ESSAYS ON DRUG AND ALCOHOL POLICIES IN THE UNITED STATES

A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

Randall Crouch

May 2015

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Randall Crouch

APPROVED:

Chinhui Juhn, Ph.D
Committee Co-Chair

Elaine Liu, Ph.D
Committee Co-Chair

Andrew Zuppann, Ph.D

Monica Deza, Ph.D
University of Texas at Dallas

Steven G. Craig, Ph.D.
Interim Dean, College of Liberal Arts and Social Sciences
Department of Economics

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Abstract

This dissertation consists of two essays that explore the unintended consequences of drug and alcohol control policies in the United States. They both examine policy changes at the state level using a difference-in-differences approach. These two studies shed light on outcomes that were not likely to be considered when policy decisions were made and may have important implications for future policies.

In the first essay, I analyze the effect of minimum legal drinking age (MLDA) laws on non-cognitive skills. The National Longitudinal Survey of Youth 1979 (NLSY79) is used to investigate the effect of changes in MLDA on the onset of regular drinking, self-esteem and self-control. Surprisingly, I find that a legal drinking environment is associated with an increase in self-esteem for females in the short-run and long-run. Then, I test several possible channels through which self-esteem may be indirectly affected by the MLDA. These channels include alcohol and drug use, marriage, sex and childbirth. Although the MLDA has a significant effect on some of these channels for females, using the channels as controls in the self-esteem analysis does not affect the magnitude or significance of the effect of the MLDA on female self-esteem.

In the second essay, I examine the effect of marijuana decriminalization in Massachusetts on the black-white gap in arrest rates for several different criminal offenses using Uniform Crime Reporting (UCR) program data. I use a difference-in-difference model that allows for a heterogeneous treatment effect by race to estimate this effect for marijuana possession and sales, non-marijuana possession and sales, violent and theft-related offenses separately for adults and juveniles. Results indicate that marijuana decriminalization leads to a decrease in the black-white gap in adult and juvenile arrest rates for marijuana possession and sales, non-marijuana sales and

adult arrest rates for theft-related offenses. These findings are consistent with decriminalization leading to a shift in police resources away from areas where blacks are more likely to be arrested.

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to Mom, Dad, Cyra and Kevin

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Chapter 1

Effect of Minimum Legal Drinking Age Laws on Non-Cognitive Skills

1.1 Introduction

The consumption of alcoholic beverages in the United States has been the focus of a long history of regulatory policies. From 1920-1933 it was illegal to sell, manufacture, transport or import alcoholic beverages in the U.S. In more recent years, the government has focused on regulating alcohol sale and consumption in an attempt to reduce youth drinking and alcohol impaired driving. A major focus for economists has been to analyze the effectiveness of these policies and to estimate the impact of alcohol consumption on individuals and society as a whole. Much economic research has analyzed the effect of alcohol consumption on mortality, health, labor, crime and education outcomes.[1]

[1] Carpenter and Dobkin 2009; Kaestner and Yarnoff 2011; Fertig and Watson 2009; Kenkel et al. 1994; Carpenter and Dopkin 2010; Koch and Ribar 2001

This study explores a possible channel through which alcohol policy affects these outcomes by analyzing the effect of Minimum Legal Drinking Age (MLDA) laws on non-cognitive outcomes using the National Longitudinal Survey of Youth 1979 (NLSY79). The MLDA laws regulate the minimum age that alcohol can be purchased and consumed. Since 1988, all states have adopted an MLDA of 21. Between 1969 and 1988, however, MLDAs varied at the state level between 18 and 21. These fluctuations are exploited as a plausibly exogenous source of variation in the MLDA. Non-cognitive outcomes are those that cannot be measured by standard achievement tests. Heckman and Rubinstein (2001) give examples of motivation, tenacity, trustworthiness, perseverance, persistence, reliability and self-discipline. "Many different personality and motivational traits are lumped into the category of non-cognitive skills" (Heckman and Rubinstein 2001). This study uses measures of non-cognitive skills available in the NLSY79, self-esteem and locus of control.

Possible channels through which the MLDA could indirectly affect non-cognitive outcomes are also investigated. One mechanism of the effect of the MLDA is through changes in drinking behavior. Other studies have thoroughly examined the effect of the MLDA on drinking prevalence and binge drinking (Cook and Moore 2001), but not much economic research has investigated the effect of the MLDA on the onset of regular drinking. A simple regression of non-cognitive outcomes on drinking variables will suffer from endogeneity. Unobserved individual characteristics may affect both drinking behavior and the development of non-cognitive skills. There could also be the problem of reverse causality; variation in non-cognitive skills could cause people to exhibit different drinking behaviors.

Medical literature suggests that drinking during adolescence adversely affects the development of brain structures associated with non-cognitive skills. Adolescent drinking appears to stunt the growth of the amygdala and the hippocampus as well as reducing the level of white matter in the frontal lobe (Clark, Thatcher and Tapert 2008). Clark et al. claim that these areas of the brain are known to be associated with emotional regulation. Alcohol consumption can also lead to abuse or dependence.[2] This could explain why an individual's legal drinking environment affects later alcohol consumption.[3] The psychology literature seems to have mixed results. While some studies find a negative correlation between alcohol consumption and self-esteem (Corbin et al. 1996; Glindemanne et al. 1999), another study finds that people who think they have consumed alcohol are more likely to give themselves positive self-evaluations (Bègue et al. 2013).

The main dataset used in this paper is the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 is a nationally representative survey of 12,686 young men and women who were between the ages of 14-22 when they were first surveyed in 1979. These same individuals were interviewed annually until 1994 and have been interviewed biennially since then. Among other things, the survey contains questions for the age at which the respondents began to drink regularly, non-cognitive skills assessments, parents' education, family composition, criminal behavior and alcohol and drug use. This study uses the individuals' state of residence available in the restricted access geocode data to match individuals with the corresponding MLDA.

[2]These two conditions are defined in the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) which is released by the American Psychiatric Association.

[3]Kaestner and Yarnoff 2011.

The data for the state MLDA laws comes from the Distilled Spirits Council of the United States (DISCUS). These MLDA changes happened at the same time the respondents in the NLSY were around the ages the laws were targeting. Individuals were between the ages of 4 and 31 during the variation of the MLDAs.

The main results of this paper provide evidence that less restrictive MLDAs lead to higher levels of self-esteem for females and whites in the short-run and for females in the long-run. Less restrictive MLDA laws also lead to an increase in self-control for blacks in the long-run. The effect of the drinking age on non-cognitive outcomes is insignificant for other subgroups. In order to test the possible channels of these surprising self-esteem results for females, this study investigates the impact of MLDA laws on drinking behavior, drug use, sexual activity, marriage and children. Although some of these channels are significantly impacted by the MLDA, they do not appear to play a significant role in the impact of the MLDA on self-esteem for females. Another important finding is that less restrictive drinking ages lead to earlier drinking onset. A one year decrease in the average MLDA between the ages of 18 and 19 leads to a 5.8% increase in the probability of becoming a regular drinker by the age of 19. This effect of the MLDA on the onset of regular drinking is further supported through the use of survival analysis.

Section 1.2 briefly reviews the alcohol and non-cognitive skills literature, section 1.3 provides a history of MLDA laws, section 1.4 describes the data, section 1.5 discusses the impact of the MLDA on drinking and drug use, section 1.6 discusses the impact of the MLDA on non-cognitive outcomes, section 1.7 explores the possible channels, and section 1.8 concludes.

1.2 Literature Review

This section reviews the literature relevant to this study. It first explores the impact of alcohol use on labor market, health, and socio-economic outcomes, and then the relationship between non-cognitive skills and health and labor market outcomes. The first vein of literature to be discussed implements a regression discontinuity (RD) framework to analyze the effect of a discontinuous increase in alcohol consumption on certain outcomes. These studies focus on the time period when the MLDA is 21 for all states (after 1988). Carpenter and Dobkin find that becoming legally allowed to drink causes an increase in mortality rates (2009), violent crimes, alcohol related crimes and city and county ordinance violations (2010).[4] Another study by Crost and Guerrero (2012) finds that becoming legally allowed to drink leads to a decrease in marijuana consumption, especially for women. Using the 1997 wave of the NLSY, Deza (2014) finds that becoming legally allowed to drink leads to a decrease in the use of hard drugs. These results are consistent with standard substitution effects.

Another vein of alcohol literature analyzes the reduced form (or difference-in-differences) effect of changes in MLDA laws on many different outcomes. These studies focus on earlier years, when the MLDA varies by state. There is some suggestive evidence that lower MLDAs have an adverse effect on fetal outcomes. Fertig and Watson (2009) use the NLSY79 to analyze the effect of having an MLDA of 18 on infant outcomes resulting from changes in the composition of births and increased prenatal drinking. They find evidence that less restrictive MLDA laws lead to worse

[4]For the both the 2009 and 2010 studies, Carpenter and Dobkin use individuals' 21st birthday as a Regression Discontinuity (RD) for becoming legally allowed to drink.

birth outcomes partially due to an increase in unplanned pregnancies. Their results are strongest for black mothers. Barecca and Page (2012) find that an MLDA of 18 does not have a significant impact on infant outcomes.[5] They do, however, find that it leads to an increase in the female to male birth ratio. This evidence suggests that increasing the MLDA could reduce fetal losses. Using MLDA laws, Kaestner and Yarnoff (2011) provide evidence that a less restrictive legal drinking environment between the ages of 18-20 is associated with an increase in later alcohol consumption and traffic fatalities for adult males.[6] Yamada et al. (1996) find that increasing the minimum drinking age of liquor improves the probability of high school graduation by decreasing liquor and wine consumption.

Other studies have used two stage least squares (2SLS) to estimate the causal effect of alcohol consumption. Using the NLSY 1979, Kenkel et al. (1994) instrument for problem drinking using its determinants,[7] and find that problem drinkers tend to earn less and are less likely to be married. Williams et al. (2003) find a negative effect of alcohol consumption on GPA, and that this effect occurs through a reduction of study hours. Using the MLDA and beer taxes to instrument for drinking, Cook and Moore (1993) find that increased alcohol consumption during high school lowers the amount of education attained after high school. Koch and Ribar (2001) use several different specifications to estimate the effect of drinking onset on education,

[5]They find an economically small decrease in birthweight, and little or no effect on other traditional measures of infant health.

[6]Using a reduced form analysis, Kaestner and Yarnoff find that moving from an environment where an individual can never drink legally to an environment where one can always drink legally is associated with a 20-33 percent increase in later alcohol consumption and a 10 percent increase in traffic fatalities for adult males.

[7]They use alcohol tax, parents' problem drinking, other relatives' problem drinking and percentage of state's population in a dry county to instrument for problem drinking.

and they conclude that this effect is likely to be small. However, they mention that their results are not necessarily inconsistent with the larger effects found in Cook and Moore (1993).[8] As a sensitivity test in their 2SLS analysis, Koch and Ribar (2001) use the MLDA as an alternative instrument for drinking onset.[9] Although they find that coefficients in the first stage regression are significantly different from zero, these models have a poor overall fit, and they conclude "that the policy variables are not strong predictors of drinking onset" (Koch and Ribar 2001). They do not explicitly report these first stage results in their paper. Several studies examine the effect of the MLDA on certain drinking behavior[10], but other than Koch and Ribar (2001), previous studies do not investigate the effect of the MLDA on regular drinking onset.

Much economic literature provides evidence of the importance of non-cognitive skills in determining labor market and health outcomes. Duncan and Dunifon measure individuals' orientation toward challenge and sense of self-control at age 21-29, and then they test the effect of these non-cognitive skills on labor market outcomes 15-25 years later. They find that both of these measures have a strong positive effect on later earnings, and orientation toward challenge also predicts future on-the-job training (Duncan and Dunifon 1998). There is also evidence that non-cognitive skills

[8]Cook and Moore(1993) analyze heavy drinking and problem drinking which may have more of an effect on educational attainment.

[9]Using the same data set as this paper (NLSY79), Koch and Ribar (2001) use the age an individual first began to drink regularly as their drinking onset variable. Since this question is asked in 1983, they also code the age of regular drinking onset as 25 for individuals who say they have "never had a drink before" in the later surveys. Coding individuals who never drink regularly as drinking regularly at age 25 is quite noisy which is why I prefer to use whether one begins to drink regularly by age 19 to measure onset. My results for drinking onset are further supported by survival analysis.

[10]Many studies use the MLDA to show an increase in alcohol consumption for RD studies (Carpenter and Dobkin 2008,2009; Deza 2014; Crost and Guerrero 2012), and Cook and Moore (2001) analyze the effect of the MLDA on drinking prevalence and intensity.

have a strong effect on schooling decisions and earnings conditional on schooling decisions (Heckman, Stixrud and Urzua 2006). Chiteji (2010) finds that self-efficacy and degree of future orientation are negatively correlated with behaviors known to depreciate health (i.e. drinking) and positively correlated with behaviors known to improve health (i.e. excercise). Another study using the NLSY79 and the Rosenberg self-esteem score finds that self-esteem has a positive effect on wages and even has more of an impact than human capital (Goldsmith, Veum and Darity 1997). Due to the evidence of the direct effect of non-cognitive ability, studying the impact of MLDA on non-cognitive outcomes could have labor market and health implications.

1.3 History of MLDA laws

The regulation of alcohol sale and consumption has been a focal point of U.S. policy since the early 1900's. After the repeal of national prohibition, the major regulations have consisted of Minimum Legal Drinking Age (MLDA) laws and drinking and driving laws. The rationale behind MLDAs is that a person should reach a certain level of maturity and responsibility before being allowed to drink. In the United States MLDAs have ranged from 18 to 21. The period between 1971 and 1988 experienced increased variation in MLDAs within states. Mainly in response to the passing of the 26th amendment in 1971, which lowered the minimum voting age from 21 to 18, 26 states lowered their MLDA to 18, 19 or 20 between 1969 and 1976. After research in the early 1980's suggested that lower MLDAs cause higher rates of traffic fatalities, the federal government passed the National Minimum Drinking Age Act of 1984. This act threatened to reduce federal highway funding for states

that did not increase their MLDA to 21. As a result, all states had adopted an MLDA of 21 by 1988, Wyoming being the last. Table 1.1 contains all the state level MLDA changes from 1967 to present (Dee 1999). Several papers demonstrate a discontinuous increase in alcohol consumption when individuals reach the legal drinking age (Carpenter and Dobkin 2009, 2010; Crost and Guerrero 2012).[11]

This paper uses the variation of the MLDA by state and month to estimate the impact of these laws on several outcomes. The data on the MLDAs go back to January of 1967, vary by state and month and are obtained from the Distilled Spirits Council of the United States (DISCUS).[12] Certain states may restrict different types of alcohol differently. For example, some states differentiate between spirits and beer or wine, others even differentiate between beers with different levels of potency and others do not differentiate at all. This paper will consider the MLDA to be the minimum age that any type of alcohol can be legally consumed, possessed and purchased. For example, through July of 1985, Kansas had an MLDA of 18 for beer with alcohol content at or below 3.2% by volume and an MLDA of 21 for beer with alcohol content above this threshold and all other types of alcohol. Beginning in June of 1985, the MLDA in Kansas was 21 for all types of alcohol. So for Kansas, this paper considers the MLDA to be 18 from January of 1967 until June of 1985, and 21 thereafter.

[11]These studies implement a regression discontinuity design after the MLDA is 21 for all states.
[12]This data was collected by Thomas Dee in previous work (Dee 1999).

1.4 Data

This study uses the NLSY79 with restricted access geocode information in order to identify the state of residence for each individual.[13] The NLSY79 is a longitudinal survey that began in 1979. The sample of 12,686 youths are between the ages of 14 and 22 in 1979. The same sample was reinterviewed annually until 1994, and biennially until 2012. Over time, some of the original sample withdraws from the survey. The NLSY asks a wide variety of questions related to personal behavior, family characteristics, labor market experiences and educational outcomes. It also provides a measure of cognitive skills, the armed forces qualifying test (AFQT), and a few measures of non-cognitive skills. The non-cognitive skills analyzed in this study measure internal versus external locus of control (Rotter Scale in 1979, and Pearlin Mastery Index in 1992), and self-esteem (Rosenberg Score in 1980, 1987 and 2006).

Individuals are also asked an extensive set of questions related to frequency and intensity of alcohol and drug use in multiple years. The questions about alcohol use are asked in the years 1982-1985, 1988 and 1989. In each of these years, individuals are asked whether they have ever had a drink, how many days they drank in the last month, and how many days they drank six or more drinks in the past month. These questions are used to measure drinking prevalence and intensity. In 1982 and 1983, individuals are asked when they began to drink on a regular basis.[14] This question (in 1983) is used as a measure of drinking onset. In the 1983 survey, 85.9% of the

[13]For observations in which an individual was interviewed, but the state is missing, the missing state is replaced with most recent non-missing year.

[14]Individuals were asked "How old were you when you first began drinking alcoholic beverages on a regular basis, that is at least once or twice a month?"

male sample claim to be regular drinkers of which the average age of regular drinking onset is 16.6. For females, 71.8% of the sample claim to be regular drinkers of which the average age of regular drinking onset is 17.44.

The non-cognitive outcomes, self-esteem and locus of control, are measured using indices constructed from self-reported questions. The Rosenberg Self-Esteem Score (Rosenberg 1965) is measured in 1980, 1987 and 2006. It is constructed from ten questions that measure an individual's self-esteem. This scale has been used in studies that measure the effect of non-cognitive skills on labor market outcomes (Heckman, Stixrud and Urzua 2006; Osborne-Groves 2005). The individuals are presented with a statement and asked whether they 1-strongly agree, 2-agree, 3-disagree or 4-strongly disagree. For example, the statement in the first Rosenberg question is "I am a person of worth". An answer of 4 (strongly disagree) for this question would indicate low self-esteem, while an answer of 4 (strongly disagree) on question 10 "I feel useless at times" would indicate high self-esteem. The answers are calibrated so that a higher Rosenberg Score indicates higher self-esteem. This score ranges from 3-30. [15] The questions used to construct the Rosenberg Score are displayed in Table **??** in Appendix 1.9.

The Pearlin Mastery Index (Pearlin and Schooler 1978) is measured in 1992. It is constructed from seven questions used to measure an individual's locus of control. This measure has been used in some of the family dynamics literature (Baruch and Barnett 1983; Carlson and Corcoran 2001). The index ranges from 7-28; a higher

[15]The Rosenberg Score is normalized; the mean in 1980 is 22.5, and the standard deviation is 4.1.

score indicates more feeling of self-control.[16] The questions used to construct the Pearlin Index are displayed in Table ?? in Appendix 1.9

The Rotter Locus of Control Scale (Rotter 1966) has also been used as a measure of non-cognitive skills in previous literature (Heckman, Stixrud and Urzua 2006). This scale is constructed using four pairs of statements pertaining to locus of control. For each pair, individuals are asked whether their opinion is more aligned with a statement indicating being in control or a statement indicating not being in control. They are then asked if their opinion is slightly closer to the chosen statement, or much closer. This creates a range of 1-4 for each statement pair. The Rotter Scale ranges from 4 to 16 where the higher number indicates a more external locus of control, or less self-control.[17] The statements and answers used to construct the Rotter Scale are displayed in Table 1.A5 in Appendix 1.9

The NLSY79 provides month and year of birth of the individuals, and DISCUS provides MLDA laws by month and state. Using the restricted NLSY79 with geocode information, respondents are linked to the MLDA in the state they reside. The MLDA data goes back to 1967, but the state of residence from the NLSY only goes back to 1979. Since the respondents are age 14-22 when the survey begins, only looking at the MLDAs from 1979 onward would ignore the MLDAs the older cohorts face at younger ages. The state in which each individual resides in the first year of the survey is used to proxy for the state of residence in prior years. This could lead to potential measurement error, but it is the best proxy available for the state of residence before the first survey. The summary statistics is presented in Table 1.2.

[16]The mean of the Pearlin Index in 1992 is 22.2, and the standard deviation is 3.2.
[17]The mean of the Rotter Scale in 1979 is 8.5, and the standard deviation is 2.4.

1.5 The Impact of MLDA Laws on Drinking and Drug Use

MLDA laws could plausibly affect non-cognitive outcomes in many different ways. This section begins by testing the direct impact of MLDA on alcohol use, and then it will focus on the indirect effect of MLDA on drug use. Several studies have investigated the effect of MLDA laws on drinking outcomes such as drinking prevalence and intensity. Using the Monitoring the Future (MTF) Survey, Thomas Dee (1999) finds that moving from an MLDA of 18 to a higher MLDA leads to an 8.4 percent decrease in the level of binge drinking among high school seniors. Using the NLSY79, Cook and Moore (2001) also find that an increase in the MLDA leads to a decrease in the probability of binge drinking and drinking prevalence.

This study will examine the effectiveness of higher MLDA laws in increasing age of drinking onset, reducing drinking prevalence and reducing binge drinking. The way these drinking outcomes are defined deserves further explanation for a clear interpretation. Drinking onset is defined in this study as the age at which an individual began to drink regularly, that is at least once or twice a month. In order to include individuals who never began to drink regularly, dichotomous variables indicating whether a person began drinking regularly by certain ages (13-20) are used to proxy for drinking onset. These variables are derived from a retrospective question asked in 1983, when respondents are between the ages 18 and 26. The cumulative distribution of regular drinking onset is displayed in Figure 1.1. Following Cook and Moore (2001), drinking prevalence is a dichotomous variable indicating whether an

individual has had any alcoholic beverages in the past 30 days. As a measure of drinking intensity, a dichotomous variable for binge drinking indicates whether the individual has had at least six alcoholic drinks on three or more occasions in the past 30 days. These variables are constructed using questions about alcohol consumption available from 1982-1985, and also 1988-1989.[18] The effect of MLDA laws on the onset of regular drinking is analyzed using the following linear probability model.

$$Drankby(age)_{isjm} = \beta_0 + \beta_1 AverageMLDA(age-1, age)_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.1}$$

For individual i in state s with birth year j and birth month m. X represents a set of controls for mother's education, race, gender, having an older sibling and AFQT score. $AverageMLDA(age-1, age)$ represents the mean of MLDAs the individual faced at the age of the outcome and the year before; (age) ranges from 13 to 20. This variable captures the MLDA variables in the two years leading up to the outcome. Merely using the MLDA at that age would be ignoring variation in the MLDA that would likely affect the outcome. For instance, if an individual drinks by 18, then by default he also drinks by 19. Including the MLDA from (age-2) in the average does not change the results in any significant way. Panel A in Table 1.3 presents the results from specification (1.1) for the outcome drank by age 19. This age was selected in order to include most of the cohorts while still allowing for potential exposure to a legal drinking environment. Since the youngest cohort was not yet 19 at the time of the 1983 survey, they were excluded from this part of the analysis. On average, a one year increase in the MLDA leads to a 4.35 percentage

[18]The drinking prevalence question is not asked in 1982, and the age of onset question is only asked in 1982 and 1983.

point (5.8%) decrease in the probability of drinking regularly by age 19. This effect is significant for females, males, Hispanics and whites. The effect for males is roughly half the size of the effect for females likely due to the fact that, on average, males begin drinking regularly earlier. An alternative specification, survival analysis, is also used to study the effect of the MLDA on the onset of regular drinking. This analysis produces results consistent with the findings in this section. This estimation method is described in Appendix 1.9.1, and the results are displayed in Table 1.A1 in Appendix 1.9.

Next, this study analyzes the effect of a binding MLDA on binge drinking and drinking prevalence. A dichotomous variable indicating whether or not it is legal to drink for each individual at the time of the survey is used to proxy for a binding MLDA. This is accomplished using the month each individual is surveyed. Respondents are asked whether they had at least one alcoholic drink in the past 30 days. This survey question is used to indicate drinking prevalence. Individuals are also asked how many days they had 6 or more alcoholic drinks in the past 30 days. This question is used to construct another binary variable indicating whether individuals binge drank (had 6 or more drinks) on at least 3 days in the last 30. This variable is used to proxy for drinking intensity. Since the drinking prevalence and intensity questions are asked in several years, it is possible to analyze the following linear probability model.

$$\begin{aligned} DrinkingOutcome_{isjmt} &= \beta_0 + \beta_1 LegaltoDrink_{isjmt} \\ &+ \beta_2 X_{isjm} + \beta_3 age_{isjmt} + \beta_4 age^2_{isjmt} + \mu_j + \delta_s + \lambda_t + \epsilon_{isjmt} \end{aligned} \tag{1.2}$$

The t subscript represents the year of the survey, and X_{isjm} is the same set of controls

as in equation (1.1), but here a quadratic age trend is also included. Fixed effects for birth year, state and year are represented by μ_j, δ_s and λ_t, respectively.

Panels B and C of Table 1.3 display the results from regressing the drinking prevalence and binge drinking variables on the legal to drink variable. A non-binding MLDA leads to a 6.4 percentage point (9.0%) increase in drinking prevalence. This effect is significant and of similar magnitudes for all subsamples. A non-binding MLDA also causes a 5.3 percentage point (36.8%) increase in the probability of binge drinking. This increase in binge drinking is significant for females (36.8%), males (36.9%), Hispanics (23.3%) and whites (45.0%). The direction of these results are consistent with Cook and Moore (2001) who use a probit model and a more extensive set of controls.[19]

The rest of this section considers the impact of the MLDA on cocaine and marijuana use. This relationship could serve as a channel through which the MLDA affects self-esteem. Possible channels will be explored further in section 1.7. Many studies have aimed to estimate whether alcohol and illicit drugs are substitutes or complements using the minimum drinking age. Some studies find support that alcohol and marijuana are complements (Farrelly et al. 1999; Williams et al. 2004; Yörük and Yörük 2011[20]), while others find evidence of their substitutability (Chaloupka and Laixuthai 1997; DiNardo and Lemieux 2001; Crost and Guerrero 2012). The literature on the relationship between cocaine and alcohol use finds evidence of their

[19]Cook and Moore (2001) also control for a more extensive definition of ethnicity, poverty status, and religious orientation.

[20]Crost and Rees (2013) find that these results are dependent on restricting the sample to individuals in the NLSY97 who used marijuana since the last interview. They find no effect when all individuals between the ages of 19 and 22 are included.

complementarity (Saffer and Chaloupka 1998; Jofre-Bonet and Petry 2008) and substitutability (Deza 2014). This part of the analysis will attempt to contribute to this literature by testing whether the MLDA has an effect on drug use. Using the 1984 survey, dichotomous variables are constructed for both drugs indicating any use in the past 30 days. The impact of the MLDA on marijuana and cocaine is analyzed using the following linear probability model.

$$DrugUse_{isjm} = \beta_0 + \beta_1 AverageMLDA(age18-19)_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.3}$$

The policy variable here is the average MLDA faced by an individual during ages 18 and 19. These ages are used because the youngest cohort is only 19 during this survey. The subscripts, fixed effects and control variables are identical to equation (1.1). Table 1.4 presents the results for cocaine use and marijuana use. On average, a one year increase in the MLDA leads to a 1.3 percentage point (i.e. 34.7%) increase in the probability of cocaine use for females and a 1.5 percentage point (i.e. 48.2%) increase in this probability for blacks. For marijuana, a one year increase in the MLDA leads to a 4.6 percentage point (i.e. 32.1%) increase in average probability of use for females, and a 3.2 percentage point (15.2%) increase for blacks. Although the magnitudes of these results appear to be quite large, the direction of the effect is consistent with the story of substitution between alcohol and marijuana and also between alcohol and cocaine for females and blacks. The results for the other subgroups are not statistically significant at conventional levels.

1.6 The Impact of MLDA on Non-cognitive Skills

This section analyzes the impact of MLDA laws on non-cognitive outcomes using a reduced form analysis. As previously mentioned, during the 1970's and 1980's, the MLDA varied between the age of 18 and 21 across states in the U.S. By controlling for state and birth cohort fixed effects, this analysis exploits the variation of the MLDA across birth cohorts within a state and across states within a birth cohort. This variation is used to estimate the causal effect of differences in the legal drinking age on measures for self-esteem and locus of control. Self-esteem is measured by the Rosenberg Score in 1980, 1987 and 2006 allowing for both short and long-run analyses. One measure of locus of control, the Rotter Scale, is measured in the 1979 survey, and the other measure, the Pearlin Mastery Index, is measured in the 1992 survey.

For the earlier outcomes (1979 and 1980), the independent variable of interest is a dichotomous variable indicating whether the individual could legally drink at the time of the survey. For later outcomes, the MLDA is not binding since these surveys occur after the respondents turn 21 (1986 for the youngest cohort). In this case, the proportion of legal months individuals could legally drink between their 18th and 21st birthday serves as the policy variable. For example, if an individual faced an MLDA of 19 for all three years, then he would be legal to drink for 24 months (age 19 and age 20), and his proportion of legal months would be $\frac{2}{3}$. The effect of the MLDA on the earlier outcomes (1979 and 1980) is measured by regressions using specification (1.4), and the effect on later outcomes (1987, 1992 and 2006) is

measured using specification (1.5).

$$Outcome_{isjm} = \beta_0 + \beta_1 LegaltoDrink_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.4}$$

$$Outcome_{isjm} = \beta_0 + \beta_1 Prop.LegalMonths[18-20]_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.5}$$

For individual i, in state s, with birth year j and birth month m. State and birth cohort fixed effects are represented by δ_s and μ_j, respectively. The vector X_{isjm} contains controls for mother's education, race, gender, having an older sibling and AFQT score. $Outcome_{isjm}$ represents the non-cognitive outcome being tested. The results from specification (1.4) and (1.5) are presented in Table 1.5 with breakdowns by race and gender. The results in panel A indicate that being legally allowed to drink does not significantly affect the Rotter (self-control) Scale in 1979. Somewhat surprisingly, being legally allowed to drink leads to a positive and significant increase in self-esteem for females. The results in panel B indicate that moving from an environment where females cannot legally drink to one where they can results in an average increase in the Rosenberg (self-esteem) Score of 0.551 points, or 13.4% of a standard deviation. [21]

Also, the results in panel C indicate that, in 1987, going from having a proportion of legal months of 0 to 1 (MLDA of 21 in all 3 years to an MLDA of 18 in all 3 years) leads to an increase in the Rosenberg Score of 1.127 points, or 27.4% of a standard deviation, for females and 0.698 points, or 17.1% of a standard deviation, for whites.

[21] This analysis was also conducted for females using the factor variable for Rotter 1979 and Rosenberg 1980 created by Heckman, Stixrud and Urzua (2006) as a dependent variable and legal to drink in 1979 as the policy variable. Although these estimates are imprecisely estimated, the sign is in the same direction as the results in panel B.

Although the coefficients are positive in 2006 for females and whites, they are not statistically significant. The effects of MLDA on self-esteem for males, blacks and Hispanics are not statistically significant at conventional levels for any of the years. Another striking result is that for blacks, a change in proportion of legal drinking months from 0 to 1 increases the Pearlin Index in 1992 by 1.439 points, or 43.1% of a standard deviation. A higher Pearlin Index indicates more self-control. These results provide evidence that less restrictive MLDA laws improve non-cognitive outcomes through higher self-esteem for females in the short-run (1980) and the long-run (1987) and higher self-control for blacks in the long-run (1992).

The next step of this analysis further investigates the effects of the drinking age on the self-esteem and self-control for females. Splitting the female sample by race and mother's education level sheds some light on which female subsamples are driving these results. The results for these subsamples are presented in Table 1.6. Although the effect of the MLDA on the Rotter Scale is not statistically significant for all females, column 6 of panel A indicates an increase in the Rotter Scale (decrease in self-control) due to a non-binding MLDA for females whose mothers' education goes beyond high school. Panel B reveals that the positive effect of being legally allowed to drink on self-esteem for females in 1980 appears to be driven by the white sample and the sample whose mothers merely completed high school. In 1987 (panel C of Table 1.6), the effect for females also appears to be driven by the white sample.

The finding that less restrictive drinking age laws lead to improved self-esteem for females is somewhat perplexing. It seems that the direct effect of increased alcohol

consumption would lead to lower self-esteem. This study further explores this peculiar result for females. The MLDA could possibly be affecting self-esteem indirectly through channels other than alcohol consumption. The next section examines the effect of the MLDA on these possible channels, and tests whether these and other channels related to drug and alcohol onset play a significant role in the effect of the MLDA on self-esteem.

1.7 Channels

Given the surprising results for female non-cognitive skills, this section will investigate whether this is being driven by the effect of MLDA on various channels. It begins by testing the effect of the MLDA on the probability of being married by 18, having sex for the first time by age 18, and having one's first child by age 19. The cumulative distributions for age at first sexual intercourse, age at first marriage and age at birth of first child are displayed in Figures 1.2, 1.3, and 1.4, respectively. The channels that are affected by the MLDA along with drug use and drinking variables will then be used as controls when testing the effect of the MLDA on the Rosenberg (self-esteem) Score for white females in 1980 and 1987. If including these channels as controls in specifications (1.4) and (1.5) has a significant impact on the MLDA coefficient, this will serve as evidence that the channels play a role in the impact of the MLDA. The impact of the MLDA on the sex and marriage channels is tested using specification (1.6), and the impact on the birth of an individual's first child is

tested using specification (1.7).

$$Channel(age)_{isjm} = \beta_0 + \beta_1 AverageMLDA(age-1, age)_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.6}$$

$$Channel(age)_{isjm} = \beta_0 + \beta_1 AverageMLDA(age-2, age-1)_{isjm} + \beta_2 X_{isjm} + \mu_j + \delta_s + \epsilon_{isjm} \tag{1.7}$$

Subscripts are the same as in specifications (1.4) and (1.5). $Channel(age)_{isjm}$ is a dummy variable indicating whether the channel variable has occurred by a certain age, and $averageMLDA(age-1, age)_{isjm}$ is the average of the MLDA that an individual faced at that age and the age before. For example, if the channel being tested is whether or not the individual was married by age 18, the policy variable would be the average MLDA faced by that individual between the ages of 17 and 18. For the first child channel, the average MLDA faced at the previous two ages is used to proxy for the legal drinking environment at the time of the decisions that result in the birth of the first child.

The main results from these regressions are presented in Table 1.7. On average, a one year increase in the average MLDA between ages 17 and 18 leads to a 2.6 percentage point (i.e. 15.9%) decrease in the probability of first marriage by age 18 for females. A one year increase in the average MLDA also leads to a 3.3 percentage point (i.e 16.5%) decrease in the probability of females having their first child by age 19. With a lower MLDA, young men and women are legally allowed to go to bars. Now that young women have access to more men, they are more likely to get married. It may also be that going to bars and marriage are complements for young

women (e.g. they use bars to search for a spouse). The MLDA does not have a statistically significant effect on first having sex by age 18 for females. The effect of the MLDA on these channels for the other samples can be found in Table 1.A2 in Appendix 1.9.

For females, the effect of the MLDA on marriage, childbirth, drug use and regular drinking onset may lead one to believe these channels are responsible for the effect of the MLDA on self-esteem. This section concludes by testing whether including these channels affects the significant results for female self-esteem in 1980 and 1987. Controls for whether individuals were married, had their first child, drank regularly, used marijuana or used cocaine by the time of the survey year are each included in separate regressions of the form (1.4) and (1.5). Since the question about regular drinking was asked in 1983, whether individuals drank regularly by 1983 is used to proxy for whether they drank regularly by 1987. The regressions also include the same control variables used previously. Tables 1.8, 1.9 and 1.10 present results from these regressions for white females in 1980, females whose mothers only graduated high school in 1980 and white females in 1987, respectively. The coefficient on the policy variables are not affected by the inclusion of any or all of the channel variables. These tests provide evidence that the surprising results indicating that a higher MLDA reduces female self-esteem are not being driven by the aforementioned channels.

1.8 Conclusion

This empirical study uses the '79 NLSY to examine the effect of the Minimum Legal Drinking Age (MLDA) on non-cognitive outcomes. The analysis begins by estimating the direct impact of the MLDA on outcomes related to drinking behavior and drug use. Less restrictive MLDAs increase the probability of binge drinking, drinking prevalence and whether or not one begins drinking regularly by age 19. To my knowledge, this is the first study that attempts to quantify the effect of the MLDA on regular drinking onset. For drugs, changes in cocaine and marijuana use in response to changes in the MLDA are consistent with standard substitution effects found in the literature (Crost and Guererro 2012; Deza 2014). Less restrictive MLDA laws lead to a decrease in the probability of cocaine use and marijuana use for females and blacks. Lower MLDA laws also lead to an increase in the probability getting married by age 18 and having a child by age 19 for females. The effects of the MLDA on having sex by age 18 are statistically insignificant.

The main analysis of this paper finds that a less restrictive MLDA leads to an increase in self-esteem for females, but not for males. Since the MLDA also affects females through various channels, these channels are tested to see whether they play a role in determining the effect of MLDA on self-esteem. Since including these variables in the self-esteem regression does not change the results for the affected subsamples, it can be concluded that they are not the channels responsible for the effect of the MLDA on self-esteem. However, it cannot be ruled out that these channels may be correlated with some unobserved determinants of self-esteem which are affected by

the MLDA differently for females. In the short-run, the increase in self-esteem could be caused by a coming of age effect, but this theory is difficult to test. The purpose of this paper was to shed light on an unintended consequence of legal drinking age laws. Although this study finds an adverse effect of a more restrictive MLDA on the self-esteem of females in the reduced form, further research is required to pinpoint the channels responsible for this relationship.

Figure 1.1: Onset of Regular Drinking

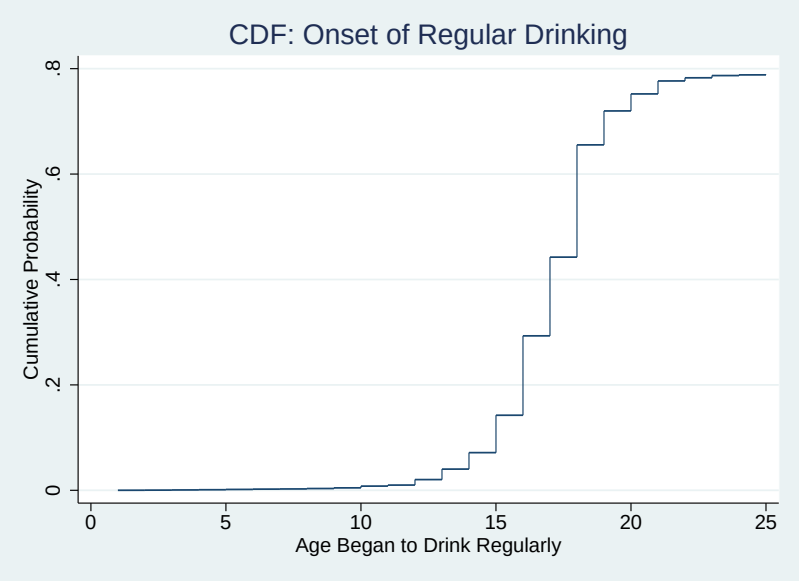

Figure 1.2: Age at First Sexual Intercourse

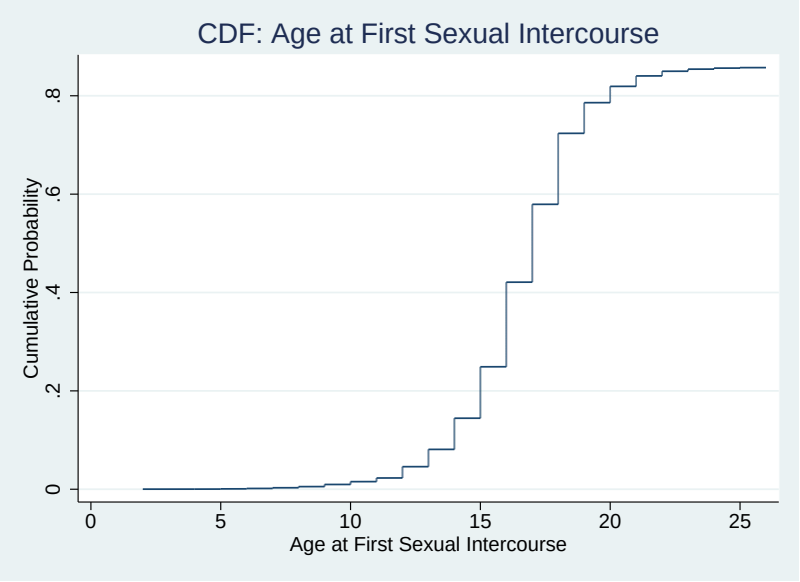

Figure 1.3: Age at First Marriage

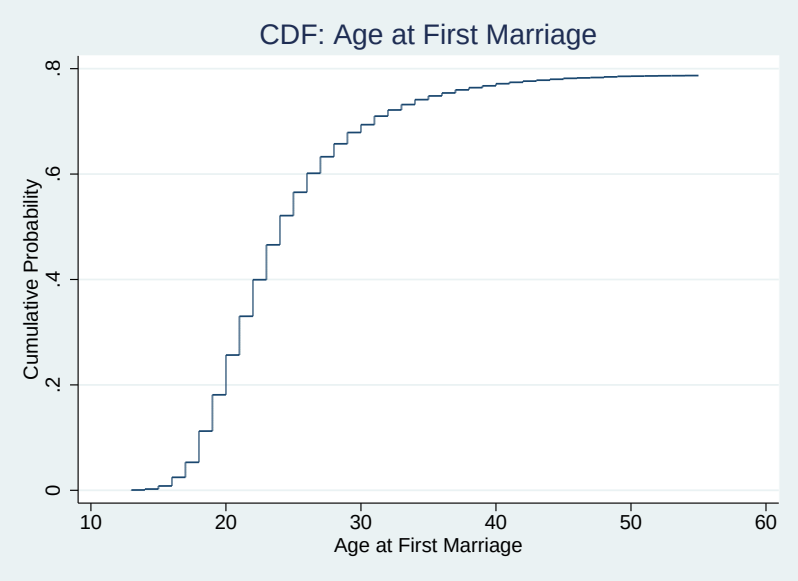

Figure 1.4: Age at Birth of First Child

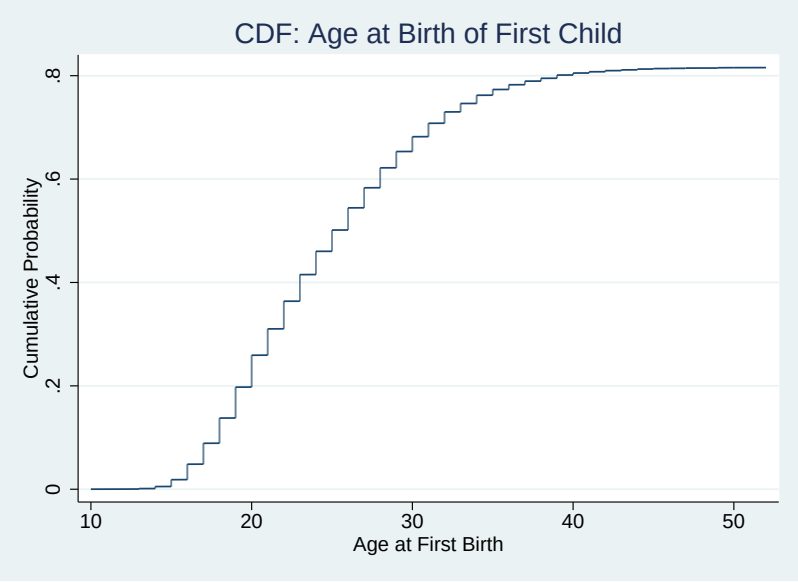

Table 1.1: MLDA Law Changes in the United States: 1967-Present

MLDA Law Changes in the United States 1967-Present

State		Initial	Decreases		Increases		
Alabama	Date	Jan-67	Jul-75		Oct-85		
	MLDA	21	19		21		
Alaska	Date	Jan-67	Sep-70		Oct-83		
	MLDA	21	19		21		
Arizona	Date	Jan-67	Aug-72		Jan-85		
	MLDA	21	19		21		
Arkansas	Date	Jan-67					
	MLDA	21					
California	Date	Jan-67					
	MLDA	21					
Colorado	Date	Jan-67			Jul-87		
	MLDA	18			21		
Connecticut	Date	Jan-67	Oct-72		Jun-82	Oct-83	Sep-85
	MLDA	21	18		19	20	21
Delaware	Date	Jan-67	Jul-72		Jan-84		
	MLDA	21	20		21		
Dist. Of Columbia	Date	Jan-67			Oct-86		
	MLDA	18			21		
Florida	Date	Jan-67	Jul-73		Oct-80	Jul-85	
	MLDA	21	18		19	21	
Georgia	Date	Jan-67	Jul-72		Sep-80	Oct-85	Oct-86
	MLDA	21	18		19	20	21
Hawaii	Date	Jan-67	Mar-72		Oct-86		
	MLDA	20	18		21		
Idaho	Date	Jan-67	Jul-72		Apr-87		
	MLDA	20	19		21		
Illinois	Date	Jan-67	Oct-73		Jan-80		
	MLDA	21	19		21		
Indiana	Date	Jan-67					
	MLDA	21					
Iowa	Date	Jan-67	Jul-72	Jul-73	Jul-78	Jul-86	
	MLDA	21	19	18	19	21	
Kansas	Date	Jan-67			Jul-85		
	MLDA	18			21		
Kentucky	Date	Jan-67					
	MLDA	21					
Louisiana	Date	Jan-67			Mar-87		
	MLDA	18			21		
Maine	Date	Jan-67	Oct-69	Jun-72	Oct-77	Jul-85	
	MLDA	21	20	18	20	21	
Maryland	Date	Jan-67	Jul-74		Jul-82		
	MLDA	21	18		21		
Massachusetts	Date	Jan-67	Mar-73		Apr-79	Jun-85	
	MLDA	21	18		20	21	
Michigan	Date	Jan-67			Dec-78		
	MLDA	18			21		
Minnesota	Date	Jan-67	Jun-73		Sep-76		
	MLDA	21	18		19		
Mississippi	Date	Jan-67			Oct-86		
	MLDA	18			21		
Missouri	Date	Jan-67					
	MLDA	21					
Montana	Date	Jan-67	Jul-71	Jul-73	Jan-79	May-87	
	MLDA	21	19	18	19	21	

MLDA Law Changes in the United States 1967-Present (Cont'd)

State		Initial	Decreases	Increases		
Nebraska	Date	Jan-67	Jul-72	Aug-80	Jan-85	
	MLDA	20	19	20	21	
Nevada	Date	Jan-67				
	MLDA	21				
New Hampshire	Date	Jan-67	Jun-73	May-79	Jun-85	
	MLDA	21	18	20	21	
New Jersey	Date	Jan-67	Jan-73	Jan-80	Jan-83	
	MLDA	21	18	19	21	
New Mexico	Date	Jan-67				
	MLDA	21				
New York	Date	Jan-67		Dec-82	Dec-85	
	MLDA	18		19	21	
North Carolina	Date	Jan-67		Oct-83	Sep-86	
	MLDA	18		19	21	
North Dakota	Date	Jan-67				
	MLDA	21				
Ohio	Date	Jan-67		Aug-82	Aug-87	
	MLDA	18		19	21	
Oklahoma	Date	Jan-67	Dec-76	Sep-83		
	MLDA	21	18	21		
Oregon	Date	Jan-67				
	MLDA	21				
Pennsylvania	Date	Jan-67				
	MLDA	21				
Rhode Island	Date	Jan-67	Mar-72	Jul-80	Jul-81	Jul-84
	MLDA	21	18	19	20	21
South Carolina	Date	Jan-67		Jan-84	Jan-85	Sep-86
	MLDA	18		19	20	21
South Dakota	Date	Jan-67	Jul-72	Jul-84	Apr-88	
	MLDA	19	18	19	21	
Tennessee	Date	Jan-67	May-71	Jun-79	Aug-84	
	MLDA	21	18	19	21	
Texas	Date	Jan-67	Aug-73	Sep-81	Sep-86	
	MLDA	21	18	19	21	
Utah	Date	Jan-67				
	MLDA	21				
Vermont	Date	Jan-67	Nov-71	Jul-86		
	MLDA	21	18	21		
Virginia	Date	Jan-67		Jul-81	Jul-85	
	MLDA	18		19	21	
Washington	Date	Jan-67				
	MLDA	21				
West Virginia	Date	Jan-67		Jul-83	Jul-86	
	MLDA	18		19	21	
Wisconsin	Date	Jan-67		Jul-84	Sep-86	
	MLDA	18		19	21	
Wyoming	Date	Jan-67	May-73	Jul-88		
	MLDA	21	19	21		

The drinking age in this table represents the youngest age that an individual can legally purchase and consume any type of alcohol. (Some states differentiate between beer/wine/liquor)

Table 1.2: Summary Statistics for Main Analysis

	Obs.	Mean	Std. Dev.
Age in 1980	12686	18.775	2.329
Birth Year	12686	1960.445	2.281
Male	12686	0.508	0.500
White	12686	0.798	0.401
Black	12686	0.139	0.346
Hispanic	12686	0.063	0.243
Proportion of Legal Drinking Months	11507	0.626	0.421
Avg. MLDA (age 18-19)	11433	19.109	1.237
Legal to drink at 1979 Survey	12183	0.437	0.496
Legal to drink at 1980 Survey	12409	0.541	0.498
Rotter (Self-Control) Score 1979	12541	8.505	2.392
Rosenberg (Self-Esteem) Score 1980	11992	22.526	4.089
Rosenberg (Self-Esteem) Score 1987	10340	23.687	4.121
Rosenberg (Self-Esteem) Score 2006	7370	23.598	4.444
Pearlin (Self-Control) Index 1992	8938	22.199	3.230
Drank regularly by year 1980	12157	0.717	0.451
Drank regularly by year 1983	12100	0.818	0.386
First Had Sex by year 1980	10735	0.717	0.451
First Had Sex by year 1987	10735	0.814	0.389
Married by year 1980	12332	0.231	0.421
Married by year 1987	12332	0.616	0.486
Had First Child by year 1980	12417	0.167	0.373
Had First Child by year 1987	12417	0.476	0.499
Used Marijuana in last 30 Days (1984)	11705	0.201	0.401
Used Cocaine in last 30 Days (1984)	12013	0.045	0.207
Ever Used Marijuana by year 1980	12367	0.564	0.496
Ever Used Marijuana by year 1987	12367	0.716	0.451
Ever Used Cocaine by year 1980	12362	0.117	0.321
Ever used Cocaine by year 1987	12362	0.341	0.474
Weighted using sample weight variable			

Table 1.3: Impact of MLDA on Drinking Onset, Prevalence and Intensity

	(1) Full Sample	(2) Female	(3) Male	(4) Black	(5) Hispanic	(6) White
Panel A: Dependent Variable- Drank Regularly by Age 19						
Average MLDA (age 18-19)	-0.0435***	-0.0561***	-0.0286**	0.0168	-0.0601*	-0.0519***
	(0.009)	(0.015)	(0.014)	(0.021)	(0.035)	(0.010)
Mean Dep. Var.	0.754	0.677	0.834	0.637	0.656	0.78
N	8477	4458	4019	2163	1355	4959
Panel B: Dependent Variable- Had at Least One Drink Last Month						
Legal to Drink	0.0644***	0.0686***	0.0615***	0.0590**	0.0423**	0.0710***
	(0.014)	(0.019)	(0.017)	(0.025)	(0.020)	(0.016)
Mean Dep. Var.	0.707	0.635	0.782	0.563	0.626	0.737
N	56463	29052	27411	14257	8867	33339
Panel C: Dependent Variable- Binge Drank 3 or More Times Last Month						
Legal to Drink	0.0527***	0.0251**	0.0811***	-0.0236	0.0291**	0.0689***
	(0.015)	(0.012)	(0.025)	(0.015)	(0.012)	(0.018)
Mean Dep. Var.	0.143	0.0681	0.22	0.0899	0.126	0.153
N	56435	29040	27395	14247	8860	33328

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted linear probability regressions. Regressions include controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), gender, race (dummies), and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18) and birth year fixed effects. In panel A, the youngest cohort is dropped since they are not yet 19 at the time of the 1983 survey. Since panels B and C analyze data across several years, these regressions control for a quadratic trend in age and year fixed effects. The outcomes in both panels B and C are available in 1982, 1983, 1984, 1985, 1988 and 1989. Legal to drink is an annual average of a monthly dummy indicating whether it is legal for that individual to drink any type of alcohol in a specific year. Since legality is determined by birth month, the construction of this variable assumes that individals are legal to drink at the beginning of the month their age reaches the MLDA.

Table 1.4: Impact of MLDA on Cocaine and Marijuana Use in 1984

	(1) Full Sample	(2) Female	(3) Male	(4) Black	(5) Hispanic	(6) White
Panel A: Dependent Variable- 1(Used Cocaine Last Month)						
Average MLDA (age 18-19)	0.00801*	0.0126*	0.0031	0.0151*	0.0025	0.0078
	(0.004)	(0.007)	(0.009)	(0.009)	(0.017)	(0.006)
Mean Dep. Var.	0.046	0.036	0.057	0.031	0.046	0.049
N	9117	4775	4342	2338	1465	5314
Panel B: Dependent Variable- 1(Used Marijuana Last Month)						
Average MLDA (age 18-19)	0.0169**	0.0462***	-0.0155	0.0320*	0.0514	0.0131
	(0.008)	(0.012)	(0.012)	(0.018)	(0.031)	(0.008)
Mean Dep. Var.	0.201	0.144	0.262	0.210	0.158	0.203
N	8890	4683	4207	2269	1432	5189

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted linear probability regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), gender, race (dummies) and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18) and birth year fixed effects. The policy variable is the average MLDA faced between the ages of 18 and 19. The drug use variables are constructed using questions about drug use in the previous 30 days in 1984.

Table 1.5: Short-term and Long-Term Impact of MLDA on Non-Cognitive Skills

	(1) Full Sample	(2) Female	(3) Male	(4) Black	(5) Hispanic	(6) White
Panel A: Dependent Variable-Rotter(Locus of Control) Scale 1979 (Lower the more self-control)						
Legal to Drink 1979	0.152	0.207	0.100	0.007	0.312	0.177
	(0.141)	(0.191)	(0.148)	(0.209)	(0.240)	(0.174)
Mean Dep. Var.	8.507	8.601	8.409	8.958	9.023	8.393
Observations	9360	4867	4493	2377	1508	5475
Panel B: Dep. Variable-Rosenberg (Self-Esteem) Score 1980 (Higher the better)						
Legal to Drink 1980	0.194	0.551**	-0.152	-0.196	0.135	0.272
	(0.190)	(0.259)	(0.298)	(0.301)	(0.368)	(0.208)
Mean Dep. Var.	22.56	22.43	22.7	22.43	21.63	22.65
Observations	9214	4803	4411	2340	1475	5399
Panel C: Dep. Variable-Rosenberg (Self-Esteem) Score 1987 (Higher the better)						
Prop. of Legal Months(18-20)	0.549**	1.127**	-0.107	0.0889	-1.211	0.698**
	(0.263)	(0.464)	(0.403)	(0.791)	(0.784)	(0.320)
Mean Dep. Var.	23.78	23.62	23.96	23.39	22.6	23.94
Observations	8581	4545	4036	2210	1361	5010
Panel D: Dep. Variable-Rosenberg (Self-Esteem) Score 2006 (Higher the better)						
Prop. of Legal Months(18-20)	0.589	0.758	0.236	0.531	-0.104	0.697
	(0.391)	(0.695)	(0.625)	(0.659)	(1.100)	(0.448)
Mean Dep. Var.	23.64	23.43	23.89	23.74	23.06	23.67
Observations	6078	3236	2842	1842	1140	3096
Panel E: Dep. Variable- Pearlin (Locus of Control) Index 1992 (Higher the more self-control)						
Prop. of Legal Months(18-20)	-0.0526	0.0845	-0.234	1.439**	-0.214	-0.222
	(0.245)	(0.371)	(0.528)	(0.625)	(0.666)	(0.285)
Mean Dep. Var.	22.25	22.19	22.32	21.8	21.91	22.36
Observations	7313	3808	3505	2175	1378	3760

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted OLS regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), gender, race (dummies) and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. Legal to drink means that the MLDA is not binding at the time of the survey. Proportion of legal months is the number of months a person was legally allowed to drink during ages 18-20 divided by the total number of months that state is not missing (up to 36 months).

Table 1.6: Impact of MLDA on Non-Cognitive Skills for Females

	(1)	(2)	(3)	(4)	(5)	(6)
		Race			Mother's Education Level	
	Black	Hispanic	White	HS Dropout	HS Grad	More than HS
Panel A: Dependent Variable-Rotter(Locus of Control) Scale 1979 (Lower the more self-control)						
Legal to Drink 1979	0.114	0.559	0.163	0.144	0.000454	0.734***
	(0.321)	(0.403)	(0.253)	(0.212)	(0.296)	(0.220)
Mean Dep. Var.	9.039	9.074	8.490	9.115	8.538	7.918
N	1226	786	2855	2216	1864	787
Panel B: Dependent Variable-Rosenberg (Self-Esteem) Score 1980 (Higher the better)						
Legal to Drink 1980	-0.514	0.00914	0.793**	-0.0934	1.032**	0.345
	(0.318)	(0.553)	(0.308)	(0.415)	(0.446)	(0.654)
Mean Dep. Var.	22.38	21.42	22.51	21.44	22.66	23.5
N	1208	774	2821	2183	1839	781
Panel C: Dependent Variable-Rosenberg (Self-Esteem) Score 1987 (Higher the better)						
Prop. Legal Months	1.281	-1.079	1.203**	0.909	0.919	1.917
	(0.797)	(1.056)	(0.558)	(0.667)	(0.665)	(1.178)
Mean Dep. Var.	23.36	22.36	23.76	22.69	23.92	24.51
N	1164	721	2660	2083	1740	722

Roubst standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted OLS regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), race(dummies) and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. Legal to drink is a dummy indicating whether it is legal to drink at the time of the survey. Proportion of legal months is the number of months a person was legally allowed to drink during ages 18-20 divided by the total months with a non-missing state (36 possible).

Table 1.7: Impact of MLDA on Marriage, Sex and Children for Females

	(1) First Married by Age 18	(2) First Had Sex by Age 18	(3) Had First Child by Age 19
Avg. MLDA (17-18)	-0.0263***	-0.0128	-0.0332***
	(0.0097)	(0.0144)	(0.00948)
Mean Dep. Var.	0.165	0.620	0.201
N	4801	4778	4899

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted linear probability regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), having an older sibling, gender and race (dummies). All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. The main covariate, MLDA (17-18) , is the average drinking age individuals face when they are age 17 and 18. This is the lowest age they can access any type of alcohol legally (some states differentiate). The dependent variables are dummies indicating whether a given channel variable occurs by a certain age.

Table 1.8: Impact of MLDA on Rosenberg(Self-Esteem) Score 1980 for White Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Legal to Drink 1980	0.980***	0.974***	0.992***	0.979***	0.984***	0.984***	0.973***
	(0.328)	(0.330)	(0.327)	(0.329)	(0.329)	(0.332)	(0.335)
Marijuana by 1980		-0.352*					-0.323
		(0.180)					(0.223)
Cocaine by 1980			-0.267				-0.140
			(0.270)				(0.282)
Married by 1980				0.107			0.540**
				(0.197)			(0.231)
Drank by 1980					-0.0555		0.0633
					(0.214)		(0.238)
Child by 1980						-0.650***	-0.930***
						(0.215)	(0.264)
Mean Dep. Var	22.53	22.53	22.53	22.53	22.53	22.53	22.53
Observations	2687	2687	2687	2687	2687	2687	2687

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted OLS regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category) and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. Legal to drink is a dummy variable indicating whether it is legal to drink any type of alcohol at the time of the survey in a given year.

Table 1.9: Impact of MLDA on Rosenberg (Self-Esteem)
1980 for Females- Mother HS Grad

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Legal to Drink 1980	1.114**	1.084**	1.115**	1.114**	1.114**	1.157**	1.125**
	(0.466)	(0.462)	(0.471)	(0.468)	(0.466)	(0.456)	(0.461)
Marijuana by 1980		-0.274					-0.273
		(0.240)					(0.242)
Cocaine by 1980			-0.0350				0.0800
			(0.478)				(0.457)
Married by 1980				0.00163			0.378
				(0.268)			(0.351)
Drank by 1980					0.0157		0.111
					(0.269)		(0.274)
Child by 1980						-0.716***	-0.880**
						(0.263)	(0.347)
Mean Dep. Var	22.69	22.69	22.69	22.69	22.69	22.69	22.69
Observations	1760	1760	1760	1760	1760	1760	1760

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted OLS regressions with controls for AFQT score, race and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. Legal to drink is a dummy variable indicating whether it is legal to drink any type of alcohol at the time of the survey in a given year.

Table 1.10: Impact of MLDA on Rosenberg(Self-Esteem) Score 1987 for White Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prop. Legal Months	1.241**	1.240**	1.240**	1.217**	1.238**	1.298**	1.262**
	(0.545)	(0.549)	(0.550)	(0.548)	(0.541)	(0.551)	(0.550)
Marijuana by 1987		0.116					-0.0426
		(0.178)					(0.196)
Cocaine by 1987			0.267				0.204
			(0.161)				(0.150)
Married by 1987				0.271			0.698***
				(0.219)			(0.258)
Drank by 1987 (83)					0.330		0.300
					(0.218)		(0.229)
Child by 1987						-0.534***	-0.792***
						(0.155)	(0.183)
Mean Dep. Var.	23.78	23.78	23.78	23.78	23.78	23.78	23.78
N	2562	2562	2562	2562	2562	2562	2562

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted OLS regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category) and having an older sibling. All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. Proportion of legal months is the number of months a person was legally allowed to drink during ages 18-20 divided by the total number of months with non-missing state (36 possible).

1.9 Appendix

1.9.1 Survival Analysis

This section explains the methodology and results from an alternative analysis of the relationship between the MLDA policy variable and the age of regular drinking onset, survival analysis. There are several advantages to using survival analysis. The dependent variable of interest in analyzing drinking onset is the age at which individuals began to drink regularly, that is once or twice per month. Survival analysis makes it possible to include the observations for respondents that either never began to drink regularly, or had not done so by the time they were surveyed. Another advantage of this method over an ordinary least squares regression is that it does not assume that the errors are normally distributed. This allows for a more flexible specification to possibly better fit the data. T is defined as a nonnegative random variable that represents the time to a specific event. The survivor function, S[t], is just the reverse cumulative distribution function of T.

$$S(t) = 1 - F(t) = Pr(T > t)$$

The survivor function in this study describes the probability that a respondent has not yet begun drinking at a specific time (t). Here, (t) represents the respondent's age. The first step in defining a survivor function is to establish a time of origin (in this case birth year) and a time of failure (in this case the year respondent begins to drink regularly). For the respondents that have not yet begun to drink regularly by 1983, the year the question was asked, they are classified as never having drunk regularly.

To empirically measure the effect of the MLDA on the onset of regular drinking, the hazard function, one closely related to the survivor function, will be used to further examine the relationship between the MLDA and drinking onset. The hazard function, h(t), determines the hazard rate or the instantaneous probability that failure will occur given that it has not yet occurred.

$$h(t) = \lim_{\Delta t \to 0} \frac{Pr(t + \Delta t > T > t | T > t)}{\Delta t} = \frac{f(t)}{S(t)}$$

Here, $f(t)$ is the probability density function. In this case the time of failure is when the respondent begins to drink regularly. The hazard function analysis allows for incorporating a set of controls along with the MLDA policy variable on the right hand side in order to interpret the effect these variables have on the hazard rate. Empirically, a parametric hazard metric is employed which means the regression takes on the following form: $h(t|x_j) = h_0(t)exp(x_j\beta_x)$ where $h_0(t)$ takes on some functional form. The Weibull model which assumes a baseline hazard function of the form $h_0(t) = pt^{(p-1)}exp(\beta_0)$ is used in this analysis.

By incorporating the Weibull model, the hazard metric takes on the following form:

$$h(t|x_j) = pt^{(p-1)}exp(\beta_0 + x_j\beta_x)$$

In this hazard analysis, the independent variable of interest is the average MLDA when the individual is 18 and 19 years old and the control variables include mother's education level, having an older sibling, gender, race, state fixed effects and birth year cohort fixed effects. The results for the hazard analysis are presented in Table 1.A1. For females and whites, a higher MLDA lowers the instantaneous probability

of beginning to drink regularly conditional on not having drunk regularly yet. The results for the other subgroups are not statistically significant.

1.9.2 Creating Channel Variables

First had sex by age 18 :

This variable is constructed using two survey questions from the 1983 survey. One indicates the age males first had sexual intercourse and the other indicates the age females first had sexual intercourse. These questions are also asked in 1984 and 1985, but since all individuals are age 18 by 1983, only the 1983 survey is used in this analysis. Using these ages, a dummy is constructed indicating whether an individual has had sex by the age of 18.

First married by age 18 :

In several years, individuals are asked at what age they began their first marriage. The NLSY combines all these survey questions to create a retrospective variable that indicates the age of first marriage for each individual. This created variable is used in this study to determine the age of first marriage. Using this age, a dummy is constructed indicating whether an individual has had their first marriage by the age of 18.

Had first child by age 19 :

Much like the age of first marriage, age at the birth of first child is asked in several years. The NLSY combines these questions to create a variable that indicates individuals age at the birth of their first child. However, this created variable has a

large number of missing observations. This is addressed in the following way. First, the values in the created variable are given precedence. Only if this value is missing are the other questions referenced. Beginning with the first year this question is asked (1982) and going forward, missing observations for the created variable are replaced by non-missing observations in each year. The age at first birth reported in each subsequent year is used if the following two conditions hold: 1) the created variable has a missing value and 2) all previous years have either a missing value or indicate that the individual has not yet had a child.

Table 1.A1: Hazard Results-Impact of MLDA on Age First Drank Regularly

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Female	Male	Black	Hispanic	White
Avg. MLDA (18-19)	-0.0718**	-0.0586*	-0.077	0.0188	0.0908	-0.0918***
	(0.032)	(0.034)	(0.062)	(0.060)	(0.122)	(0.036)
Obs.	9231	4824	4407	2340	1480	5411

Standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

The reported coefficients are the natural logs of the hazard ratios. The hazard functions are estimated using the Weibull Distribution. Controls include: race, gender, mothers' education level, AFQT Score, having an older sibling, and fixed effects for birth year and state of residence.

Table 1.A2: Impact of MLDA on Channels for Full, Male, Black and Hispanic Samples

Panel A: Dependent Variable: 1(Married by Age 18)					
	(1) Full Sample	(2) Male	(3) Black	(4) Hispanic	(5) White
Avg. MLDA (17-18)	-0.00545	0.0124**	-0.0167*	0.0425	-0.00505
	(0.0057)	(0.0061)	(0.0093)	(0.0318)	(0.0063)
Mean Dep. Var.	0.103	0.0388	0.0495	0.163	0.107
N	9252	4451	2327	1481	5444
Panel B: Dependent Variable: 1(Had Sex by Age 18)					
Avg. MLDA (17-18)	-0.00363	0.00608	0.0145	-0.00778	-0.00471
	(0.0107)	(0.0150)	(0.0242)	(0.0472)	(0.0138)
Mean Dep. Var.	0.679	0.741	0.836	0.648	0.655
N	9126	4348	2294	1468	5364
Panel C: Dependent Varaible- 1(Had Child by Age 19)					
Avg. MLDA (17-18)	-0.00336	0.0216***	-0.0145	-0.0184	-0.00273
	(0.00500)	(0.00582)	(0.0176)	(0.0232)	(0.00606)
Mean Dep. Var.	0.135	0.0670	0.287	0.220	0.104
N	9410	4511	2392	1518	5500

Robust standard errors in parentheses are clustered at the state level
* p<0.10 ** p<0.05 *** p<0.01

Note: This table represents results from weighted linear probability regressions with controls for AFQT score, mother graduating high school, mother surpassing high school (mother high school dropout is the excluded category), having an older sibling, gender and race (dummies). All regressions contain state fixed effects (state when respondent was age 18), and birth year fixed effects. The main covariate, MLDA (17-18) , is the average monthly drinking age individuals face when they are age 17 and 18. This is the lowest age they can access any type of alcohol legally (some states differentiate).

Table 1.A3: Questions from Rosenberg Self-Esteem Score (1980;1987;2006)

Rosenberg Statements
- I am inclined to feel that I am a failure
- I feel I do not have much to be proud of
- I wish I had more self-respect
- I feel useless at times
- I sometimes think I am "no good" at all
- I have a positive attitude
- I am satisfied with myself
- I am as capable as others
- I am a person of worth
- I have a number of good qualities
Possible Answers
1 Strongly Agree
2 Agree
3 Disagree
4 Strongly Disagree

Table 1.A4: Questions from Pearlin Mastery Index (1992)

Pearlin Statements
- No way I can solve the problems I have
- I sometimes feel I'm being pushed around
- I have little control over what happens to me
- I often feel helpless in dealing with problems of life
- Little I can do to change important things in my life
- I can do just about anything I really set my mind to
- What happens to me in the future mostly depends on me
Possible Answers
1 Strongly Disagree
2 Disagree
3 Agree
4 Strongly Agree

Table 1.A5: Statements for Rotter Scale (1979)

Statement Pair 1	A. What happens to me is my own doing B. Sometimes I feel that I don't have enough control over the direction my life is taking
Statement Pair 2	A. When I make plans, I am almost certain that I can make them work B. It is not always wise to plan too far ahead, because many things turn out to be a matter of good or bad fortune anyhow
Statement Pair 3	A. In my case, getting what I want has little or nothing to do with luck B. Many times we might just as well decide what to do by flipping a coin
Statement Pair 4	A. Many times I feel that I have little influence over the things that happen to me B. It is impossible for me to believe that chance or luck plays an important role in my life
Individual Response: Respondents choose A or B for each pair, and then they choose whether their choice is much closer or slightly closer to their opinion	
Each pair receives a score ranging from 1-4:	
	1) Statement consistent with internal locus of control much closer to opinion 2) Statement consistent with internal locus of control slightly closer to opinion 3) Statement consistent with external locus of control slightly closer to opinion 4) Statement consistent with external locus of control much closer to opinion

Chapter 2

Effect of Marijuana Decriminalization in Massachusetts on the Black-White Gap in Arrest Rates

2.1 Introduction

Race relations for black Americans in the United States are uniquely characterized by a deep rooted history of oppression that began with slavery. Over the years, many policies that aimed to provide equal opportunities and freedoms to all Americans have significantly improved conditions for blacks. The Civil Rights Movement, a social movement between 1954 and 1968, inspired many of these equal rights policies.

A few years after the Civil Rights Movement, the War on Drugs began in the United States in 1971. The War on Drugs consists of a political regime and series

of law changes designed to increase the government's effort in enforcing and intensifying laws that restrict the consumption, production or distribution of illegal drugs. Opponents of the War on Drugs claim that it specifically targets black Americans and is used as a form of racial oppression. (Tonry 1994; Fellner 2009). If this is true, the War on Drugs could be indirectly undermining the progress toward the equal treatment of blacks.

The goal of this paper is to empirically examine the effect of the decriminalization of small amounts[1] of marijuana in Massachusetts on the black-white gap in arrest rates for several different crime categories. In terms of the War on Drugs, the decriminalization of marijuana can be thought of as one of its battles. Before going any further, it will be helpful to provide a brief history of the marijuana regulations in the United States. Officially, the prohibition of marijuana began in the 1920's, and it has been classified as a schedule 1 narcotic[2] by the federal government since 1970. At the state and local levels, the liberalization of marijuana policy has occurred in three different forms, the legalization of recreational use, the legalization of medical marijuana and the decriminalization[3] of small amounts.

The legalization for recreational use is the most permissive and has only been implemented in recent years. Since Washington and Colorado first legalized recreational use in November of 2012, Alaska, Oregon, Washington D.C. and a few cities have followed suit. States that have legalized medical marijuana allow doctors to

[1] less than 1 ounce

[2] A schedule 1 narcotic is defined as a drug that has a high potential for abuse, currently has no accepted medical use in treatment in the U.S., and that lacks accepted safety of use under medical supervision (Drug Enforcement Agency n.d.).

[3] This is different from legalization in that there is still a legal consequence, just not incarceration.

prescribe marijuana as medicine to their patients. These states also issue licenses for dispensaries to cultivate and distribute marijuana to patients. Beginning with California in 1996, 22 more states and the District of Colombia have since legalized the use of smoked marijuana for medical reasons.[4] The decriminalization of marijuana reduces the legal consequences of possessing small amounts of marijuana for personal use. The degree of this reduction varies[5], but all states that are considered decriminalized merely issue a fine for the possession of small amounts of marijuana in lieu of incarceration. In the 1970's eleven states decriminalized small amounts of marijuana including Oregon, Alaska, Colorado, California, Maine, Minnesota, Ohio, Mississippi, Nebraska, New York and North Carolina (Thies and Register 1993). The next set of states to decriminalize marijuana was Nevada in 2002 and Massachusetts in 2009 (Scott 2010). This study focuses on decriminalization in Massachusetts due to the stark reduction in penalties, the offense no longer going on an individual's criminal record and a subsequent policy that prohibits the use of the smell of marijuana as probable cause.

The policy of interest in this study is the decriminalization of marijuana in Massachusetts that took effect on January 1, 2009. States that passed decriminalization policies reduced punishments to varying degrees. The following description of decriminalization policy is specific to Massachusetts. Before decriminalization, adults could be arrested, charged with a misdemeanor and spend several days in jail for

[4]The information on the legalization of recreational and medical use is obtained from the the White House website (Marijuana Resource Center n.d.)

[5]Some decriminalized states have reduced the offense to a civil infraction and others still consider it a misdemeanor. There is also variation in the fine associated with this offense after decriminalization.

even a first offense. This crime would also be recorded on their criminal records. Afterward, the possession of less than one ounce of marijuana for people at least 18 years old would result in a mere civil infraction that carries a $100 fine and is no longer recorded on criminal records. For juveniles, the punishment for the possession of less than one ounce is also a $100 fine, but in addition, they are required to attend a drug awareness program and perform community service. Their parents are also notified by the authorities. If a juvenile does not complete the drug awareness and community service, the civil penalty will then be raised to $1,000 for which both the parents and juvenile are responsible for paying (Massachusetts Law 2015). In response to decriminalization, the Supreme Judicial Court in Massachusetts also made it unlawful for police to use the smell of burnt marijuana as probable cause in police searches in 2011. This case law is the result of Commonwealth v. Cruz which involved an incident that occurred on June 24, 2009 (Commonwealth v. Cruz).

Using a difference-in-difference model that allows for a heterogeneous treatment effect for blacks and whites, the effect of this policy on the difference between black and white arrest rates is analyzed at the state and agency-levels for the years 2006-2012. This study begins by estimating the direct effect of the policy on the black-white gap in marijuana possession arrest rates. Then, the indirect effects of the policy on the arrest-rate gap in sales of marijuana, possession and sales of non-marijuana drugs, violent and theft-related offenses is also tested. These indirect effects could potentially shed light on the substitution of arrests between crimes after decriminalization. The arrest-rate data is obtained from the Uniform Crime Reporting (UCR) program administered by the Federal Bureau of Investigation (FBI) and contains

data for over 18,000 law enforcement agencies.[6] It is the most comprehensive data on U.S. arrests currently available. Because the UCR is based on voluntary reporting, some agencies do not report in every year for each offense. To address this issue, only agencies that report arrests for a given offense in every year of the study are included in the preferred specification. The control states are restricted to those without marijuana policy changes during the time of the study, and only black and white arrests are analyzed.

Decriminalization in Massachusetts leads to a decrease in the black-white gap in marijuana possession arrest-rates of 339.7 arrests per 100,000 people for adults and 192 arrests per 100,000 people for juveniles in the preferred specification. Since blacks are arrested for marijuana possession at a higher rate than whites before the policy, these results are not surprising. There is also evidence that decriminalization also reduces the black-white arrest-rate gap for other crimes including the sale of marijuana and non-marijuana drugs for adults and juveniles and theft-related crimes for adults. Overall the results are consistent with a shift in police resources away from poor black neighborhoods after decriminalization of marijuana in Massachusetts.

Section 2.2 provides a review of the related literature, section 2.3 describes the data, section 2.4 outlines the empirical methodology, section 2.5 discusses the results, section 2.6 covers robustness checks, section 2.7 provides a brief discussion and section 2.8 concludes.

[6](Uniform Crime Reports n.d.)

2.2 Literature Review

This section will explore the literature related to both racial disparity in arrests and also other criminal behavior that may be affected by marijuana policy changes. African-Americans are over-represented in incarceration and arrest rates, especially for drug offenses (Blumstein 1993; Tonry 1995; Duster 1997; Beckett et al. 2005; Ramchand et al. 2006). Even if we assume that whites and blacks have identical marijuana usage and aside from bias in policing, other cultural factors may expose black users of marijuana to more opportunities for arrest. Because black marijuana users are more likely to live in urban neighborhoods than whites, they have less access to private space which makes it more difficult for them to conceal their drug use (Mitchell and Caudy 2015). According to self-reports in the 2002 National Survey on Drug Use and Health, blacks are almost twice as likely as whites to buy marijuana outdoors and three times as likely to buy it from a stranger after controlling for demographic variables, current and past drug use, and general drug market covariates (Ramchand et al. 2006). In urban black neighborhoods a relatively large portion of drug transactions occur outdoors in plain sight. Therefore, police with a goal of maximizing drug arrests should focus their attention in these areas. It is also easier for them to perform undercover operations due to the social disorganization of these neighborhoods relative to predominantly white urban or suburban neighborhoods (Tonry 1994).

2.2.1 Racial Bias in Policing

Several studies that examine the black-white gap in arrests attempt to identify racial bias in policing. A substantial literature across several disciplines examines racial bias in motor vehicle searches. For a more in depth review of this literature, please see Harcourt (2004). Knowles, Persico and Todd (2001) develop a model in which police behaving efficiently will arrive at an equilibrium where the percentage of successful searches[7] are equal across races. Conducting searches at this equilibrium can be explained by statistical discrimination, while deviating from this equilibrium can be explained by racial animus which they model as a taste for discrimination. According to their model, they find no evidence of racial animus in searches that yield contraband, but they actually find evidence of a taste for discrimination against white motorists in searches that yield large amounts of drugs.[8]

Racial bias in policing, especially related to illegal drug markets, could potentially be occurring off the roadways which is beyond the scope of motor vehicle search studies. Donohue and Levitt (2001) analyze the effect of the racial composition of a city's police force on the racial patterns of arrests. They find that increasing the amount of officers from a given race is associated with an increase in arrests of suspects of different races, but it has little impact on arrests of suspects of the same race. Using the NLSY 1997, Mitchell and Caudy (2015) test whether differences in drug arrests are explained by observed race differences in drug usage, drug sales, nondrug

[7]This is defined in different ways; the main results use searches that produce contraband and searches that produce large quantities of drugs.

[8]This model was tested on a sample of all motor vehicle searches conducted on a stretch of Interstate 95 in Maryland between 1995 and 1999.

offending, or neighborhood contextual features. They find that after controlling for these alternative explanations, roughly 85% of the baseline black-white gap in drug arrests remains. They conclude that these results are consistent with racial bias in law enforcement. Another study examines the Seattle needle exchange to explore the racial disparity in drug arrests (Beckett et al. 2005). Their findings suggest that this disparity is driven by Seattle law enforcement's focus on black and Latino users of crack cocaine. This effect remains when they control for specific characteristics of the crack cocaine market that make it more visible than other drugs. These characteristics include the relatively high frequency which crack cocaine is exchanged, more exchanges being outdoors and other race neutral factors.

In this paper, one reason decriminalization may affect the racial composition of arrest rates is through the redistribution of police resources in response to the policy. Following the logic in Knowles, Persico and Todd (2001) a shift in police resources could occur through the channel of statistical discrimination. Shifting resources due to statistical discrimination would occur if the probability, or the perceived probability, of a successful police effort (i.e. search, stop, investigation) changes across races. If decriminalization increases the relative cost or decreases the relative benefit of police effort in predominantly black neighborhoods, shifting resources out of these neighborhoods can be explained by statistical discrimination.

2.2.2 Marijuana, Other Drug Use and Crime

Another vein of literature analyzes the effect of increases in marijuana use on other crime outcomes. These relationships should be carefully considered when determining the best marijuana control policy to adopt. Empirically, users of marijuana are more likely to use harder, more dangerous drugs. This relationship could either be due to a stepping stone effect [9] or unobserved heterogeneity that makes a person more likely to use both soft drugs, like marijuana, and harder drugs. Deza (2015) estimates a dynamic discrete choice model to determine whether correlations in soft and hard drug use are driven by a stepping stone effect from soft to hard drugs or by unobserved heterogeneity. She finds a modest sized stepping stone effect between marijuana and harder drugs, but she also finds a similar effect between alcohol and harder drugs. The legal environment in which drugs are consumed could potentially drive these moderate stepping stone effects. For example, in order to purchase marijuana when it is criminalized, a user will inevitably be exposed to the illegal underground drug market. If this exposure makes the user more likely to use other drugs available in this market, then criminalization could potentially foster the transition from soft to hard drug use.

Many studies set out to estimate the effect of marijuana policy on non-drug crimes. Morris et al. (2014) find evidence that the legalization of medical marijuana leads to decreases in homicide and assault rates. They did not find that medical marijuana laws had an enhancing effect for any of the crime types they analyzed.[10]

[9] This is characterized by state dependence between drugs, also known as the gateway drug effect.

[10] They analyzed homicide, rape, robbery, assault, burglary, larceny and auto theft using the FBI Uniform Crime Reports.

Using the 1992, 1993 and 1994 National Crime Victimization, Markowitz (2005) finds that states which have decriminalized marijuana have higher probabilities of assaults and robberies. Many different theories can explain associations between drug use and violence, and these theories can be extended to other types of crimes as well.[11] The relationship between marijuana decriminalization and arrests for violent and theft-related crimes could occur through changes in usage or through changes in police behavior discussed earlier. If decriminalization were to affect marijuana usage, this could in turn affect a potential criminal's tendency to commit a crime, or it could also affect a potential victim's susceptibility to certain personal crimes, especially robbery and rape, through reduced awareness. Since decriminalization reduces the consequences of marijuana use, this should cause demand to increase and marijuana use to also increase. However, there is little evidence of the effect of decriminalization on marijuana use (Model 1993[12]), while several studies find evidence of little to no effect (Johnston et al 1981; Thies and Register 1993; MacCoun and Reuter 1997, 2001; Single, Christie and Ali 2000). Decriminalization could potentially free up resources for police to be able to focus on other crimes. This would result in an increase in arrests for other crimes even if the rate of offending is constant. This change in enforcement may also affect the existing criminal networks which would likely have an effect on various crime rates (Markowitz 2005; Goldstein et al. 1989[13])

This study will contribute to the existing literature by attempting to quantify

[11]For a more complete discussion of drug use and violence, please see Goldstein (1985) and Roth et al. (1993).

[12]The data used in this study only includes hospital emergency room visits.

[13]Although this paper discusses law enforcement and criminal networks as they pertain to crack-cocaine, the same concept , although arguably on a much smaller scale, can be applied to marijuana decriminalization.

the effect of marijuana decriminalization in Massachusetts on the racial disparity in arrests. It examines both the direct effect of this policy on the black-white gap in marijuana possession arrests and the indirect effect on the black-white gap of other types of arrests, including possession and sales of drugs other than marijuana, violent crimes and theft-related crimes.

2.3 Data

The main data set used in this paper is from the Uniform Crime Reporting (UCR) Program which is organized by the Federal Bureau of Investigation (FBI).[14] The UCR publishes several different breakdowns of this data. This study uses the yearly summary of arrests for 43 criminal offense categories[15] by age, sex and race at the level of the reporting agency. To say that the data has arrests by age, sex and race is a bit misleading. Agencies report arrests by age and sex, and they also report arrests by race for adults and juveniles. The UCR does not report race-sex or race-age breakdowns of arrests. The data is voluntarily reported to the FBI on a monthly basis by city, county and state law enforcement agencies.[16] If an offender commits multiple crimes, only the most severe crime is reported to the UCR. This would be concerning if changes in the arrests for one crime affect the reported arrests for another. Since marijuana possession is considered one of the least severe criminal

[14]The data was obtained from the Inter-university Consortium for Political and Social Research (ICPSR) maintained by the University of Michigan.

[15]There are 39 individual offenses reported and 4 offense subtotals. The complete list of reported offenses and subtotals are displayed in Appendix 2.9.

[16]Information on study design is from ICPSR website. Agencies either report directly to FBI, or they report to their state which in turn reports to the FBI. Questionnaires are either mailed or conducted on-site.

offenses, this should not be problematic when analyzing marijuana decriminalization.

The UCR also contains several characteristics of the reporting agencies. Each agency is identified by its state and one of nine U.S. regions. Agencies also report the population of their coverage area. Some types of agencies including colleges and universities, highway patrols, state police, drug enforcement task forces and school districts systematically report a population of zero. Agencies are also classified as MSA/non-MSA, suburban/non-suburban and city/county.

This study aggregates the agency-level data to the state level in the main analyses. The data starts with 2,345,820 agency-offense-year observations.[17] The following steps were taken to prepare the data for the state-level analyses. First, only black and white arrest rates were used in this analysis, and observations with missing offense information are excluded.[18] Also, if any arrest observations were missing for one race and not the other, they were both set to missing.[19] Summary statistics at the state level for agencies that report in all years are displayed in Table 2.1.

Agencies in states other than Massachusetts that passed marijuana policies during the years of the study were also excluded from the main analysis.[20] The reason this study focuses on decriminalization in Massachusetts rather than multiple treatment states is due to the heterogeneous degrees of treatment across states. After decriminalization, some states still assign a hefty fine (e.g. $700 in Nevada), and others still

[17]This study uses data for the years 2006 to 2012.

[18]This amounts to 42,803 observations. Also, a few observations recorded a negative number for arrests; these were changed to missing.

[19]Changing negatives and observations with one race and not the other to missing amounted to 11 observations for adults and 17 observations for juveniles.

[20]These states are Arizona, Connecticut, Delaware, Michigan, New Jersey, New Mexico and Rhode Island. Washington D.C. is also excluded.

charge offenders with a misdemeanor which could affect their criminal record. Also, a majority of these states decriminalized marijuana in the 1970's. Although it may limit the external validity of the study, examining a more recent policy may provide a more relevant insight regarding future policy decisions.

In order to calculate arrest rates, I use alternative measures of population. First, I use population estimates from the American Community Survey (ACS). The ACS is a mandatory[21] survey conducted by the U.S. Census Bureau. They survey approximately 1 in 38 U.S. households annually about detailed information including age, race, gender, income and education. This analysis uses race, age and sample weights. The value of the sample weights in the ACS is the number of people in the U.S. that are represented by a given observation. Aggregating the weights at the state level conditional on race and age group provides a proxy for state-level population corresponding to the demographic groups in the UCR arrest data. However, since I sum agencies who report in every year, the arrest rate is likely to be too low. To make the population estimates correspond to arrest rates, I also use agency level population from the UCR to sum to the state-level population.

The sample in this study is restricted to the years 2006-2012. Years before 2006 are excluded because the Boston Police Department, one of the largest agencies in Massachusetts, did not report arrests to the UCR between 2003 and 2005. Years after 2012 are not included because 2012 is the most recent year currently available for this version of the UCR data.

[21]The Census Bureau conducts an extensive follow up process to ensure all surveys are completed. If a respondent does not submit the survey, a Census staff member will call or even visit the household in person. (U.S Census Bureau)

2.4 Empirical Methodology

In this section I describe my estimation strategy to analyze the effect of marijuana decriminalization on arrest rates in Massachusetts. Using a difference-in-difference model that allows for heterogeneous effects on blacks vs. whites, this analysis estimates the effect of this policy on black-white gap in state-level arrest rates. The following regression equation is estimated in the state-level arrest rate analysis.

$$\begin{aligned} ArrestRate_{stj} = \alpha + \beta_1(Black * After2008 * MA)_{stj} + \beta_2(Black * MA)_{sj} \\ +\beta_3(Black * After2008)_{tj} + \beta_4(After2008 * MA)_{st} \\ +\beta_5 After2008_t + \beta_6 Black_j + \beta_7 MA_s + \mu_s + \delta_t + \epsilon_{stj} \end{aligned} \tag{2.1}$$

For state s, year t and race j. State and year fixed effects are represented by μ_s and δ_t respectively. $Black$ is a dichotomous variable that takes the value of 1 for black arrests and 0 for white arrests. $After2008$ takes on a value of 1 after the policy takes effect and 0 before, and MA takes the value of 1 for Massachusetts and 0 otherwise.[22] The ordinary least squares estimate of β_1 identifies the effect of marijuana decriminalization on the gap between black and white arrest rates in Massachusetts. This effect is estimated using adult and juvenile arrest rates for several offense categories including marijuana possession, marijuana sales/manufacturing, non-marijuana drug

[22]The control states for regressions with 588 observations (e.g. Marijuana Possession in Table 2.2) are Alabama, Alaska, Arkansas, California, Colorado, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin and Wyoming. In regressions with only 574 observations (e.g. Marijuana Sales in Table 2.2), Alabama is dropped due to missing reports for the respective crime.

possession, non-marijuana drug sales/manufacturing,[23] theft-related crimes and violent crimes. The estimate of β_2 represents the average difference between black arrest rates in Massachusetts before 2009 and all other arrest rates before 2009.[24] The estimate of β_3 represents the average difference between black arrest rates in the control states after 2008 and all other arrest rates in the control states.[25] The estimate of β_4 represents the average difference between white arrest rates in Massachusetts after 2008 and all other white arrest rates. This is essentially the effect of the decriminalization policy on white arrest rates. In order to estimate the effect of the policy on black arrest rates, the coefficient estimates for β_1 and β_4 are added together, and an F-test is used to test whether this sum is significantly different from zero. The F-statistic and p-values from this test are reported for every regression. Standard errors are robust and clustered at the state level.

I use alternative estimates of the arrest rate at the state level. In the first specification, I include arrests from agencies that report arrests in all seven years for a specific offense and aggregate to the state level. For the denominator, I use population estimates from the ACS data for each demographic group. For example, in order to calculate the marijuana possession arrest rate for black juveniles in Texas, I divide the number of black juvenile marijuana possession arrests in Texas by the population of blacks in Texas under the age of 18.

[23] In the interest of space, sales/manufacturing offenses will be referred to as sales.

[24] This includes white arrest rates in all states and black arrest rates in the control states.

[25] This includes white arrest rates for control states in all years and black arrest rates in control states before 2009.

$$ArrestRate_{stj} = \frac{\sum_a Arrests_{astj}}{ACSpopulation_{stj}}$$

Where a represents the agency.

For the second specification, I use the same method for summing arrests as before but divide this arrest measure by the agency-level population summed to the state level. Because the agency population is not broken down by race or age, the state-level ACS data is used to scale the arrest rate by the fraction of the relevant demographic group in each state.

$$ArrestRate_{stj} = \frac{\sum_a Arrests_{astj}}{\sum_a UCRpopulation_{astj}} * \frac{ACSpopulation_{stj}}{ACSpopulation_{st}}$$

I also exclude agencies such as colleges and universities, highway patrols, state police, drug enforcement task forces and school districts that report zero population from both arrest and population calculations in the second specification.

In the third specification, I expand the sample to also include agencies that report in all pre-policy years (2006-2008) while setting the post-policy years' values to zero for marijuana possession arrests if they are missing.[26]

[26] $$ArrestRate_{stj} = \frac{\sum_a Arrests_{astj}}{ACSpopulation_{stj}}$$

2.5 Results

The main purpose of this paper is to estimate the effect of marijuana decriminalization on arrest rates for several different criminal offense categories. Using the model defined in equation (2.1), this analysis attempts to quantify the effect of the decriminalization policy in Massachusetts on the difference between black and white arrest rates. Both juvenile and adult arrest rates are analyzed separately for each offense category.

2.5.1 The Effect of Decriminalization on Marijuana Arrest Rates

State-level regression results for marijuana arrest rates are displayed in Table 2.2 for adults and Table 2.3 for juveniles. These results implement the arrest-rate specifications outlined in section 2.4. In columns 1 and 2, only agencies that report in every year are included in the arrest rate calculation. In columns 3 and 4, only agencies that report in every year and report a positive population are included. Also, this specification calculates population based on agency reported population. Columns 5 and 6 extend the sample to agencies that report in all pre-policy years (2006-2008). Since some agencies are likely to stop reporting marijuana possession arrests after decriminalization, the missing marijuana possession arrest rates after the policy are recoded as zero in this specification.

The coefficients in the first row of Table 2.2 correspond to OLS estimates of β_1 in equation (2.1). This can be interpreted as the effect of decriminalization on the

black-white marijuana arrest-rate gap for adults in Massachusetts. Restricting to agencies that report in all years, the policy leads to a decrease in the difference between black and white adult arrest rates of 339.7 arrests per 100,000 adults for marijuana possession as seen in column 1, and 13.54 per 100,000 adults for marijuana sales as seen in column 2. For the second arrest-rate specification, decriminalization decreases the black-white gap in adult arrest rates by 610.0 per 100,000 for marijuana possession and by 31.39 per 100,000 for marijuana sales. When the sample is extended to include agencies that report in all pre-policy years, decriminalization leads to a decrease in the black-white gap in adult arrest rates of 328.4 per 100,000 for marijuana possession as seen in column 5 and 12.34 per 100,000 for marijuana sales as seen in column 6. For adults, the policy substantially reduces the difference between black and white marijuana possession and sales arrest rates. The estimate of β_2 represents the average difference between black arrest rates in Massachusetts before 2009 and all other arrest rates before 2009.[27] On average, blacks in Massachusetts have lower arrest rates than the excluded category for marijuana possession and higher arrest rates for sales. The estimate of β_3 represents the average difference between black arrest rates in the control states after 2008 and all other arrest rates in the control states.[28] These estimates are not statistically significant for adult possession or sales arrest rates in Table 2.2. The estimate of β_4 represents the effect of the policy on white arrest rates. Decriminalization reduces the white arrest rate for possession in all three specifications and the white sales arrest rate in the second specification as seen in column 4. The other estimates of β_4 for sales

[27]This includes white arrest rates in all states and black arrest rates in the control states.

[28]This includes white arrest rates for control states in all years and black arrest rates in control states before 2009.

are also negative but insignificant. The effect of the policy on black arrest rates is calculated by adding the estimates of β_1 and β_4, and the significance is tested using an F-test. The policy reduces the black arrest rates for marijuana possession and sales in all three specifications.

The results for juvenile marijuana arrests are displayed in Table 2.3. Restricting to agencies that report in all years, decriminalization decreases the black-white gap in juvenile arrests by 192.0 per 100,000 for marijuana possession as seen in column 1 and by 14.57 per 100,000 for marijuana sales as seen in column 2. In the second arrest-rate specification, the policy leads to a decrease in the black-white gap in juvenile arrest rates of 349.0 per 100,000 for marijuana possession as seen in column 3 and 34.19 per 100,000 for marijuana sales as seen in column 4. When all agencies that report in every pre-policy year are included in the sample, the policy decreases the black-white gap in juvenile arrest rates by 165.4 per 100,000 for marijuana possession as seen in column 5 and by 13.45 per 100,000 for marijuana sales as seen in column 6.

For both adults and juveniles, the results demonstrate a consistent pattern that decriminalization leads to a decrease in the black-white gap in arrests for both marijuana possession and marijuana sales. It should come as no surprise that the black-white gap in marijuana possession arrest rates falls. Decriminalization should theoretically cause arrest rates for both blacks and whites to fall to zero. Since blacks are arrested at a higher rate before the policy, it makes sense that the gap would be reduced. The question of what happens to other arrests and why is much more

interesting. Since there is evidence that marijuana usage does not change after decriminalization,[29] it may be that the substitution occurs on the side of law enforcement through the shifting of resources. Other literature suggests that black drug market participants have less access to private space than their white counterparts and are more likely to participate in open air drug markets (Mitchell and Caudy 2015). If police are shifting some of their resources out of poor minority neighborhoods after decriminalization due to changes in the cost-benefit structure of searches, this could explain why blacks who sell marijuana appear to benefit more from this decriminalization policy than whites.

2.5.2 The Effect of Decriminalization on Non-Marijuana Arrest Rates

This section analyzes the indirect effect of marijuana decriminalization on the black-white gap in state-level arrest rates for other drugs, crimes related to theft and violent crimes. It is plausible that marijuana decriminalization could indirectly affect the black-white gap in non-marijuana arrests rates, especially through the following channels. The policy is likely to influence the use of marijuana as probable cause in the investigation of other crimes.[30] If police were using marijuana as probable cause for blacks at a relatively higher rate than whites before the policy, this would help to explain a decrease in the black-white gap in arrest rates for other crimes. Through

[29]Johnston et al. 1981; Thies and Register 1993; MacCoun and Reuter 1997, 2001; Single, Christie and Ali 2000

[30]As a direct result of decriminalization, the Supreme Judicial Court in Massachusetts made it unlawful for police to use the smell of burnt marijuana as probable cause for police searches in 2011 (Commonwealth v. Cruz 2015).

changes in the costs and benefits of police efforts, the policy could also affect law enforcement behavior and how police allocate their resources.

For drugs other than marijuana, the offenses (sales and possession) are divided into three categories: cocaine/opium and their derivatives, truly addicting synthetic narcotics and other dangerous non-narcotic drugs. The total arrests for these offenses are calculated by totaling the arrests for the three offenses in each agency-year observation. Missing arrests are treated as zeros unless all three arrest categories are missing for an agency-year observation in which case that observation is coded as missing. For the state-level analysis, the agency-level drug possession and sales arrest totals are aggregated to the state level and then divided by ACS state-level population to form the arrest rates. Equation (2.1) is estimated for adults and juveniles using the same arrest rate specifications used in section 2.5.1.

Adult arrest-rate results for all drugs other than marijuana are displayed in Table 2.4. Decriminalization does not significantly affect the black-white gap in adult arrests for non-marijuana possession. For the arrest-rate specification that includes all agencies that report in every year, the policy decreases the adult black-white gap in non-marijuana sales arrest-rates by 67.4 arrests per 100,000 adults as seen in column 2. For the specification that excludes agencies that report zero population, decriminalization decreases the black-white gap in non-marijuana sales arrest rates by 119.3 adult arrests per 100,000 adults. When the sample is extended to agencies that report in all pre-policy years, the policy leads to a decrease in the adult arrest-rate gap of 64.55 arrests per 100,000 for non-marijuana drug sales.

State-level results for juvenile arrest rates for drugs other than marijuana are

displayed in Table 2.5. The effect of decriminalization on the black-white gap in juvenile arrest rates for possession of non-marijuana drugs is small and statistically insignificant for all three arrest-rate specifications. For non-marijuana sales, decriminalization leads to a decrease in the black-white arrest-rate gap of 29.85 juvenile arrests per 100,000 juveniles in the first specification, a marginally significant decrease of 42.26 per 100,000 juveniles in the second specification and a decrease of 28.65 per 100,000 in the third specification. This further supports the claim that blacks are more susceptible to drug sales arrests compared to whites due to the prevalence of open air drug markets and less access to private space in poor minority neighborhoods. Decriminalization can potentially affect the visibility of criminal behavior as well as police incentives to target these neighborhoods.

After decriminalization, the differences in sales arrest rates for both adults and juveniles are dampened significantly for marijuana and other drugs. Police seem to be substituting away from drug sales arrests for blacks. It seems plausible that before decriminalization, police were more likely to use the smell of marijuana as probable cause in arrests for drug sales. Unable to use marijuana as probable cause[31], police may be shifting resources away from black neighborhoods, where drug activity is more visible, due to an increase in the relative cost of successful police searches in these areas.

The shifting of police resources and changes in the use of marijuana as probable

[31](Commonwealth v. Cruz 2015)

cause in response to the policy could also lead to an indirect effect of decriminalization on violent and theft-related crimes. Results for the black-white gap in violent[32] and theft-related[33] arrest rates are displayed in Tables 2.6 and 2.7. Table 2.6 contains results for adults, and Table 2.7 contains results for juveniles. The way that individual crimes in the violent and theft-related categories are aggregated is the same as the non-marijuana drug arrest totals.

The effect of the policy on the gap in adult arrest rates for violent crimes is not statistically significant in any of the specifications. Restricting to agencies that report in every year, decriminalization decreases the black-white gap in adult theft-related arrest rates by 210.6 arrests per 100,000 adults as seen in column 1. In the second specification, the policy leads to a decrease in the black-white gap in the adult theft-related arrest rates of 229.6 per 100,000. Restricting to agencies that report in all pre-policy years, the policy decreases the adult arrest-rate gap for theft related crimes by 195.0 arrests per 100,000 adults. For juveniles, the effect of decriminalization on the black-white gap in theft-related and violent arrest rates is not statistically significant in any of the specifications.

The results in Tables 2.6 and 2.7 provide strong evidence that the decriminalization of marijuana causes the black-white gap in theft related arrest rates to decrease for adults. No effect is found for adult violent crimes or juvenile theft-related or violent crimes. Another interesting result, is the estimate of β_4, or the effect of

[32]The violent arrest total consists of: murder/non-negligent manslaughter, negligent manslaughter, forcible rape, aggravated assault and other assaults.

[33]The term theft-related may be somewhat unclear. These crimes have something to do with taking something that belongs to someone else. They consist of: robbery, burglary/breaking and entering, larceny/theft (not motor vehicle), motor vehicle theft, forgery/counterfeiting, fraud, embezzlement and buying/selling/receiving stolen property.

the policy on the white arrest rate. For both adult and juvenile theft-related and violent arrest rates, decriminalization leads to a statistically significant increase in white arrest rates in all three specifications. Not only does it appear that police are substituting away from theft-related arrests for blacks after decriminalization, but also that they are arresting more whites for theft-related and violent crimes.

This study finds evidence that decriminalization differentially affects blacks and whites through changes in their arrest rates. Decriminalization leads to a decrease in the black-white gap in adult arrest rates for marijuana possession, marijuana sales, non-marijuana drug sales and theft-related crimes. It also leads to a decrease in the black-white gap in juvenile arrest rates for marijuana possession, marijuana sales and non-marijuana drug sales. This evidence is consistent with the pattern that police are shifting resources out of predominantly black neighborhoods in response to the decriminalization policy. These results provide suggestive evidence that drug control policies disproportionately target blacks as proposed by several other studies (Tonry 1994; Fellner 2009; Nunn 2002; Beckett et al. 2005).

2.6 Robustness Checks

This section will check whether the main analysis is robust to certain changes in specification. For all crime categories, the sample is extended to all agencies that report to the UCR instead of just agencies that report arrests in every year for a given offense. Here, missing observations are treated as zero in the state-level aggregation of arrests. Only states with at least one agency reporting in every year are included

in this portion of the analysis. This utilizes more of the information in the data set, but may suffer from bias due to misreporting. These results are displayed in Table 2.A1 and Table 2.A2 in Appendix 2.9. These coefficient estimates are consistent with the pattern found in the main results. Decriminalization leads to a fall in the black-white gap in arrests for marijuana possession, marijuana sales and non-marijuana drug sales for adults and juveniles and a fall in the gap in theft-related arrest rates for adults.

Also, the effect of decriminalization on the black-white gap in marijuana arrest rates is estimated using states adjacent to Massachusetts as controls instead of all states without a marijuana policy during the study. This attempts to capture the regional characteristics that make adjacent states similar to each other, but it comes with a loss of statistical power. The control states here are Connecticut, Maine, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island and Vermont. These results are displayed in Table 2.A3 and correspond to the specification in Table 2.2. These results still find a significant decrease in the black-white gap in adult marijuana possession arrest rates, but the results for marijuana sales, although similar to Table 2.2 in sign and magnitude, are not statistically significant. This is likely to be due to the reduction in power with this specification.

Lastly, the analysis is conducted at the agency level for agencies that report a positive population and report arrests in every year of the study. The sample is exactly the same as in columns 3 and 4 of Tables 2.2-2.7 in the state-level analysis. For the agency level analysis, the following regression specification is estimated.

$$ArrestRate_{astj} = \alpha + \beta_1(Black * After2008 * MA)_{stj} + \beta_2(Black * MA)_{sj} + \beta_3(Black * After2008)_{tj} + \beta_4(After2008 * MA)_{st} + \beta_5 After2008_t + \beta_6 Black_j + \beta_7 MA_s + \mu_s + \delta_t + \epsilon_{astj} \quad (2.2)$$

Standard errors are robust and clustered at the state level. Each agency-level observation is weighted by its agency-reported population. Agency-level arrest rates are calculated by dividing agency level arrests by the agency reported population. Because each agency only reports one population for all ages and demographic groups, this agency-level arrest rate is scaled by the fraction of the state population represented by each demographic group which is imputed from the ACS data.

$$ArrestRate_{astj} = \frac{Arrests_{astj}}{UCRpopulation_{ast}} * \frac{ACSpopulation_{stj}}{ACSpopulation_{st}}$$

These results are displayed for all the offense categories in Table 2.A4 for adults and Table 2.A5 for juveniles in Appendix 2.9. These findings are nearly identical in magnitude and significance to the state-level analysis in columns 3 and 4. The main results in section 2.5 appear to be robust to the different specifications explored in this section.

2.7 Discussion

To simplify this discussion, arrests can be thought of as an interaction between arrestees and police. For arrest patterns to change, it must be that either the behaviors

of arrestees (or potential arrestees) are changing, or that the behaviors of police are changing. Potential arrestees can change the rate they commit crimes or they may have some control over their likelihood of police interaction (i.e. the way crimes are committed, visibility or suspiciousness). Criminals may also attempt to evade arrest which would decrease arrests if successful or if unsuccessful, may increase the severity of charges. Police may change their behavior for several different reasons. It may be that their behavior changes due to a restructuring of enforcement (i.e. law changes, political influence, departmental orders), changes in the perceived probability of successful arrests (i.e. statistical discrimination) , changes in personal preference (i.e. discretion in arrests or racial bias), or some combination of the three.

Because the existing literature provides evidence that marijuana decriminalization does not significantly affect usage (Johnston et al 1981; Thies and Register 1993; MacCoun and Reuter 1997, 2001; Single, Christie and Ali 2000), it seems unlikely that the change in the pattern of arrest rates is due to changes in usage altering criminal behavior. However, decriminalizing marijuana may alter a potential arrestee's way of using marijuana, or even decrease a criminal's incentive to evade arrest, especially since it is no longer used as probable cause for searches.

Unless law enforcement decreases its overall size in response to the policy, more resources (i.e. police officers, courts, jail space) should be available to enforce other crimes after decriminalization. The changes in patterns of arrests found in this study may shed some light on how they are utilizing these excess resources. Literature suggests that drug use and drug transactions are relatively more visible in poor minority neighborhoods because users have less access to private space and drug

transactions are more likely to occur outdoors (Mitchell and Caudy 2015). Without marijuana possession as a reason for probable cause, the cost to police of focusing their attention in these neighborhoods should increase relative to the benefit. The results in this study are consistent with the pattern that police are shifting resources out of these neighborhoods. Further investigation of changes in arrest patterns and other outcomes in response to drug control policies is necessary for the optimal implementation of future policies.

2.8 Conclusion

Using a difference-in-difference model that allows for a heterogeneous treatment effect on blacks vs. whites, this study finds that the decriminalization of marijuana in Massachusetts leads to a significant decrease in the black-white gap in arrest rates for both drug crimes and non-drug crimes. The policy decreases the black-white gap in adult and juvenile arrest rates for possession and sales of marijuana and sales of non-marijuana drugs and adult arrest rates for theft-related offenses. The main analysis is conducted at the state level using three different specifications of arrest rates. These results are robust to a change in sample selection, using states adjacent to Massachusetts as controls, and conducting the analysis at the agency level.

The effect decriminalization in Massachusetts has on the black-white arrest-rate gap for several criminal offenses is consistent with the theory that stricter drug control policies affect blacks relatively more than whites (Tonry 1994; Fellner 2009; Nunn 2002). These empirical findings are consistent with decriminalization causing a shift

in police resources out of areas where blacks are more likely to be arrested. Such a shift in resources could be due to differences in the visibility of drug markets across races (Mitchell and Caudy 2015) and changes in the use of marijuana as probable cause in the investigation of other crimes (Commonwealth v. Cruz 2015). Further investigation of decriminalization in other contexts, other types of drug control policies and potential responses in police and criminal behavior could potentially paint a more complete picture of the racial implications of these policies.

Table 2.1: State-level Summary Statistics for Agencies that Report in Every Year

	Obs	Mean	Std. Dev.
Number of Agencies	3,514	124.80	147.20
Agency Reported State Population[a]	3,514	4,031,005	6,162,870
ACS State Population[b]	3,514	6,072,295	6,953,021
Year	3,514	2009.00	2.00
White Adult Arrest Rates			
Marijuana Possession	294	166.28	86.73
Marijuana Sales	287	16.14	13.65
Other Drugs Possession	294	125.47	118.23
Other Drugs Sales	294	32.53	24.06
Violent Crimes	294	430.79	187.94
Theft Related Crimes	294	509.72	215.01
Black Adult Arrest Rates			
Marijuana Possession	294	785.43	554.69
Marijuana Sales	287	97.23	79.63
Other Drugs Possession	294	536.02	443.38
Other Drugs Sales	294	244.20	201.55
Violent Crimes	294	2182.13	1257.88
Theft Related Crimes	294	2071.60	1131.47
White Juvenile Arrest Rates			
Marijuana Possession	294	129.80	81.01
Marijuana Sales	287	9.72	13.87
Other Drugs Possession	294	25.47	18.64
Other Drugs Sales	294	6.18	5.24
Violent Crimes	294	242.91	123.79
Theft Related Crimes	294	445.46	239.96
Black Juvenile Arrest Rates			
Marijuana Possession	294	311.84	595.65
Marijuana Sales	287	28.01	43.61
Other Drugs Possession	294	61.78	80.50
Other Drugs Sales	294	32.76	66.70
Violent Crimes	294	1294.08	2219.87
Theft Related Crimes	294	1946.04	2714.09

[a]This population measure is calculated by summing the population reported by each agency at the state level.

[b]This population measure is calculated by summing the sample weights in the ACS data at the state level.

*Arrest Rates are Arrests per 100,000 people in corresponding demographic group.

Table 2.2: State-level Adult Arrest Rates: Marijuana

Adult Arrests per 100,000 Adults						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years[2]	
	(1) Possession	(2) Sales	(3) Possession	(4) Sales	(5) Possession	(6) Sales
Black*After2008*MA	-339.7***	-13.54***	-610.0***	-31.39***	-328.4***	-12.34**
	(30.28)	(4.300)	(46.15)	(11.39)	(31.07)	(4.579)
Black*MA	-225.3**	34.42***	-213.2	70.34**	-242.2***	34.87***
	(85.43)	(11.90)	(166.0)	(27.95)	(84.13)	(11.89)
Black*After2008	-18.92	2.069	-49.81	5.558	-29.86	-0.306
	(30.28)	(4.300)	(46.15)	(11.39)	(31.07)	(4.579)
After2008*MA	-58.99***	-1.112	-74.29***	-3.227**	-84.68***	-0.848
	(4.601)	(0.950)	(6.581)	(1.575)	(5.212)	(0.965)
After2008	4.215	2.470	-11.77	3.147	-6.227	0.206
	(18.81)	(2.924)	(25.10)	(7.313)	(19.25)	(2.946)
Black	639.9***	79.26***	981.2***	179.1***	659.9***	80.91***
	(85.43)	(11.90)	(166.0)	(27.95)	(84.13)	(11.89)
MA	383.2***	22.62***	204.8***	7.086	254.0***	52.51***
	(39.42)	(5.788)	(73.71)	(13.66)	(39.19)	(5.765)
F-test	157.50	9.73	197.60	9.33	156.40	7.08
P-value	0.000	0.003	0.000	0.004	0.000	0.011
Avg. Black Arrest Rate	785.43	97.23	1155.04	213.76	806.20	99.20
Avg. White Arrest Rate	166.28	16.14	215.69	30.19	173.61	17.53
S.D. Black Arrest Rate	554.69	79.63	986.45	183.50	547.75	80.55
S.D. White Arrest Rate	86.73	13.65	90.36	17.97	85.59	13.98
Observations	588	574	588	574	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each

[2] For possession arrests in column 5, missing arrests after the policy year are coded as zero arrests.

Robust standard errors in parentheses are clustered at the state level.

* p<0.10 ** p<0.05 *** p<0.01

Table 2.3: State-level Juvenile Arrest Rates: Marijuana

Juvenile Arrests per 100,000 Juveniles						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years[2]	
	(1) Possession	(2) Sales	(3) Possession	(4) Sales	(5) Possession	(6) Sales
Black*After2008*MA	-192.0**	-14.57***	-349.0***	-34.19***	-165.4**	-13.45***
	(72.87)	(4.709)	(95.11)	(10.99)	(72.03)	(4.681)
Black*MA	11.77	28.20***	101.4*	62.71***	-17.43	26.97***
	(40.74)	(5.875)	(59.28)	(13.40)	(40.79)	(5.839)
Black*After2008	19.93	-9.819**	9.181	-20.50*	17.93	-10.94**
	(72.87)	(4.709)	(95.11)	(10.99)	(72.03)	(4.681)
After2008*MA	-49.44***	-3.205***	-86.23***	-5.233***	-90.06***	-3.777***
	(4.236)	(0.596)	(6.184)	(1.152)	(4.359)	(0.595)
After2008	-10.63	-0.0350	-27.84	-5.022	-13.84	-1.207
	(31.91)	(2.690)	(42.25)	(6.018)	(31.70)	(2.874)
Black	173.0***	23.42***	264.0***	51.89***	175.3***	24.09***
	(40.74)	(5.875)	(59.28)	(13.40)	(40.79)	(5.839)
MA	148.3***	11.92***	81.76**	16.39***	152.1***	20.18***
	(25.26)	(1.718)	(32.34)	(3.780)	(25.39)	(1.731)
F-test	11.02	13.14	20.82	11.87	12.66	12.630
P-value	0.002	0.001	0.000	0.001	0.001	0.001
Avg. Black Arrest Rate	311.84	28.01	435.33	58.55	316.63	29.00
Avg. White Arrest Rate	129.80	9.72	168.37	17.32	133.70	10.64
S.D. Black Arrest Rate	595.65	43.61	761.82	91.15	595.68	44.10
S.D. White Arrest Rate	81.01	13.87	76.99	16.87	80.96	14.01
Observations	588	574	588	574	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each

[2] For possession arrests in column 5, missing arrests after the policy year are coded as zero arrests.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.4: State-level Adult Arrest Rates:
All Drugs Excluding Marijuana

Adult Arrests per 100,000 Adults						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years	
	(1) Possession	(2) Sales	(3) Possession	(4) Sales	(5) Possession	(6) Sales
Black*After2008*MA	56.92	-67.40***	95.16	-119.3***	62.58	-64.55***
	(45.55)	(14.22)	(58.76)	(38.65)	(45.80)	(14.72)
Black*MA	-223.8***	242.3***	-341.6***	475.7***	-233.4***	236.6***
	(74.59)	(34.58)	(90.76)	(65.53)	(74.28)	(35.24)
Black*After2008	-231.2***	-98.94***	-325.8***	-208.7***	-238.8***	-105.5***
	(45.55)	(14.22)	(58.76)	(38.65)	(45.80)	(14.72)
After2008*MA	-1.909	-3.116**	7.545	-6.455***	1.442	-2.369*
	(5.181)	(1.369)	(6.689)	(1.977)	(5.489)	(1.345)
After2008	-72.07***	-22.25***	-105.0***	-39.80***	-81.12***	-25.18***
	(19.30)	(6.927)	(25.50)	(11.84)	(19.19)	(6.721)
Black	547.2***	263.4***	775.7***	500.4***	558.1***	273.0***
	(74.59)	(34.58)	(90.76)	(65.53)	(74.28)	(35.24)
MA	302.2***	153.8***	210.5***	270.3***	200.0***	141.3***
	(25.07)	(13.79)	(31.00)	(23.15)	(25.09)	(13.98)
F-test	1.29	23.37	2.73	10.25	1.72	19.76
P-value	0.262	0.000	0.106	0.003	0.197	0.000
Avg. Black Arrest Rate	536.02	244.20	753.80	444.92	547.62	252.28
Avg. White Arrest Rate	125.47	32.53	171.11	54.04	130.68	34.81
S.D. Black Arrest Rate	443.38	201.55	518.37	352.06	443.06	204.29
S.D. White Arrest Rate	118.23	24.06	114.19	26.36	117.79	24.16
Observations	588	588	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each state.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.5: State-level Juvenile Arrest Rates:
All Drugs Excluding Marijuana

Juvenile Arrests per 100,000 Juveniles						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years	
	(1) Possession	(2) Sales	(3) Possession	(4) Sales	(5) Possession	(6) Sales
Black*After2008*MA	-2.154	-29.85***	8.199	-42.26*	-1.186	-28.65***
	(9.262)	(7.800)	(18.55)	(24.11)	(8.990)	(7.833)
Black*MA	-20.38**	37.25***	-46.18**	55.98**	-21.61***	36.44***
	(7.771)	(11.56)	(17.74)	(27.03)	(7.706)	(11.60)
Black*After2008	-20.21**	-24.60***	-39.67**	-62.21**	-20.91**	-24.84***
	(9.262)	(7.800)	(18.55)	(24.11)	(8.990)	(7.833)
After2008*MA	-2.896*	-2.913***	-2.270	-5.061***	-2.800*	-3.102***
	(1.544)	(0.360)	(2.652)	(0.662)	(1.530)	(0.384)
After2008	-16.94***	-10.28**	-28.39***	-29.24*	-18.19***	-10.87***
	(4.653)	(3.855)	(8.358)	(15.55)	(4.730)	(3.851)
Black	48.37***	40.15***	84.29***	93.32***	49.01***	40.16***
	(7.771)	(11.56)	(17.74)	(27.03)	(7.706)	(11.60)
MA	32.08***	19.61***	24.37***	37.20***	23.45***	19.16***
	(3.378)	(3.705)	(5.495)	(7.297)	(3.312)	(3.718)
F-test	0.24	16.67	0.09	3.80	0.16	15.50
P-value	0.626	0.000	0.772	0.058	0.691	0.000
Avg. Black Arrest Rate	61.78	32.76	95.34	69.50	63.06	33.19
Avg. White Arrest Rate	25.47	6.18	34.71	10.98	26.53	6.75
S.D. Black Arrest Rate	80.50	66.70	127.86	166.72	80.30	66.89
S.D. White Arrest Rate	18.64	5.24	20.74	6.87	19.08	5.42
Observations	588	588	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each state.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.6: State-level Adult Arrest Rates: Non-drug Crimes

Adult Arrests per 100.000 Adults						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years	
	(1) Theft Related	(2) Violent	(3) Theft Related	(4) Violent	(5) Theft Related	(6) Violent
Black*After2008*MA	-210.6***	-89.49	-229.6***	-102.0	-195.0***	-73.15
	(65.29)	(89.45)	(82.73)	(107.8)	(66.66)	(90.92)
Black*MA	-430.5**	-77.13	-757.1***	-292.6	-457.7**	-106.2
	(173.8)	(194.5)	(211.4)	(214.4)	(170.5)	(190.3)
Black*After2008	-99.67	-150.7*	-134.3	-171.9	-119.2*	-172.7*
	(65.29)	(89.45)	(82.73)	(107.8)	(66.66)	(90.92)
After2008*MA	53.68***	35.82***	64.15***	46.42***	62.99***	42.33***
	(9.609)	(6.010)	(12.52)	(8.650)	(11.96)	(7.739)
After2008	48.19	-20.61	43.23	-37.19	26.80	-36.49
	(40.60)	(44.44)	(47.20)	(50.69)	(43.42)	(47.43)
Black	1632.0***	1840.5***	2146.2***	2308.5***	1665.2***	1876.9***
	(173.8)	(194.5)	(211.4)	(214.4)	(170.5)	(190.3)
MA	1092.3***	1301.7***	286.0***	1123.2***	585.7***	821.1***
	(80.75)	(89.47)	(97.50)	(100.4)	(80.50)	(88.73)
F-test	5.26	0.35	3.64	0.25	3.42	0.11
p-value	0.027	0.558	0.063	0.617	0.072	0.742
Avg. Black Arrest Rate	2071.60	2182.13	2694.57	2730.40	2106.48	2215.69
Avg. White Arrest Rate	509.72	430.79	646.29	528.49	522.91	440.98
S.D. Black Arrest Rate	1131.47	1257.88	1284.87	1350.95	1112.57	1240.62
S.D. White Arrest Rate	215.01	187.94	176.02	146.48	206.22	179.94
Observations	588	588	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each state.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.7: State-level Juvenile Arrest Rates: Non-drug Crimes

Juvenile Arrests per 100,000 Juveniles						
	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years	
	(1) Theft Related	(2) Violent	(3) Theft Related	(4) Violent	(5) Theft Related	(6) Violent
Black*After2008*MA	-24.48	-28.86	49.45	3.102	-19.82	-19.82
	(347.1)	(286.6)	(455.8)	(351.9)	(346.9)	(286.5)
Black*MA	-856.6*	-409.8	-1294.4**	-610.5	-865.3*	-422.9
	(432.1)	(336.2)	(572.6)	(417.1)	(430.4)	(335.1)
Black*After2008	-305.7	-273.2	-434.8	-349.3	-315.7	-284.1
	(347.1)	(286.6)	(455.8)	(351.9)	(346.9)	(286.5)
After2008*MA	33.85**	23.69***	49.82***	31.54***	38.13***	25.89***
	(13.48)	(7.597)	(16.60)	(8.935)	(13.65)	(7.649)
After2008	50.17	-29.22	50.96	-25.97	39.72	-36.27
	(189.5)	(149.9)	(248.8)	(181.7)	(189.6)	(150.1)
Black	1696.0***	1217.4***	2262.0***	1536.2***	1714.1***	1232.5***
	(432.1)	(336.2)	(572.6)	(417.1)	(430.4)	(335.1)
MA	937.8***	736.6***	367.7**	761.0***	719.5***	593.6***
	(128.8)	(99.45)	(169.9)	(121.4)	(128.1)	(99.04)
F-test	0.00	0.00	0.05	0.01	0.00	0.00
p-value	0.979	0.986	0.829	0.922	0.958	0.983
Avg. Black Arrest Rate	1946.04	1294.08	2537.00	1611.58	1964.79	1306.28
Avg. White Arrest Rate	445.46	242.91	553.63	289.44	451.97	246.50
S.D. Black Arrest Rate	2714.09	2219.87	3498.02	2688.69	2707.70	2216.15
S.D. White Arrest Rate	239.96	123.79	223.16	111.10	239.17	122.87
Observations	588	588	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1]Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each state.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

2.9 Appendix

Table 2.A1: State-level Adult Arrest Rates: All Agencies

Adult Arrests per 100,000 Adults						
	Marijuana Arrests		Non-Marijuana Drug Arrests		Theft-Related and Violent Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	Possession	Sales	Possession	Sales	Theft-Related	Violent
Black*After2008*MA	-362.8***	-16.13**	58.41	-76.60***	-231.8***	-65.81
	(31.77)	(6.060)	(45.81)	(14.52)	(71.18)	(94.23)
Black*MA	-251.3***	29.53**	-245.0***	232.4***	-468.4***	-109.4
	(83.51)	(12.40)	(73.53)	(37.34)	(169.0)	(190.8)
Black*After2008	-3.024	4.037	-238.1***	-95.25***	-73.27	-144.6
	(31.77)	(6.060)	(45.81)	(14.52)	(71.18)	(94.23)
After2008*MA	-97.65***	-1.999	-3.546	-3.267	51.29***	49.05***
	(5.621)	(1.481)	(6.507)	(3.154)	(16.57)	(10.78)
After2008	11.36	4.751	-73.79***	-22.21***	59.03	-17.06
	(21.01)	(3.809)	(20.16)	(7.881)	(44.92)	(51.69)
Black	678.2***	93.48***	574.9***	293.8***	1722.5***	1938.9***
	(83.51)	(12.40)	(73.53)	(37.34)	(169.0)	(190.8)
MA	232.5***	16.53***	185.1***	147.8***	427.8***	690.3***
	(39.10)	(6.054)	(24.93)	(16.13)	(80.50)	(88.62)
F-test	184.20	7.072	1.24	25.26	5.29	0.03
P-value	0.000	0.011	0.272	0.000	0.027	0.865
St. Dev. of Dep. Var.	513.1	77.74	387.1	201.2	1151.1	1280.5
Mean Dep. Variable	523.9	71.86	363.4	165.8	1403.3	1404
Observations	588	574	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates are calculated by dividing state aggregated arrests for every reporting agency by ACS state population for each race and age group. States without at least one agency reporting in every year are excluded. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

Robust standard errors in parentheses are clustered at the state level.

* p<0.10 ** p<0.05 *** p<0.01

Table 2.A2: State-level Juvenile Arrest Rates: All Agencies

Juvenile Arrests per 100,000 Juveniles						
	Marijuana Arrests		Non-Marijuana Drug Arrests		Theft-Related and Violent Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	Possession	Sales	Possession	Sales	Theft-Related	Violent
Black*After2008*MA	-169.6**	-14.78***	-0.120	-30.31***	-37.94	-26.02
	(72.01)	(4.872)	(9.347)	(7.713)	(356.5)	(287.1)
Black*MA	-15.74	27.79***	-22.68***	36.51***	-857.7*	-423.9
	(40.64)	(5.849)	(8.017)	(11.57)	(438.4)	(334.3)
Black*After2008	19.06	-11.45**	-22.12**	-24.65***	-299.2	-271.8
	(72.01)	(4.872)	(9.347)	(7.713)	(356.5)	(287.1)
After2008*MA	-98.83***	-3.072***	-3.457**	-3.502***	31.43**	29.24***
	(4.651)	(0.695)	(1.582)	(0.452)	(15.04)	(8.362)
After2008	-8.414	0.602	-16.52***	-10.72**	58.73	-38.61
	(32.06)	(3.021)	(4.644)	(4.044)	(194.4)	(149.9)
Black	175.5***	25.48***	49.85***	40.79***	1747.0***	1252.1***
	(40.64)	(5.849)	(8.017)	(11.57)	(438.4)	(334.3)
MA	151.3***	16.05***	23.33***	20.81***	703.2***	593.0***
	(25.22)	(1.759)	(3.320)	(3.696)	(128.4)	(98.03)
F-test	13.99	12.48	0.12	17.73	0.00	0.00
P-value	0.001	0.001	0.732	0.000	0.986	0.991
St. Dev. of Dep. Var.	433.3	34.81	61.13	49.41	2101.8	1653.5
Mean Dep. Variable	235.3	23.65	47.7	22.38	1253.7	805.8
Observations	588	574	588	588	588	588

All regressions contain state and year fixed effects. Arrest rates are calculated by dividing state aggregated arrests for every reporting agency by ACS state population for each race and age group. States without at least one agency reporting in every year are excluded. All arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

Robust standard errors in parentheses are clustered at the state level.

* p<0.10 ** p<0.05 *** p<0.01

Table 2.A3: State-level Adult Marijuana Arrest Rates: Adjacent Control States

Adult Arrests per 100,000 Adults

	Agencies that Report in All Years		Agencies that Report Positive Population in All Years[1]		Agencies that Report in All Pre-policy Years[2]	
	(1) Possession	(2) Sales	(3) Possession	(4) Sales	(5) Possession	(6) Sales
Black*After2008*MA	-304.2***	-13.56	-605.2***	-36.03	-301.3***	-13.08
	(49.38)	(7.959)	(51.19)	(20.28)	(50.04)	(9.209)
Black*MA	-114.9	36.58	146.4**	104.3*	-115.3	35.85
	(65.21)	(26.21)	(47.63)	(53.23)	(65.92)	(26.77)
Black*After2008	-54.45	2.083	-54.55	10.20	-57.03	0.431
	(49.38)	(7.959)	(51.19)	(20.28)	(50.04)	(9.209)
After2008*MA	-71.12***	-1.162	-93.99***	-2.175	-100.2***	-0.541
	(7.467)	(0.925)	(6.091)	(1.749)	(7.623)	(0.935)
After2008	0.249	-3.391	-1.300	-12.88	-0.590	-5.293
	(37.18)	(3.974)	(34.24)	(13.66)	(36.85)	(3.836)
Black	529.6***	77.10**	621.5***	145.2**	533.0***	79.93**
	(65.21)	(26.21)	(47.63)	(53.23)	(65.92)	(26.77)
MA	-89.19***	14.04	-39.06**	30.41	-57.85**	12.54
	(24.03)	(12.61)	(16.45)	(24.47)	(24.17)	(12.78)
F-test	53.26	3.61	194.00	4.04	59.97	2.19
P-value	0.000	0.094	0.000	0.079	0.000	0.177
St. Dev. of Dep. Var.	282.1	62.0	326.0	122.4	282.8	63.1
Mean Dep. Variable	388.0	55.3	462.7	107.0	393.5	58.4
Observations	126	126	126	126	126	126

All regressions contain state and year fixed effects. Arrest rates in columns 1-2 and 5-6 are calculated by dividing state aggregated arrests by ACS state population for each race and age group. All arrest rates are arrests per 100,000 people. The states adjacent to Massachusetts are used as control states. F-test and P-value are from a test of the effect of the policy on black arrest rates.

[1] Arrest rates in columns 3-4 are calculated by dividing state level arrests by state-aggregated agency reported total population and scaling by the fraction of the respective demographic group for each state.

[2] For possession arrests in column 5, missing arrests after the policy year are coded as zero arrests.

Robust standard errors in parentheses are clustered at the state level.

* p<0.10 ** p<0.05 *** p<0.01

Table 2.A4: Agency-level Adult Arrests Rates

Adult Arrests per 100,000 Adults (Agencies that Report in Every Year)

	Marijuana Arrests		Non-Marijuana Drug Arrests		Theft-Related and Violent Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	Possession	Sales	Possession	Sales	Theft-Related	Violent
Black*After2008*MA	-623.1***	-7.142	190.8**	-119.1***	-220.2***	-140.7***
	(34.14)	(20.54)	(71.62)	(32.19)	(45.83)	(47.68)
Black*MA	111.9	73.83**	-433.1***	509.0***	-392.5*	372.5*
	(86.62)	(28.95)	(92.68)	(59.60)	(202.8)	(199.5)
Black*After2008	-36.52	-18.66	-421.3***	-208.5***	-143.9***	-134.0***
	(34.14)	(20.54)	(71.62)	(32.19)	(45.83)	(47.68)
After2008*MA	-65.15***	-1.219	35.00	-1.032	85.90***	54.31***
	(13.63)	(1.041)	(22.27)	(4.963)	(15.59)	(10.58)
After2008	-51.06	-2.284	-164.5***	-53.04***	-35.59	-89.75***
	(35.55)	(5.448)	(45.17)	(13.13)	(28.38)	(24.71)
Black	655.8***	175.6***	867.0***	466.5***	1781.6***	1643.7***
	(86.62)	(28.95)	(92.68)	(59.60)	(202.8)	(199.5)
MA	39.20	-2.842	203.3***	246.3***	88.19	794.6***
	(55.24)	(11.80)	(20.24)	(23.54)	(94.98)	(90.38)
F-test	219.60	0.15	6.25	11.57	8.80	3.32
P-value	0.000	0.697	0.017	0.002	0.005	0.076
St. Dev. of Dep. Var.	1030.7	272.4	1247.1	576.9	2510.0	2272.5
Mean Dep. Variable	521.0	114.8	598.6	254.9	1520.5	1346.3
Observations	77678	26272	66086	33342	97892	100342

All regressions contain state and year fixed effects and are weighted by agency-reported population. Agency level arrest rates are calculated by dividing agency level arrests by agency reported population and scaling by the fraction of the respective race and age group represented in the state's population which are obtained from the ACS. Regressions with agency level arrest rates exclude agencies that do not report a positive population. Each agency-level observation is also weighted by the agency-reported population. Arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.A5: Agency-level Juvenile Arrest Rates

Juvenile Arrests per 100,000 Juveniles (Agencies that Report in Every Year)						
	Marijuana Arrests		Non-Marijuana Drug Arrests		Theft-Related and Violent Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)
	Possession	Sales	Possession	Sales	Theft-Related	Violent
Black*After2008*MA	-323.5***	-38.82***	3.830	-54.29***	-233.3***	-171.7***
	(12.86)	(4.688)	(10.58)	(12.01)	(57.54)	(49.46)
Black*MA	228.7***	69.08***	-14.54	74.31***	-576.0***	-75.75
	(35.72)	(9.010)	(17.54)	(21.84)	(197.4)	(137.2)
Black*After2008	-16.41	-15.75***	-35.22***	-50.06***	-152.4**	-175.0***
	(12.86)	(4.688)	(10.58)	(12.01)	(57.54)	(49.46)
After2008*MA	-73.01***	-5.193***	1.550	-5.368***	62.17***	42.59***
	(8.837)	(0.716)	(1.586)	(0.449)	(21.49)	(13.28)
After2008	-47.37**	-5.033**	-27.16***	-16.35***	-169.2***	-148.9***
	(18.45)	(2.127)	(3.292)	(3.405)	(49.42)	(29.18)
Black	136.8***	45.39***	52.56***	74.81***	1543.7***	1001.6***
	(35.72)	(9.010)	(17.54)	(21.84)	(197.4)	(137.2)
MA	2.502	14.38***	7.027	30.68***	84.79	530.3***
	(20.19)	(3.298)	(6.496)	(7.156)	(88.46)	(62.26)
F-test	814.2	78.1	0.32	23.35	7.05	6.77
P-value	0.000	0.000	0.577	0.000	0.011	0.013
St. Dev. of Dep. Var.	572.0	106.3	144.8	170.1	2735.5	1773.4
Mean Dep. Variable	229.8	39.1	57.4	35.9	1247.7	746.6
Observations	77676	26270	66086	33342	97892	100342

All regressions contain state and year fixed effects and are weighted by agency-reported population. Agency level arrest rates are calculated by dividing agency level arrests by agency reported population and scaling by the fraction of the respective race and age group represented in the state's population which are obtained from the ACS. Regressions with agency level arrest rates exclude agencies that do not report a positive population. Each agency-level observation is also weighted by the agency-reported population. Arrest rates are arrests per 100,000 people. F-test and P-value are from a test of the effect of the policy on black arrest rates.

Robust standard errors in parentheses are clustered at the state level.
* p<0.10 ** p<0.05 *** p<0.01

Table 2.A6: Uniform Crime Reports Offense Categories

Reported Individual Offenses:

1. Murder
2. Negligent Manslaughter
3. Forcible Rape
4. Robbery
5. Aggravated Assault
6. Breaking and Entering
7. Larceny- theft (not Auto)
8. Motor Vehicle Theft
9. Other assaults
10. Arson
11. Forgery and Counterfeiting
12. Fraud
13. Embezzlement
14. Stolen property (buying, selling or receiving)
15. Vandalism
16. Weapons (Carrying, Possessing, etc.)
17. Prostitution and vice
18. Sex offenses (Not Rape or Prostitution)
19. Sale/Manufacture of Opium or Cocaine
20. Sale/Manufacture of Marijuana
21. Sale/Manufacture of Synthetic Narcotics
22. Sale/Manufacture of Other Non-narcotic Drugs
23. Possession of Opium or Cocaine
24. Possession Marijuana
25. Possession of Synthetic Narcotics
26. Possession of Other Non-narcotic Drugs
27. Bookmaking (Horse and Sports)
28. Number and Lottery
29. All other Gambling
30. Offenses against Family/Children
31. Driving Under the Influence
32. Liquor Laws
33. Drunkenness
34. Disorderly Conduct
35. Vagrancy
36. All other Non-traffic Offenses
37. Suspicion
38. Curfew and Loitering
39. Runaways

Reported Offense Subtotals:

Total Drug Offenses 19-26

Total Drug Sales/Manufacture 19-22

Total Drug Possession 23-26

Total Gambling 27-29

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