, the anticipated sign of the estimates is negative.

1.5.2 Policy Exogeneity

To conduct a study using the MDA as an exogenous treatment, policy must be exogenous with respect to the measured outcome variable: youth crime rates. One fear is that states experiencing increases in youth crime are more apt to raise the MDA in an attempt to decrease youth crime or other factors that may be related to youth crime. (High school dropout rates, for example, may be correlated with youth crime rates.) Since the MDA affects 16- and 17-year-olds, any concern of MDA policy being endogenous to crime rates should be limited to crime at the ages of 16 and 17. To investigate this issue, I examine state documentation for why states consider changing the MDA. I also use my data on 16-, 17-, and 18-year-old crime rates to see if youth crime trends can predict changes in state MDAs.

[15] Results are robust to other weightings.

Why do states change the MDA?[16] Proponents paternalistically argue that a higher MDA will improve the lifetime employment, health, and general welfare of otherwise myopic, impulsive youth by decreasing dropout rates, better preparing would-be dropouts for an increasingly competitive job market that values skilled labor (National Association of Secondary School Principals, 2010). For example, a Maryland task force established when the state began considering raising its MDA from 16 to 18 reports that their objective is to "enable children to succeed, maximize their human potential, and lead productive lives" (Maryland State Board of Education, 2007). Opponents question the effectiveness and costliness of raising the MDA, also often arguing that the policy violates the rights of individuals and their families to decide when to leave school (NEA Education Policy and Practice Department, 2010). No documentation appears to cite worries about youth crime trends as a reason for raising the MDA.

If there were selection into higher MDA policy based upon upward youth crime trends, we would expect that states would be more likely to adopt higher MDAs when youth crime is increasing. It could then be possible to predict state changes in the MDA by using lagged youth crime rates. Table 1.2 shows the results of regressing an indicator for increasing the state MDA on five lags of youth crime rates and state population covariates. Columns (1) and (2) show the results for 16-year-old property and drug crime, respectively. Columns (3) and (4) show results for 17-year-old crime, and (5) and (6) show results for 18-year-old crime. A test for joint significance of the lagged crime rates shows that youth crime rates have no distinguishable predictive power for MDA policy changes.[17]

If changes in the MDA are correlated with other factors (aside from schooling) that would cause observed crime rates to increase or decrease, my estimates will be biased away from zero. For example, if states that raise the MDA also increase policing efforts, higher arrest rates will accompany higher MDAs, lending a positive bias to my estimates. This, however, does not appear to be the case for the relevant time period. Anderson (2012) shows

[16] Messacar and Oreopoulos (2012) provide a thorough summary of the debate over raising the MDA, upon which much of this discussion is based.

[17] Inclusion of fewer lags remain insignificant.

that there is no evidence that recent MDA laws are correlated with higher levels of police enforcement measured by per capita expenditures and sworn officers. If anything, higher MDAs may be associated with lower police expenditures per capita, consistent with the idea of a binding state budget (Lochner and Moretti, 2004). If, on the other hand, additional schooling due to the MDA makes criminals more capable of committing crimes that go unreported or without capture, then my results would be biased downward. This outcome, however, is also unlikely. In an attempt to capture any other unobserved heterogeneity across states that will affect my results, I include state-specific time trends in my empirical specification.[18]

Identification is achieved from state variation in MDA policy over time. The differences-in-differences approach removes the effects of time-invariant county factors and year-specific factors that could influence county crime rates. I also control for a vector of observable, time-varying county factors in the regression and include state-specific time trends.

1.6 Results

1.6.1 Male crime

Property crime

Figure 1.3 illustrates the male property crime results, with the top panel showing results for Equation (1.1), and the second panel showing the results for Equation (1.2). Tables 1.3 and 1.4 show the estimated age-specific policy effects for alternative specifications of Equation (1.1) and Equation (1.2), respectively. In each table, each column represents a separate regression under a different specification. Because many of the point estimates are individually insignificant, I test for joint significance during the incapacitation period (ages 16 and 17 for an MDA of 18) and the long-run period (ages 19 and above). The F-statistics and p-values from the joint significance tests are reported for each specification.

Results for the base specification are shown in column (4), where county and year

[18]The main specification includes only a linear, state-specific time trend. Results are robust to the inclusion of a quadratic trend.

fixed effects, demographic covariates, and state time trends are included in the regression. Comparing these results to other specifications, it appears that spatial heterogeneity across time is important: there are factors other than the MDA that change over the sample period and may be contributing to the decrease in crime over time. In column (1), the crime rate is regressed only on the policy interaction with age and county and year fixed effects. The coefficient on the policy indicator is statistically significant and negative. The age-specific policy effects are each negative, though the precision of the estimates diminishes at older ages. These results indicate that cohorts experiencing higher MDAs are arrested less in both the short- and long-run. The inclusion of linear state time trends in column (2) and of time-varying covariates in column (3) both diminish the magnitude of the policy effects, capturing some of the decrease in property crimes. I maintain the specification in column (4), including both state time trends and covariates, as the preferred specification to account for as much spatial heterogeneity as possible. Lastly, adding a quadratic time trend in column (5) to the base specification has no significant effect on the estimates, so I do not include it in the preferred specification.

Estimates for Equation (1.2) in Table 1.4 follow the same pattern of sensitivity to different combinations of covariates as estimates for Equation (1.1). Each specification has two columns: one showing the interactions for an indicator variable of an MDA of 17, and one showing interactions for an MDA of 18. For each specification, the estimates are of similar magnitude to those in Table 1.3 but slightly less precisely estimated. As would be expected, the magnitude of the policy effects are larger for an MDA of 18 than an MDA of 17.

Incapacitation period As can be seen in Table 1.3 and Figure 1.3, cohorts bound by high MDAs have lower property crime rates during the compulsory schooling period (ages 16 and 17). I find that male cohorts who must stay in school until age 17 or 18 are arrested for property crimes significantly less than other cohorts while they are within the compulsory schooling age range: 17-year-olds in treated cohorts have an arrest rate that is 4.1 percentage points below untreated cohorts. When I examine MDAs of 17 and 18 separately, I find that

an MDA of 18 causes a larger decrease in arrests than an MDA of 17, though the estimates are not significantly different.

These empirical results support the incapacitation story and are comparable in size to those in the existing literature. Jacob and Lefgren (2003) and Luallen (2006) estimate that daily juvenile property crime decreases by 14% and 28.8% on school days, respectively. Anderson (2012) finds that annual 16-18 year-old property crime decreases by 9.9% relative to contemporaneous 15-17 year-old crime when the MDA is raised. My estimates may be smaller than day-to-day incapacitation effect estimates if daily incapacitation simply causes youth to move delinquent behavior to another day. In that case, crime rates on school days may be lower only because crime rates on non-school days rise.[19] This problem would bias the incapacitation effect to appear larger using daily crime data.

Long-run period The decrease in property crime that is observable during the incapacitation period lingers at age 18, with treated cohorts having arrest rates lower by 5.7%, but then soon dissipates. When policy effects are allowed to differ across MDAs of 17 and 18, I again find that any negative effect on crime dissipates in early adulthood. Tests for joint significance at ages 19 and above for any MDA above 16, MDAs of 17, and MDAs of 18, show no significance in the long-run for any of the minimum dropout age policies. For the aggregated property crime measure of this analysis, which includes larceny, burglary, motor vehicle theft, and arson, I find no long-run effects of the minimum dropout age after the compulsory schooling period.

It is difficult to explain the non-persistent reduction in property crimes with the human capital hypothesis. The human capital hypothesis, regardless of whether working through wages and employment, preferences for crime, or skills to evade arrest, predicts a consistent decline in crime. This non-persistence observed in the results could be explained if the amount of human capital attained through compulsory schooling as a teenager is limited.

Both cognitive and non-cognitive skills are essential to economic outcomes and are

[19] Jacob and Lefgren (2003) do find weak evidence that property crime may be shifted to other days of the week.

developed early in life, but investments made in early childhood have much higher returns than investments later in life (Heckman and Masterov, 2007). Oreopoulos (2009) finds that cohorts gain an average of 0.12 additional years of schooling through the MDA, however, perhaps the human capital developed during that time has limited impact on criminal behavior. Would-be dropouts have lower non-cognitive skills, and therefore may not maintain legitimate employment for long despite additional cognitive skills gained from an additional year of compulsory schooling.[20]

The long-run results also align well with dynamic incapacitation: incapacitation at compulsory schooling ages may diminish the intensity with which cohorts commit crime later in life because criminal behavior is state-dependent. This channel may work through the criminal labor market or habit formation. If each year of criminal experience or networking during adolescence is important for criminal opportunity in early adulthood, incapacitation during adolescence has implications for crime later in life and can explain why an MDA of 18 has stronger effects than an MDA of 16 or 17. Dynamic incapacitation may not last long for property crime because property crimes are not strongly state-dependent. For example, larceny, the most frequent property crime, requires little prior experience or knowledge. I investigate this hypothesis further by comparing the long-run effects of simple and serious property crimes.

Minor vs. serious property crime

Figure 1.4 illustrates the results for minor male property crimes, comprised of larceny and arson, while Figure 1.5 shows results for serious male property crimes: burglary and motor vehicle theft. The top panels show results for Equation (1.1), and the second panel showing the results for Equation (1.2).

Minor and serious property crimes exhibit different responses to the compulsory schooling age. In particular, the incapacitation effect is stronger for serious than for minor crimes. Similarly, serious crimes exhibit a slightly longer dynamic incapacitation effect in the long-

[20]Heckman and Rubinstein (2001) find that non-cognitive skills may explain the lower employability of GEDs and high school graduates.

run, most notable when MDA policies are separated out by age. This corroborates the story that crimes that require more experience and are more state-dependent, such as serious property crimes, will exhibit larger long-run effects of the MDA.

Drug crime

Figure 1.6 shows the results for male drug crime. Tables 1.5 and 1.6 display the estimated policy effects for alternative specifications of Equation (1.1) and (1.2), respectively. Many of the interacted policy and age terms are individually significant, especially during the incapacitation period. Specification (1) runs the estimating equation with only the policy indicator(s) and county and year fixed effects. The policy effects are statistically significant and negative at almost all ages. The estimates are not very sensitive to alternative specifications.

Incapacitation period The effects for drug crime are more pronounced than for property crime. Male drug crime decreases by nearly 20% during the incapacitation period and remains below the control group by 4.6%-7.9% in the early 20s. Comparing single-age policies (MDA of 16 vs. 17 vs. 18), I find that an MDA of 18 persistently decreases crime by a larger margin than an MDA of 17. In fact, an MDA of 17 does not have a statistically significant impact on drug crime.

Long-run period The age-specific policy effects on drug crime remain jointly significant at the 95% level for ages 19 and above for MDAs of 18 and are jointly significant for MDAs of 17 at the 90% level. These results again can be explained with dynamic incapacitation. For drug crimes, this channel may work through the criminal labor market or habit formation and addiction. Drug sales may show strong dynamic incapacitation effects if each year of drug sale experience or networking during adolescence is important for criminal opportunity in future years. For example, it takes time to establish buyer and seller relationships. Drug possession crime may also be heavily affected by this channel due to addiction and habit formation. Intervention of drug use during adolescence may have a significant impact on

future drug use if adolescence is a critical time for individuals to become introduced to drugs.

Violent crime

Figure 1.7 shows the results for male violent crime. Tables 1.7 and 1.8 display the estimated policy effects for alternative specifications of Equation (1.1) and (1.2), respectively. Specification (1) runs the estimating equation with only the age-interacted policy indicator(s) and county and year fixed effects. The policy effects are statistically insignificant, but negative at almost all ages. The introduction of covariates in specification (2) captures some of the factors that may have been associated with the negative effect of the MDA at all ages. The estimated policy effects become statistically significant and positive. Similarly, the introduction of a linear state time trend in specification (2) also makes the estimated coefficients more positive. This shows that the time trend captures a secular decline in violent crime. The combination of a linear state time trend and covariates in base specification (4) has statistically significant and positive coefficients, with the addition of quadratic state time trends in (5) doing little to influence the estimates.

Incapacitation period Column (4) of Table 1.7, our base specification, shows that male violent crime increases by 5 in 1,000 at age 15. The effects of the policy appear concentrated at younger ages. This increase is jointly significant in all specifications. Comparing single-age policies (MDA of 16 vs. 17 vs. 18) in Table 1.8, I find that an MDA of 18 is responsible for the significant effect, with an MDA of 17 causing a statistically insignificant increase in violent crimes in the short run.

These results do not follow the same incapacitation explanation as property and drug crimes. While school may draw students away from opportunities to commit property and drug crime, it may actually provide additional opportunities or incentives to engage in violence by creating social interactions among students that generate violent activity. The larger effects observed at younger ages may indicate that the presence of more senior students causes younger male students to act more aggressively. Jacob and Lefgren (2003)

find that a school day increases juvenile violent crime by 28%, while Luallen (2006) measure a 31.5% increase in violent crime. Gilpin and Pennig (2012) also find that the MDA increases in-school violent crime. In contrast, Anderson (2012) discovers that an MDA of 18 decreases violent crime among 16-18 year-olds. This could be, however, due to the increase in violence committed by 15 year-olds: members of his control group.

Long-run period Using the social interaction hypothesis from the short-run results, it is difficult to anticipate what long-run effects the MDA will have on violent crime. I find that there is not statistically significant long-run effect: all point estimates are positive, but none are significant. There is also no joint significance for ages 19 and above under any specifications. This could indicate that the increase in violent crime that is observed during the incapacitation period is short-lived aggression due to school skirmishes, and not indicative of the development of violent careers.

1.6.2 Heterogenous effects by demographic populations

The results presented so far reflect the average policy effect across all county cohorts, but a second area of interest for this analysis is what demographic groups may experience a stronger or weaker policy effect. For example, groups who have many students who would dropout (were it not for the MDA) should show stronger treatment effects than groups for whom the MDA makes no difference in their schooling decision. Only 56% of African-American students graduate from high school, compared to 78% of white non-Hispanice (Greene, 2001). Low-income youth are five times more likely than high-income youth to dropout (National Center for Education Statistics, 2012). To allow for differential treatment effects by income and racial minority status, I interact the policy indicators with an indicator for cohorts who are below the median county income per capita or above the median African-American population density.

Income per capita

The property crime results by county income per capita (above or below the national median) are plotted in Figures 1.8 through 1.9. Interestingly, it appears that property crime rates of lower income counties are relatively unaffected by higher MDAs. There does not appear to be any incapacitation or long-run effect for low-income cohorts, while cohorts in the upper half of the income distribution show the same pattern of results as in the base specification: there is a school incapacitation effect that lingers until age 19. For drug crimes, shown in Figures 1.10 and 1.11, there may be a weak incapacitation effect on drug crimes for low income cohorts, but it disappears immediately after the compulsory schooling period at age 18. The results for cohorts at the upper half of the income per capita distribution, however, show an incapacitation effect that persists well into the early 20s.

African-American population density

Estimates by the African-American male share of the population are plotted in Figures 1.12 through 1.15. These results show that the crime rates of cohorts that have a higher concentration of blacks are more sensitive to the MDA than other cohorts. Property crime in cohorts with higher black concentrations experience an incapacitation effect, while cohorts with lower black concentrations are less strongly affected. Figures 1.14 and 1.15 show that drug crime experiences a reduction at all ages for cohorts with higher black concentrations, while other cohorts show a small decrease in crimes during the incapacitation period which disappears completely by age 20.

1.7 Conclusion

This paper informs the current policy debate faced by many states as they consider raising the MDA to 18. I use recent changes in the MDA as a quasi-experiment to estimate the effect of recent MDA policies on the crime-age profile. I measure the effect of the MDA on a cohort's crime through adolescence and early adulthood. I find that higher

compulsory schooling ages decrease male property and drug crimes during adolescence, but the effects diverge across crime types in early adulthood. At age 17, property crime decreases by 9.2%, while drug crime decreases by 19%. The effects on property crime peter out during young adulthood, particularly for the less serious crimes like larceny; the effect is more persistent for serious burglary, theft, and drug crimes. These results add to the evidence for the incapacitation effect of school and show that the human capital effect of compulsory schooling on crime may be limited, since the effect is not consistent. Rather, it appears that extra schooling received as a teenager can decrease cohort crime, but mainly by incapacitating individuals during their teenage years and affecting the trajectory of criminal careers in crimes that require the accumulation of criminal networks and skills.

These results have implications for education policy as a way to control crime and our understanding of criminal career development. This paper adds to evidence from Heckman and Masterov (2007) that early investments in education are highly cost effective for controlling crime. This paper shows that extra schooling received as a teenager does not appear to systematically decrease crime, but it can impede the development of criminal careers by functioning as a program that keeps adolescents away from criminal opportunities. When considering how to invest in programs to deter crime, following Heckman and Masterov (2007), it appears that investments in early childhood would be much more cost-effective than schooling investments in adolescence.

1.8 Chapter 1 Appendix

Figure 1.1: Policy variation: 1972-2009

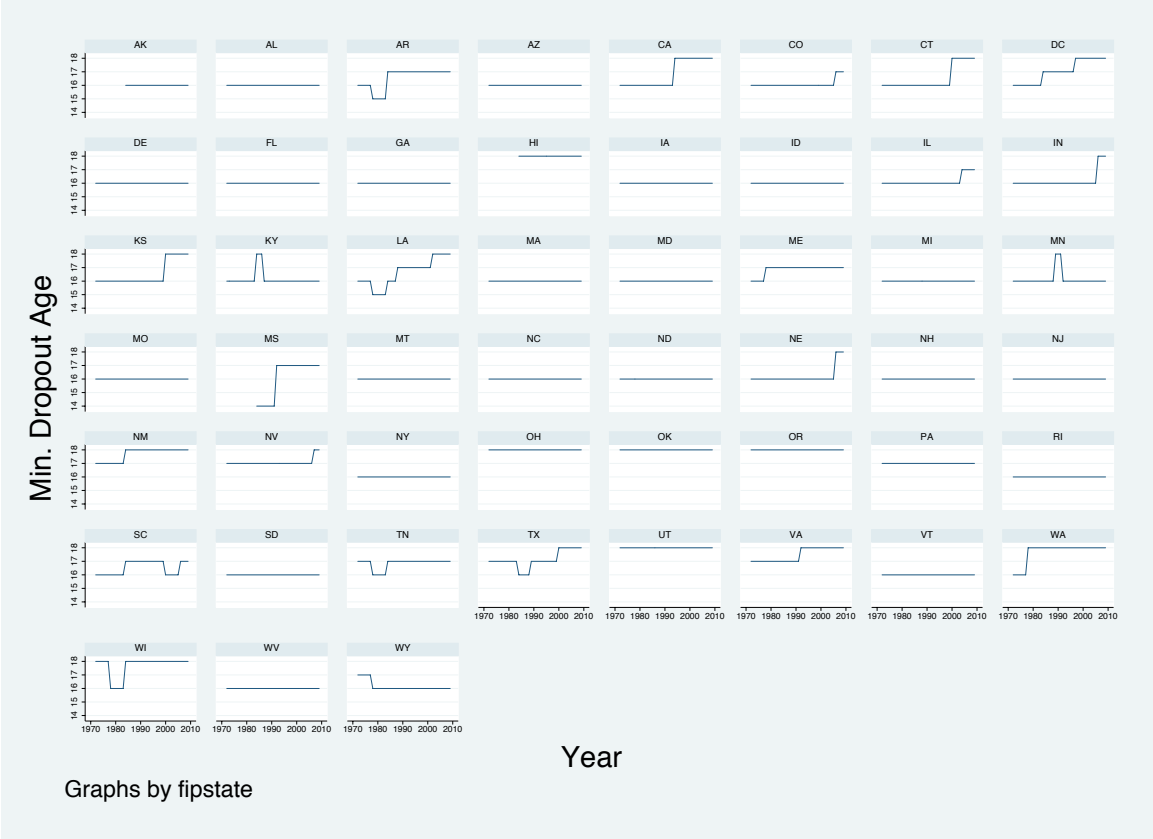
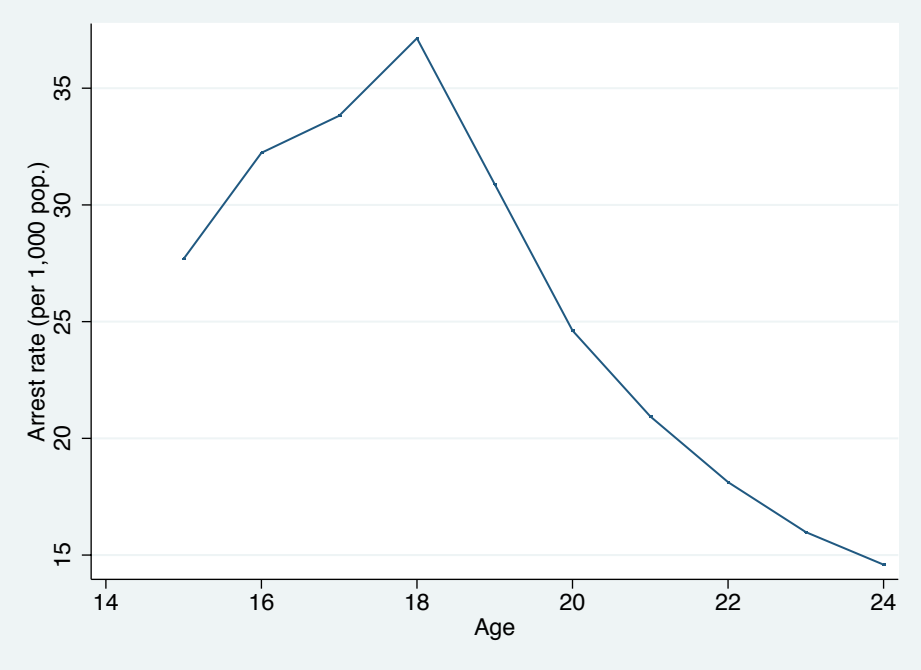
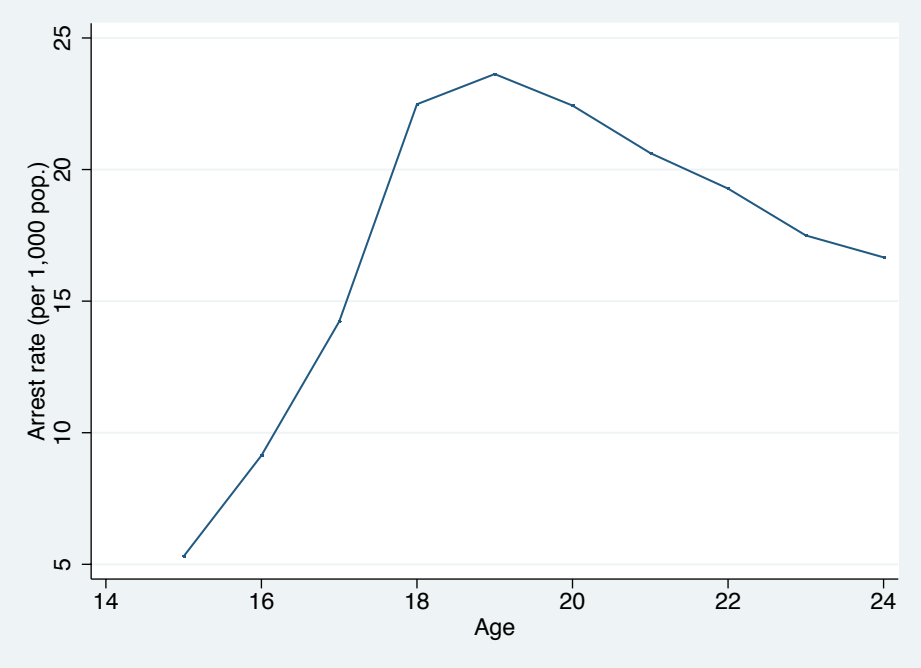


Figure 1.2: Summary crime statistics



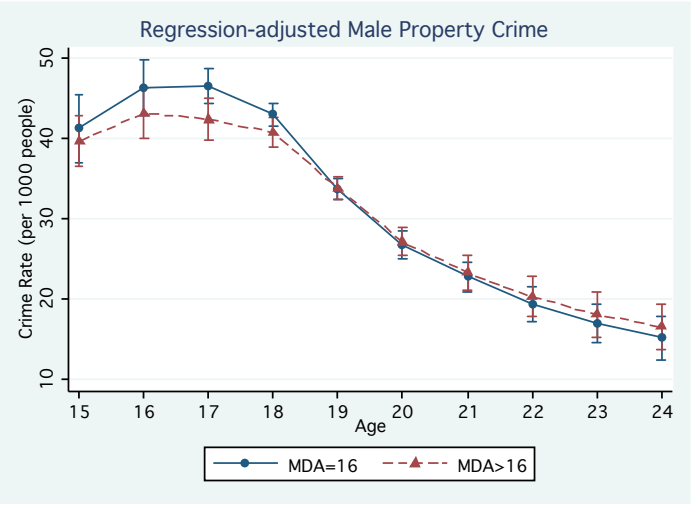
(a) Property crime arrest rate per 1,000 by age



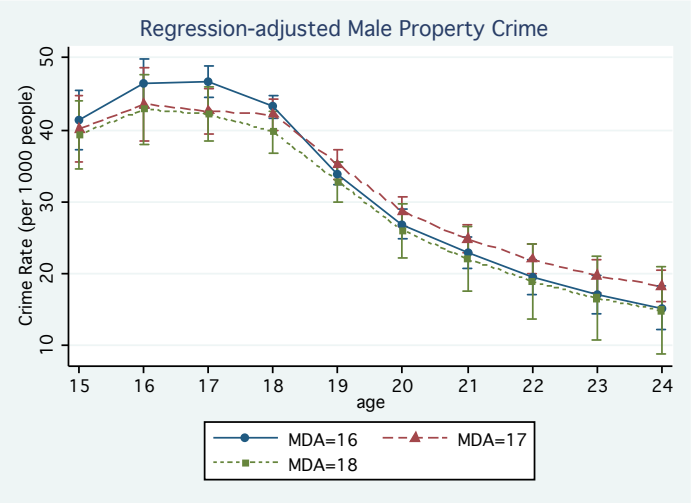
(b) Drug crime arrest rate per 1,000 by age

Unconditional average county-level arrest rate by age. Data source: UCR agency arrest data, 1980-2009. Data is aggregated to the county level.

Figure 1.3: Male property crime rates per 1,000 people, Base specification



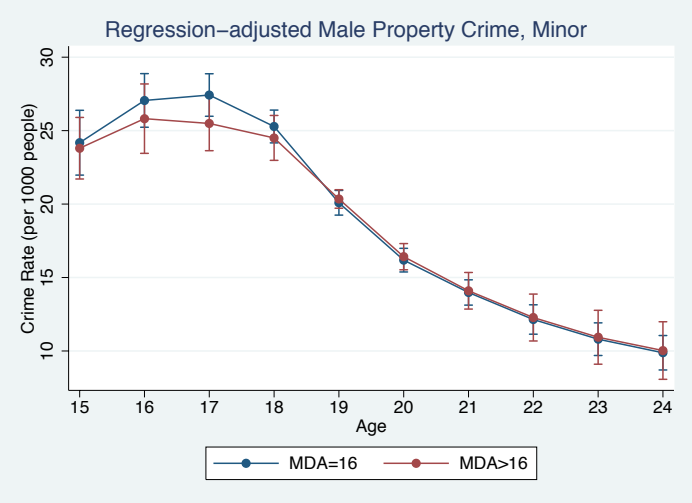
(a) MDA 16 vs. MDA>16



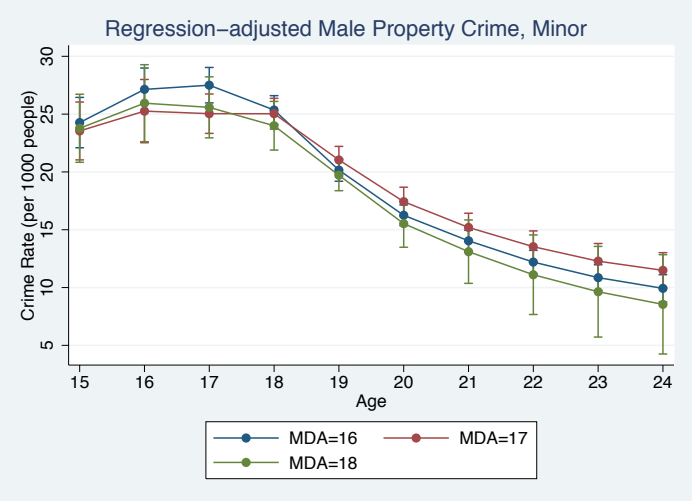
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the male property crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the male property crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.4: Minor male property crime rates per 1,000 people, Base specification



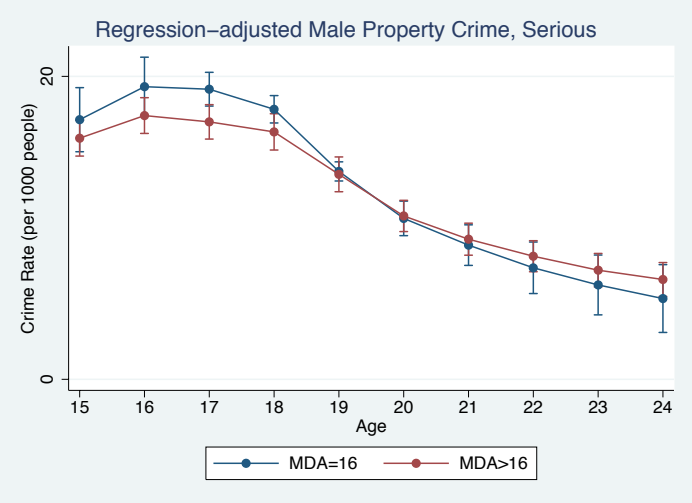
(a) MDA 16 vs. MDA>16



(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the male property crime rate for minor crimes (larceny and arson) at age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the male property crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.5: Serious male property crime rates per 1,000 people, Base specification

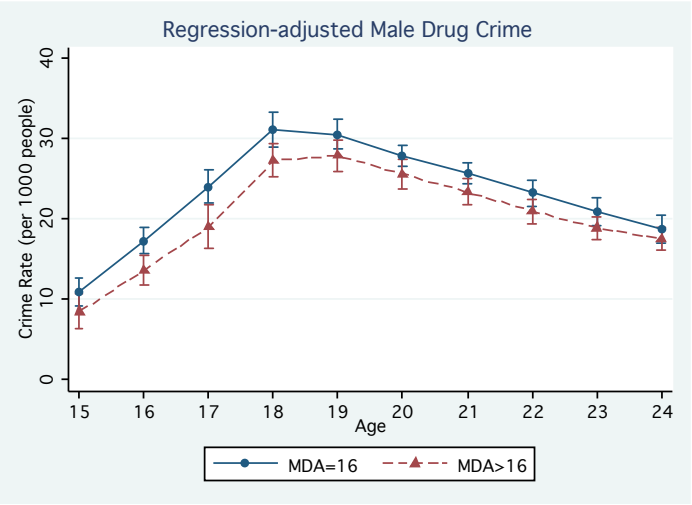


(a) MDA 16 vs. MDA>16

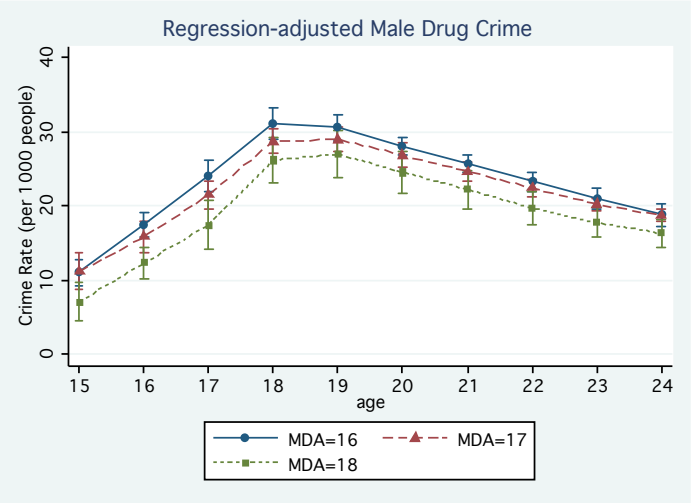
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the male property crime rate for serious crimes (burglary and motor vehicle theft) at age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the male property crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.6: Male drug crime rates per 1,000 people, Base specification



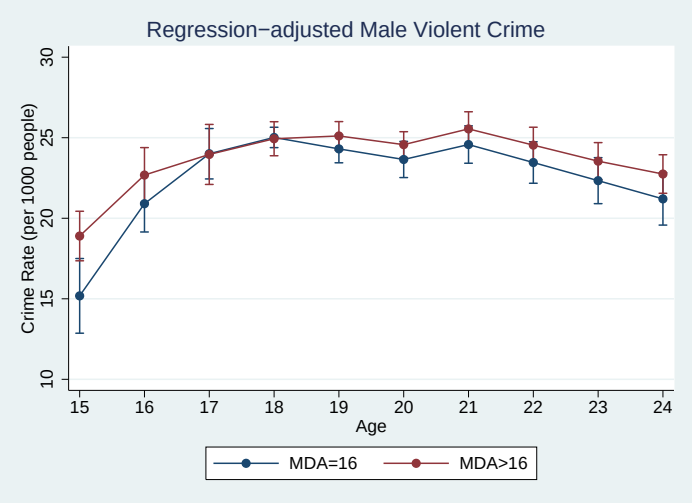
(a) MDA 16 vs. MDA>16



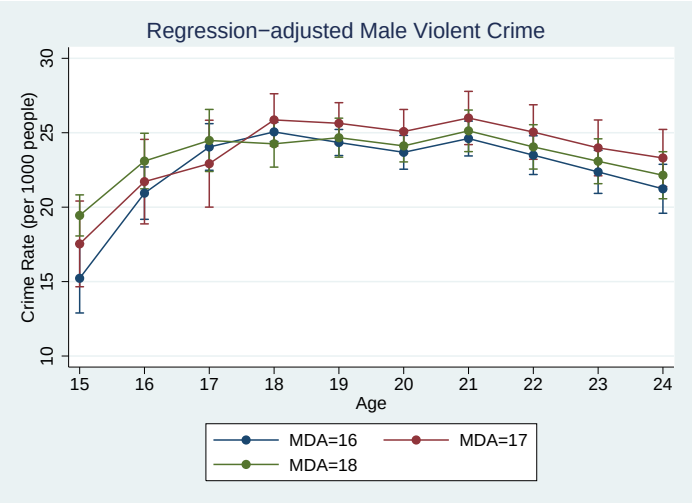
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the male drug crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the male drug crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.7: Male violent crime rates per 1,000 people, Base specification



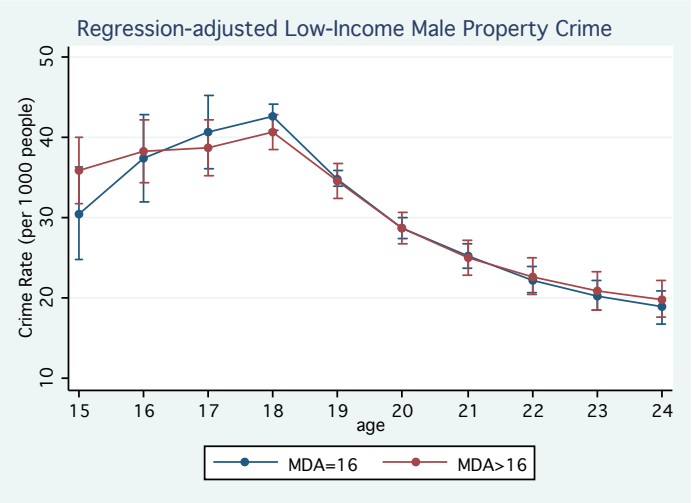
(a) MDA 16 vs. MDA>16



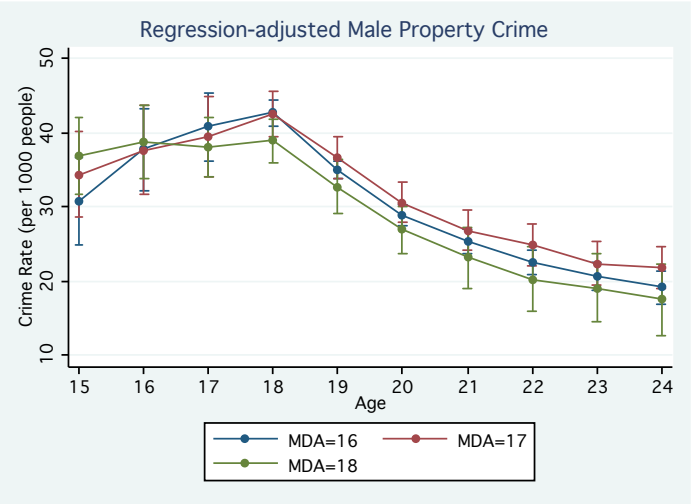
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the male violent crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the male violent crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.8: Male property crime, Low income per capita



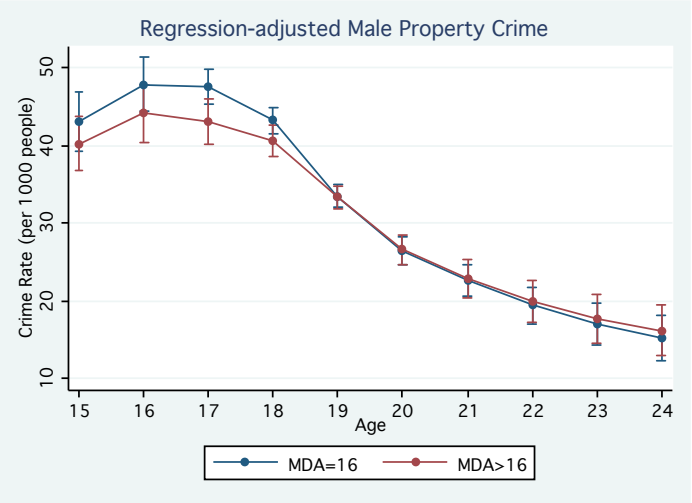
(a) MDA 16 vs. MDA>16



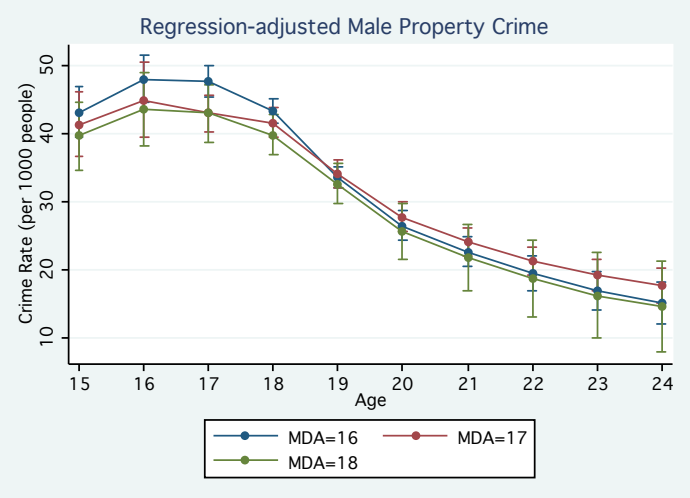
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1) with the addition of an indicator for cohorts in counties above the median income per capita during adolescence interacted with the policy term: the dependent variable is the male property crime rate; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows similar regression results from Equation (1.2): the dependent variable is the male property crime rate; regressors are the same as in the top panel, but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.9: Male property crime, High income per capita



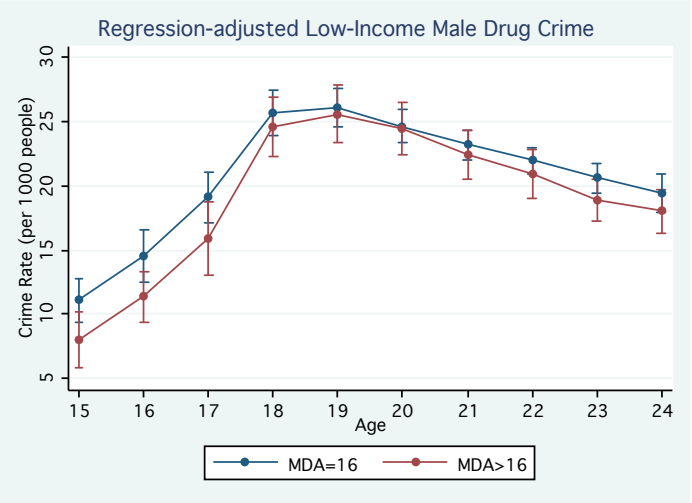
(a) MDA 16 vs. MDA>16



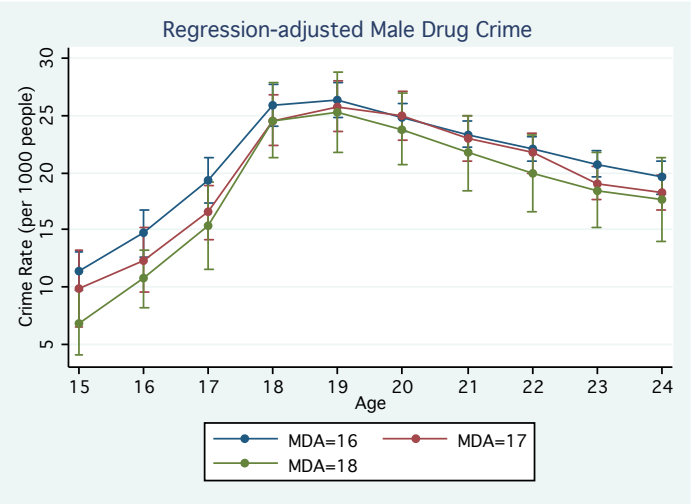
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1) with the addition of an indicator for cohorts in counties above the median income per capita during adolescence interacted with the policy term: the dependent variable is the male property crime rate; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows similar regression results from Equation (1.2): the dependent variable is the male property crime rate; regressors are the same as in the top panel, but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.10: Male drug crime, Low income per capita



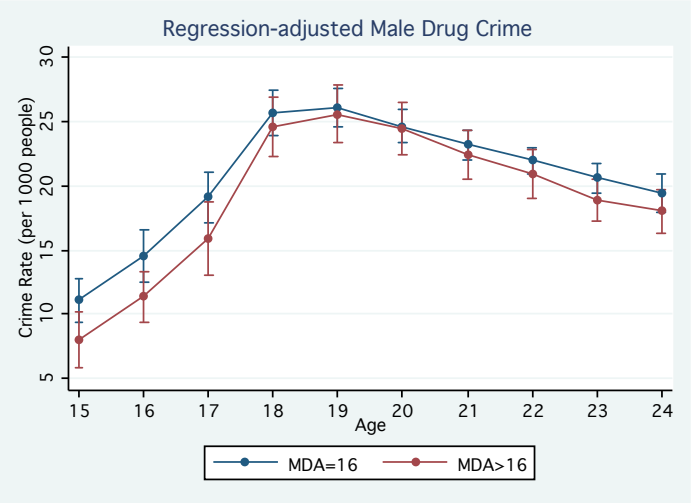
(a) MDA 16 vs. MDA>16



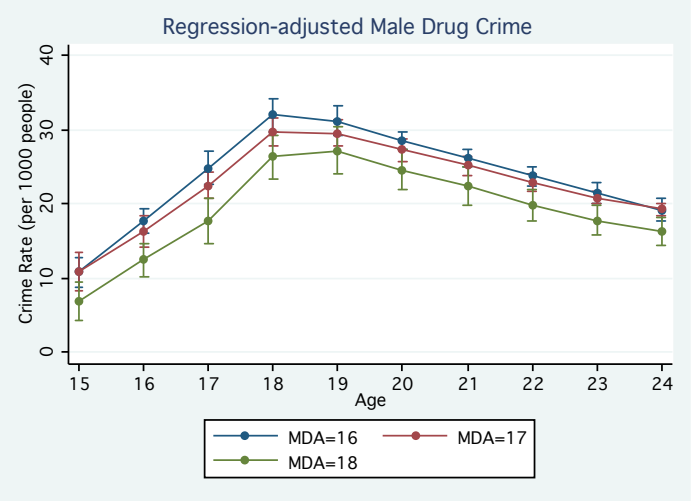
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1) with the addition of an indicator for cohorts in counties above the median income per capita during adolescence interacted with the policy term: the dependent variable is the male drug crime rate; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows similar regression results from Equation (1.2): the dependent variable is the male drug crime rate; regressors are the same as in the top panel, but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.11: Male drug crime, High income per capita



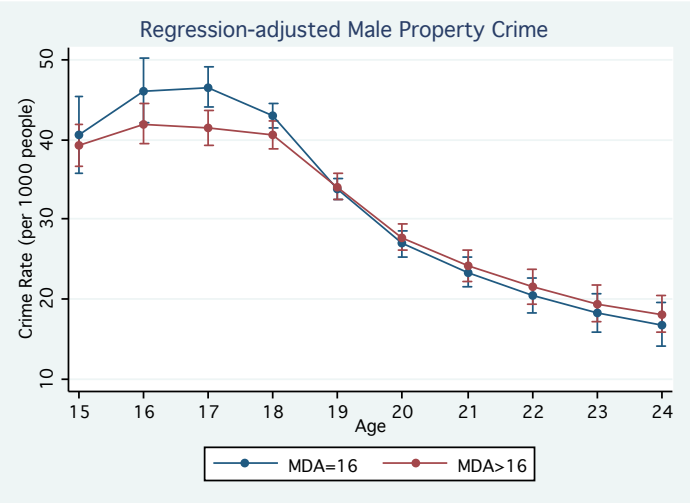
(a) MDA 16 vs. MDA>16



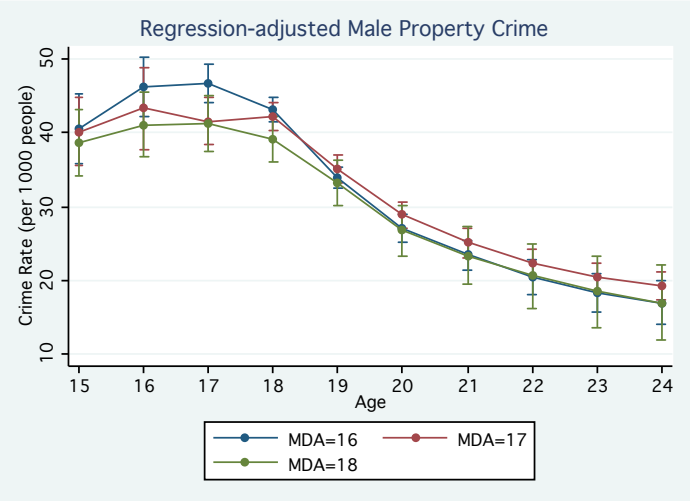
(b) MDA 16 vs. MDA>16

Notes: The top panel shows regression results from Equation (1.1) with the addition of an indicator for cohorts in counties above the median income per capita during adolescence interacted with the policy term: the dependent variable is the male drug crime rate; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows similar regression results from Equation (1.2): the dependent variable is the male drug crime rate; regressors are the same as in the top panel, but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.12: Male property crime, High black population ratio



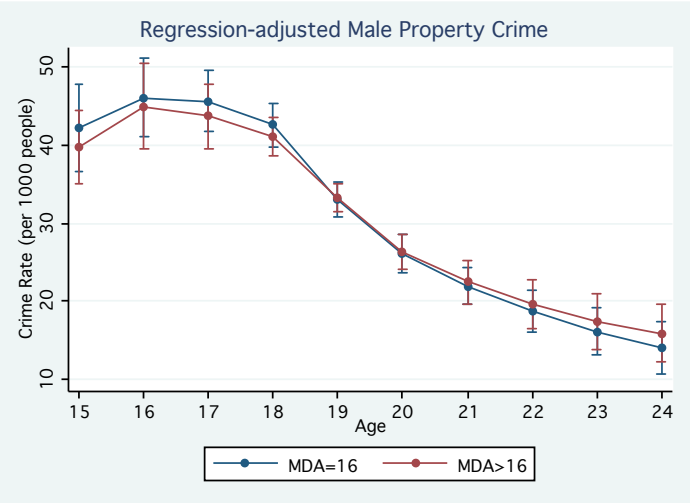
(a) MDA 16 vs. MDA>16



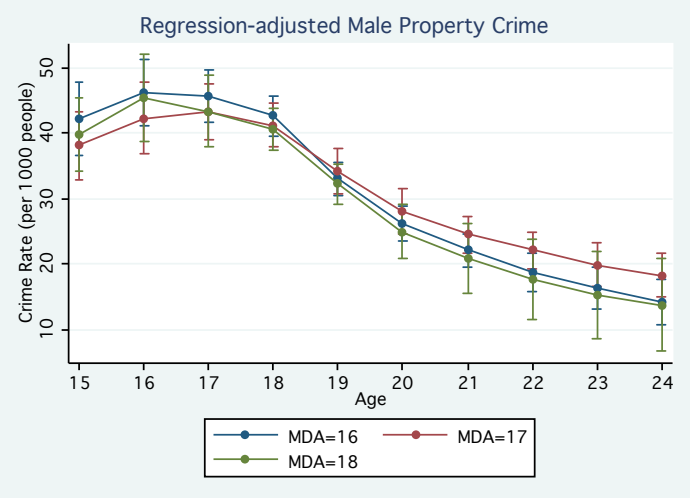
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.13: Male property crime, Low black population ratio



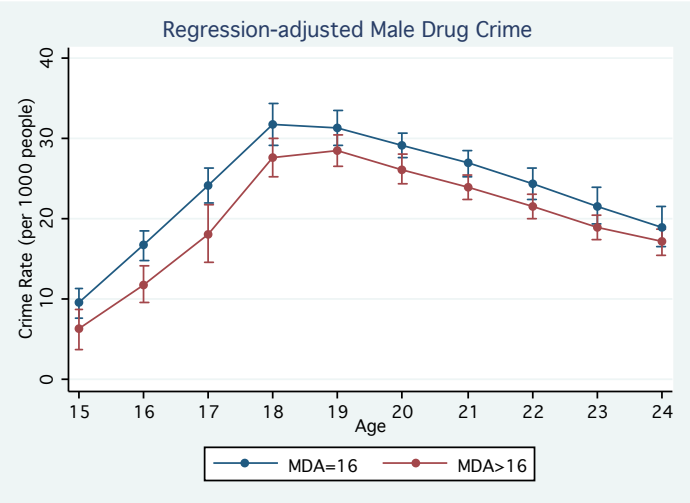
(a) MDA 16 vs. MDA>16



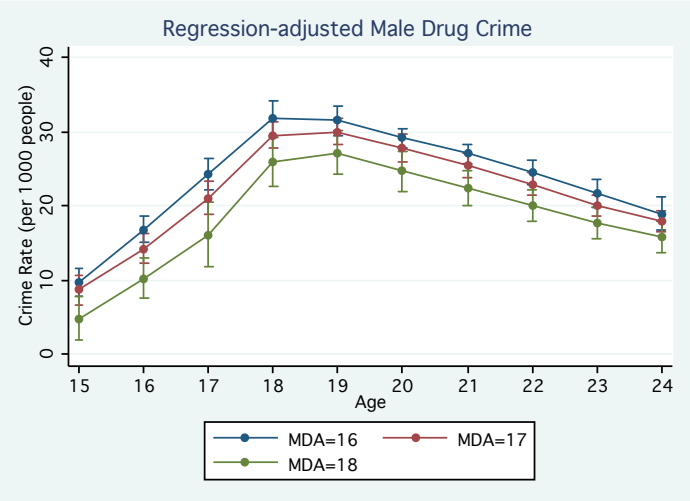
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.14: Male drug crime, High black population ratio



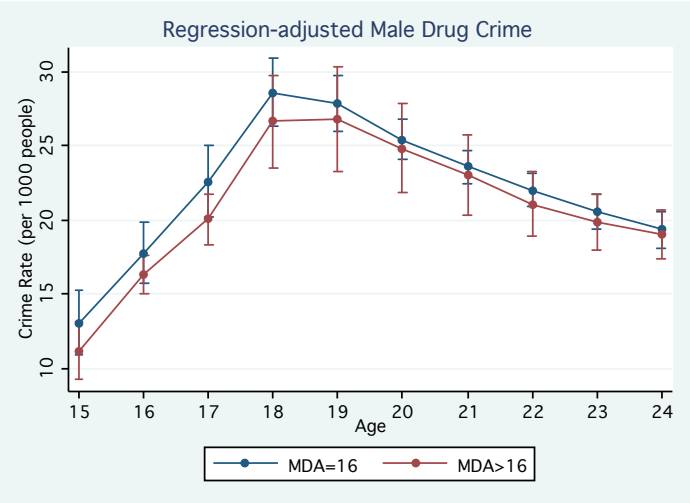
(a) MDA 16 vs. MDA>16



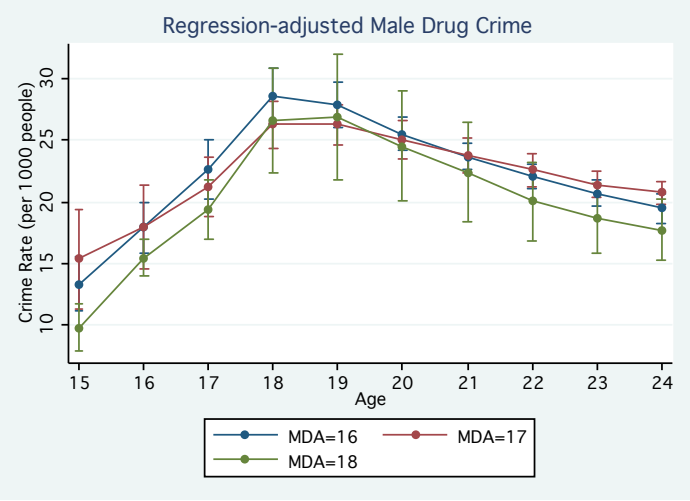
(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Figure 1.15: Male drug crime, Low black population ratio



(a) MDA 16 vs. MDA>16



(b) MDA 16 vs. 17 vs. 18

Notes: The top panel shows regression results from Equation (1.1): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are an indicator for a cohort being treated by an MDA above age 16, county and year fixed effects, state-specific linear time trends, and demographic covariates as described earlier. The bottom panel shows regression results from Equation (1.2): the dependent variable is the female drug crime rate for age a in county c at time t; regressors are the same as in Equation (1.1), but the policy indicator is split into two: one for a cohort being treated by an MDA of age 17 and one for cohort MDA treatment of age 18. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

Table 1.1: Data cohorts by MDA

MDA	County cohorts (N)	Min.	Max.	Average cohort birth year
16	58,296	1956	1991	1973.4
17	18,173	1956	1991	1974.4
18	25,065	1956	1993	1988.7

Cohorts are assigned the MDA that most likely was binding during adolescence, and cohorts for whom this assignment is too unclear are removed from the data, leaving 101,534 cohorts who experienced MDAs of 16, 17, or 18.

Table 1.2: Predicting MDA Increases Using Youth Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)
Current crime	0.058	0.196	-0.062	0.130	-0.074	0.193
	(0.391)	(0.889)	(0.414)	(0.682)	(0.445)	(0.568)
Crime, 1st lag	-0.227	-0.624	-0.084	-0.569	0.042	-0.288
	(0.452)	(0.986)	(0.494)	(0.825)	(0.524)	(0.732)
Crime, 2nd lag	0.147	-0.512	0.236	0.079	0.157	0.006
	(0.481)	(0.921)	(0.535)	(0.916)	(0.488)	(0.723)
Crime, 3rd lag	0.234	0.851	0.260	0.413	0.281	0.095
	(0.462)	(1.096)	(0.499)	(0.938)	(0.452)	(0.706)
Crime, 4th lag	-0.220	-1.293	-0.140	-0.673	-0.090	-0.418
	(0.399)	(0.831)	(0.435)	(0.786)	(0.455)	(0.662)
Crime, 5th lag	-0.008	1.290*	-0.297	0.599	-0.225	0.510
	(0.332)	(0.736)	(0.337)	(0.632)	(0.354)	(0.538)
N	1,117	1,117	1,117	1,117	1,117	1,117

Notes: Standard errors in parentheses. Logistic regression is run on a panel of state-level data from 1980-2009. Marginal effects reported. Dependent variable is an indicator for increasing state MDA in year t. Regressors are age-specific male property or drug rates at time t and 5 lagged periods and state demographic information at time t: total male population, black male population, state employment ratio, and income per per capita. Columns (1) and (2) show the predictive margins for 16-year-old property and drug crime, respectively. Columns (3) and (4) show predictive margins for 17-year-old crime, and (5) and (6) show results for 18-year-old crime.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3: Alternative Specifications of Equation (1.1), Male Property Crime

	(1)	(2)	(3)	(4)	(5)
Age 15, MDA>16	-6.088*	-1.692	-3.614	-1.606	-1.71
	(3.383)	(2.589)	(2.64)	(2.585)	(2.332)
Age 16, MDA>16	-7.785**	-3.221	-5.266**	-3.156	-3.138
	(3.447)	(2.504)	(2.581)	(2.485)	(2.205)
Age 17, MDA>16	-8.835***	-4.094**	-6.287***	-4.102**	-3.976**
	(2.534)	(1.883)	(1.717)	(1.813)	(1.565)
Age 18, MDA>16	-6.633***	-1.882	-4.448***	-2.278*	-2.101*
	(2.547)	(1.376)	(1.519)	(1.303)	(1.112)
Age 19, MDA>16	-4.13**	0.598	-2.046*	0.0596	0.259
	(1.628)	(1.469)	(1.111)	(1.279)	(1.353)
Age 20, MDA>16	-3.793***	0.924	-1.68	0.393	0.628
	(1.403)	(1.796)	(1.327)	(1.609)	(1.792)
Age 21, MDA>16	-3.652***	1.008	-1.511	0.502	0.733
	(1.394)	(1.958)	(1.484)	(1.789)	(1.994)
Age 22, MDA>16	-3.271**	1.359	-1.092	0.894	1.133
	(1.477)	(2.201)	(1.75)	(2.045)	(2.273)
Age 23, MDA>16	-2.976*	1.57	-0.813	1.101	1.327
	(1.539)	(2.425)	(1.973)	(2.267)	(2.495)
Age 24, MDA>16	-2.557	1.911	-0.473	1.404	1.623
	(1.628)	(2.617)	(2.145)	(2.446)	(2.671)
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Covariates	N	N	Y	Y	Y
State linear time trend	N	Y	N	Y	Y
State quadratic time trend	N	N	N	N	Y
R-squared	0.38	0.404	0.395	0.413	0.422
Observations (N)	530,980	530,980	524,421	524,421	524,421
Number of counties	2,933	2,933	2,909	2,909	2,909
Incapacitation F-test	11.47	2.95	10	3.40	3.80
	p = 0.0001	p = 0.0621	p = 0.0002	p = .0417	p = 0.0293
Long-run F-test	2.08	0.89	2.02	1.03	1.10
	p = 0.0726	p = 0.5093	p = 0.0814	p = 0.4206	p = 0.3762

Notes: Standard errors in parentheses. The dependent variable is the male property crime rate for age a in county c at time t. Each column represents are different specification. Covariates included in specifications (3)-(5) demographic covariates as described earlier. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. Incapacitation F-tests are performed for the joint significance of the policy effect at ages 16 and 17. Long-run effects are tested for by joint significance at ages 18 and above. All regressions are weighted by average county population. Standard errors are clustered by state. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

Table 1.4: Alternative Specifications of Equation (1.2), Male Property Crime

	(1)		(2)		(3)		(4)		(5)	
	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18
Age 15, Treated	-5.088	-6.39*	-1.516	-2.113	-1.337	-2.087	-1.394	-2.082	-3.202	-3.849
	(3.828)	(3.437)	(3.124)	(3.141)	(3.109)	(3.12)	(2.759)	(2.652)	(3.288)	(2.997)
Age 16, Treated	-6.547*	-8.204**	-3.113	-3.623	-2.945	-3.628	-2.981	-3.461	-4.727	-5.554*
	(3.673)	(3.694)	(3.093)	(3.2)	(3.056)	(3.169)	(2.673)	(2.608)	(3.179)	(3.149)
Age 17, Treated	-7.509***	-9.32***	-4.2**	-4.389*	-4.08**	-4.487*	-4.153**	-4.131**	-5.759***	-6.568***
	(2.287)	(2.774)	(2.054)	(2.522)	(1.963)	(2.45)	(1.786)	(1.946)	(1.86)	(2.21)
Age 18, Treated	-3.92**	-8.045***	-0.732	-2.977	-1.039	-3.443*	-1.165	-2.963*	-2.605	-5.58***
	(1.964)	(2.575)	(1.369)	(2.019)	(1.369)	(1.916)	(1.111)	(1.624)	(1.632)	(1.767)
Age 19, Treated	-1.415	-5.626***	1.636	-0.463	1.215	-1.09	1.139	-0.598	-0.193	-3.241**
	(1.61)	(1.531)	(1.491)	(2.327)	(1.39)	(2.15)	(1.467)	(2.362)	(1.532)	(1.429)
Age 20, Treated	-0.763	-5.584***	2.187	-0.329	1.754	-0.933	1.648	-0.353	0.456	-3.124
	(1.56)	(1.421)	(1.628)	(2.899)	(1.509)	(2.712)	(1.607)	(3.052)	(1.62)	(1.9)
Age 21, Treated	-0.569	-5.582***	2.293	-0.304	1.855	-0.859	1.714	-0.254	0.627	-3.024
	(1.538)	(1.557)	(1.621)	(3.243)	(1.508)	(3.067)	(1.591)	(3.434)	(1.605)	(2.251)
Age 22, Treated	0.0431	-5.505***	2.81	-0.14	2.405	-0.646	2.239	0.00437	1.246	-2.845
	(1.615)	(1.875)	(1.737)	(3.677)	(1.629)	(3.512)	(1.721)	(3.886)	(1.742)	(2.713)
Age 23, Treated	0.345	-5.345**	3.024	0.0125	2.624	-0.498	2.447	0.146	1.516	-2.646
	(1.759)	(2.153)	(1.919)	(4.036)	(1.805)	(3.87)	(1.9)	(4.25)	(1.919)	(3.091)
Age 24, Treated	0.806	-5.144**	3.436*	0.207	3.027	-0.367	2.86	0.274	1.916	-2.485
	(1.847)	(2.436)	(2.074)	(4.36)	(1.929)	(4.187)	(2.011)	(4.552)	(2.026)	(3.404)
County FE	Y		Y		Y		Y		Y	
Year FE	Y		Y		Y		Y		Y	
Covariates	N		N		Y		Y		Y	
State linear time trend	N		Y		N		Y		Y	
State quadratic time trend	N		N		N		N		Y	
R-squared	0.381		0.405		0.414		0.422		0.396	
Observations (N)	530,980		530,980		524,421		524,421		524,421	
Number of counties	2,933		2,933		2,909		2,909		2,909	
Incapacitation F-test	3.18	10.30	1.01	2.12	0.93	2.51	1.24	2.66	2.21	7.94
	p = 0.0810	p = 0.0002	p = 0.3193	p = 0.1313	p = 0.3400	p = 0.3400	p = 0.2704	p = 0.0800	p = 0.1435	p = 0.0010
Long-run F-test	1.11	2.99	1.58	0.23	1.53	0.31	1.35	0.29	1.30	1.39
	p = 0.3701	p = 0.0147	p = 0.1730	p = 0.9631	p = 0.1893	p = 0.9302	p = 0.2552	p = 0.9405	p = 0.2765	p = 0.2362

Notes: Standard errors in parentheses. The dependent variable is the male property crime rate for age a in county c at time t. Each set of columns represents are different specification based on Equation (1.2), with odd columns showing the coefficients associated with an indicator for an MDA of 17 and even columns showing coefficients for an MDA of 18. Covariates included in specifications (3)-(5) are demographic covariates as described earlier. Incapacitation F-tests are performed for MDA=18 for the joint significance of the policy effect at ages 16 and 17. For MDA=17, the F-test is essentially a t-test at age 16. Long-run effects are tested for by joint significance at ages 19 and above. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01. Standard errors in parentheses.

Table 1.5: Alternative Specifications of Equation (1.1), Male Drug Crime

	(1)	(2)	(3)	(4)	(5)
Age 15, MDA>16	-3.828	-2.61*	-2.45*	-2.531	-1.941
	(2.494)	(1.584)	(1.46)	(1.593)	(1.603)
Age 16, MDA>16	-5.208**	-3.907***	-3.71***	-3.776***	-3.21**
	(2.404)	(1.486)	(1.352)	(1.412)	(1.442)
Age 17, MDA>16	-6.611***	-5.24***	-5.015***	-5.097***	-4.495***
	(2.555)	(1.893)	(1.788)	(1.739)	(1.597)
Age 18, MDA>16	-5.336**	-3.926**	-3.799**	-3.884**	-3.293**
	(2.458)	(1.975)	(1.928)	(1.826)	(1.693)
Age 19, MDA>16	-4.142*	-2.671	-2.652	-2.728*	-2.178
	(2.129)	(1.747)	(1.721)	(1.613)	(1.449)
Age 20, MDA>16	-3.901*	-2.373	-2.321	-2.401*	-1.878
	(2.068)	(1.503)	(1.474)	(1.343)	(1.236)
Age 21, MDA>16	-3.946*	-2.353*	-2.28*	-2.367**	-1.89
	(2.164)	(1.36)	(1.307)	(1.156)	(1.157)
Age 22, MDA>16	-3.948*	-2.29*	-2.212*	-2.269**	-1.875
	(2.353)	(1.376)	(1.32)	(1.164)	(1.276)
Age 23, MDA>16	-3.677	-2.019	-1.94	-2.001*	-1.611
	(2.383)	(1.37)	(1.319)	(1.168)	(1.305)
Age 24, MDA>16	-2.928	-1.293	-1.26	-1.303	-0.943
	(2.403)	(1.421)	(1.387)	(1.232)	(1.378)
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Covariates	N	N	Y	Y	Y
State linear time trend	N	Y	N	Y	Y
State quadratic time trend	N	N	N	N	Y
R-squared	0.298	0.33	0.341	0.353	0.314
Observations (N)	530,980	530,980	524,421	524,421	524,421
Number of counties	2,933	2,933	2,909	2,909	2,909
Incapacitation F-test	3.49	3.98	4.22	4.42	3.96
	p = 0.0386	p = 0.0251	p = 0.0205	p = 0.0173	p = 0.0256
Long-run F-test	4.27	5.34	4.91	4.97	4.22
	p = 0.0016	p = 0.0003	p = 0.0005	p = 0.0005	p = 0.0017

Notes: Standard errors in parentheses. The dependent variable is the male drug crime rate for age a in county c at time t. Each column represents are different specification. Covariates included in specifications (3)-(5) demographic covariates as described earlier. Incapacitation F-tests are performed for the joint significance of the policy effect at ages 16 and 17. Long-run effects are tested for by joint significance at ages 19 and above. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01. Standard errors in parentheses.

Table 1.6: Alternative Specifications of Equation (1.2), Male Drug Crime

	(1)		(2)		(3)		(4)		(5)	
	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18
Age 15, Treated	-0.706	-5.024**	-0.257	-3.939**	0.174	-3.929***	0.88	-4.281***	0.604	-3.016*
	(1.63)	(2.558)	(1.596)	(1.619)	(1.491)	(1.513)	(1.646)	(1.48)	(1.352)	(1.81)
Age 16, Treated	-2.389	-6.379**	-2.053	-5.128***	-1.583	-5.098***	-0.95	-5.443***	-1.065	-4.2**
	(1.456)	(2.492)	(1.423)	(1.538)	(1.329)	(1.413)	(1.373)	(1.308)	(1.222)	(1.656)
Age 17, Treated	-3.387***	-8.139***	-3.169**	-6.716***	-2.639*	-6.69***	-2.147*	-7.038***	-1.998*	-5.791***
	(1.209)	(2.745)	(1.522)	(2.065)	(1.466)	(1.947)	(1.292)	(1.684)	(1.191)	(1.872)
Age 18, Treated	-3.001*	-6.442***	-2.896*	-4.851**	-2.416	-4.968**	-2.065	-5.27***	-1.673	-4.145**
	(1.795)	(2.458)	(1.541)	(2.309)	(1.521)	(2.237)	(1.279)	(2.023)	(1.611)	(1.874)
Age 19, Treated	-2.18	-5.061**	-2.014	-3.377	-1.623	-3.626	-1.308	-3.899*	-0.913	-2.835
	(1.697)	(2.156)	(1.329)	(2.265)	(1.328)	(2.208)	(1.157)	(2.023)	(1.519)	(1.755)
Age 20, Treated	-1.666	-5.061**	-1.432	-3.299*	-1.113	-3.444*	-0.867	-3.683**	-0.444	-2.681*
	(1.552)	(1.974)	(1.058)	(1.939)	(1.065)	(1.875)	(0.879)	(1.632)	(1.319)	(1.428)
Age 21, Treated	-1.58	-5.269***	-1.281	-3.402**	-1.053	-3.447**	-0.856	-3.674***	-0.434	-2.744**
	(1.604)	(1.93)	(0.91)	(1.664)	(0.872)	(1.586)	(0.678)	(1.279)	(1.224)	(1.188)
Age 22, Treated	-1.355	-5.53***	-1.006	-3.545**	-0.819	-3.546**	-0.655	-3.704***	-0.248	-2.907**
	(1.669)	(2.102)	(0.885)	(1.499)	(0.839)	(1.393)	(0.677)	(1.002)	(1.241)	(1.147)
Age 23, Treated	-1.175	-5.271**	-0.859	-3.204**	-0.707	-3.17**	-0.587	-3.309***	-0.116	-2.576**
	(1.65)	(2.185)	(0.842)	(1.452)	(0.802)	(1.332)	(0.711)	(0.902)	(1.225)	(1.165)
Age 24, Treated	-0.455	-4.612**	-0.179	-2.483*	-0.0729	-2.492*	0.0269	-2.581***	0.534	-1.951
	(1.611)	(2.267)	(0.872)	(1.501)	(0.847)	(1.385)	(0.768)	(0.958)	(1.236)	(1.256)
County FE	Y		Y		Y		Y		Y	
Year FE	Y		Y		Y		Y		Y	
Covariates	N		N		Y		Y		Y	
State linear time trend	N		Y		N		Y		Y	
State quadratic time trend	N		N		N		N		Y	
R-squared	0.3		0.331		0.342		0.355		0.315	
Observations (N)	530,980		530,980		524,421		524,421		524,421	
Number of counties	2,933		2,933		2,909		2,909		2,909	
Incapacitation F-test	2.69	4.44	2.08	5.93	1.42	6.90	0.48	10.02	0.76	4.79
	p = 0.1074	p = 0.0170	p = 0.1556	p = 0.0050	p = 0.2395	p = 0.0023	p = 0.4923	p = 0.0002	p = 0.3877	p = 0.0127
Long-run F-test	2.40	7.42	2.82	9.28	2.46	9.36	2.19	12.71	2.40	7.97
	p = 0.0411	p = 0.0000	p = 0.0197	p = 0.0000	p = 0.0372	p = 0.0000	p = 0.0601	p = 0.0000	p = 0.0413	p = 0.0000

Notes: Standard errors in parentheses. The dependent variable is the male property crime rate for age a in county c at time t. Each set of columns represents are different specification based on Equation (1.2), with odd columns showing the coefficients associated with an indicator for an MDA of 17 and even columns showing coefficients for an MDA of 18. Covariates included in specifications (3)-(5) are demographic covariates as described earlier. Incapacitation F-tests are performed for MDA=18 for the joint significance of the policy effect at ages 16 and 17. For MDA=17, the F-test is essentially a t-test at age 16. Long-run effects are tested for by joint significance at ages 19 and above. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01. Standard errors in parentheses.

Table 1.7: Alternative Specifications of Equation (1.1), Male Violent Crime

	(1)	(2)	(3)	(4)	(5)
Age 15, MDA>16	0.968	3.251**	2.915**	4.952**	4.141***
	(1.359)	(1.33)	(1.219)	(2.011)	(1.31)
Age 16, MDA>16	-0.0186	2.237*	0.865	1.778	2.202**
	(1.282)	(1.239)	(0.999)	(1.188)	(1.107)
Age 17, MDA>16	-1.564	0.676	-0.954	-0.0436	0.371
	(1.251)	(1.349)	(0.957)	(1.236)	(1.160)
Age 18, MDA>16	-1.282	0.888	-0.962	-0.0808	0.309
	(1.616)	(0.995)	(0.798)	(0.757)	(0.640)
Age 19, MDA>16	-0.36	1.762	-0.0447	0.803	1.169
	(1.475)	(1.081)	(0.747)	(0.816)	(0.795)
Age 20, MDA>16	-0.233	1.859*	0.0835	0.921	1.253
	(1.506)	(1.115)	(0.825)	(0.895)	(0.865)
Age 21, MDA>16	-0.124	1.897*	0.161	0.972	1.25
	(1.679)	(1.148)	(0.962)	(0.981)	(0.915)
Age 22, MDA>16	-0.0537	1.905	0.282	1.08	1.338
	(1.732)	(1.171)	(1.027)	(1.024)	(0.974)
Age 23, MDA>16	0.0466	1.951	0.45	1.21	1.42
	(1.721)	(1.202)	(1.076)	(1.077)	(1.049)
Age 24, MDA>16	0.387	2.265*	0.794	1.543	1.734
	(1.693)	(1.247)	(1.128)	(1.139)	(1.140)
Covariates	N	N	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
State linear time trend	N	Y	N	Y	Y
State quadratic time trend	N	N	N	N	Y
R-squared	0.089	0.17	0.147	0.185	0.203
Observations (N)	617,354	617,354	532,914	532,914	532,914
Number of counties	2,969	2,969	2,932	2,932	2,932
Incapacitation F-test	5.18	7.12	7.38	7.83	8.82
	p = 0.0092	p = 0.002	p = 0.0016	p = 0.0011	p = 0.0005
Long-run F-test	1.42	1.42	1.36	1.31	1.35
	p = 0.2176	p = 0.2177	p = 0.2447	p = 0.2656	p = 0.2502

Notes: Standard errors in parentheses. The dependent variable is the male violent crime rate for age a in county c at time t. Each column represents are different specification. Covariates included in specifications (3)-(5) demographic covariates as described earlier. Incapacitation F-tests are performed for the joint significance of the policy effect at ages 16 and 17. Long-run effects are tested for by joint significance at ages 19 and above. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01. Standard errors in parentheses.

Table 1.8: Alternative Specifications of Equation (1.2), Male Violent Crime

	(1)		(2)		(3)		(4)		(5)	
	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18	MDA = 17	MDA = 18
Age 15, Treated	-1.198	1.904	2.123	3.594***	0.625	3.869***	2.311	4.223***	3.085*	4.481***
	(1.591)	(1.528)	(1.824)	(1.287)	(1.743)	(1.184)	(1.812)	(1.216)	(1.868)	(1.26)
Age 16, Treated	-1.767	0.843	1.325	2.536*	-0.878	1.655	0.772	2.155*	1.568	2.374**
	(1.584)	(1.441)	(1.807)	(1.305)	(1.397)	(1.019)	(1.683)	(1.242)	(1.546)	(1.18)
Age 17, Treated	-3.238**	-0.623	-0.356	1.104	-2.677*	-0.074	-1.122	0.437	-0.428	0.670
	(1.624)	(1.549)	(1.866)	(1.454)	(1.465)	(1.021)	(1.722)	(1.315)	(1.678)	(1.246)
Age 18, Treated	-0.887	-1.613	1.78	0.101	-0.63	-1.309	0.806	-0.799	1.371	-0.543
	(1.449)	(1.9)	(1.271)	(1.215)	(1.018)	(0.955)	(1.069)	(0.976)	(0.844)	(0.843)
Age 19, Treated	-0.143	-0.58	2.337*	1.167	-0.0412	-0.185	1.289	0.323	1.813*	0.558
	(1.412)	(1.805)	(1.241)	(1.276)	(1.003)	(0.899)	(0.985)	(0.993)	(1.025)	(0.927)
Age 20, Treated	0.192	-0.611	2.513*	1.191	0.151	-0.108	1.396	0.433	1.805	0.687
	(1.497)	(1.859)	(1.364)	(1.262)	(1.16)	(0.946)	(1.129)	(1.005)	(1.142)	(0.924)
Age 21, Treated	0.399	-0.593	2.541*	1.215	0.193	-0.015	1.38	0.517	1.693	0.744
	(1.663)	(2.037)	(1.456)	(1.301)	(1.325)	(1.072)	(1.27)	(1.099)	(1.231)	(0.948)
Age 22, Treated	0.695	-0.732	2.644*	1.118	0.43	-0.000505	1.551	0.554	1.789	0.803
	(1.731)	(2.121)	(1.533)	(1.326)	(1.388)	(1.163)	(1.341)	(1.143)	(1.313)	(0.978)
Age 23, Treated	0.785	-0.665	2.628*	1.183	0.549	0.191	1.615	0.717	1.786	0.939
	(1.759)	(2.135)	(1.58)	(1.347)	(1.444)	(1.203)	(1.4)	(1.176)	(1.38)	(1.019)
Age 24, Treated	1.308	-0.546	3.083*	1.328	1.026	0.392	2.071	0.91	2.223	1.115
	(1.831)	(2.139)	(1.671)	(1.374)	(1.517)	(1.272)	(1.488)	(1.231)	(1.501)	(1.102)
County FE	Y		Y		Y		Y		Y	
Year FE	Y		Y		Y		Y		Y	
Covariates	N		N		Y		Y		Y	
State linear time trend	N		Y		N		Y		Y	
State quadratic time trend	N		N		N		N		Y	
R-squared	0.091		0.171		0.148		0.186		0.203	
Observations (N)	617,354		617,354		532,914		532,914		532,914	
Number of counties	2,969		2,969		2,932		2,932		2,932	
Incapacitation F-test	1.24	4.63	0.54	7.73	0.39	7.78	0.21	8.42	1.03	9.09
	p = 0.2703	p = 0.0145	p = 0.4671	p = 0.0012	p = 0.5328	p = 0.0012	p = 0.6484	p = 0.0007	p = 0.3155	p = 0.0005
Long-run F-test	0.97	2.47	1.45	2.35	1.33	2.52	1.28	2.59	1.33	2.40
	p = 0.4636	p = 0.0299	p = 0.207	p = 0.0379	p = 0.2585	p = 0.0274	p = 0.2807	p = 0.0238	p = 0.2549	p = 0.0343

Notes: Standard errors in parentheses. The dependent variable is the male violent crime rate for age a in county c at time t. Each set of columns represents are different specification based on Equation (1.2), with odd columns showing the coefficients associated with an indicator for an MDA of 17 and even columns showing coefficients for an MDA of 18. Covariates included in specifications (3)-(5) are demographic covariates as described earlier. Incapacitation F-tests are performed for MDA=18 for the joint significance of the policy effect at ages 16 and 17. For MDA=17, the F-test is essentially a t-test at age 16. Long-run effects are tested for by joint significance at ages 19 and above. Data are from the 1980-2009 agency-level FBI Uniform Crime Reports aggregated to the annual, county level. All regressions are weighted by average county population. Standard errors are clustered by state.

* p ¡ 0.1, ** p ¡ 0.05, *** p ¡ 0.01. Standard errors in parentheses.

Chapter 2

Crime and the Minimum Wage

2.1 Introduction

Does raising the minimum wage have the unintended effect of increasing crime? Research in "the new economics of the minimum wage" shows that although increases in the minimum wage have a small net effect on employment, the absence of a net effect conceals displacement of lower-skilled workers as employers substitute toward higher-skilled workers.[1] There is growing evidence that raising the minimum wage causes higher levels of unemployment among youth and workers with weak labor attachment.[2] Moreover, increases in the minimum wage raise the probability that teenagers will be idle: they are more likely to leave school and, conditional on not being in school, more likely to be unemployed.[3] We ask: Do some youth influenced by a change in the minimum wage turn to crime?

Economic reasoning and literature lead us to believe that the answer is yes. Numerous studies have shown that idle youth are more likely to engage in crime, whether because they are not in school or not working.[4] The existing evidence for the causal relationship between

[1] Neumark and Wascher (2006) conduct a review of studies that examine the employment effect of changes in the minimum wage.

[2] Currie and Fallick (1996), Ahn et al. (2011), Burkhauser et al. (2000)

[3] Neumark and Wascher (1995) use matched CPS data to study the effect of minimum wages on employment and enrollment decisions of youth. They find that increases in the minimum wage raise the likelihood that lower-skilled teenagers will become unemployed, replaced by higher-skilled teenagers who leave school. They also find an increase in the probability that displaced workers will be not only unemployed but also not enrolled in school.

[4] Jacob and Lefgren (2003) and Luallen (2006) estimate that daily juvenile property crime decreases by

the minimum wage and crime is somewhat limited, however. Hashimoto (1987) provides national time-series evidence that a positive relationship between the minimum wage and crime does exist in the United States. A limitation, however, of using nationally aggregated data to examine crime is that much of the variation in crime is lost. Additionally, national changes in the minimum wage may not be exogenous with respect to low-skill labor markets and crime-employment trends. We expand on the evidence by using micro-level panel data on the criminal activity of minimum wage workers.

We employ panel data from the National Longitudinal Survey of Youth 1997 (NLSY97) cohort to identify the effect of changes in the minimum wage on participation in crime. We estimate the effect of an increase in the minimum wage on an affected worker's probability of committing crime. We exploit changes in state and federal minimum wage laws between 1997 and 2010. The NLSY97 data allows our study to make several contributions. First, due to the detailed nature of the employment and crime history in the data, we are able to test if movement in or out of crime is due to changes in employment status. Next, the fine level at which the NLSY is collected allows us to control for levels of heterogeneity that would otherwise be lost at higher levels of aggregation. For instance, unobservable ability might be correlated with both criminal behavior and being employed at the minimum wage. Our ability to control for such heterogeneity suggests that our work can be considered a micro-level complement to Hashimoto (1987). Lastly, the data allows us to directly identify individuals who were bound by changes in the minimum wage, rather than approximating the treatment group based on general demographics.

We find compelling evidence that an increase in the minimum wage both displaces youth from licit employment and increases criminal activity among not only the unemployed but also the employed. In particular, crimes related to monetary gain: drug sale and stealing both increase. Our estimates show that workers who are affected by a change in the minimum wage are 1.5 to 1.7 percentage points more likely to be idle and 2.4 percentage points less

14% and 28.8% on days that students must be in school, respectively. Raphael and Winter-Ember (2001) and Gould et al. (2002a) find that declining labor opportunities cause an increase in crime. In particular, Raphael and Winter-Ember (2001) find that an increase in unemployment causes a rise in property crimes, which are crimes often associated with illicit income.

likely to be employed. Moreover, the probability of being unemployed and committing property theft increases by 0.1 percentage points (the average probability is 3.85%). The probability of being employed and stealing increases by 0.7 percentage points (relative to an average probability of 7.53%).

These findings have implications for policy regarding both the low-wage labor market and criminal activity. Our results raise the hope of using policies that encourage employment to reduce crime in the short and long term, given that current market work both decreases current criminal activity and raises the opportunity costs of future crime.[5] The findings also point toward the short and long-term dangers of policies which increase unemployment among those on the margin of licit and illicit work. Regardless of overall net-employment effects, it appears minimum wage increases also increase crime. Given the contemporaneous costs of crime and especially the long-term consequences (generating "criminal" human capital, future arrests and recidivism), minimum wages as a policy for fighting poverty appear quite unattractive along this dimension.

2.2 Data

The National Longitudinal Survey of Youth is an ideal data set for studying the effects of the minimum wage on crime because it allows us to identify workers affected by changes in the minimum wage and control for individual-level heterogeneity. It is an annual survey that collects detailed information about youth educational and labor market experiences, as well as family background, relationships, and personal behavior (e.g. criminal behavior).

The NLSY97 follows a cohort of nearly 9,000 respondents who were between the ages of 12 to 16 years old as of 1997. The data spans from 1997-2010, during which time there were four increases in the Federal minimum wage and several changes at the state level. We use NLSY97 data linked to confidential state geocoded information to match respondents to the binding minimum wage during each survey wave as in Currie and Fallick (1996). The

[5]Raphael and Winter-Ember (2001), Gould et al. (2002b), and Machin and Meghir (2004) demonstrate that criminal activity responds to both employment and wages.

binding minimum wage in a given state-year is determined by the maximum of the state and federal minimum wage at that time.

We are able to identify individuals affected by changes in the binding minimum wage by observing employment histories and wages. Bound workers are identified by three criteria. First, they must have lived in a state that experienced a change in the minimum wage during the years directly before and after the change. Second, in the year preceding a minimum wage increase, the individual must have been employed in a job where his/her nominal wage was less than the upcoming nominal minimum wage but not less than the current minimum wage. Lastly, the minimum wage job must be in an industry covered by the minimum wage. In our data, we consider jobs reported as agricultural, military, self-employed, or public administration to be uncovered.[6]

In each wave, survey respondents were asked about their participation in criminal activity, including selling illegal drugs and stealing. We are confident in the use of self-reported criminal activity: self reports have been found to be accurate representations of official crime reports (Hindelang, 1981). Further, we don't want to use arrest data because of the possible endogeneity of policy changes and policing.[7] We denote criminal activity by an indicator that respondents reported having engaged in selling drugs or stealing since the date of their last interview. For respondents who ever report criminal activity, missing values are replaced by zeros under the assumption that any lack of response is due to inactivity.[8]

Table 2.1 provides summary statistics of employment, wage, and individual characteristics for the full data sample as well as by age. Approximately 60 percent of the individual-year observations have wage information that can be used to assign minimum wage worker status. Of those, approximately 4% are records for individuals in a state and year where they were bound by a minimum wage change. Minimum wage employment is most common at ages 14-19, as is generally known to be the case (Bur, 2011). The crime statistics reflect the usual

[6]Respondents to the NLSY97 report up to 11 wages in a given survey year. A respondent is considered to be bound by a minimum wage change if at least one of the reported jobs fits the aforementioned criteria. Jobs with reported wages of zero dollars are considered invalid and excluded from the data.

[7]We are unable to make use of national incident-based data, such as National Incident-Based Reporting System data, due to changes in jurisdictional reporting during the time period that we study.

[8]Alternative treatments of missing values did not impact our analysis.

age-crime profile, with highest criminal activity occurring during teenage years. Wage and hour information based on the job at which respondents work the most hours per week show increasing wages and hours with age.

2.3 Empirical Strategy

We are interested in whether a change in the minimum wage causes bound workers to turn to crime, possibly from losing their jobs and becoming idle. We first separately estimate how changes in the minimum wage contemporaneously affect the probabilities that affected workers will commit crime and become unemployed. Our basic estimating equation in the crime regressions is

$$\mathbb{1}\{Crime_{it}^{c}=1\}=\alpha_0^c+\sum_{a=1}^{4}\beta_a^c\mathbb{1}\{MWBound_{it}\}\times\mathbb{1}\{agegrp_{it}=a\}+\gamma^c X_{it}+e_{it}^c \qquad (2.1)$$

where the dependent variable is an indicator that respondent i committed crime c (drug sale, property theft) in year t, the year of the minimum wage increase. The treatment indicator for being bound by the minimum wage change is defined above. The treatment is interacted with a vector of indicators for age groups defined as 14-16, 17-19, 20-24, and 25-30 years old to allow the effect of the minimum wage change to vary across ages. X_{it} includes gender, race, and vectors of year fixed effects and age fixed effects to control for national trends in crime and the crime-age profile. These variables will absorb any fixed differences in the propensity towards crime or under-reporting of crime associated with any of these characteristics. The coefficients of interest are the vector of βs.

Low-wage Worker and Minimum Wage Change Effects One concern with the baseline estimates may be that they miss a low-wage worker effect or an endogeneity problem with increases in the minimum wage. The low-wage worker effect would be present if workers who receive low remuneration from licit labor are more likely to commit crime or lose their

job, regardless of whether they are affected by a change in the minimum wage. The policy endogeneity problem may occur if states that raise the minimum wage are also the states with the largest crime, employment, or enrollment problems. In both cases, our minimum wage worker indicator alone cannot differentiate these effects from our focus: the effect of a minimum wage increase on workers bound by the change.

To address this problem, we also specify models that include an indicator for low-wage workers ($LowWage$) and for living in a state where the minimum wage increased ($ChangeMW$). In the event that some changing states have higher crime rates than non-changing states, we also include state fixed effects. If any of these effects is the driving factor behind what we observe in our baseline results, the coefficient on $MWBound$ should lose significance with the appropriate indicators absorbing the effect. Low wage workers are defined as individuals who had a wage within \$0.36 of the binding minimum wage, even if there is no change in the minimum wage. We use \$0.36 because on average, workers bound by the minimum wage have wages \$0.36 below the new binding minimum wage. We also add controls for observable individual-level characteristics, such as ability measured by math PIAT score in 1997, mother's education, and household income in 1997.

Individual Fixed Effects In the event that there are unobservable, time-invariant characteristics (δ_i^c) that are associated with an individual's likelihood of committing crime or experiencing a binding minimum wage change, the NLSY data allow us to estimate a specification including individual fixed effects. This specification takes the following form:

$$\mathbb{1}\{Crime_{it}^c = 1\} = \alpha_0^c + \sum_{a=1}^{4} \beta_a^c \mathbb{1}\{MWBound_{it}\} \times \mathbb{1}\{agegrp_{it} = a\} + \gamma^c X_{it} + \delta_i^c + e_{it}^c \quad (2.2)$$

The estimating equations for employment uses the same form, replacing the dependent variable with indicators for employment after the wage change.[9]

The estimation procedure is limited to individuals who were working in the year before

[9]Missing data for employment are replaced as zeros for respondents who have reported this information in any other year of the survey.

the change in the minimum wage. Individuals who were not working cannot be included because it is impossible to assign minimum wage worker status to someone who has no reported wages. Moreover, we are interested in movement from licit to illicit labor, so the correct starting group is individuals involved in licit labor.

2.4 Results

2.4.1 Employment Effects

We first present estimates of the effect of the minimum wage on employment using both OLS and logit specifications. OLS estimates provide interpretable marginal effect measures of the minimum wage on the probability of each of these crimes. We also present logit estimates take into account the non-linearities associated with the binomial dependent variable.

Disemployment The top panel of Table 2.2 presents OLS estimates, while the bottom panel presents logit estimates. In each panel, Column (1) presents the estimates of Equation 2.1. Column (2) adds state fixed effects, controls for changing the minimum wage, low-wage status, and individual controls. Column (3) presents the individual fixed effects regression with the inclusion of the minimum wage change and low wage indicators. Table 2.2 shows the increase in the minimum wage has a negative effect on the employment of minimum wage workers. In these regressions, employment is defined as working any type of job, whether self-employed or as an employee. Teenagers experience a decline in employment of 4 percentage points, while even adults become less employable by about 3 percentage points. These results align with the competitive model of the labor market and suggest that we may be able to anticipate affects on crime.

The results for youth are directly comparable in magnitude to what Currie and Fallick (1996) find using NLSY79 data. Zavodny (2000) also finds a negative effect on the likelihood of employment, though not as large. Although few studies have observed disemployment effects of the minimum wage among adults in their 20s, that may be due to the fact that

those studies depend upon data at a higher aggregation level and cannot identify which workers were bound by changes in the minimum wage. Because a minority of adults work at the minimum wage, it stands to reason that aggregate data may understate or overlook the effect that we find here.

Weeks employed To take a closer look at the disemployment effects, we examine how the time spent employed, measured in weeks, is affected by increases in the minimum wage. This measurement allows us to observe disemployment effects at a finer level of detail than the binary employment measure. For example, if it is the case that some individuals become unemployed but find employment within the same year, the effect would be captured in the weeks worked measurement but overlooked by the binary measurement. This may also explain why, although most of the point estimates in the binary regressions are negative, not all are statistically significant.

Table 2.3 shows the results of regressing weeks employed at time t on an increase in the minimum wage. Columns (1) and (2) show tobit regressions of weeks worked conditional on being employed previous to a change in the minimum wage. Column (1) uses the controls of Equation 1. Column (2) adds state fixed effects, controls for changing the minimum wage, low-wage status, and individual controls. Columns (3) and (4) show a linear regression of weeks worked conditional on being employed both before and after the minimum wage change. Column (3) contains the full set of controls, and Column (4) adds individual fixed effects.

Column (1) shows that, with the inclusion of individuals who become unemployed, an increase in the minimum wage decreases the time spent employed by nearly 2 weeks for 17-19 year-olds by nearly 2 weeks, 4 weeks for 20-24 year-olds, and over 11 weeks for 25-30 year-olds. With the inclusion of additional controls in Column (2), these effects persist. Part of the effects observed in Column (1) may be attributable to a low-wage worker effect: low-wage workers work nearly 2 weeks less than higher wage workers. However, the change in the minimum wage still has a negative effect on bound workers with the magnitude of the effect increasing with age.

Columns (3) and (4) focus on the employment hours of workers who remain employed after a change in the minimum wage. Rather predictably, these estimates are smaller in magnitude than those in Columns (1) and (2). In Column (3), it appears that young adults who are bound by a change in the minimum wage and remain employed still experience some disemployment effect. When we control for individual-level heterogeneity in Column (4), however, it appears that these effects are concentrated among teenagers ages 14-16, who work about 2 weeks less than similar teens who were not affected by a change in the minimum wage.

These results, when combined with the binary employment results presented earlier, suggest that an increase in the minimum wage raises the likelihood that individuals will become unemployed and experience longer spells of unemployment. This raises the important question of whether those who remain employed or find employment following a minimum wage increase also experience an increase in incentives to commit crime, such as during extended periods of unemployment.

2.4.2 Crime

We estimate linear probability and logit models on self-reported drug sale, theft of items worth less than $50, and theft of items worth $50 or more. The results are presented in Tables 2.4 through 2.6.

Drug Sale OLS estimates in Table 2.4 shows that 14-16 year-olds who are bound by a change in the minimum wage experience a significant increase in the probability of selling drugs of approximately 5 percentage points relative to their unaffected peers. The presence of an effect among only teens may reflect that teens' drug-related decisions are more sensitive to changes in the minimum wage than older workers.

Logistic results confirm the significance of the 14-16 year-old results, though they are only significant at the 90% level in logistic regressions. The shrinking coefficients and loss of significance across columns should be expected as additional fixed effect controls are added: observations are lost when any fixed effect perfectly predicts crime. For example,

several observations are lost between columns (1) and (2) due to some states only containing individuals who never commit drug crimes. Column (3) shows that the positive relationship between experiencing a minimum wage increase and selling drugs does not hold up using individual level variation in crime. This could be because individual heterogeneity drives our results, but also using fixed effects means we condition on having at least one year of crime and one year without, reducing our sample size considerably and raising the size of the standard errors on our estimates.

Stealing Items of Less than $50 Value Tables 2.5 present results for theft of items worth less than $50. Younger and older teens are both more likely to steal when affected by an increase in the minimum wage. In particular, 14-16 year-olds are about 4 percentage points more likely to commit a theft, while older teens 17-19 increase crime by 1-2 percentage points.

Stealing Items of $50 Value or More Tables 2.6 contains results for theft of items worth $50 or more. Again we see that the effect of an increase in the minimum wage is concentrated amongst teenagers: 14-16 year-olds increase higher value theft by 2.3 to 2.5 percentage points, and 17-19 year olds increase higher value theft by about 1 percentage point. These effects are slightly smaller than the effect on stealing cheaper items, suggesting that displaced minimum wage workers who move into criminal activity may start with smaller crimes. Additionally, these effects persist when controlling for individual heterogeneity, indicating that youth committing crime in the past will be more prone to theft when the minimum wage rises.

2.5 Multinomial Choice

Our reduced form estimates show that an increase in the binding minimum wage has a causal effect on employment and criminal activity related to monetary gain among minimum wage workers. We are interested in knowing to what degree the increase in crime is due to

substitution with licit work. We estimate a multinomial choice model where individuals decide over four choices: being unemployed and not in crime (E0C0), being an unemployed criminal (E0C1), being employed and not in crime (E1C0), and being an employed criminal (E1C1).

The utility for each choice j made by individual i in period t is

$$U_{ijt} = \alpha_j + \beta_j \mathbb{1}\{MWBound_{it}\} + \delta_j X_{it} + e_{ijt} \tag{2.3}$$

Our covariates include an indicator for being bound by a minimum wage change, which captures wage and disemployment effects, such as losing one's job or experiencing a change in work hours. We also include controls for living in a state that changed the minimum wage, low-wage status, gender, race, age group, and year. Assuming that the error term for each choice follows an extreme-value distribution, we can estimate this model using a multinomial logit.

Rewriting the equation in general form as

$$U_{ijt} = Z_{it}\theta_j$$

The probability of each choice j is $\frac{e^{Z\theta_j}}{1+\sum_{k=2}^{4} e^{Z\theta_k}}$. We are interested in the marginal effect of being bound by a minimum wage change on each choice probability, so we evaluate the derivate of each choice probability with respect to a change in the minimum wage.

Given the competitive model of labor markets, we expect to see positive effects on choices without employment (E0). The substitution hypothesis tells us that as youth are forced out of employment, either through losing a job or a reduction in hours, they will move into illicit labor. This gives us a second expected result on choices that involve crime (C1).

2.5.1 Results

Tables 2.7 through 2.8 present the average marginal effect on the probability of each employment-crime choice for raising the minimum wage or being a worker bound by a change

relative to others in a state that has changed the minimum wage, respectively. Each table row represents separate estimates on each type of crime studied: property theft of low and high values and drug sale. Standard errors for the marginal effects are calculated by the delta method. We also report the results of a significance test for the relevant variable, given as a Wald test statistic and the corresponding p-value.

Table 2.7 shows the marginal effects associated with a change in the minimum wage. These can be interpreted as the change in choice probabilities that occurs for workers in states that change the minimum wage relative to workers who live in states where the minimum wage did not change. The results indicate an increase in crime among the unemployed when the minimum wage increases. Most notably, property theft increases by 0.2 percentage points among the unemployed, an increase that is significant at the 95% confidence level. The fact that this result is present for any workers, rather than only workers bound by the minimum wage change, suggests that as workers queue for minimum wage jobs they turn to crime. A significance test confirms that the indicator for a policy change is significant in that regression.

The marginal effects of an increase in the minimum wage for workers bound by the change are given in Table 2.8. These results represent the change in choice probabilities for bound workers relative to other workers who experienced an increase in the minimum wage but were unaffected due to being in an uncovered industry or having wages above the wage floor. For each crime, the indicator for bound worker status is significant. We see an increased movement from employment to idleness, with the probability of being an employed non-criminal falling by 2.4-3.2 percentage points and the probability of being idle increasing by 1.5 to 1.9 points. Of those who would have remained employed and out of crime, those who do not lose their jobs turn to crime. Again this result is strongest among property theft of any value, for which crime among the employed increases by 1.5 percentage points.

These results align with the competitive model prediction that the minimum wage displaces some workers who are more likely to commit crime. The increase in crime among employed workers supports the story of substitution between employment and crime as

periods of unemployment extend for minimum wage workers, even those who do find a job following the increase in the wage floor.

2.6 Conclusion

Did raising the minimum wage increase crime in the United States over the past 15 years? The evidence we present suggests the answer is yes. Further, our results indicate that this increase in crime occurs among both the unemployed and employed as a means for income, seen by increases in income-related crimes: drug sale and property theft. Among the employed this seems to occur due to a decrease in labor income due to a reduction in work.

Our results highlight the importance of providing employment opportunities for young, unskilled-youth given the evidence for a substitution between licit and illicit work. They also point to the dangers both to the individual and to society from policies that restrict the already limited employment options of this group. Our results indicate that youth crime will increase by 0.7 percentage points as the minimum wage increases. With average crime rates of drug sale at 5.6% and stealing between 2 and 4.7%, this is a substantial increase. The social costs to raising the minimum wage may not appear in net employment or unemployment changes, but nonetheless appear non-trivial.

2.7 Chapter 2 Appendix

Table 2.1: Summary Statistics

	All	Ages 14-16	Ages 17-19	Ages 20-24	Ages 25-30
% Assignable wage info	61.98	10.41	60.43	77.45	78.81
% MW bound	3.91	7.81	7.47	2.39	3.1
Hourly wages	14.28	7.13	7.66	15.09	18.9
Hours per week	32.4	17.0	27.1	33.5	36.4
% Sold drugs	5.61	8.08	7.40	4.78	3.56
% Stole value $<$ \$50	4.65	13.87	8.52	2.94	1.19
% Stole value $\geq$ \$50	2.03	5.25	3.11	1.39	0.65
% Male	50.3	58.08	51.46	49.55	49.84
% Female	49.7	41.92	48.54	50.45	50.16
% Black	24.8	21.94	22.87	24.9	26.53
% Hisp	20.75	17.7	18.83	21.28	21.77
% Mixed, non-Hisp	0.9	0.86	0.9	0.92	0.86
% Not black, non-Hisp	53.55	59.5	57.39	52.9	50.84
Observations	62,878	1,627	14,733	29,174	17,344

Table 2.2: Effects on Employment

Linear Probability Model Estimates

	(1)	(2)	(3)
14-16	-0.0439*	-0.0399*	-0.0442*
	(0.024)	(0.024)	(0.024)
17-19	-0.0135	-0.0104	-0.0034
	(0.009)	(0.009)	(0.009)
20-24	-0.0105	-0.0083	0.0042
	(0.010)	(0.010)	(0.010)
25-30	-0.0375***	-0.0331***	-0.00875
	(0.011)	(0.011)	(0.011)
$\mathbb{1}\{ChangeMW\}$		-0.0042	-0.0035
		(0.003)	(0.003)
$\mathbb{1}\{LowWage\}$		0.0035	0.0110***
		(0.003)	(0.004)
Observations	60,354	60,354	60,354
R-squared	0.018	0.024	0.258

Logit Estimates

	(1)	(2)	(3)
14-16	-0.254	-0.21	-0.31
	(0.229)	(0.233)	(0.300)
17-19	-0.173	-0.136	-0.0385
	(0.117)	(0.122)	(0.151)
20-24	-0.177	-0.137	0.0648
	(0.153)	(0.159)	(0.184)
25-30	-0.551***	-0.483***	-0.13
	(0.153)	(0.154)	(0.179)
$\mathbb{1}\{ChangeMW\}$		-0.0356	-0.0116
		(0.054)	(0.060)
$\mathbb{1}\{LowWage\}$		0.0573	0.188***
		(0.053)	(0.064)
Observations	60,354	60,343	20,203

Binary employment outcome on (1) an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects; (2) controls of (1) with indicators for state change in minimum wage and low-wage worker status, mother's education, math PIAT score, household income in 1997, and state fixed effects; (3) all aforementioned controls with individual fixed effects. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Weeks Employed

	(1)	(2)	(3)	(4)
14-16	-1.61	-1.373	0.186	-2.390*
	(2.746)	(2.738)	(1.472)	(1.387)
17-19	-1.897**	-2.336**	-0.609	0.308
	(0.995)	(1.018)	(0.510)	(0.489)
20-24	-4.053***	-3.884***	-1.538***	0.443
	(1.153)	(1.181)	(0.562)	(0.546)
25-30	-11.43***	-10.79***	-3.888***	0.00015
	(1.320)	(1.320)	(0.631)	(0.614)
$\mathbb{1}\{ChangeMW\}$		-0.23	-0.0533	-0.217
		(0.392)	(0.179)	(0.162)
$\mathbb{1}\{LowWage\}$		-1.688***	-0.687***	0.390**
		(0.395)	(0.193)	(0.190)
Observations	60,200	60,200	56,794	56,794
R-squared			0.1	0.098
Number of PUBID				8,359

Columns (1) and (2) show tobit regressions of weeks worked in the year of a MW increase on (1) an indicator for being bound by a change in the minimum wage and year, age, race, and gender fixed effects; (2) controls of (1) with indicators for state change in minimum wage and low-wage worker status, mother's education, math PIAT score, household income in 1997, and state fixed effects. Column (3) results from an OLS model of hours conditional on employment with same controls as column (2). Column (4) results from an OLS model of income conditional on employment with same controls as column (2). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Probability of Selling Illegal Drugs

Linear Probability Model Estimates

	(1)	(2)	(3)
14-16	0.0503**	0.0465**	0.0292
	(0.022)	(0.022)	(0.022)
17-19	0.0095	0.0051	-0.0060
	(0.008)	(0.008)	(0.008)
20-24	-0.0019	-0.0061	-0.0133
	(0.011)	(0.011)	(0.012)
25-30	-0.0030	-0.0043	-0.0132
	(0.014)	(0.014)	(0.014)
$\mathbb{1}\{ChangeMW\}$		-0.0020	-0.0012
		(0.004)	(0.004)
$\mathbb{1}\{LowWage\}$		0.0036	-0.0014
		(0.003)	(0.004)
Observations	42,082	42,082	42,082
R-squared	0.015	0.019	0.357

Logit Estimates

	(1)	(2)	(3)
14-16	0.549*	0.484*	0.344
	(0.287)	(0.290)	(0.430)
17-19	0.121	0.0582	-0.19
	(0.12)	(0.124)	(0.178)
20-24	-0.0535	-0.129	-0.311
	(0.234)	(0.24)	(0.303)
25-30	-0.134	-0.173	-0.544
	(0.366)	(0.368)	(0.464)
$\mathbb{1}\{ChangeMW\}$		-0.0498	-0.0652
		(0.08)	(0.096)
$\mathbb{1}\{LowWage\}$		0.0624	-0.0329
		(0.06)	(0.085)
Observations	42,081	41,996	10,100

Binary crime outcome regressed on (1) an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects; (2) controls of (1) with indicators for state change in minimum wage and low-wage worker status, mother's education, math PIAT score, household income in 1997, and state fixed effects; (3) all aforementioned controls with individual fixed effects. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Probability of Stealing Items Worth < $50

Linear Probability Model Estimates

	(1)	(2)	(3)
14-16	0.0428**	0.0387**	0.0425**
	(0.020)	(0.020)	(0.020)
17-19	0.0214***	0.0144**	0.0095
	(0.007)	(0.007)	(0.008)
20-24	-0.0032	-0.0089	-0.0157
	(0.009)	(0.010)	(0.010)
25-30	0.0168	0.0185	-0.0095
	(0.012)	(0.012)	(0.012)
$\mathbb{1}\{ChangeMW\}$		-0.0015	-0.0004
		(0.003)	(0.003)
$\mathbb{1}\{LowWage\}$		0.0015	-0.0008
		(0.003)	(0.003)
Observations	48,026	48,026	48,026
R-squared	0.034	0.039	0.29

Logit Estimates

	(1)	(2)	(3)
14-16	0.31	0.278	0.497
	(0.245)	(0.249)	(0.392)
17-19	0.231**	0.137	0.0752
	(0.106)	(0.112)	(0.164)
20-24	-0.137	-0.266	-0.342
	(0.286)	(0.293)	(0.35)
25-30	0.891**	0.977***	0.366
	(0.352)	(0.355)	(0.395)
$\mathbb{1}\{ChangeMW\}$		-0.180**	-0.158
		(0.089)	(0.104)
$\mathbb{1}\{LowWage\}$		0.0798	-0.0265
		(0.058)	(0.079)
Observations	48,025	47,903	11,979

Binary crime outcome regressed on (1) an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects; (2) controls of (1) with indicators for state change in minimum wage and low-wage worker status, mother's education, math PIAT score, household income in 1997, and state fixed effects; (3) all aforementioned controls with individual fixed effects. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Probability of Stealing Items Worth $\geq$ \$50

Linear Probability Model Estimates

	(1)	(2)	(3)
14-16	0.0252*	0.0237*	0.0174
	(0.013)	(0.013)	(0.015)
17-19	0.0104**	0.0068	0.0106*
	(0.005)	(0.005)	(0.006)
20-24	-0.0038	-0.0075	-0.0050
	(0.007)	(0.007)	(0.008)
25-30	0.011	0.0103	0.0026
	(0.009)	(0.009)	(0.010)
$\mathbb{1}\{ChangeMW\}$		-0.0024	-0.0014
		(0.002)	(0.003)
$\mathbb{1}\{LowWage\}$		0.0012	-0.0022
		(0.002)	(0.002)
Observations	41,307	41,307	41,307
R-squared	0.012	0.015	0.237

Logit Estimates

	(1)	(2)	(3)
14-16	0.448	0.383	0.499
	(0.362)	(0.366)	(0.532)
17-19	0.296*	0.177	0.454*
	(0.167)	(0.174)	(0.244)
20-24	-0.398	-0.559	-0.274
	(0.509)	(0.518)	(0.556)
25-30	0.997*	0.977*	0.605
	(0.532)	(0.535)	(0.602)
$\mathbb{1}\{ChangeMW\}$		-0.205	-0.303*
		(0.144)	(0.163)
$\mathbb{1}\{LowWage\}$		0.0563	-0.206*
		(0.093)	(0.122)
Observations	41,306	41,093	5,175

Binary crime outcome on (1) an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects; (2) controls of (1) with indicators for state change in minimum wage and low-wage worker status, mother's education, math PIAT score, household income in 1997, and state fixed effects; (3) all aforementioned controls with individual fixed effects. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Marginal Effects of Changing the Minimum Wage

	Average marginal effects				Wald test	
	E=0, C=0	E=1, C=0	E=0, C=1	E=1, C=1	χ^2 statistic	P-value
Steal items worth < \$50	-0.0013	-0.0055	0.0016*	0.0053*	6.00	0.1115
	(0.0037)	(0.0046)	(0.0010)	(0.0030)		
Steal items worth ≥ \$50	-0.0017	-0.0017	0.0014*	0.0020	4.43	0.2185
	(0.0043)	(0.0047)	(0.0008)	(0.0022)		
Steal any value	-0.0037	-0.0048	0.0029**	0.0057	8.19	0.0422
	(0.0042)	(0.0057)	(0.0012)	(0.0041)		
Drug sale	-0.0015	-0.0027	0.0007	0.0036	1.43	0.6977
	(0.0042)	(0.0056)	(0.0010)	(0.0038)		

Multinomial logit controlling for an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects, and indicators for state change in minimum wage and low-wage worker status. Each crime regression is run separately. Average marginal effects reported. Standard errors calculated by the delta method are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Marginal Effects of Raising the Minimum Wage on Bound Workers

	Average marginal effects				Wald test	
	E=0, C=0	E=1, C=0	E=0, C=1	E=1, C=1	χ^2 statistic	P-value
Steal items worth $<$ \$50	0.0172**	-0.0251***	0.0006	0.0072	10.39	0.0155
	(0.0068)	(0.0082)	(0.0017)	(0.0050)		
Steal items worth $\geq$ \$50	0.0176**	-0.0240***	-0.0008	0.0072*	11.12	0.0111
	(0.0073)	(0.0082)	(0.0013)	(0.0039)		
Steal any value	0.0190***	-0.0323***	-0.0011	0.0145**	14.42	0.0024
	(0.0071)	(0.0095)	(0.0020)	(0.0067)		
Drug sale	0.0159**	-0.0236**	0.0013	0.0064	7.69	0.0529
	(0.0071)	(0.0095)	(0.0018)	(0.0066)		

Multinomial logit controlling for an indicator for being bound by a change in the minimum wage interacted with age group indicators and year, age, race, and gender fixed effects, and indicators for state change in minimum wage and low-wage worker status. Each crime regression is run separately. Average marginal effects reported. Standard errors calculated by the delta method are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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