

Homicides and the Age of Criminal Responsibility: A Density Discontinuity Approach

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Abstract

We employ a density discontinuity design to evaluate the deterrence effect of more severe punishments around the legal age of criminal responsibility in Brazil. Motivated by the criminology literature, we propose a novel proxy based on the inherent risk underlying criminal activities. Using violent death rates as a proxy for an individual's involvement in violent crime, we find no discernible deterrence effects. We additionally study arrest data from the country's third most populous state, Rio de Janeiro, and discuss the advantages of our proxy in light of potential underreporting biases from using criminal records.

JEL Codes: J18, J22, K14, K42, O12.

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1 Introduction

Crime is a prominent public concern in urban areas worldwide. Violent crime participation and victimization are known to be higher among young males, especially those from disadvantaged neighborhoods. Although the problem is not restricted to developing countries, it takes dramatic contours in Latin America. In the region, more than a hundred cities exceed twenty murders per one hundred thousand inhabitants annually. Additionally, public safety is listed in opinion pools by almost every Latin American country as the top public concern.¹ As part of an effort to reduce youth involvement in violent crime, lawmakers and the general public have considerably debated the age of criminal responsibility, that is, the age above which a young offender is subjected to the stricter legal treatment of adults.²

An analysis of criminal behavior and the deterrent effects of punitive policies is part of a long literature that, among economists, has strong roots in Becker's (1968) rational model of crime and punishment. The general focus has been on understanding the responses of potential criminals to incentives, in particular to severe or more likely punishments. The legislation regarding violent crimes in many countries and U.S. states determines that only people older than a particular age cutoff – the age of majority – are processed by the adult justice system. People younger than this cutoff are subject to the juvenile justice system, usually with more lenient punishments. The *Beckerian* model would imply that the severity of potential punishments faced by people when they reach the age cutoff would reduce their propensity for involvement in violent criminal acts.³⁴

We aim to provide novel empirical evidence of the crime-deterrent effects of stronger punishments using data from a country in which 16 people per thousand people under the age of nineteen were murdered in 2013 alone (Waiselisz, 2014): Brazil. Our approach seeks to complement previous studies by using a new proxy variable for criminal involvement in a natural quasi-experiment design.

The common difficulty for every empirical study of criminal behavior is that individual

¹See Igarapé (2016) and FBI (2012) for the murder rate and Latinobarometro (2015) for opinion poll data. Also, more than thirty cities in the United States display homicide rates above this threshold, indicating the problem of urban violence is not restricted to developing countries.

²We will employ (with some abuse of legal terminology) the terms “criminal responsibility” and “criminal majority” interchangeably. From a legal standpoint, it could be more precise to define the age of criminal responsibility as the age above which children are responsible for their offenses and can be tried in court (currently set at twelve years in Brazil), even if still under a special juridical system, and criminal majority as the point above which people are subject to the penal treatment assigned to adults.

³See Chalfin and McCrary (2014) for a recent review of the empirical tests of this theoretical prediction.

⁴Based on this argument, many states in the United States reduced the age of majority in an attempt to deter juvenile criminality (Rubin, 2003). But given the absence of consensual evidence on the effectiveness of this reduction, some states have recently revised the age threshold upward (Brown, 2012), this time with a stronger focus on rehabilitating young offenders (Loeffler and Chalfin, 2015).

propensities to participate in crimes are, by nature, not directly observable. Thus, we need to resort to proxy variables. The most natural and commonly used proxies lie in criminal records, such as arrest and conviction data. Our main concern, previously shared by Lee and McCrary (2009) and Chalfin and McCrary (2014), is that too many obstacles lie between a criminal offense and an official record identifying the offender. Importantly, the severity of most of these obstacles depends on enforcement efforts and the discretion of authorities, both of which may vary with the offender's characteristics or the type of crime, leading to potential biases.⁵

For policy evaluation purposes, the main threat is that, conditional on a crime committed, the propensity of the criminal justice system to generate a formal arrest or conviction record is likely to depend on the procedures and potential sanctions the law itself prescribes. For example, in responding to a possible offense, a public agent might issue only an informal warning if the offender is a minor, subject to an unlikely conviction; whereas, if the offender were an adult, the agent might instead choose to arrest him. In such cases, the likelihood of a crime committed by a minor showing up in arrest records is lower than if the same crime is committed by an adult. Therefore, proxies based on criminal records have a potential *underreporting bias* of unknown magnitude that would compromise any age-based estimates. We first provide new evidence of this bias and its magnitude by studying the comprehensive police arrest records from Rio de Janeiro, the third largest Brazilian state.

To circumvent this differential underreporting bias of criminal records, we propose a novel proxy for an individual's involvement in violent crime: the change in the number of violent deaths by age.⁶ We base our proxy on the well-documented fact from the criminology literature that criminal offending and violent victimization partially overlap. We know that if an individual is more likely to engage in criminal activities, he will offend more often and will therefore be more likely to be arrested, all else equal. This is in fact the rationale behind the use of police records as proxies for criminal activity. Analogously, potential offenders are also more likely to become victims of a violent death while committing a crime, whether by the hands of law enforcement agents or by those of other criminals, like rival gang members. In other words, violent death rates by age, although far from being uniquely determined by individual offenses, increase on the degree to which the individual is engaged in violent criminal acts (Jennings *et al.*, 2012). Our proxy originates from the evidence that this overlap

⁵Curry and Klumpp (2009) show that a bias, manifested through differential conviction rates by offenders' characteristics (e.g., race, gender, wealth, and age), can emerge with a Bayesian jury.

⁶Although the use of murder rates is common in the literature as a proxy for general crime rates, to the best of our knowledge, this is the first paper to use the previously documented overlap between victimization and offending at the crime scene to suggest the use of violent death as a proxy for individual episodes of violent crime.

between offending and victimization is not only due to selection but also depends on actual individual behavior.⁷ This is particularly true for riskier and more severe crimes; for example, Carvalho and Soares (2016) find that the two-year mortality rate of gang members in Rio de Janeiro is as high as 20%. We thus look for evidence of higher propensities to engage in violent criminal activities in Brazil through the effect they may have on violent mortality. The behavior of this measure around the age of criminal majority also might be of direct interest for public policy.

We focus on Brazil for two reasons. First, we have access to reliable and detailed data from the universe of death certificates for the country as whole as well as police records for its third largest state by population. Second, the country has one of the strictest rules regarding the age of criminal responsibility. Under no condition can people younger than eighteen years be prosecuted, tried, or sentenced under the standard penal law. Instead, these young offenders are subject to special treatment under the juvenile justice system, governed by the *Estatuto da Criança e do Adolescente* (Statute of Children and Adolescents). Among other provisions (see, e.g., Shecaira, 2004), minors must be sent to specialized police departments and preference is then given to rehabilitating measures like warnings, damage reparations, and social services if a minor is convicted. In a few cases in which the judge may rule for detention, the minor is nonetheless sent to special detention facilities, where he will stay for a maximum of three years and no later than his 21st birthday. To put this in perspective, the penalty for first-degree murder by an individual eighteen years of age or older (to whom we will refer here as an “adult”) is imprisonment for a period ranging from six to twenty years.

The sharpness of the age rule for criminal prosecution in Brazil lends itself to a natural quasi-experiment, which we exploit to measure the degree to which a more strict punishment may influence the decision to engage in a crime. More specifically, we employ methods of density discontinuity designs (McCrory, 2008; Jales and Yu, 2017) to the frequency of (male) violent deaths by age using national death records in Brazil from 1996 to 2013 and looking for evidence of a discontinuous fall at the age of eighteen.⁸ The key difference between our analysis and the traditional regression discontinuity design lies in that we are not working with a dependent variable that is a function of other covariates that determine treatment

⁷The criminology literature has noted that participation in crimes increases the odds of violent victimization (Lauritsen *et al.*, 1991; Nieuwbeerta and Piquero, 2008; Muftić and Hunt, 2013; Pyrooz *et al.*, 2014, , among others). Homicides are also frequently used as the best proxies for overall crime rates in other contexts. As UNDOC (2011) argues, “homicide data are considered among the most representative and comparable crime indicators” and because of its more scrupulous recording, “may be considered not only as a single phenomenon in isolation, but also as a reasonable proxy for violent crime in general”. See, e.g., the argument in Kleck (2004) for choosing deaths by firearms as the best proxies to gun ownership and diffusion in the U.S.

⁸The idea that discontinuities in density functions also could be used for econometric identification has been exploited in different contexts (such as Meyer and Wise, 1983; Saez, 2010).

assignment. Instead, we have data on violent deaths by fine measures of individual ages and wish to investigate whether its *frequency* changes at eighteen years old.

The central identifying assumption is that all factors affecting violent death rates have continuous effects on the number of violent deaths around the cutoff age; therefore, the only factor discontinuously affecting violent death rates around an individual's 18th birthday must be his propensity to engage in criminal behavior as a result of the sudden change in penal sanctioning. Importantly, this strategy does not even require that a large share of violent deaths coincide with the deceased's criminal behavior, only the continuous behavior of other death probability determinants.⁹

We discuss and investigate, to the furthest possible extent, potential threats to this assumption. These threats include other rules believed to change at the eighteenth birthday. First, we survey the literature on the legal drinking age in Brazil, also set at eighteen years of age, and alcohol abuse. We complement the general consensus of extremely weak compliance with direct evidence of a continuous behavior of alcohol-related deaths around that age. Second, we discuss the right to apply for a drivers license. In this case, the lengthy and costly process mitigates coincidence concerns, especially regarding the low-income at-risk young population. We also provide direct evidence that this should be a lesser concern for identification by documenting the absence of a discontinuity of traffic-related deaths around the eighteenth birthday. Third, we show that the military conscription process is not a sharp function of the age. Fourth, we clarify that the compulsory education age in Brazil is far from the 18th-birthday threshold. Fifth, we discuss the minor changes in labor market regulation that take place at that same threshold. Last, we provide direct evidence on potential changes in risky behaviors of young people, which could be affected by a symbolic psychological change brought by a milestone "coming of age" event. Overall, there are mitigating reasons and complementary direct evidence that reduces concerns about these threats.

Beyond identification issues, the use of any proxy raises concerns about the power of the statistical tests being performed. In our particular case, any randomness in underlying violent victimization that has origins unrelated to an individual's criminal engagement adds noise to the test statistics, reducing power and creating the need for a large sample for good statistical inference. That we are dealing with a very large sample (all violent deaths in a

⁹Note that an increase in criminal activity makes one more prone to suffering a violent death very close after the commission of the criminal act – because of the immediate reaction of the police or third-parties – or later on because of revenge (e.g., by a gang) or after the police investigation. Only the former link between offending and victimization would discontinuously change at the age of criminal majority; the latter link, victimization due to past crimes, continuously evolves at the age cutoff and would be captured by the density's underlying polynomial.

large country over a 18-year period) helps mitigate concerns over the potentially low power of tests based on the proxy. We calculate the power of the main exercise and propose a simple model linking criminal involvement and victimization. This calculation rules out deterrent effects on criminal involvement greater than 10%, under standard power and significance levels.

We find no discernible change in the density of violent deaths at the age of criminal responsibility in the Brazilian metropolitan areas. We interpret this result as additional evidence of weak deterrent effects of the threat of stricter adult sentencing rules on violent criminal behavior around the cutoff age. Our finding is also true among the more vulnerable group of black males. We perform a series of additional robustness and placebo tests.

We also find evidence of underreporting bias when using the police arrests records. For that, because of data availability, we restrict our focus to the state of Rio de Janeiro between the years of 2010 and 2014. For the category of “Other Crimes” – offenses that police officers may have more leeway on deciding whether or not to register a transgression – we find a 30% *increase* in the frequency of offenses at eighteen years of age, an increase that is statistically significant at 1%. We also observe a 9.5% reduction in the frequency of drug-related crimes at the age cutoff, but we find no evidence of a change in arrests related to “Violent” or “Property Index Crimes”.¹⁰ An increase in reports of less damaging crimes, with no effect or a drop in reports of serious crimes, is an expected consequence of a differential underreporting bias.

These main findings are in line with the bulk of the empirical literature that investigates the deterrent effects of judging juveniles as adults. Many studies have already employed a regression discontinuity strategy, mostly to estimate the effect of the age of majority on criminal recidivism. To our knowledge, all of these use either official criminal records in the United States as a proxy for criminal behavior or self-reported data. Two studies in particular come the closest to our own. Lee and McCrary (2009) follow a longitudinal cohort of youngsters and use an RD design to search for evidence of a discontinuous drop in offense rates after the cohort reaches the age above which they are judged in a standard adult court. They find a low deterrent effect, illustrated by a reduced-form elasticity of -0.05. Loeffler and Grunwald (2015b) find that processing young offenders on drug-related felonies reduces the probability of recidivism, but that the effect is concentrated on the least risky adolescents. Loeffler and Grunwald (2015a) and Loeffler and Chalfin (2015) study the recent rise in the age cutoffs in Illinois and Connecticut, respectively, and find no discernible change in the number of offending juveniles.¹¹

¹⁰Index Crimes follows the Uniform Crime Reports (UCR) system and include seven crimes fundamental to comparing crime rates: murder and non-negligent manslaughter, rape, burglary, aggravated assault, larceny, motor vehicle theft, and arson.

¹¹Using self-reported criminal behavior, Hjalmarsson (2009) finds that, although people perceive an increase

As far as we know, this is the first study of the deterrent affects of the stricter adult punishment outside a developed country using a causal identification framework. The Brazilian setup brings a sharp change in *de jure* criminal treatment and a much higher violence incidence. Also, to the best of our knowledge, this is the first paper to employ a different proxy – violent deaths – to estimate the deterrent effect of the age of criminal majority.¹² Our work also differs from the literature cited above in at least one other important way. Because their strategy employs criminal records, which exist only for offenders and not the public in general, they exclusively follow people who have already been prosecuted, either as minors or as adults. Their focus on deterring *recidivism* specifically targets a high criminal propensity segment of the population.

2 Proxies: Criminal and Mortality Data

Deterrence effectiveness and individual criminal propensities are not directly observable, so quantitative research has to focus on the frequency of documented crime involvement. The most natural and commonly used proxies lie in individual criminal records. If a treatment has a deterrent effect, then we would expect to see a decrease in the number of people arrested.

There are, however, a few hurdles that an offense has to clear in order to appear in the criminal records and be made available for study. First, the crime needs to be detected and the offender, identified. However, the officers in charge might still find themselves with enough discretion to determine whether to conduct the formal procedures that generate an arrest record or to release the individual. At later stages, and depending on the location, there also might be laws preventing access or effectively erasing the criminal records that apply to minors, to arrests that later led to acquittals, or to first-time offenders.

The propensity to generate a formal arrest record might itself depend on age and the potential sanction to which the offender is subject. This generates a non-standard measurement error that biases the estimates using the age threshold. In Brazil, where a strict potential punishment change is centered on the individual's 18th birthday, it is generally recognized that police officers have a weaker motivation for arresting minors than they have for adults, mostly due to differences in criminal procedures and potential sanctions.

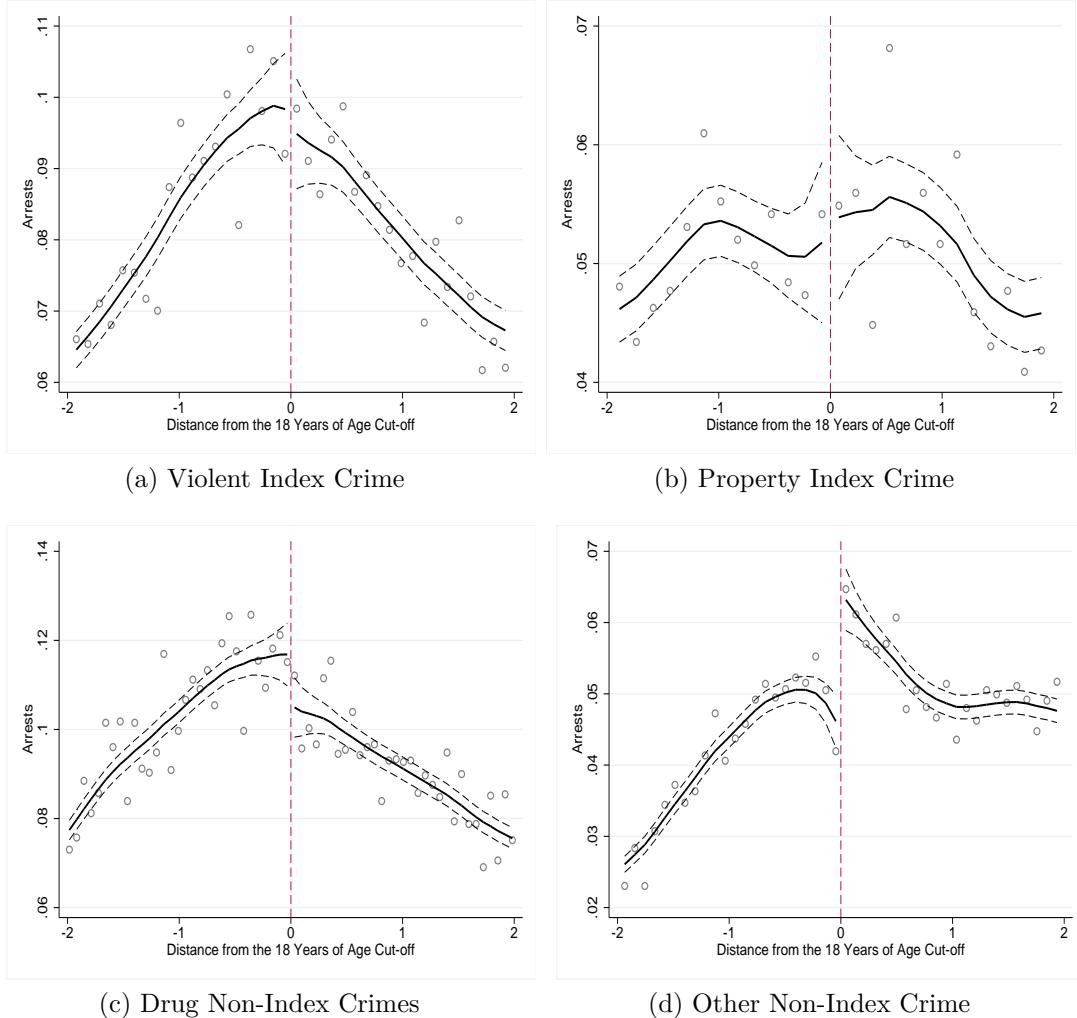
Lee and McCrary (2009) acknowledge this problem in their Florida youth delinquency study and, to address it, restrict attention to reoffenders (who already have an open criminal record) and index crimes, for which stricter reporting procedures limit discretion in releasing

in the expected punishment at the age of criminal majority, harsher punishments only deter juveniles from committing small crimes (less than US\$50) in the United States.

¹²The literature on deterrent effects of criminal law in Brazil is still small and has focused more on correlations between aggregate variables than on identifying causality (e.g., Araujo Jr *et al.*, 2014; Nadanovsky, 2009).

a suspect. The behavior of non-index crimes reported in their paper provides strong evidence of biases affecting less serious crimes (see figures 3 and 4 in Lee and McCrary, 2009). The magnitude of the bias and its relevance for index crimes, however, cannot be directly assessed, a fact that is a significant threat to identification.

Figure 1: Police Arrest by Type of Offense – Rio de Janeiro



Local linear density estimates, by crime type, following FBI's classification of index and non-index crimes. The bandwidth is of one year to each side and the year is decomposed into 25 bins. Panel (a) shows the number of arrests disaggregated for murder, rape, robbery, and aggravated assault, Panel (b) burglary, larceny, and motor vehicle theft, Panel (c) drug-related crimes and Panel (d) shows arrests for all other non-index felony offense. Source: ISP-RJ.

We use restricted-access data on all arrests registered by the police in the state of Rio de Janeiro, in Brazil, in order to study the behavior of criminal data around the age of criminal responsibility. Figure 1 plots local-linear density estimates centered on the 18th birthday.¹³

¹³Section 3.1 presents a formal description of the data and a more through analysis.

The only striking, significant, and robust feature of these estimates is the large increase in arrests for non-drug-related non-index crimes, that is, the least damaging and the least violent offenses. The magnitude of this estimated increase is higher than 25%. Its presence is in line with the results obtained in Lee and McCrary (2009). We interpret it as evidence of a strong underreporting bias for minor crimes committed by young offenders.¹⁴ More generally, the possibility that a large reporting bias could affect all criminal data is what motivates our search for a different proxy of criminal involvement.

The criminology literature already provides us with some indications by identifying a strong pattern of overlap between victimization and offending behavior.¹⁵ Our proxy follows from the conclusion that this overlap is not only due to selection on the demographics of the most vulnerable groups but also depends on the fundamental degree of actual individual behavior: choosing to participate in criminal activities increases one's odds of violent victimization.¹⁶ Whenever an individual chooses to engage in crime, especially violent forms, he accepts the risks of being a victim of a violent death whether brought on by the reactions of his potential victims, law enforcement agents, rival criminals, or intervening third-parties. We therefore look for those discontinuities in mortality data that would mirror the potential discontinuities in criminal involvement and its associated risk-taking. A further advantage of this proxy is that any measurement error on death reporting is likely to be orthogonal to the age of the victim around the threshold. That is, the probability that a homicide is reported as a function of the victim's age is smooth around the threshold. If, on the one hand, this proxy is unlikely to suffer from the endogeneity that arrests and convictions have with relation to the penal code itself; on the other hand, it is more indirect and, therefore, noisier. This makes necessary a discussion of the statistical power of the tests conducted (see Section 5.2).

Additionally, if engagement in illicit activities increases one's risk of suffering a violent death, this is particularly more intense for offenses generally regarded as more serious: rape, murder, kidnapping, armed assaulting, or involvement in drug-related conflicts. Those are the crimes for which we expect a violent death to be a more probable outcome when compared to less serious offenses, such as speeding, or shoplifting. Consequently, changes in violent victimization are expected to serve as a better proxy for an individual's involvement in violent

¹⁴We do not find any evidence that incentives for involvement in minor crimes would be weaker, constraints tighter, or punishments stronger just before the 18th birthday. Section 5.1 presents a more thorough discussion of confounding effects.

¹⁵See Jennings *et al.* (2012) for a survey of 34 recent studies on the overlap pattern and its robustness across countries. The literature is at least as old as Wolfgang (1958), who in a early study of homicide victims documented that homicide was often a consequence of another crime perpetrated by its victim.

¹⁶See, for instance, Lauritsen *et al.* (1991); Wittebrood and Nieuwbeerta (1999); Nieuwbeerta and Piquero (2008). Pyrooz *et al.* (2014) study the role of gang participation in contributing to the victim-offender overlap, and Muftić and Hunt (2013) study the role of victim-precipitated homicides.

crime than for broader notions of illegal behavior.¹⁷ This has an important bearing on the interpretation of the estimates, which we take into account in the following sections, as only changes in violent criminal engagement are likely to be detectable.

For instance, a drop in death numbers following the threshold at eighteen can be less the result of people abandoning crime altogether and more of them turning away from riskier and more serious crimes toward less severe ones. We stress, however, that this substitution effect should still be regarded as a deterrent impact from harsher penalties at eighteen years of age, that is, the very thing we are ultimately trying to measure. We revisit this discussion when presenting the results in Section 4.

3 Data and Empirical Strategy

This section discusses the data employed and our empirical approach. As a rule, when we speak of “people” we are referring to males. We focus on males because the period between the ages of sixteen and twenty coincides with the age interval in which young women are more likely to be victims of assault, particularly of a sexual nature (Waiselisz, 2013), adding a particular amount of noise to the proxy. This could severely weaken links between victimization and criminal involvement.

3.1 Data

Death Records. In Brazil, violent deaths (homicides, suicides, and accidents) are reported following a unified set of rules determined by a National Law (number 6.015/1973). In particular, no burials can be performed without a death certificate (*Declaração de Óbito*), which contains information such as the full identification of the deceased (with date of birth exactly as stated in his ID), some of his social and personal characteristics (race, gender, etc.), and, most relevantly to our purposes, the precise date, the location, and the probable cause of death.¹⁸

These death certificates are consolidated into a national database called *Sistema de Informações sobre Mortalidade* (SIM or Mortality Information System). As of 2011, its coverage had reached over 96.1% of the total annual deaths inferred using the National

¹⁷If deterrent incentives are the only factor changing discontinuously at the 18th birthday, a drop in mortality would be the sum of the death drop induced by lower participation in high-risk criminal activities and by lower participation in low-risk criminal activities. The former plausibly dominates. This heterogeneity in the sensitivity of victimization to different crimes does not threaten our identification (see Section 3.2).

¹⁸A useful description of the procedures, as well as reports on how they have changed over the years, can be found, among other sources, in Borges *et al.* (2013). Accounts on the limitations of the data generated by these procedures and how they can be improved can be found in Cerqueira (2013).

Census. In our sample, the classification of death causes follows the 10th revision of the International Statistical Classification of Disease and Related Health Problems (ICD-10), endorsed by the World Health Organization.¹⁹

Our interest lies in those deaths deemed unnatural or violent, that is, in those that occurred as the result of aggression. For these deaths, the law requires an autopsy and the determination of a “basic cause” of death..

For the broader definition and classification of homicides, we consider the choice of ICD codes for deaths known to be the result of aggression by others (homicides) and those of which intention (whether self-inflicted or accidental) could not be determined. These are all under ICD’s Chapter XX, “External causes of morbidity and mortality”. We use this broader category of violent deaths, following Cerqueira (2013), to attenuate concerns about the misclassification of death causes. Table C.1 in the appendix lists these codes and provides a brief description of the instrumental cause of death.²⁰ For robustness, we also adopt alternative and narrower definitions of homicides and verify if results hold if we restrict attention to deaths by firearms. The conclusions are always qualitatively equivalent.²¹

Criminal Records. In Brazil, most investigative police work is the responsibility of the state government, and different state records are not integrated in a national repository. Thus, for the tests on our assumptions and specifications, we focus on the criminal records of one state, Rio de Janeiro, where we know there is reliable and sufficiently large data. More specifically, we consider its local official records from the *Instituto de Segurança Pública* (ISP), at the Bureau of Public Security.

The data consists of arrests of identified crime suspects as registered at police stations from 2010 to 2014, which together represent around 8% of all offense registries. As we discussed in the previous section, identifying the offender is always a hurdle in generating full criminal records. Because of other technical issues, to identify offenders at their individual level, we restrict attention to records issued in districts and specialized police stations that were already part of the central information system – *Sistema de Delegacia Legal*²² – by 2010. This amounts to 118 police stations, covering a territory that was home to 74.5% of the Rio de Janeiro state’s population, according to the 2010 demographic census.²³ Finally, we use

¹⁹As of March 2018, the database and the coverage statistics were available at http://tabnet.datasus.gov.br/cgi/sim/dados/cid10_indice.htm.

²⁰Note that this classification includes, e.g., deaths classified as homicides but with “unknown” (or undetermined) death instruments.

²¹Appendix Table C.2 displays the summary statistics of violent deaths by age ranges and other covariates.

²²This is the computerization and modernization program of the police stations in the state of Rio de Janeiro. It involves standardizing records and procedures within a central database.

²³The total population served by these specialized stations is likely to be underestimated, since the calculation does not take into account the people who are assisted by the police in a given area but live outside of it.

only records that have at least the following information: color of skin, gender, and date of birth of the offender.

Like Lee and McCrary (2009), we divide crimes into four categories: Violent Index Crimes, Property Index Crimes, Drug-Related Non-Index Crimes, and Other Non-Index Crimes.²⁴

3.2 Empirical Strategy

The strictness of the age-based rule for criminal responsibility in Brazil, as well as the sharp contrast between minor and adult penal systems, lend themselves naturally a discontinuity design approach. Framing our discussion in the language of the “Rubin causal model” (Rubin, 1974), the treatment effect to be identified is the consequence of the more rigorous set of sanctions to which adult offenders in Brazil are subject when compared to juveniles. The treatment group ($D = 1$) then comprises young adults who have just turned eighteen, whereas the control group ($D = 0$), which will serve as the counterfactual for comparisons, comprises young people who are just short of reaching eighteen years of age.²⁵

In the ideal sampling design, the researcher would observe a random sample of the pair (Y, X) , where $Y(D)$ is a propensity to commit crimes (assumed to be binary for simplicity) for each assignment into group $D \in \{0, 1\}$ and X is age, normalized to zero at the cutoff, the variable that sharply assigns people to treatment and control groups. Then, given the treatment assignment around zero, the difference in the height of the conditional mean functions at this cutoff identifies the local average treatment effect (Hahn *et al.*, 2001).

Researchers in the literature often have no access to data on the pair (Y, X) . Instead, suppose one observes only the density of X (age) conditional on $Y = 1$ (crime engagement). Jales and Yu (2017) show that it is possible to identify the local average treatment effect of this policy in this setup. Under the standard RDD assumptions,²⁶ we have

$$\lim_{X \searrow 0} \ln f(X|Y=1) - \lim_{X \nearrow 0} \ln f(X|Y=1) = \ln \frac{E[Y(1)|X=0]}{E[Y(0)|X=0]}, \quad (1)$$

as scaling factors involving the density at $X = 0$ and the overall probability of $Y = 1$ cancel out.²⁷ Thus, the difference in the limits of the log-density at the threshold is equal to the standard local average treatment effect of the policy. In other words, a discontinuity in the conditional mean of Y given X at the threshold will necessarily imply a discontinuity at the

²⁴See footnote 10 for definitions.

²⁵We thank an anonymous referee for suggesting the current presentation of the empirical strategy.

²⁶Most notably, the unconditional density $f(X)$ is continuous and positive at the critical age $X = 0$. Additionally, $Pr\{Y(D)|X=x\}$ is continuous in x for $D \in \{0, 1\}$.

²⁷We have that $f(x|Y=1) = \frac{\Pr\{Y=1|X=x\}f(x)}{\Pr\{Y=1\}}$, so the result above follows easily.

density of X given $Y = 1$. Consequently, despite not observing the pair (Y, X) , we have enough information to infer whether there is a discontinuity at the conditional mean of Y given X by estimating this function.

Yet another challenge presents itself. Criminal propensities Y are not observable, so proxies are required. Suppose that instead of observing the pair (Y, X) the researcher observes the density of X (age) given that $W = 1$ (a violent death occurred). The goal is still to infer whether the policy has any effect on (unobservable) criminal propensities, Y . However, we observe another variable W that is caused by Y ; that is, Y lies in the causal path between the treatment and W . Assume that

$$P[W = 1|Y, X] = g(Y, X, u), \quad (2)$$

that is, that the probability of death by a violent cause is a function of criminal behavior, age itself, and other unobserved factors u . Under continuity of the function g with respect to the last two arguments, if we only observe the density of X given $W = 1$, then the best we can do is to look at discontinuities of density around the threshold. If we do that, under the standard continuity assumptions of the typical discontinuity design approach and following the arguments in Jales and Yu (2017), we identify the following object

$$\rho = \lim_{X \searrow 0} \ln f(X|W = 1) - \lim_{X \nearrow 0} \ln f(X|W = 1) = \ln \frac{E[g(Y(1), X, u)|X = 0]}{E[g(Y(0), X, u)|X = 0]}. \quad (3)$$

This is the **local** treatment effect of age of majority on the number of deaths, mediated by criminal behavior changes, as measured in terms of a log change. This expression makes it clear that the difference between the two directional limits with respect to age (given that the individual has suffered a violent death) is a combination of the effect that the harsher punishment has on the criminal behavior itself and the effect that criminal behavior has on the probability of dying from a violent death. The value cannot be zero for the proxy to be valid.²⁸ We discuss the power of this approach in Section 5.2.

We follow McCrary (2008) in our estimation procedure of ρ . To avoid confusion, an important observation to have in mind is that although we are thinking of people and the number of deaths, we estimate a relation between histogram bin midpoints and their associated frequencies. This will have an important bearing on the interpretation of the

²⁸Again, we restate that all studies on criminal offenses employ proxies. Those looking at the frequencies of arrests in particular are implicitly assuming that more arrests are the result of more people engaging in crime when the right control variables are used. Using the language developed above, our key concern is that for arrest data, $P[\text{arrest} = 1|Y, X]$ jumps up at $X = 0$ for any intensity of criminal activity Y . As a consequence, estimates based on arrest data show a bias for the causal effect of the policy on the number of arrests as mediated by criminal behavior changes.

results: the estimates for ρ are then estimates of how much *the frequency of a bin* with predetermined binwidth b would immediately change after it began to include people (and only those) whose ages have surpassed the eighteenth threshold. The trade-off is clear: as we increase the bandwidth window (which spans several bins of a histogram), each bin becomes lengthier, and we are thus looking at a larger group of young adults, but at the cost of using observations that are farther away from the cutoff, thus potentially biasing estimates. This way of correctly interpreting results shall become clearer as we turn to specific examples.

Additional Estimation Issues. A first estimation concern is with the bandwidth selection for the local regressions above: when trying to provide estimates at a single point, we aim at finding the right balance between the competing goals of using observations as close as possible to the cutoff, as demanded by the identifying assumptions in Section 3.2, while assuring ourselves that enough data are being used. This is true for all regression discontinuity designs, but since we are dealing here with possible discontinuities in density functions, moving away from the cutoff gives us a larger sample and thus a histogram that is smoother and closer to the actual distribution of frequencies. Unfortunately, there is no widely agreed on method for selecting optimal bandwidths in the semi-parametric regression discontinuity context (Ludwig and Miller, 2007), and so our strategy is to run the regressions and report the results for a broad range of candidate bandwidths. Our most local estimates have a window of three months on each side of the eighteen-year mark, and our least local, a window of seventeen months on each side. Still, to find some formal guidance on which bandwidth to choose, we calculate the rule-of-thumb bandwidth selection proposed by Fan and Gijbels (1996). We find that the suggested bandwidth using an uniform kernel is 12 months.

A second issue has to do with the choice of functional forms. In our preferred specifications we allow for higher-order polynomial forms of f^+ and f^- , like in McCrary (2008). However, having in mind that the main recommendation in Gelman and Imbens (2014) favors linear polynomials, we run a density test with linear polynomials in the robustness section.²⁹

4 Results

This section reports the main results using death records and compares them with those obtained from the criminal records in Rio de Janeiro. Like all other discontinuity designs, we are less interested in the significance of each individual estimate and more interested in identifying significant effects that do not depend on a particular combination of bins,

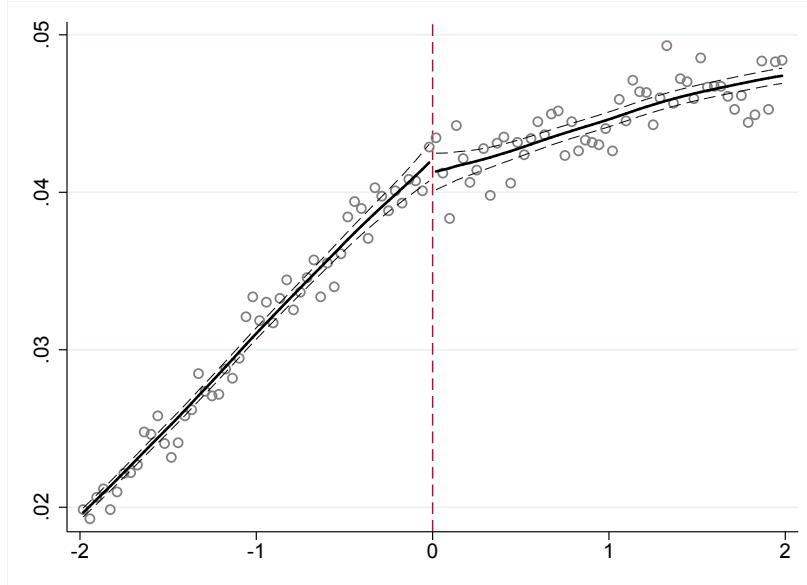
²⁹Their basic argument is threefold: the implicit weights for high-order (third, fourth, or higher) polynomials are not attractive; the results are too sensitive to the choice of polynomial order; and traditional inference in these settings has poor properties.

bandwidth, and functional form, but instead show up across different specifications. To target urban crime, we investigate violent death by age for the combination of all the large metropolitan areas and state capitals in the country. We focus on this subset because one could be reasonably concerned that motivations for violent crime in rural areas have more to do with acts of impulse and less with crime being a rational, economically driven choice.

4.1 Main Results

As an almost defining characteristic of RD designs, a graphical illustration provides a basic understanding of the approach. Figure 2 presents the distribution of male homicides across ages between 16 and 20 years in Brazil. We normalize age relative to the cutoff of 18 years old, so -2 means 16 years of age. The pairs of dashed lines denote the 95% confidence intervals. The figure shows the age threshold at which our regressions will look for sudden changes in an otherwise continuous relationship. In the absence of treatment effects (i.e., heavier punishments decreasing the number of homicides through discouraging criminal behavior), we would expect to see the relationship continuing on its path in a smooth fashion, without any jumps at the treatment change point.

Figure 2: Distribution of Violent Deaths (All Metropolitan Areas)



This figure plots the frequency of violent deaths of males per age of death (in the horizontal axis: age of death minus 18 years old) for all victims who died older than 16 and younger than 20 years old in the metropolitan areas and state capitals of Brazil, between 1996 and 2013. The solid line is the n-order polynomial that best fits the scatter points and the two dashed lines represent the confidence intervals. The red vertical dashed line marks the age of criminal majority. This figure was generated using the default bandwidth and procedures from McCrary (2008). Source: SIM/Datasus.

From (3), ρ is our measure of discontinuity in the number of violent deaths for varying specifications; it is the log difference of the height of the density function at the age cutoff.³⁰ For completeness, the tables that follow also include the total number of deaths across the chosen bandwidth and the number of deaths within the closest bins to the left and to the right of the cutoff. Bandwidths are typically a few months long, whereas bins are much shorter, spanning less than a week and reflecting the granularity of our data.

Table 1 Panel A presents our main results, the estimates of $\hat{\rho}$ from equation (3) considering all violent deaths of men across all metropolitan areas and state capitals in Brazil. Our preferred bandwidth, calculated with the rule-of-thumb bandwidth selection proposed by Fan and Gijbels (1996), is approximately 12 months around the age cutoff (97.007% of a year to be precise). The first column of Table 1 presents the results for this case. The point estimate implies a fall in violent death occurrences of approximately 2% when one crosses the criminal responsibility threshold. The density used in this test is based is discretized into several bins that are only 3 days long, with observations more than 12 months away (and therefore outside of the bandwidth) receiving no weight for the estimation. The drop obtained is quantitatively small and has no statistical significance (t-statistic equal to 0.95).

³⁰Therefore, using a standard first-order approximation, a small point estimate in log-points can be interpreted as a percentage change.

Table 1: Density Discontinuity on the Frequency of Violent Deaths by Age

	<i>Bandwidth in months (from the point of cutoff)</i>			
	± 12	± 3	± 6	± 17
	(1)	(2)	(3)	(4)
<i>Panel A. Total Violent Deaths</i>				
$\hat{\rho}$	-.02 (.021)	.02 (.042)	.006 (.03)	-.025 (.017)
Total Deaths	41150	10577	21139	60349
<i>L & R bins</i>	133/191	0/68	63/125	263/295
<i>Panel B. Deaths in confrontation with the police</i>				
$\hat{\rho}$.021 (.168)	-.208 (.371)	-.096 (.237)	.049 (.136)
Total Deaths	709	178	350	1001
<i>L & R bins</i>	21/22	4/2	9/11	30/25
<i>Panel C. Homicides</i>				
$\hat{\rho}$	-.009 (.023)	-.008 (.046)	.004 (.032)	-.018 (.019)
Total Deaths	35183	9089	18123	51497
<i>L & R bins</i>	170/204	0/55	54/105	222/246
<i>Panel D. Deaths by firearm (any)</i>				
$\hat{\rho}$	-.012 (.024)	-.002 (.048)	-.003 (.034)	-.019 (.02)
Total Deaths	31862	8248	16408	46676
<i>L & R bins</i>	154/180	0/47	51/92	201/219
<i>Panel E. Homicides by firearm</i>				
$\hat{\rho}$	-.003 (.024)	.007 (.049)	.007 (.035)	-.013 (.02)
Total Deaths	30498	7896	15738	44666
<i>L & R bins</i>	149/172	0/44	49/87	192/209
<i>Panel F. Homicides in the street</i>				
$\hat{\rho}$	-.011 (.036)	.065 (.072)	.038 (.051)	-.023 (.029)
Total Deaths	14042	3627	7230	20539
<i>L & R bins</i>	86/95	23/43	42/64	139/163

This table presents the discontinuity estimate and standard errors (in parenthesis) from the discontinuity density test proposed by McCrary (2008). Output variable is the number of violent deaths by age around 18 years old according to death cause as indicated in each panel. The discontinuity estimate, $\hat{\rho}$, is the log difference in height just before and after the 18 years old cutoff. Our sample are all violent deaths of males between 16 and 20 years old who died in Brazilian metropolitan areas. Columns present estimates for different bandwidths measured in months (rounded). Bandwidth in column (1) is the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996); precise bandwidth is .970066 of a year. Remaining columns show results with a 1/4, 1/2 and 1.5 times bandwidth and binwidths in column 1. From columns 1 to 4, binwidth (in days) is equal to 3, 1, 1 and 4. *Total Deaths* is the number of deaths in the chosen bandwidth and *L & R bins* displays the number of deaths in the closest bins to the left and to the right of the cutoff. Source: SIM/Datasus. Significance levels: *10%, **5%, ***1%.

We interpret a 2% frequency drop as small in light of the evidence presented in the recent literature. Biderman *et al.* (2010) study the consequences of a late-night dry law on mortality and finds a causal drop of 10% in homicides.³¹ Callaghan *et al.* (2014) and Carpenter and Dobkin (2009) study the consequences of the minimum drinking age in Canada and the United States, respectively, and estimate increases in mortality after crossing the age threshold of 14.2% in the former and 9% in the latter.³²

For robustness, we also consider bandwidths that represent multiples of the optimal bandwidth of Fan and Gijbels (1996). In particular, we consider smaller bandwidths, with a quarter and a half of a year in columns 2 and 3 and larger bandwidth of 1.5 times the reference bandwidth in column 4. The trade-off here is that at the same time that smaller bandwidths compare death rates of more similarly aged agents, they also include fewer observations, producing less precise estimates. As we see in columns 2 to 4, estimates of density discontinuity are all small in magnitude, change signs, and all indicate a failure to reject a continuous behavior of the violent death rate across the age of criminal responsibility. Table 1 Panels B through F repeat this exercise using five different classifications of violent deaths; all estimates are statistically equal to zero. Results are also consistent with a series of placebo tests and robustness checks we perform and report in Web Appendix B.³³

These initial results, however, may mask a great deal of variation not only along the characteristics of the population in the different regions in the country but also along other local metropolitan characteristics. We thus run the same exercise for each of the 32 metropolitan areas and state capitals. In Appendix Figure C.1, we plot the t-statistics from each estimated $\hat{\rho}$ using the different bandwidths and the histogram of these statistics. The estimates are centered around zero and show a pattern of statistical insignificance. Only in few cases the estimated effect is statistically significant, including the single metropolitan area in the state of Rio de Janeiro, but is consistent with the occurrence of 5% of false positives.

In sum, our theoretical starting point is that heavier punishments decrease the net benefits from crime participation, a decrease that should then be reflected in the number of homicides. We see little evidence of this when looking at all deaths in Brazil between 1996 and 2013 and in the near totality of metropolitan regions and state capitals.

³¹De Mello (2015) suggests a large connection between criminal activity related to drug trafficking and homicide rates, attributing 30% of the cross-sectional variation in homicides to the dispersion in the prevalence of crack cocaine in the state of São Paulo.

³²Our exercise would have power to identify an effect of this magnitude on mortality (see Section 5.2).

³³In Web Appendix B, we use lower-order polynomials as recommended by Gelman and Imbens (2014); we break our exercise in different time windows; we estimate placebo exercises using age cutoffs of 17 and 19 years old; and we run a regression discontinuity exercise on death rates by cohort using the Brazilian Demography Censuses from 2000 and 2010.

4.2 Results Using Criminal Records

We now turn to the estimation of a possible discontinuity on the distribution of criminal involvement using criminal record data. In particular, we use data on all arrests registered by the police in the state of Rio de Janeiro. As we showed in Section 2, Figure 1 plots local density estimates of criminal offenses by age for different types of crime, always centered on the 18th birthday. This figure reveals a large increase in offenses for non-drug-related non-index crimes (panel (d)) and a drop in the frequency of drug-related non-index crimes in panel (c).

Columns 2 to 5 in Table 2 present the estimates from (3) and define precise magnitudes for these observations. First, we find a 0.35 log-point increase in the density of “other” crimes that is statistically significant at 1%. If we interpret this observation as criminal involvement, it would follow that young adults are discontinuously 30% more likely to commit crimes when they turn eighteen years of age. Like Lee and McCrary (2009), however, we do not interpret these results as such. Instead, we see it as evidence consistent with a strong underreporting bias for minor crimes committed by young offenders. “Other” crimes are the least violent offenses – non-violent, non-property, and non-drug-related crimes – where police officers may use a particularly large degree of discretion on whether to follow the official procedures or simply issue a warning before releasing the offender. It should thus represent a reasonable upper bound on the differential underreporting bias.

On the other hand, we also estimate a 0.1 log-point drop in the frequency of drug-related crimes that is statistically significant at the 10% level. Rio de Janeiro is one of the most violent states in Brazil, and its capital records the largest absolute number of violent deaths. Crime in the metropolitan area of Rio de Janeiro is also uniquely distinguishable as a large drug-consuming market with drug-dealer gangs that are in a near-permanent state of turf wars (see Cerqueira, 2014; Carvalho and Soares, 2016). Also, with the strong evidence of underreporting bias, the estimated effects represent a lower bound for the underlying effect on criminal involvement.

To shed additional light on the magnitude of this reporting bias, we confront the point estimates with the density discontinuity estimates using violent deaths as proxy for criminal involvement, restricting the sample to the metropolitan area of Rio de Janeiro. The result is shown in column 1 in Table 2, where we estimate a 0.13 log-point drop in the frequency of violent deaths at the cutoff of 18 years old. This effect is also statistically significant at the 10% level and approximately of the same magnitude as the reported drop in drug-related arrests. It is, however, in contrast with the nationwide results.

Two possible interpretations exist. First, Rio de Janeiro might represent a particular scenario of detectable deterrence effects. Second, and more convincing in the absence of

Table 2: **Density Discontinuity on the Frequency of Violent Deaths and Police Arrests by Age – Rio de Janeiro, 2010 to 2014**

<i>Deaths</i>	<i>Arrests (2010-2014)</i>				
	<i>Index Crime</i>		<i>Non-Index Crime</i>		
	<i>Violent</i>	<i>Property</i>	<i>Drug</i>	<i>Other</i>	
	(1)	(2)	(3)	(4)	(5)
$\hat{\rho}$	-.13*	-.05	.00	-.10**	.35***
	(.07)	(.07)	(.12)	(.05)	(.06)
Bandwidth (months)	8	9	9	8	7
Binwidth (days)	7	7	14	7	7

This table presents the discontinuity estimate and standard errors (in parenthesis) from the discontinuity density test proposed by McCrary (2008). The discontinuity estimate, $\hat{\rho}$, is the log difference in height just before and after the 18 years old cutoff. In *Column 1*, the output variable is the number of violent deaths of men between 1996-2013 in Rio de Janeiro metropolitan area. In *Columns 2 to 5*, the output variable is the number of police arrests by crime type, following FBI's classification of index and non-index crimes, between 2010-2014 of men in Rio de Janeiro state, as in 1. Bandwidths and binwidths calculated using the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996). Source: SIM/Datasus and ISP-RJ. Significance levels: *10%, **5%, ***1%.

definitive evidence on the singularity of this metropolitan area, it is likely to simply represent a false positive among a set of large Brazilian urban areas. In light of the evidence from Appendix Figure C.1, we favor the second interpretation.

5 Discussion

In this section, we first discuss challenges to the identification of deterrent effects through the methodology we have employed. Then, we evaluate the statistical power of tests based on violent mortality to detect changes in underlying criminal engagement.

5.1 Threats to Identification

The identifying assumption for the treatment effect of stronger punishment after the age of eighteen is that any cause of violent death risk other than participation in crimes *continuously* changes with age at the 18th birthday threshold. We now assess some possible threats to the validity of that assumption, with particular focus on other rights and legal treatment changes that could occur around the same age threshold.

The legal drinking age. The minimum legal drinking age (MLDA) in Brazil is also set to eighteen years old. Evidence suggests a the link between drinking, criminal engagement,

and (both directly and indirectly) violent victimization.³⁴ As such, if compliance with the law were strong, one would expect an increase in the number of violent deaths at the age of eighteen due to a sudden change in alcohol consumption at 18 years of age. However, evidence suggests weak compliance with the MLDA determination in Brazil.

Romano *et al.* (2007) documents that underage test subjects were able to purchase alcohol in their first attempt more than 80% of the time in the metropolitan area of São Paulo. Souza *et al.* (2005, see Figure 2, in particular) presents evidence of high alcohol consumption and continuous growth with age among adolescents in the Cuiabá metropolitan region. Several other studies, like Madruga *et al.* (2012), reinforce the failure of MLDA compliance and document a dramatic prevalence of alcohol consumption and binge drinking among underage adolescents in Brazil. In related work, Cerqueira (2014), for instance, finds that alcohol does not have a relevant role in explaining either the rise or the fall in homicide rates in Brazil for the past three decades.

We, therefore, view the minimum legal drinking age as a minor, but still potentially relevant, threat to the identification of deterrent effects. We directly assess this point by employing our empirical strategy to test for a discontinuity in the density of deaths related to alcohol around the age of majority.³⁵ As can be seen in Table 3, Panel A, we find no statistically significant change in the number of deaths involving alcohol. The results are qualitatively the same if we include illegal drug abuse.

Driver licenses and traffic accidents. Another legal allowance that follows a threshold rule at eighteen years of age is the right to drive standard cars and motorcycles.³⁶ Immediately after turning 18 years old, people might start a training process in specialized driving schools and, after its completion, take a practical examination to obtain a restricted-use temporary license.³⁷ The training takes a few months, and the costs involved are significant (equal to around one and a half months of work at minimum wage), especially in light of low-income at-risk groups. These costs and delays, in turn, make a sudden increase in mobility and traffic exposure when turning eighteen an unlikely threat to identification.

Besides its role as a placebo outcome, we are also interested in assessing any concurrent

³⁴See, Carpenter and Dobkin (2009) and Callaghan *et al.* (2014) for impacts of the MLDA on mortality.

³⁵These are deaths directly related to alcohol abuse and any other death causes (e.g., car crashes and other accidents) where non-trivial levels of alcohol were found in the blood at the autopsy. We discuss below deaths related to risky behavior more generally. These could, potentially, increase after the 18th birthday if a large increase in binge drinking occurred, since this is known to induce impulsive behavior. We find no evidence of an increase in these more general measures either.

³⁶The process for buses, trucks, and other specialized heavy-duty vehicles requires a minimum of one or two years in these previous categories. Additionally, a stricter 21-year minimum age rule applies for the categories with the heaviest vehicles.

³⁷The theoretical effect of driving and licensing on criminal behavior is ambiguous; at the same time, it increases non-crime-related job opportunities and also expands the types of crimes the individual can commit.

effects of the minimum legal age for driving. We run density discontinuity tests looking at deaths caused by car accidents and related causes.³⁸ In Table 3, Panel B, we see that reaching the age of criminal majority has no effect on the frequency of deaths by vehicle accident.

Congscription, military draft. In the first six months of each year, male Brazilian citizens who turn eighteen on that calendar year have to present themselves to an office of the Armed Forces and officially make themselves available to join the military. Essentially, after a series of brief stages without any significant time dedication, only those who wish to follow a military career and have proved their mental, physical, and health competencies are drafted. If selected, the young male is incorporated into the force and actually starts military service in the following calendar year.³⁹ However, because of a so-called “excess military contingent”, more than 95% of young males are discharged throughout the procedure.⁴⁰

The main concern is that a training internship and other forms of time dedication would incapacitate the young male at his 18th birthday, affecting his criminal activities. First, given its restricted reach, the military draft should be a limited cause for concern in our case. Even more importantly, a serious threat to the RD design requires any confounding treatment under analysis to be a sharp function of the age. The military draft is not this: when presenting for the draft, people lie randomly from zero to eleven months away from their eighteenth birthday; those roughly 5% actually drafted had a 18th birthday happen six to twelve months prior to the start of military service.

Compulsory education. During the period of study, the compulsory education threshold in Brazil was set at fourteen years of age and was, therefore, outside even our largest bandwidth window. As a consequence, we do not see education policies as a threat to the identification of deterrent effects.⁴¹

Labor market regulation. The other set of legal rights that change at the majority threshold and could be relevant to our identification strategy are labor regulations. Starting at age 16, people can already be formally employed with full rights,⁴² but with some special working conditions. In particular, minors cannot work more than 40 hours a week (extra hours are not allowed), work in an unhealthy workplace, or work at night shifts. Although these changes do expand the set of job opportunities at age eighteen, we find it unlikely that the

³⁸We considered fatal accidents by car, motorcycles, and trucks, and explicitly discarded buses since they are essentially means of public transportation.

³⁹For example, someone who turned 18 in 2010 had to present himself to the Armed Forces between January and June 2010 and, if drafted, started his military service in January 2011.

⁴⁰According to the Brazilian Army (Exército Brasileiro, 2010), in 2006, 4.5% of agents that signed up were drafted at the end of the process. The estimated figure for recent years is roughly the same.

⁴¹It was only recently, with the signing of Law number 12.796 in April 2013 (and following a Constitutional amendment from 2009), that mandatory education was set to last until the age of seventeen.

⁴²Teenagers between 14 and 16 years old can be formally hired, but need the consent of the parents.

Table 3: Density Discontinuity on the Frequency of Non-Violent Deaths by Age

	<i>Bandwidth in months</i>			
	± 12 (1)	± 3 (2)	± 6 (3)	± 17 (4)
<i>Panel A. Deaths caused by alcohol abuse</i>				
$\hat{\rho}$.208 (.76)	.802 (1.305)	1.466 (1.352)	.356 (.642)
Total Deaths	31	6	17	44
<i>L & R bins</i>	4/5	1/2	1/4	6/8
<i>Panel B. Deaths by vehicle accidents</i>				
$\hat{\rho}$.086 (.088)	.151 (.176)	.135 (.125)	.068 (.072)
Total Deaths	2630	632	1279	4008
<i>L & R bins</i>	31/48	7/14	15/25	49/64
<i>Panel C. Deaths caused by accidents (excl. vehicle)</i>				
$\hat{\rho}$	-.121 (.077)	-.246 (.16)	-.218** (.111)	-.096 (.062)
Total Deaths	3184	805	1653	4737
<i>L & R bins</i>	56/41	11/13	21/23	79/63
<i>Panel D. Suicides</i>				
$\hat{\rho}$.018 (.122)	.414 (.257)	.042 (.177)	.011 (.1)
Total Deaths	1325	322	667	2015
<i>L & R bins</i>	24/35	5/5	10/11	37/53
<i>Panel E. All other non-violent deaths</i>				
$\hat{\rho}$	-.072 (.047)	-.023 (.096)	-.116* (.067)	-.028 (.038)
Total Deaths	8513	2139	4296	12781
<i>L & R bins</i>	70/92	12/30	34/53	115/120

This table presents the discontinuity estimate and standard errors (in parenthesis) from the discontinuity density test proposed by McCrary (2008). Output variable is the number of deaths by age around 18 years old according to death cause as indicated in each panel. The discontinuity estimate, $\hat{\rho}$, is the log difference in height just before and after the 18 years old cutoff. Our sample are all non-violent deaths of men between 16 and 20 years old who died in Brazilian metropolitan areas. Columns present estimates for different bandwidths measured in months (rounded), calculated as in Table 1. *Total Deaths* is the number of deaths in the chosen bandwidth and *L & R bins* displays the number of deaths in the closest bins to the left and to the right of the cutoff. Source: SIM/Datasus. Significance levels: *10%, **5%, ***1%.

age threshold of labor law by itself would lead to a substantial increase in hiring in the formal sector at eighteen substantially affecting criminal behavior, particularly in a setting where the informal sector is so large. In the period of this study, the degree of informality in the labor market in metropolitan areas in Brazil ranged between 38% and 49% percent (PNAS/IBGE). As illustrated, using data from the Ministry of Labor and Employment (RAIS/MTE) to plot the age distribution of new hires in the formal sector in 2000, we find that the greatest jump in hires happened at 19 years old (see Figure C.4 in the Web Appendix).

Symbolism and psychological triggers. Last, the 18th birthday may be charged with symbolism, marking a milestone “coming of age” event that could discontinuously affect individual behavior. This could affect not only violent criminal behavior specifically, but also risky behavior and non-violent crimes, more generally. These psychological considerations, which point to greater responsibility, would reinforce the potential deterrent effects of criminal majority. Still, we fail to estimate any discontinuity for violent deaths by age.

We assess potential discontinuous changes in risky behavior at the majority age by testing for a discontinuity on other death causes in Table 3. First, in Panel C, we assess the number of deaths in unintentional accidents excluding vehicles accidents (category “W” in ICD-10 excluding vehicles and codes related to violent deaths). These include deaths that could be related to imprudent behavior, such as unintentional falls, being struck by/against large objects, drowning, and electric discharges. Since psychological triggers could also affect the likelihood of self-harm, we also directly look at the age distribution of suicides in Panel D. Last, we run our density discontinuity tests by looking at deaths from all other non-violent deaths in Panel E. We see that reaching the age of criminal majority has no effect on the frequency of deaths in any of these cases. We thus interpret that as evidence that a psychological or a social mechanism does not seem to impose a major threat to our identification strategy.

5.2 Power Calculation

Our proxy hinges on the link between a reduction in violent criminal behavior and a decrease in violent deaths. If this link is too weak, we could be facing a weak proxy issue, which would manifest itself in large standard errors for a given sample size.

While we cannot directly assess the power of this link, we can calculate the power of our test to identify a drop in the density of violent deaths directly. Appendix Figure C.3 presents the minimum detectable effect (approximately measured in percentage points) against the power of the test, for three different significance levels. At 5% significance level and power of 80%, our exercise would be able to detect a drop in the density function of around 5%. A minimum detectable effect of 10% has power close to one for the three standard significance

levels. This means our empirical strategy has enough power to identify meaningful effects of the age of criminal majority on the frequency of violent deaths among young adults.

In Appendix A, we provide additional meaning to this exercise by laying out a model linking the identification discussion and the power calculation of our estimates for the treatment of interest: a reduction in criminal involvement induced by the stronger penal code. The model clarifies that the key parameter to this calculation is a non-identified nuisance parameter γ . This parameter relates, for people at the critical age, the ratio of the baseline victimization rate to the part of the victimization rate that is due to criminal behavior under the less strict juvenile code.

In the limit case in which $\gamma = 0$, violent victimization is only a function of criminal involvement, and detectable falls in death rates are related one-to-one to changes in this treatment ratio. Away from that case, with knowledge of γ , one could still move from detectable effects on violent victimization toward detectable effects on criminal involvement. In particular, the minimum detectable effect of criminal involvement is a linear function of γ and the minimum detectable effect on violent deaths. This means that even when the baseline victimization rate is equal to the victimization rate due to criminal behavior under the less strict juvenile code ($\gamma = 1$), a minimum detectable effect of 5% on violent deaths (at 5% significance level and power of 80%) relates to a detectable effect on criminal involvement equal to 10.25%. That is, we are able to rule out relatively small deterrent effects.

6 Coming to terms with insignificant deterrent effects

We have searched for evidence of criminal deterrence effects from stronger potential punishments through the use of a novel proxy and a density discontinuity test. We studied Brazilian metropolitan areas using potential changes in the frequencies of young male violent death as a proxy for criminal involvement. Legal adults convicted of crimes are subject to stronger potential sanctions when compared to minors, with the distinction in legal treatment being defined by a sharp rule at the eighteenth birthday. Strong deterrence effects should lead to a drop in criminal involvement and, given the high risk involved in violent criminal activities, to a downward jump in the frequency of violent deaths around the eighteenth birthday. We have discussed advantages and potential shortcomings of this approach, especially in comparison to the more traditional use of arrest and conviction records as proxies for criminal involvement.

The data on several violent mortality measures show no jump in the frequency of violent deaths around the age of criminal responsibility. Based on the well-documented partial overlap between victimization and criminal engagement, this evidence adds to a body of research that concludes that deterrent effects of harsher punishments for youth violent crime

are weak.

Insignificant deterrent effects of more severe penalties can still be reconciled with rational theories of criminal behavior. These theories prescribe that for sanctions to serve their fundamental purpose of deterring crime, punishment must be considered in all three of its main dimensions: severity, certainty, and celerity. A change only in the severity with which young offenders are punished probably falls short of its intention to deter crime, due to the extremely low crime resolution rates prevalent in Brazilian cities. For instance, a report by the Brazilian Ministry of Justice and the Public Prosecutor's Association (ENASP, 2012, p. 22) indicates a homicide resolution rate below 10%, in sharp contrast with the rates obtained in developed countries of 65% for the United States and 90% for the United Kingdom.

Our results are also consistent with the general theory of crime of Gottfredson and Hirschi (1990), who point out that criminal behavior is often impulsive and reflects a lack of self-control. Moffitt *et al.* (2011) show that individuals with poor self-control more often make mistakes as adolescents, such as getting involved in crimes. In these cases, the threat of harsher punishments would have an even smaller deterrent effect in practice.

Lastly, we emphasize that while the current discussion focuses on punishment-based policies, some alternatives focus more on the benefit side of rational calculations. Many policies may reduce criminal involvement by increasing the payoff from staying away from crime, such as education and social inclusion programs. Thus there still remains an important role for future research on the effectiveness of particular policies guided by this logic.

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A Appendix: Power Calculation

To provide additional meaning to this exercise, let us return to the identification discussion from Section 3.2. Participation in a crime is an individual decision that depends on the treatment (assigned deterministically as a function of the age) $Y(D)$ and, potentially, over other characteristics. Assume that the probability of death ($W = 1$) for an individual of age X and unobservable characteristics u takes the following form

$$Pr(W = 1|X, u) = \alpha(X, u) + \beta Y(D),$$

where $Y(D) \in \{0, 1\}$ is the criminal involvement of the agent for $D \in \{0, 1\}$. In this set-up, the probability of dying from a violent cause at age X is the combination of the baseline victimization rate if the agent does not engage in crime – $\alpha(X, u)$ – and the additional propensity of being killed when participating in a criminal activity – $\beta Y(D)$. The key functional assumption is then that the effect of criminal involvement on the probability of violent victimization is separable and constant across individuals.

It follows that the density discontinuity parameter we estimate satisfies

$$\rho = \lim_{X \searrow 0} \ln f(X|W=1) - \lim_{X \nearrow 0} \ln f(X|W=1) = \ln \left(\gamma + \frac{E[Y(1)|X=0]}{E[Y(0)|X=0]} \right) - \ln(\gamma + 1), \quad (4)$$

where $\gamma = \frac{E[\alpha(X,u)|X=0]}{\beta E[Y(0)|X=0]}$ is a non-identified nuisance parameter. This parameter relates, for individuals at the critical age, the ratio of the baseline victimization rate to the part of the victimization rate which is due to criminal behavior under the less strict juvenile code ($D = 0$).

Notice that the term $E[Y(1)|X=0]/E[Y(0)|X=0]$ in equation 4 is the treatment ratio of interest: the reduction in criminal involvement induced by the stronger penal code.

Appendix (Not for Publication)

B Placebos and Robustness Checks

The basic question we address in this section is whether in Brazil young individuals who have just turned eighteen are different from those who are slightly younger than this threshold in other aspects besides being subject to different penal systems. Before discussing it, let us first consider another way of dealing with RD estimates on frequency data that has been already advanced by the literature.

B.1 Linear Density Test

As we mentioned in section 3.2, the estimation procedure we employ (McCrary, 2008) uses high-order polynomials. But more recently, Gelman and Imbens (2014) makes the case that RD designs should favor low-order polynomials. As a robustness check, we then investigate whether the results above carry over when we run a analogous density test but with low-order polynomials for the density functions f^+ and f^- and a rectangular kernel. Standard errors used are bootstrapped to properly take into account the non-standard data generating process and small-sample properties of the estimator. In this exercise, we use $K = 25$ bins on each side.

Table C.3 presents the results. Note that the coefficients in this table are expressed in number of deaths. Its estimates should always be interpreted relative to the average sizes of the bins that are constructed. For the ease of interpreting the magnitude in the drop (or jump if positive) at 18 years, we include in brackets the size of the coefficient relative to the frequency of the closest bin to the left of the cutoff. We find no evidence of discontinuity in the frequency of violent deaths at the age cutoff.

We show more specifications using low-order polynomials.

B.2 Regression Discontinuity Design

One could be reasonably concerned that, by pooling together deaths from different years, we may be comparing rates across cohorts of different sizes. We use the Brazilian Demography Censuses from 2000 and 2010 which contain the cohort size per age in each metropolitan region in these two years. In the census data we observe the age of individuals in number of years, so it is not possible to observe the cohort size for each date of death. To circumvent this, we interpolate the size of the cohort to calculate the death rate by age in weeks for every metropolitan region in 2000 and 2010. We use this data to estimate a standard regression discontinuity as Calonico *et al.* (2014), with the results shown in Table C.6. Again we find no evidence of a drop in violent mortality which could argue in favor of a deterrence effect around the age of majority.

B.3 Different Time Windows

Total death numbers have time trends of their own, either natural or influenced by policy interventions or economic activity. Also, the death record database is itself in a continuous improvement process, increasing its coverage and generating more detailed data

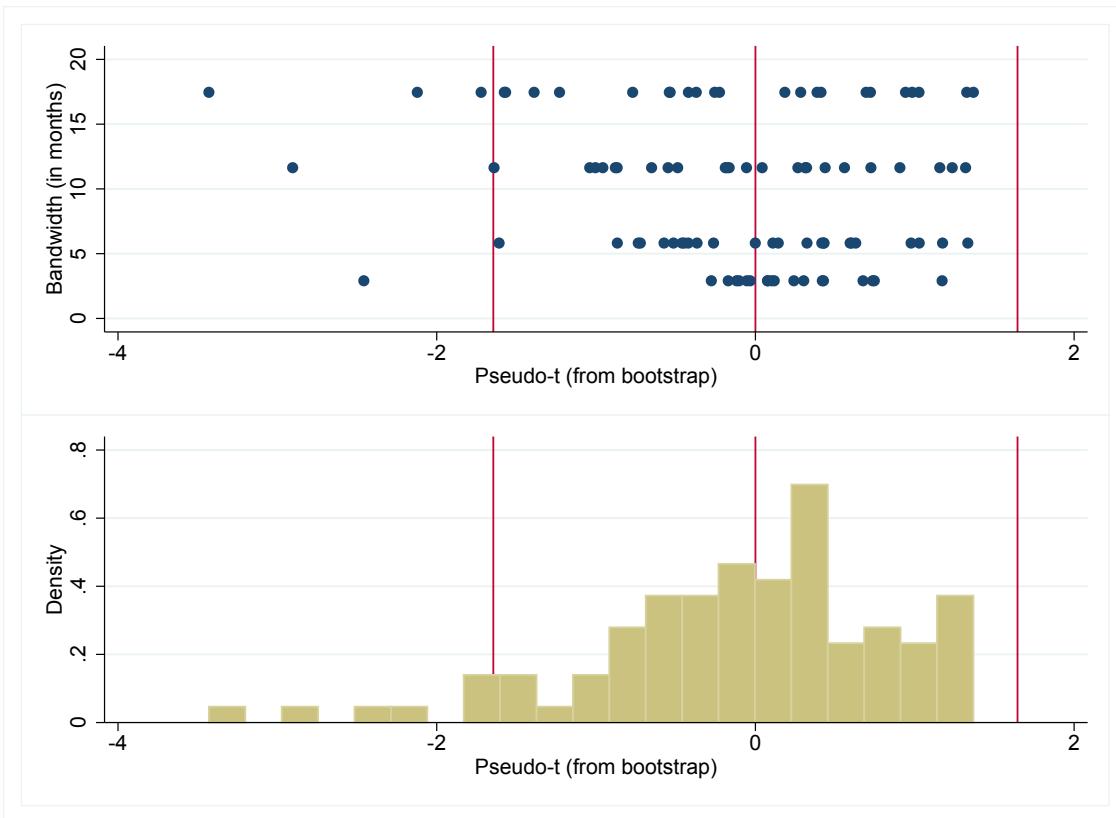
When we pool death observations from 1996 to 2013, these trends in death numbers and death records do not violate our identification assumption unless there were distinct trends for deaths just below or just above the cutoff at eighteen. For instance, if in this ten-year period policies had been enacted that had reduced crime among recent adults but not among soon-to-be adults, or if death records had improved solely for one of the two groups. To the best of our knowledge, this does not seem to be the case. Still, in Table C.4 we divide our original time interval into four different time periods. The null effects for metropolitan regions in Brazil remain in all time periods.

B.4 Placebo Age Cutoffs

We also evaluate alternative cutoff points besides the age of eighteen to check if our data would point to a discontinuity in a value of our assignment variable that we know is *not* the true cutoff for treatment assignment. Figure C.2 plots the local density of violent deaths for four different age cutoffs: 17, and 19 years old. Table C.5 summarizes the results from the formal procedures. We find a zero effect for every placebo cutoff.

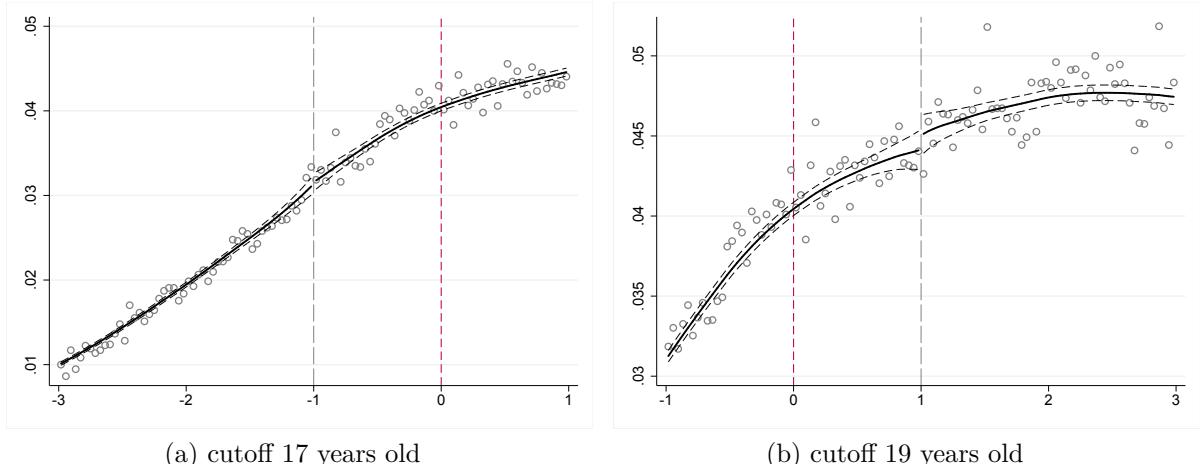
C Additional Figures and Tables

Figure C.1: t Statistics and Histogram (All Metropolitan Areas)



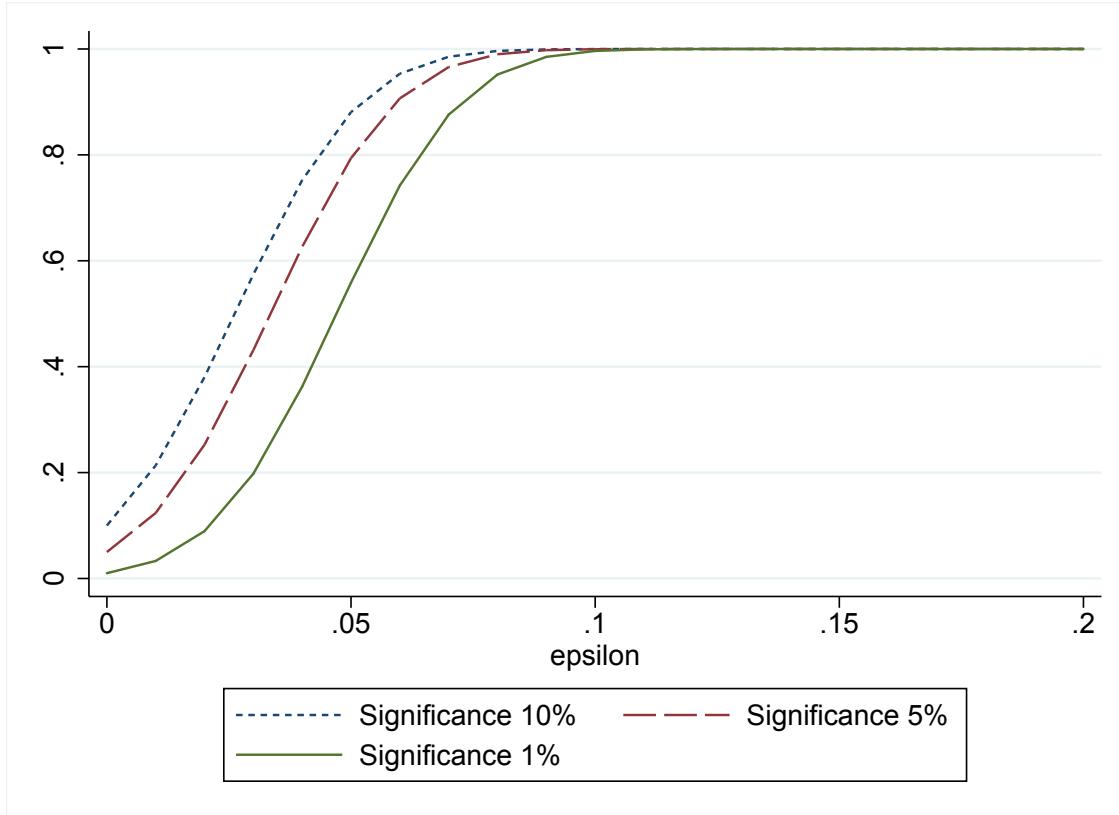
This figure plots the t statistics calculated for violent deaths of males estimated separately for each metropolitan area and state capital in Brazil, estimated in an analogous way as in Table 1. We present the t statistic calculated for different bandwidths (3, 6, 12 and 17 months) as indicated in the vertical axis. Source: SIM/Datasus.

Figure C.2: Placebo cutoffs



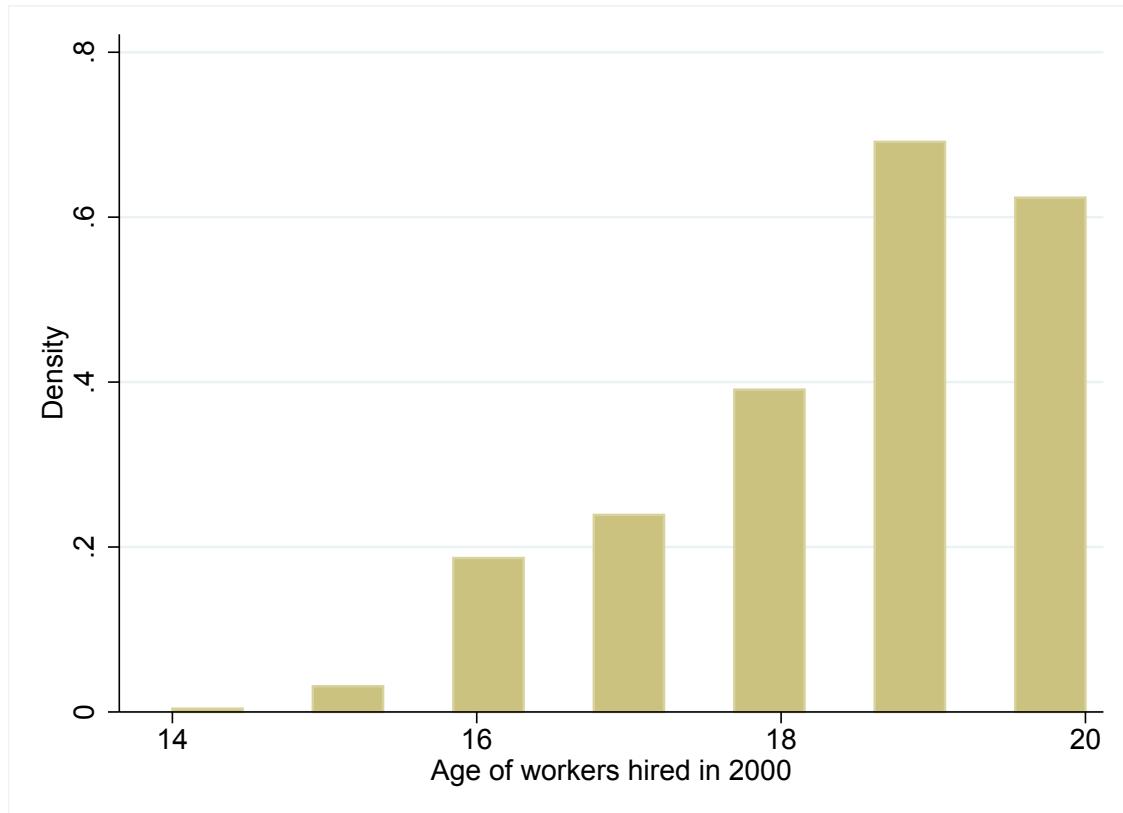
This figure plots the frequency of violent deaths of young males per age of death (in the horizontal axis: age of death minus 18 years old) for all victims who died in the metropolitan areas and state capitals of Brazil, between 1996 and 2013. Each graph looks for a discontinuity in the death number for placebo age thresholds: 17 (figure a), and 19 (figure b) years old. We present the number of deaths per age of death for two years around the placebo year threshold. The solid line is the n-order polynomial that best fits the scatter points and the two dashed lines represent the confidence intervals. in each graph, the red vertical dashed line marks the age of criminal majority (0 in the vertical axis); the gray vertical line marks the placebo threshold. Figures generated using the default bandwidth and procedures from McCrary (2008). Source: SIM/Datasus.

Figure C.3: Power Test



This figure plots the power of our test to identify a drop in the density of violent deaths at eighteen years of age. It plots the minimum detectable effect, ϵ^{MDE} , defined by $\ln(1 - \epsilon^{MDE}) = \ln f^- - \ln f^+$. We consider three different significance levels as indicated.

Figure C.4: Histogram of age of new hires in 2000



This figure plots the histogram of the age of hires in formal jobs in 2000. Sample males in metropolitan areas and state capitals. Absolute number in each age: 588 (14); 4,549 (15); 27,223 (16); 34,861 (17); 57,015 (18); 100,845 (19); and 90,976 (20). Source: RAIS/MTE.

Table C.1: Codes from the ICD-10 and Categorization According to Intention

Intention	Characterizing Instrument	Subdivision of the ICD-10
	Poisoning	X85-X90
	Hanging	X91
	Drowning	X92
	Firearm	X93-X95
		Y350
	Impact	X96, Y01-Y03
Homicide		Y351
	Fire	X97-X98
		Y352
	Blunt	Y00, Y04-Y05
		W50
		Y353
	Unknown	Y06-Y09
		Y356, Y357
	Poisoning	Y10-Y19
	Hanging	Y20
	Drowning	Y21
	Firearm	Y22-Y24
		W32-W34
Indetermined	Impact	Y25, Y30, Y32
	Fire	Y26-Y27
	Sharp	Y28
		W25-W26
	Blunt	Y29
	Unknown	Y33-Y34

Notes: Since we are following Cerqueira (2013), this table is part of his more detailed table 1, which includes suicides and accidents (not related to motor vehicles).

Table C.2: Summary Statistics Homicides in Brazil (Largest Metropolitan Areas)

	<i>Bandwidth in months (from the point of cut-off)</i>							
	-12	+12	-3	+3	-6	+6	-17	+17
<i>Total Deaths</i>	26246	30453	7227	7362	14160	14844	36442	47018
<i>Marital Status</i>								
Single	21965	25189	6055	6040	11852	12276	30586	38820
Married	2826	3420	787	869	1542	1705	3861	5337
Others	678	989	187	247	366	477	914	1538
Missing	777	855	198	206	400	386	1081	1323
<i>Last Level of Schooling Completed</i>								
1 st -4 th Year	650	761	164	162	337	364	878	1171
5 th -8 th Year	2736	3154	740	760	1451	1542	3808	4989
High School	7382	7894	1997	1896	3978	3830	10364	12001
Incomp. College or more	5398	6543	1488	1572	2926	3166	7432	10198
Missing	10080	12101	2838	2972	5468	5942	13960	18659
<i>Type of Place Where Death Took Place</i>								
Hospital	9712	11367	2686	2765	5222	5538	13643	17403
Domicile	2167	2561	618	609	1191	1239	2960	3958
Public Venue	13784	15881	3756	3829	7425	7766	19062	24633
Missing	115	135	34	39	61	64	146	209
<i>Source of Information for the Death Declaration</i>								
Police Report	9687	11136	2650	2696	5196	5474	13441	17039
Hospital	801	931	225	198	415	443	1127	1430
Other	1895	2208	513	543	1027	1092	2681	3433
Missing	13863	16178	3839	3925	7522	7835	19193	25116
<i>Autopsy</i>	19796	22884	5429	5544	10587	11196	27464	35343

This table presents the number of violent death declarations for each value of the covariates listed. The distance to the point of cut-off at the age of 18 is measured in months. 1st year of school in Brazil starts usually at age 6 and High School generally encompasses the ages between 14 and 18.

Table C.3: Density Discontinuity on the Frequency of Violent Deaths by Age – Lower order Polynomials

Bandwidth in months (from the point of cutoff)				
	± 3	± 6	± 9	± 12
	(1)	(2)	(3)	(4)
<i>Panel A: Linear</i>				
$\hat{\rho}$	2.87 (8.33) [1.5%]	1.18 (11.86) [.3%]	-21.71 (14.45) [-3.6%]	-15.68 (16.65) [-1.8%]
<i>Panel B: Quadratic</i>				
$\hat{\rho}$	5.73 (12.66) [2.9%]	1.75 (17.83) [.4%]	8.91 (21.64) [1.5%]	-23.09 (25.09) [-2.6%]
<i>Panel C: Cubic</i>				
$\hat{\rho}$	25.06 (16.82) [12.9%]	5.63 (24.07) [1.3%]	14.66 (29.01) [2.4%]	17.21 (34.11) [2%]
<i>L & R bins</i>	195\244	440\478	607\653	878\890

Notes: RD estimates from a (local) *linear*, *quadratic* or *cubic* regression given a choice of bandwidth indicated in the columns and using 25 age bins on *each* side of the cutoff. Bootstrapped standard errors are in parentheses (10000 replications). The percentage increase (if positive) or decrease (if negative) in the frequency of homicides relative to the size of the closest bin to the left of the cutoff are shown in brackets. *L & R bins* displays the number of deaths in the closest bins to the left and to the right of the cutoff. Significance levels: *10%, **5%, ***1%.

Table C.4: Homicides in Brazil (Largest Metropolitan Areas) – different years

	Bandwidth in months (from the point of cutoff)			
	± 12	± 3	± 6	± 17
<i>Panel A: Years 1996-1999</i>				
$\hat{\rho}$	-.011 (.047)	.083 (.094)	.04 (.066)	-.005 (.038)
Total Deaths	8224	2161	4266	12021
<i>L & R bins</i>	333/353	333/353	333/353	333/353
<i>Panel B: Years 2000-2003</i>				
$\hat{\rho}$.006 (.041)	-.048 (.084)	.016 (.058)	-.016 (.033)
Total Deaths	11405	2847	5796	16814
<i>L & R bins</i>	400/399	400/399	400/399	400/399
<i>Panel C: Years 2004-2008</i>				
$\hat{\rho}$	-.028 (.042)	.039 (.082)	.007 (.059)	-.035 (.034)
Total Deaths	10545	2702	5392	15422
<i>L & R bins</i>	440/451	440/451	440/451	440/451
<i>Panel D: Years 2009-2013</i>				
$\hat{\rho}$	-.067 (.046)	-.087 (.091)	-.063 (.065)	-.059 (.038)
Total Deaths	8531	2232	4392	12412
<i>L & R bins</i>	466/438	466/438	466/438	466/438

This table presents the discontinuity estimate and standard errors (in parenthesis) from the discontinuity density test proposed by McCrary (2008). Each panel restricts the sample to deaths in the years indicated. Output variable is the number of violent deaths by age around 18 years old. The discontinuity estimate, $\hat{\rho}$, is the log difference in height just before and after the 18 years old cutoff. Our sample are all violent deaths of men between 16 and 20 years old who died in Brazilian metropolitan areas. Columns present estimates for different bandwidths measured in months (rounded). Bandwidth in column (1) is the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996). *Total Deaths* is the number of deaths in the chosen bandwidth and *L & R bins* displays the number of deaths in the closest bins to the left and to the right of the cutoff. Source: SIM/Datasus. Significance levels: *10%, **5%, ***1%.

Table C.5: Homicides in Brazil (Largest Metropolitan Areas) – different cutoffs

	Bandwidth in months			
	± 12	± 3	± 6	± 17
<i>Panel A: Cutoff at 17 years old</i>				
$\hat{\rho}$.008 (.024)	-.07 (.048)	-.006 (.034)	.032 (.02)
Total Deaths	31997	8104	15932	47228
<i>L & R bins</i>	684/653	684/653	684/653	684/653
<i>Panel B: Cutoff at 19 years old</i>				
$\hat{\rho}$.02 (.02)	-.005 (.041)	.015 (.029)	.018 (.017)
Total Deaths	46022	11492	23130	68844
<i>L & R bins</i>	903/874	903/874	903/874	903/874

This table presents the discontinuity estimate and standard errors (in parenthesis) from the discontinuity density test proposed by McCrary (2008). Output variable is the number of violent deaths by age around different age cutoffs (in panels). The discontinuity estimate, $\hat{\rho}$, is the log difference in height just before and after the indicated age cutoff. Our sample are all violent deaths of men between 16 and 20 years old who died in Brazilian metropolitan areas. Columns present estimates for different bandwidths measured in months (rounded). Bandwidth in column (1) is the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996). *Total Deaths* is the number of deaths in the chosen bandwidth and *L & R bins* displays the number of deaths in the closest bins to the left and to the right of the cutoff. Source: SIM/Datasus. Significance levels: *10%, **5%, ***1%.

Table C.6: Regression Discontinuity Estimates of Violent Death Rates by Age Using Census

	Bandwidth (in months)			
	± 7	± 2	± 3	± 10
	(1)	(2)	(3)	(4)
$\hat{\rho}$	1.16 (1.29)	.62 (2.75)	1.81 (2.43)	-.33 (1.59)
Number of observations	1374	340	688	2036

Estimates of a regression discontinuity design, as Calonico *et al.* (2014), of the violent deaths rate by age around 18 years old. All deaths in 2000 and 2010. Death rates calculated using interpolation of metropolitan area-cohort population using Censo data. Columns present estimates for different bandwidths measured in months (rounded). Bandwidth in column (1) is the rule-of-thumb bandwidth proposed by Fan and Gijbels (1996). *Total Deaths* is the number of deaths in the chosen bandwidth. Total number of observations 17153. Source: SIM/Datasus. Significance levels: *10%, **5%, ***1%.