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ABSTRACT

Ban-the-box (BTB) laws, which prevent employers from asking prospective employees about their criminal histories at initial job screenings, have been adopted by 25 states and the District of Columbia. Using data from the National Incident-Based Reporting System, the Uniform Crime Reports, and the National Longitudinal Survey of Youth 1997, this study is the first to estimate the effect of BTB laws on crime. We find some evidence that BTB laws are associated with an increase in property crime among working-age Hispanic men. This finding is consistent with employer-based statistical discrimination as well as potential moral hazard. A causal interpretation of our results is supported by placebo tests on policy leads and a lack of BTB-induced increases in crime for non-Hispanic whites and women. Finally, we find that BTB laws are associated with a reduction in property crime among older and white individuals, consistent with labor-labor substitution toward those with perceived lower probabilities of having criminal records (Doleac and Hansen 2017).

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1. Introduction

More than 2.2 million Americans were incarcerated (Bureau of Justice Statistics 2016), representing 24 percent of the world's prison population (World Prison Brief 2017). Between 2000 and 2015, the male incarceration rate rose from 904 to 1,600 inmates per 100,000 population, with African Americans and Hispanics consistently representing a disproportionate share of inmates (Bureau of Justice Statistics 2016, 2001).[1] Real public spending on incarceration reached over $80 billion in 2013, with the total costs of the criminal justice system reaching over $250 billion (Executive Office of President of the United States 2016).

Recidivism rates among the incarcerated population are quite high. Over three-quarters (77 percent) of released prisoners are rearrested within five years (National Institute of Justice 2014). The lack of labor market opportunities for those with criminal records has been posited as one rationale for high recidivism rates (Executive Office of President of the United States 2016). Observational studies show that those with criminal records are less likely to be employed (Grogger 1995; Pager 2003; Pager et al. 2009) and earn less (Nagin and Waldfogel 1998; Geller et al. 2006) than their non-incarcerated counterparts. While this relationship can be explained, at least in part, by difficult-to-measure personal characteristics (Grogger 1995), recent audit experiments suggest a causal link (Pager et al. 2009).

With the goal of improving labor market opportunities of ex-offenders, 25 states and the District of Columbia — along with over 150 cities and counties — have implemented "ban-the-box" (BTB) laws, which require employers to remove questions regarding the prospective employee's criminal history from job applications (National Employment Law Project 2017). Proponents argue that by withholding information about criminal histories from initial job

[1] African Americans and Hispanics represent over 50 percent of the U.S. prison population despite representing just 12 percent and approximately 20 percent, respectively, of the U.S. population (Raphael and Stoll 2013).

application screenings, employers may be more willing to hire those with criminal records because their perceptions of a criminal record may be changed by social engagement with the applicant (Doleac and Hansen 2017; National Employment Law Project 2017). On the other hand, employers may react to less information on an applicant's criminal history by engaging in statistical discrimination against demographic groups with (perceived) higher rates of criminal arrests.[2]

There are a number of channels through which BTB laws may affect crime. If BTB laws are effective in increasing employment among those with criminal records, this could reduce the gains to criminal activity, resulting in less crime. On the other hand, if BTB laws induce statistical discrimination against racial minorities, then BTB laws could have the unintended consequence of increasing crime rates among low-skilled minorities who have fewer job options. BTB laws may also affect criminal behavior of those who are less likely to have criminal records (e.g. older or more highly educated individuals) if firms engage in labor-labor substitution toward such individuals. Finally, BTB laws could also generate moral hazard if such laws lower the future cost of crime commission by reducing the likelihood that criminal histories will become known to prospective employers.

To our knowledge, this study is the first to estimate the effect of BTB laws on crime. Using data from the National Incident Based Reporting System (NIBRS) from 2004 to 2014 and a difference-in-differences approach, we find that BTB laws are associated with a 10 to 20 percent increase in property crime offenses committed by Hispanic men ages 25 and older, the same demographic groups that saw declines in employment in response to BTB laws (Doleac

[2] In addition, the higher administrative costs imposed on firms could result in firms choosing to hire fewer employees of all race/ethnicities or differentially fewer racial minorities, to whom it might devote disproportionately larger shares of resources for additional background checks.

and Hansen 2017). A causal interpretation of our findings is supported by falsification tests on leads of BTB laws as well as the absence of increases in crime committed by non-Hispanic whites and females. Finally, we uncover evidence that BTB laws may reduce property crime among some older African American and non-Hispanic white males, consistent with labor-labor substitution toward those less likely to have criminal records. Supplemental analyses using county-level data from the Uniform Crime Reports and individual-level longitudinal data from the National Longitudinal Survey of Youth 1997 provide a qualitatively similar pattern of results.

2. Background and Literature

2.1 Background

Approximately one-third of all U.S. adults have some type of criminal record (Bureau of Justice Statistics 2014). A 2012 survey by the Society of Human Resources Management found that 69 percent of employers used criminal background checks for some or all hiring decisions, representing a large increase over the last two decades (Holzer, Raphael and Stoll, 2006; Society for Human Resources Management 2012).

Following release, ex-offenders may face difficulties finding employment for a variety of reasons (Holzer, Raphael and Stoll 2003). Time spent incarcerated prevents individuals from gaining work experience, may depreciate previously accumulated labor market skills, and may interrupt schooling investments (Western, Kling and Weiman 2001). In addition, imprisonment may also lead to an erosion of social capital, reducing the likelihood of finding future employment (Sampson and Laub 1993). Moreover, employers may use a prospective worker's prior criminal conviction as an observable indicator of lower average productivity or higher expected liability costs from reoffending (Raphael 2011a; Freeman 2008; Blumstein and

Nakamura 2009).[3,4] A widely cited 2003 survey of California employers found that 71 percent of respondents said that they would "probably not" or "definitely not" hire an applicant with a criminal background (Raphael 2011b).

While some of the adverse labor market effects of incarceration may be partly offset by in-prison schooling and job training programs (Kling 2006; Landersø 2015), ex-offenders still have worse labor market outcomes than their non-incarcerated counterparts (Grogger 1995; Pager 2003; Pager et al. 2009; Geller et al. 2006). An audit study by Pager et al. (2009) finds that randomly assigning a criminal record to otherwise identical job applications is associated with a 50 percent lower likelihood of an interview request or job offer. Observational studies estimate earnings differentials between those with and without incarceration histories to be 10 to 40 percent (Geller et al. 2006; The Pew Charitable Trusts 2010). These differentials appear largest for those over age 30 and grow over time (Nagin and Waldfogel 1998). Among offenders, longer prison sentences are negatively related to labor market outcomes. Mueller-Smith (2014) exploits randomized judge assignments in Harris County, Texas and finds that longer sentences are associated with reductions in both employment and earnings.

Poor labor market prospects may also cause crime (Becker 1968). There is robust evidence of a positive relationship between local unemployment rates and crime (Raphael and Winter-Ebmer 2001; Gould et al. 2002; Machin and Meghir 2004; Levitt 2004; Öster and Agell 2007; Lin 2008), as well as a positive relationship between business cycle contractions and crime (Bushway, Cook, and Phillips 2010).

[3] Several studies show that employers may overestimate the magnitudes of these associations (Roberts et al. 2007; Blumstein and Nakamura 2009).
[4] Insuring against employee misconduct and malfeasance through "fidelity bonds" is often not possible if the employee has a criminal record (Stafford 2006).

Studies examining the effect of labor market opportunities on ex-offenders' recidivism rates are relatively rare. Using data on 1.7 million offenders released from California prisons from 1993 to 2008, Schnepel (2017) finds that construction and manufacturing job availability is negatively related to recidivism rates. Mears et al. (2015) finds that African American ex-offenders are more likely to engage in subsequent violent crime in response to higher African American unemployment rates, while white ex-offenders are more likely to reoffend in property crime in response to rising unemployment. The authors interpret this finding as evidence that African American ex-offenders may face elevated prejudice-driven frustration that manifests itself in "expressive criminal behavior" such as violent crime.

2.2 Prior Literature on BTB Laws

In response to difficulties ex-offenders face in securing gainful employment and the increase in recidivism that could result, half of all U.S. states have adopted "ban the box" laws, which seek to strategically withhold information on job applicants' criminal histories from prospective employers at first interview.[5] The first BTB law, adopted by Hawaii in 1998, applied to both public and private firms hiring new employees. Under the Hawaii statute, employers are not permitted to inquire into any job applicant's criminal history until a "conditional offer of employment" was made (National Employment Law Project 2017). Moreover, if a criminal conviction occurred within the previous decade (or if incarceration occurred at any point), the conditional employment offer can only be rescinded if it can be

[5]Other initiatives include reducing the breadth of crimes for which incarceration is a prescribed punishment, increasing human capital acquisition among those who are incarcerated (see, for example, Hall et al. 2016), improving employer contacts with inmates (Center for the Study of Social Policy 2012), reducing the number of jobs for which licensure is required (Hall et al. 2016), and juvenile justice reform.

shown that the conviction bears a “rational relationship” to the duties and responsibilities of the position.

Following the adoption of the Hawaii statute, 24 additional states and the District of Columbia — along with over 150 cities and counties — adopted a BTB law. Estimates suggest that approximately 211 million Americans, roughly two-third of the U.S. population, live in a jurisdiction covered by a BTB law (National Employment Law Project 2017). Nine states, the District of Columbia, and 15 localities have adopted BTB laws that apply to both private and public employers. Sixteen (16) states have adopted BTB laws that apply only to public employees or to private employees with government contracts.[6]

A handful of studies have examined the effects of BTB laws, almost all focusing on labor market outcomes.[7] Agan and Starr (2018) conduct a randomized control trial in which they randomly assign a criminal record to otherwise identical fictitious online job applications and send them to two sets of employers—those who previously included a box on their applications for criminal histories and those who did not—before and after BTB laws were enacted. Prior to the implementation of BTB laws, the authors find that whites who applied to affected employers in New York City and New Jersey were 7 percent more likely to receive callbacks for employment than African Americans. After the passage of BTB laws, the African American-white callback gap increased over six-fold to 45 percent. This suggests that BTB laws induce employers without information on applicants’ criminal histories to statistically discriminate against demographic groups with perceived higher rates of criminal behavior.[8]

[6] Still other BTB laws include broader “fair-chance” employment provisions (including Hawaii) that require employers to consider the job-relatedness of a conviction, mitigating circumstances, and evidence of rehabilitation (National Employment Law Project 2017).
[7] See also Mungan (2017) for a theoretical discussion.
[8] This finding is also consistent with prior work that found that increased availability of criminal records information is positively related to employment opportunities for low-skilled African American males (Holzer et al. 2006; Finlay, 2009; Stoll, 2009)

Doleac and Hansen (2017) reach a similar conclusion using quasi-experimental methods. Drawing CPS data from 2004 to 2014 and exploiting temporal variation across states, counties, and cities in the adoption of BTB laws, the authors find that BTB laws decrease the employment of less-educated (< high school degree) Hispanic men ages 25-to-64 by approximately 3 to 5 percent. Doleac and Hansen also find evidence of labor-labor substitution toward older and more educated individuals. While some younger African American men see a decline in employment in response to BTB laws, African Americans ages 35 and older experience an increase in employment, as do more highly educated (≥ college degree) African American women. This result suggests that employers may be discriminating in favor of demographic groups with a lower perceived propensity for crime. Along the same lines, Jackson, Sullivan and Zhao (2017) study a reform to the Massachusetts Criminal Offender Record Information (CORI), which included a BTB-style reform, and find that contrary to proponents' intentions, the reform reduced employment opportunities for ex-offenders.

Finally, using data from the 2007 to 2014 American Community Survey (ACS) and Origin-Destination Employment Statistics from the Longitudinal Employer-Household Dynamics series, Shoag and Vueger (2017) find that BTB laws are associated with a 4 percent increase in employment among African American men living in census tracts with high crime rates. They interpret this finding as evidence that BTB laws are effective in increasing employment opportunities for vulnerable individuals. Shoag and Vueger (2017) also find evidence of upskilling by African American men, as well as negative employment spillovers to African American women, who are much less likely to have criminal records than men.

Only one study of which we are aware has examined in the impact of BTB laws on crime. D'Alessio et al. (2015) study the 1998 Hawaii BTB legislation and find that following the passage of the law, the percent of all prosecuted defendants in Hawaii who were repeat offenders fell by 57 percent. However, because this study relies on a before-after estimator, it is unclear whether this estimate may be contaminated by time-varying factors.

3. Data

Given the inherent difficulties in measuring crime, a limitation not unique to our study, we utilize multiple datasets collected by different entities, in different settings, and for different purposes. Our analyses use administrative and survey data from three sources — the National Incident-Based Reporting System (NIBRS), the Uniform Crime Reports (UCR), and the National Longitudinal Survey of Youth 1997 (NLSY97) — to estimate the relationship between BTB laws and crime. Each of these datasets, which we briefly discuss below, offers distinct advantages designed to complement the others. We draw conclusions from the weight of the evidence across the separate analyses.

Our primary data source is the National Incident-Based Reporting System (NIBRS). We draw agency-by-month data from the NIBRS between 2004 and 2014. Local, state, and federal agencies generate and report information for the NIBRS to the FBI, based on administrative records of criminal incidents reported to these agencies. To ensure data quality, our main analysis sample consists of a balanced panel of agencies and months, though broader definitions of sample selection (e.g. agencies that reported in at least half the years covering the sample period or agencies serving counties of at least 20,000 population) produced a similar pattern of results. We generate counts of criminal incidents committed by male arrestees by age and measure total crimes, property crimes, and violent crimes, as well as specific subcategories of property and violent crimes.[9]

An important advantage of the NIBRS data is that we are able to measure crimes committed by demographic subgroups disaggregated by age, including working-age African American and Hispanic men. The most notable drawback of the NIBRS is its limited coverage across the United States. As of 2014, 37 states and the District of Columbia participated in the NIBRS, representing a coverage of roughly 93 million U.S. residents (FBI National Press Office 2015). However, by 2012, just 15 states were reporting all of their crime data through NIRBS (FBI 2012). Additionally, while the NIBRS data do include detailed information on criminal incidents (including characteristics of both the victim and arrestee), there are no data on education levels, which prevents an examination of racial minorities of lower skill levels. Moreover, there are no data on prior arrests, which do not allow us to disaggregate the impacts of BTB laws on recidivism versus first-time crime commission.

Table 1A shows means of agency-by-month criminal incidents involving male arrestees, by age and race/ethnicity. While average incident counts are higher for non-Hispanic white males relative to African American and Hispanic males, when these counts are adjusted for the respective sizes of age- and race/ethnicity-specific subpopulations, crime rates are 1.3 to 3.4 times higher for Hispanic and African Americans relative to non-Hispanic whites.[10] Across race/ethnicity, crime rates are also higher for younger as compared to older individuals.

[9] We also experiment with collecting information on offenders involved in incidents for which they are not arrested. The results are qualitatively similar.

[10] Given that NIBRS estimates are not representative of the US population (or any specific state), we present unweighted estimates. Weighting the regressions by county-specific population or agency-specific population served produces a similar pattern of results.

Because of the limited coverage of the NIBRS across the United States, we next turn to the Uniform Crime Reports (UCR), also over the period from 2004 to 2014. We measure county-by-month criminal arrest counts for adults ages 18 and older for African Americans and whites (inclusive of Hispanic whites). In contrast to the NIBRS, county-level UCR data cover roughly 98 percent of the US population in all 50 states and the District of Columbia (FBI 2015) and, when weighted, are representative of the U.S. population. Thus, use of the UCR allows additional identifying variation in BTB laws.

The chief disadvantage of the UCR is that these data do not disaggregate adult crimes by both gender and race/ethnicity. Given that BTB laws are likely to have smaller effects on women than men, estimated policy impacts may be muted. Moreover, the UCR does not identify Hispanics, a population for which BTB laws have been found to reduce employment (Doleac and Hansen 2017). Arrests by Hispanics and non-Hispanic whites are aggregated, which may be problematic if Hispanics and whites are differentially impacted by BTB laws. Table 1B presents weighted arrest counts (Panel I) and arrest rates (Panel II) for working-age adults in the UCR. The pattern of findings is consistent with the NIBRS, with higher relative crime rates for non-whites.

We supplement our administrative crime data with self-reported individual-level longitudinal data drawn from the NLSY97. The NLSY97, sponsored by the Bureau of Labor Statistics (BLS), consists of a national sample of youths who were 12-16 years of age as of December 31, 1996. We focus on young working-age adults over the period between 2004 and 2014, drawn from Rounds 7-16 of the NLSY97.

These data offer a number of distinct advantages. First, the data are nationally representative and allow us to measure not only criminal arrests, but also self-reported criminal behavior undetected by law enforcement. Second, the use of longitudinal data permits us to control for individual-level heterogeneity via estimation of individual fixed effects models. Third, the data contain information on the respondent's educational attainment, which allows us to assess whether any potential effects are concentrated among the least educated minorities, whose employment prospects may be most adversely impacted through statistical discrimination.

The NLSY97 also has some important disadvantages, including a limited sample of about 9,000 youth. Hence, there are relatively few Hispanic and African American men by county and survey wave, which is likely to lead to less precise estimates of policy impacts and also reduces statistical power among finer cuts of the sample. Furthermore, while survey data may pick up criminal behaviors not captured administratively, measurement error with self-reported criminal histories may also contribute to imprecision in the estimates.

Additionally, owing to the longitudinal cohort design, the age range in the NLSY97 does not perfectly coincide with the UCR and NIBRS analyses. During the analysis period from 2004 to 2014, NLSY respondents are between the ages of 19 and 34. Nevertheless, this age range is salient for analyzing the effects of BTB laws given that criminal activity typically peaks during the late teens to early 20s (Loeber and Farrington 2014) and young adults are also forming or have formed strong labor force attachment. One limitation of this cohort design is that restricting the age range by construction also restricts the analysis period; hence, differential effects across the age distribution may also reflect heterogeneity over time and/or differences due to a decrease in the identifying policy variation.

Each of our NLSY-based crime outcomes is dichotomous in nature: (i) *Arrest*, set equal to 1 if the respondent reported being arrested since their prior interview, and set equal to 0 otherwise[11], (ii) *Destroy Property*, set equal to 1 if the respondent "purposely damaged or destroyed property not belonging to [him/her]" and 0 otherwise, (iii) *Minor Theft*, set equal to 1 if the respondent stole something worth less than $50 and 0 otherwise, (iv) *Major Theft*, set equal to 1 if the respondent stole something worth more than $50 and 0 otherwise, (v) *Other Property Crime*, set equal to 1 if the respondent had possessed or received stolen property, or sold something for more than it was worth and 0 otherwise, and (vi) *Assault*, set equal to 1 if the respondent had attacked or assaulted someone. We also construct a composite measure of *Any Property Crime*, capturing criminal activities (iii) through (v).[12]

In Table 1C we present weighted means for the variables from the NLSY97 for our analysis sample. Consistent with the patterns noted in the NIBRS and the UCR, we find that arrest-rates are consistently highest among non-Hispanic African American males, followed by Hispanic males and then non-Hispanic white males. Furthermore, crime declines with age, with both arrests and participation in various criminal activities significantly higher among adults ages 19-to-26 compared with older adults ages 27-to-34. This is consistent with the age distribution of crime, such that criminal activity typically peaks between late adolescence into early adulthood and then declines (Loeber and Farrington 2014; Farrington 1986). Arrests and criminal engagement are also substantially lower for females.[13] Arrest rates derived from the

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[11] Respondents also provide data on the year and month of the arrest, though these are not always available for all respondents. The BLS makes available event history files for each respondent containing information on the number of arrests by year/month. Following Round 7, arrest dates are imputed based on the midpoint of the reference period since the date of the last interview (see www.nlsinfo.org). In alternate analyses, we utilized data from the arrest event history files to match the BTB policy data based on month/year of arrest. Our estimates remain robust. Since the remainder of the criminal activity measures are available only based on date of last interview, we present analyses for arrests using the same reference period for consistency and ease of comparison across models.
[12] Beginning in Round 8, questions regarding criminal behaviors (property crime, assault, etc.) were no longer asked of all respondents, but rather only those who had ever reported being arrested and a control group of approximately 10% of respondents (see: www.nlsyinfo.org). Wave fixed effects, included in all models, capture this change in the sampling frame, though our estimates remain robust to restricting the analyses to Round 8 onwards.
[13] The mean prevalence of being arrested among females in the NLSY97 ranged from 1.9 percent (Hispanics ages 19-34) to 2.1 percent (both non-Hispanic Whites and African Americans). In comparison (see Table 1C), arrest

NLSY97 are an order of magnitude lower than those derived from the UCR. This may reflect underreporting and measurement errors as well as the NLSY sample being representative of the non-institutionalized population and excluding those who are under detention or incarcerated.

IV. Methods

We begin by using agency-level month-specific crime data from the NIBRS from 2004 to 2014, focus on crimes committed by Hispanic and African American men, and estimate a Poisson model. Apart from adjusting for the skewness of the crime distribution and accounting for the discrete nature of the crime counts, the Poisson model can accommodate fixed effects well as it does not suffer from the "incidental parameters" problem.[14] Our specification takes the following form:

$$C_{jcst} = E_{jcst} \exp\left(\beta_0 + \beta_1 BTB_{cst} + \beta_2 \mathbf{X}_{cst} + \beta_3 \mathbf{Z}_{st} + \alpha_j + \tau_t + \varepsilon_{jcst}\right) \qquad (1)$$

where C_{jcst} is the crime count among males in agency *j* located in county *c* in state *s* at calendar time *t* (in months from 1 to 132) and BTB_{cst} is an indicator set equal to 1 if there is a BTB law in effect in county *c* at time *t* due to a state law, a county law, or a city BTB law.[15] The vector $\mathbf{X}_{cst}$ includes county-level controls, including the natural log of the population served by the reporting agency, the share of population that was African American and Hispanic, the average age of the

rates among males ages 19-34 were three to five times higher, ranging from 6.1 percent (non-Hispanic Whites) to 6.4 percent (Hispanics) to 10.1 percent (non-Hispanic African Americans).

[14] If, in equation (1), $\exp(\varepsilon_{jcst})$ follows a gamma distribution with mean of 1 and variance σ, then (1) represents a negative binomial model; if σ is assumed to equal 0, then the negative binomial becomes a poisson regression model (Grootendorst 2002). We have experimented with a negative binomial regression, with a similar pattern of results. In equation (1), exposure for each unit is represented by *E*, which can be proxied by the population served by the reporting agency.

[15] If the law were enacted mid-month, BTB_{cst} is set equal to 0 for that month and 1 thereafter. The results using the value of the share of month t that the law was in effect in county *c* are highly similar.

population, and the natural log of personal per capita income; and the vector $\mathbf{Z}_{st}$ is a vector of state-level observables, including the percent of the population ages 25 and over with a Bachelor's degree, the natural log of per-capita police expenditures, the natural log of per capita police employees, indicators for the presence of concealed carry permit laws, and the state minimum wage.[16] Finally, α_a is a vector of agency-level dummies and τ_t is a set of time-dummies measured at the month-level.

The marginal effect, $[\exp(\beta_1)-1] \times 100$ can be interpreted as the percent change in $E(C_{jcst})$ associated with a one-unit change in the ban-the-box law. In addition, we also experiment with interacting BTB_{cst} with an indicator for whether the BTB law applies to private employers or private employers with government contracts. This will allow us to disentangle the impact of a BTB law that applies to only public employers (the vast majority of laws) and those that extend to private employers.

For our UCR-based analysis, we estimate a Poisson model comparable to equation (1) using county-level data:

$$C_{cst} = E_{cst} \exp\left(\beta_0 + \beta_1 BTB_{cst} + \beta_2 \mathbf{X}_{cst} + \beta_3 \mathbf{Z}_{st} + \alpha_c + \tau_t + \varepsilon_{cst}\right) \qquad (2)$$

where α_c is a set of county dummies. In addition to the above controls, we add to the vector $\mathbf{X}_{cst}$ a measure for the number of reporting agencies per county-month and the natural log of the age-, gender-, and race/ethnicity-specific population.

[16] Means of the independent variables are presented in Appendix Table 1. County-level demographic controls are generated using population data from the National Cancer Institute's Surveillance Epidemiology and End Results (SEER) Program. The county-level personal per capita income data are collected from the Bureau of Economic Analysis. The share of the state population ages 25 and over with a Bachelor's degree is generated using data from the Current Population Survey Outgoing Rotation Groups; the state per-capita police expenditures and per capita police employees are collected from the Bureau of Justice Statistics; the concealed carry permit laws are collected from www.usacarry.com and the state minimum wages are collected from the Department of Labor. The personal per capita income, per-capita police expenditures and minimum wages are in current dollars.

Finally, we turn to self-reported longitudinal data from the NLSY97 to estimate an individual fixed effects model of the following form via least squares:

$$C_{icst} = \beta_0 + \beta_1 BTB_{cst} + \beta_2 \mathbf{X}_{icst} + \beta_3 \mathbf{P}_{st} + \alpha_c + \nu_i + \tau_t + \varepsilon_{ist} \tag{3}$$

where C_{icst} measures the criminal activity of individual *i* residing in county *c* in state *s* in year *t*, BTB_{cst} is an indicator for whether there is a BTB law in effect in the respondent's county, $\mathbf{X}_{icst}$ is a set of individual observables comparable to those described above. While we estimate these models with fixed effects at the level of the policy variation (county of residence; α_c) for comparison with the NIBRS and the UCR analyses, in extended specifications, we also fully exploit the longitudinal nature of the data and alternately include person fixed effects, ν_i. This captures all observed and unobserved time-invariant heterogeneity across individuals, including joint fixed determinants of criminal and labor market activities. As discussed above, the use of individual data will allow us to explore heterogeneity in crime effects of BTB laws by race and other factors, most notably educational attainment.

Identification of β_1 in equations (1) through (3) comes from state, city, and county-specific changes in BTB laws. Over the period from 2004 to 2014, 12 states, 78 cities, and 21 counties enacted BTB laws. Twenty (20) of these laws bind for both public and private employers while the vast majority only bind for public employers. Appendix Table 2 shows each of the laws adopted over the 2004 to 2014 period as well as the sources of identifying variation across each of our three datasets. In the NIBRS, 59 jurisdictions contribute to identifying variation, in the UCR 86, and in the NLSY97 74.

The credibility of our identification strategy rests on the parallel trends assumption. We take a number of tacks to test this assumption. First, we examine how crime was trending before and after the adoption of BTB laws in "treatment" versus "comparison" jurisdictions using an event study analysis. If BTB laws are implemented exogenously to crime commission by non-white men, we would expect crime in the years leading up to the adoption of BTB laws in treatment jurisdictions to be no different from crime in comparison jurisdictions. Moreover, because the impact of BTB laws may take time to unfold, we generate a set of mutually exclusive BTB indicators for (i) each of the two years leading to the adoption of a BTB law, (ii) the year of the BTB law, (iii) the year following the law's adoption, and (iv) two years or more following the effective date of the BTB law.

As a second test of the parallel trends assumption, we estimate falsification-type tests by focusing on populations whose criminal behavior should be less or differently affected by BTB laws. Specifically, we examine the impact of BTB laws on crime of non-Hispanic whites and females. While these demographic groups might be affected by BTB laws via labor-labor substitution by employers and could be impacted through moral hazard-related channels, the effects of BTB laws on crime rates of this group are expected to be smaller or of the opposite sign.

IV. Results

Tables 2 through 9 show our main results. We focus on estimates of β_1 in these tables, though coefficient estimates on the controls are available upon request. Standard errors are clustered at the state-level (Bertrand et al. 2004; Doleac and Hansen 2017).

4.1 NIBRS Main Results

Panel I of Table 2 shows our main Poisson regression results from the NIBRS. We find that BTB laws are associated with a 7.1 percent [$e^{(0.069)} - 1$] increase in the number of total criminal incidents (property plus violent crimes) involving Hispanic men (column 1, row 1). This effect appears to be driven by property crimes (row 2), where we find that BTB laws are associated with an 11.7 percent increase in such incidents. Looking across the age distribution (columns 2 through 4), we find that the adoption of BTB laws are associated with a (statistically insignificant) 5.1 percent increase in property crimes for Hispanic males under age 25, a 16.5 percent increase in property crimes for those ages 25-to-34 and a 17.5 percent increase in property crimes for those ages 35-to-64. These results are consistent with the hypothesis that BTB laws may induce statistical discrimination by potential employers (Agan and Starr 2016; Doleac and Hansen 2017) as well as potential moral hazard. We find little evidence that BTB laws increase violent crimes. Estimated effects are uniformly smaller for violent as compared to property crimes (row 3).

Turning to African American men (columns 5 through 8), we find little evidence of BTB-induced increases in property crimes. In fact, coefficients are negative, though they are not significantly different from zero at conventional levels. For older African American men, this could be due to labor-labor substitution, a result found by Doleac and Hansen (2017). As with Hispanics, we find little evidence that BTB laws affect violent crime among African American males.

In Panel II of Table 2, we restricted the sample to those agencies reporting at least one crime per period, essentially focusing on locales with relatively higher shares of each demographic group. The pattern of results in columns (1) through (8) is similar to Panel I, though in Panel II we find stronger evidence that BTB laws reduce property crime among older Black men, consistent with the labor-labor substitution finding of Doleac and Hansen (2017).

In the remaining columns of Table 2 (columns 9 through 12), we examine non-Hispanic white males, a population for whom BTB laws are expected to have much smaller effects. Consistent with this hypothesis, we find no evidence that BTB laws increase property or violent crimes for this racial group. Estimates of β_1 are uniformly statistically insignificant at conventional levels, and the magnitudes of β_1 in property crime regressions are, in three of four cases, much smaller than the estimates obtained for Hispanic males.[17,18]

In Table 3, we examine whether the crime effects of BTB laws differ by the breadth of their coverage. Consistent with Doleac and Hansen (2017), the findings in Panel II show that the property crime effect for Hispanic men is driven by BTB laws applying to public employers. For violent crime, we find that adding private firms is associated with increases in violent crimes for younger Hispanic males. This result could be explained by more widespread statistical discrimination, generating more expressive/emotional crimes. For African Americans and non-Hispanic whites, we find no evidence of increases in crime following the enactment of either public or private BTB statutes.

Together, the findings in Tables 2 and 3 suggest that statistical discrimination against Hispanic males (and perhaps toward older African Americans) may be an important mechanism to explain the crime effects of BTB laws, consistent with recent scholarship on BTB laws. Our findings might also partly reflect moral hazard. Empirical evidence in other contexts, notably

[17] We find a similar pattern of results when using a larger sample of agencies reporting more than five years during the sample period between 2004 to 2014 (see Appendix Table 3A).

[18] In Appendix Table 3B, we examine the sensitivity of estimates in Panel II of Table 2 to estimating ordinary least squares (OLS) models with the dependent variable redefined as the natural log of the crime rate per 1,000 population. Our findings suggest a qualitatively similar pattern of results.

with respect to automobile insurance (Cohen and Dehejia 2004), workers' compensation (Fortin and Lanoie 2000), and health insurance (Dave and Kaestner 2009; Dave et al. 2015), is generally supportive of moral hazard effects. Similarly, Bamberger and Donohue (1999) find that workplace discipline practices involving "last chance agreements," which govern reinstatement of discharged employees and reduce the cost to the employee of wrongdoing, can lead to more wrongdoing and discharges. Hence, BTB laws may also conceivably generate such moral hazard by indirectly reducing the negative consequences of criminal engagement.

4.2 Placebo Tests

Next, we explore trends in crime before and after the enactment of BTB laws. Figures 1 and 2 report results of an event-study analysis showing 95 percent confidence intervals of crime trends up to three years before the enactment of a BTB law and up to three or more years after the law's adoption.[19] We find no evidence that property crime (Figure 1) or violent crime (Figure 2) crimes were trending differently prior to the enactment of BTB laws for any age group (Figure 1). For Hispanic men ages 25 and older, we find that property crimes increase one and two years after the adoption of a BTB law, a pattern of findings consistent with a causal impact of BTB laws. For older African American and non-Hispanic whites, we find noisy declines in property crime following BTB enactment.

Tables 4A through 4C present coefficients on two years of leads and lags of BTB laws after controlling for the full set of observable state- and county-specific time-varying controls.

[19] Specifically, we estimate a Poisson model where the dependent variable is the counts of crime and the right hand-side variables include a set of dummies for each agency, a set of times for each time period (months from 1 through 132), the natural log of the population in the jurisdiction served by the agency, and indicators for three, two, and one year prior of the adoption of a BTB law, the year of the BTB law change, and one, two, and three or more years following the adoption of a BTB law.

The pattern of results is consistent with the event-study analysis, suggesting that BTB laws are associated with increases in property crimes for Hispanic males. A test of the joint significance of the sum of lagged BTB law effects on property crimes for Hispanic males suggests a longer-run impact that is statistically different from zero at the 10 percent level of significance. For African American and white males, there is little evidence that BTB laws are associated with increases in property or violent crimes.

In Table 5, we explore whether the crime effects of BTB laws extend to female racial minorities, a population for whom there is less evidence of statistical discrimination (Doleac and Hansen 2017), but for whom moral hazard or labor-labor substitution is still possible. In the main, we find weaker evidence that BTB laws increase crime for Hispanic (columns 1 through 4), African American (columns 5 through 8), or non-Hispanic white (columns 9 through 12) females. Unlike for Hispanic men, the estimated impacts are often small in magnitudes and uniformly statistically indistinguishable from zero. These placebo-type tests add a degree of confidence to a causal interpretation of our findings in Table 2.[20]

Together, the results presented thus far suggest that BTB laws may have the unintended consequences of increasing property crimes among Hispanic males. Table 6 examines which specific crimes are driving these findings. For Hispanic males, BTB laws are associated with a 6.3 to 19.7 percent increase in larcenies. We also find that BTB laws are associated with an 11.0 to 23.2 percent increase in weapons law violations, which may have occurred during the commission of property crimes. For African American and non-Hispanic white males, we find

[20] In addition, we explore the robustness of findings to the inclusion of state-specific linear time trends. While less precisely estimated, the results continue to point to positive effects of BTB laws on property crime for Hispanic men ages 25-to-34 and a decline in property crime for older African American men.

little evidence of changes in specific crimes in response to BTB laws, though notably the effects on larceny and stolen property are negative for African American men.[21]

4.3 UCR and NLSY97 Results

As noted above, one of the limitations of the NIBRS is its limited coverage. We turn to the UCR to exploit greater identifying variation in BTB laws as well as greater external validity. However, we are unable to disaggregate crime counts by gender-race/ethnicity-age to mirror the analysis in the NIBRS. To the extent that women, who are less likely to be impacted by BTB laws, are included, estimated policy impacts will be smaller in magnitude. Moreover, white adults may not serve as a credible control to the extent that Hispanics are included in their crime counts.

We find no evidence that BTB laws are associated with increases in overall property or violent crime among African American adults (Table 7A, column 1). For property crimes, estimated effects are generally negative, though never significantly different from zero. Only for aggravated assaults is there some (marginal) evidence for positive crime effects among African Americans. For white adults (Table 7A, column 2), analysis from the UCR uncovers some evidence of BTB law-induced declines in property crime. This finding is consistent with labor-labor substitution toward whites. An examination of public BTB laws versus BTB laws that extend to private employers (Table 7B) provides some support for the hypothesis that crime falls more among white adults when the BTB law extends to private employers.[22]

[21] The estimated crime effects of BTB laws are robust to the controls of state-specific unemployment rates (see Appendix Table 4).

[22] Data collection and reporting practices in the NIBRS and UCR vary across agencies (Anderson 2014; Barnett-Ryan and Swanson 2008; Gould et al. 2002). In addition, under-reporting is more prevalent in smaller jurisdictions than larger ones (Lynch and Jarvis 2008). We experimented with restricting the sample to (i) those agency-months where the agency-specific count was no more than two standard deviations from the agency-specific mean and (ii) those agencies serving larger than 25,000 population. The findings were qualitatively similar to those reported.

Finally, we analyze data from the NLSY97. Table 8A reports estimates of the effects of BTB laws on arrests for Hispanic, African American, and white males.[23] In line with the previous results, we find that among Hispanic males (Panel I), BTB laws are associated with an increase in the probability of being arrested, on the order of about one to two percentage points (about 13-to-25 percent relative to the baseline mean), though these estimates are very imprecise for the broader age group of 19-to-34. We do not find any significant effects for African American or white males.

Since criminal activity declines with age, in Panel II, we restrict our attention to those aged 19-to-26, an age group for which the prevalence of criminal behaviors is relatively high. For the NLSY97 cohort, the prevalence of arrest for 19-to-26 year-olds is significantly higher relative to older males 27-to-34 years of age (7.3% vs. 4.5% for Hispanics; 11.3% vs. 7.7% for African Americans; and 6.9% vs. 4.3% for whites). Models in Panel II of Table 8A show a consistent and significant increase in the probability of an arrest for Hispanic males under age 27; there is little effect for those ages 27 and older (Panel III). Estimates are fairly robust across all three specifications. Again, we do not find any significant effects for younger African American men. Importantly, estimates for younger white males are close to zero and do not suggest any meaningful shifts in arrests associated with the BTB laws, suggesting that statistical discrimination in employment may be an important mechanism for Hispanic males.

To further probe whether BTB laws are affecting certain types of crime, in Table 8B, we estimate the effects of these laws on active criminal participation based on reported engagement

[23] Model 1 includes fixed effects at the level of the policy variation (county of residence) and Model 2 alternately includes person-level fixed effects. Model 3 includes both county and person fixed effects, which is possible as a subset of individuals have relocated to a different county of residence over the sample period. Hence, the latter controls account for both spatial and individual (time-invariant) heterogeneity, including any fixed factors that affect locational choices and sorting.

in various criminal behaviors. We specifically consider effects on economically motivated criminal engagement, a broad measure of "property crime", and then consider specific components (minor theft, major theft, other property crime, destroy property) underlying this measure available in the NLSY97. We qualify our discussion by noting that these estimates are highly imprecise due to the drop in sample size by more than half compared with the analyses for arrests.[24] Among Hispanic males, there is a suggestive increase in property crime on the order of about 1.5 (ages 19-to-34) to 1.8 (ages 19-to-26) percentage points, approximately an 18 percent increase relative to the sample mean.

This overall increase masks some heterogeneity across specific types of property crime. Most notably, we find that BTB laws may have reduced minor theft, especially among younger Hispanic males (by about 1.6 percentage-points), but increased major theft (defined in the NLSY97 as stealing anything valued at $50 or greater), thus shifting criminal behaviors to more intense property crime. Effects on destroying property are close to zero in magnitude. Hence, the net increase in property crime for Hispanic males is reflective of a strong increase in major theft and in other forms of property crime, which more than compensate for any decline in minor theft. We do not find any consistent or significant patterns for African American males.

Given that BTB laws affect the labor market prospects of individuals with a propensity to commit crime, we would expect first-order effects, if any, on crimes with an economic motivation. However, for reasons noted earlier, there may also be spillover or second-order effects on violent crime. When we turn to assaults, we find a small suggestive increase of about 1.7 percentage-points among younger Hispanic males.

[24] As noted above, this is due to the change beginning in Round 8 in the universe of respondents who report on their criminal behaviors. We therefore report models with county fixed effects to conserve degrees of freedom. Estimates with person fixed effects or person plus county fixed effects are similar, though standard errors inflate further.

There is less evidence of statistical discrimination among female minorities in the literature (Doleac and Hansen 2017), consistent with gender-based crime patterns. Females are far less likely to commit crimes, and racial/ethnic differences in criminal behavior is substantially lower among females. It is validating that estimates reported in Appendix Table 5 generally confirm this prediction across all measures of criminal behavior.

Finally, owing to the cohort design of the NLSY97, most of the sample is low-educated and many have not completed their education. For instance, among Hispanic males ages 19-to-34, only eight (8) percent of the analysis sample have attained at least some college education, and over 22 percent have less than a high school education. In unreported results available upon request, we assessed whether effects of the BTB laws are higher among the least-educated males by including an interaction between the law and an indicator for less than a high school education.[25] One might expect larger crime effects for less educated individuals for whom Doleac and Hansen (2017) found BTB law-induced adverse employment effects were concentrated.

For arrests, the broader measure of property crime, as well as minor and major theft, we find significantly larger and positive effects for low-educated Hispanic males. In fact, for property crime, it appears that the effect is driven by less-than-high school educated males, consistent with the hypothesis that statistical discrimination may be an important mechanism.[26,27]

[25] We use a very strict definition of low-educated, having attained less than a high school degree, since the sample is ages 19 and above. Hence, virtually all individuals in this age range should have completed high school.
[26] We test for differential effects across educational attainment through an interaction term and by alternately stratifying the sample based on high school completion. These specifications suggest that BTB laws are associated with a statistically significant 4 percentage points increase in arrests among Hispanic males (ages 19-34) with less than a high school education; for higher educated Hispanic males, the effect is insignificant and essentially nil (coefficient of -0.007). Among younger Hispanics (ages 18-26), less-than-high school educated males are about 2 to 9 percentage points more likely to be arrested relative to higher educated males, as a result of the BTB laws. The effects of BTB laws on property crime, minor theft, and major theft produce a qualitatively similar pattern of results.
[27] We also conducted analyses on employment outcomes from the NLSY97. For younger African American males, we find that the BTB laws are associated with about a 6 percentage point decline in private sector employment (p-

V. Conclusions

Recent studies suggest that there may be unintended consequences of BTB laws, including statistical discrimination in employment against racial minorities. This study is the first to examine the effect of BTB laws on crime using data from three national data sources, the NIBRS, UCR and NLSY97. We find that BTB laws are associated with about a 10 to 20 percent increase in crime among working-age Hispanic men, driven primarily by property crimes. This result is consistent with economically motivated crimes due to statistical discrimination-driven diminished employment opportunities and, perhaps, moral hazard. For African American and non-Hispanic white men, we find no evidence that BTB laws increase crime. In fact, for some older African American and white men, we find that BTB laws are associated with a decline in property crime. This result is consistent with labor-labor substitution toward those with observable characteristics (e.g. older age and white) with lower perceived propensities for criminal records. These findings pass placebo tests on leads of BTB laws and are smaller for women.

Together, these findings are consistent with decreased employment prospects for some racial minorities possibly due to statistical discrimination. They may also partly reflect potential moral hazard (Bamberger and Donohue 1999), whereby BTB laws reduce the cost of crime commission. The magnitudes of our effects suggest that BTB laws are associated with an important unintended consequence that may generate significant social costs. Estimates of the

value = 0.19) but no decline in any employment, suggestive of a shift in the composition of jobs. Some estimates are also suggestive of an increase in the number of jobs worked, possibly reflecting (in conjunction with the decline in private sector employment) a shift from the formal to the informal sector. For younger Hispanic males, there are no significant or strong effects on employment at the extensive margin, though estimates are suggestive of about a 6-7% decrease in the number of weeks worked [p=value = 0.24] among those employed. For whites we do not find any strong effects on employment at the extensive margin but a significant 11% increase in weeks worked at the intensive margin.

per-offense total cost of property crime is approximately $5,263 in 2016 dollars (McCollister et al. 2010).[28] Based on NLSY97 estimates of increases in crime among younger Hispanic males (Panel II column 3), we obtain back-of-the-envelope BTB-induced additional crime costs (for 19-to-26 year-olds of $1.1 billion at the upper bound. If crime among non-Hispanic whites ages 27-to-34 falls, on average about 0.9 to 2.1 percentage-points (Panel III, columns 7 through 9), the net crime costs fall to $322 million. The findings from this study should be interpreted as suggestive, but they appear to add to some emerging evidence that BTB laws, while well-intentioned, may adversely impact certain individuals that they are intended to benefit by further perpetuating the cycle of criminality.

[28] According to Uniform Crime Report data from 2010, approximately 24 percent of property crime was burglary, 68 percent was larceny/theft, and 8 percent was motor vehicle theft. Using these breakdowns and the total per-crime costs in Table 5 of McCollister et al. (2010), we obtain total cost per property crime offense of about $4,721 in 2008 dollars or $5,263 in 2016 dollars. See also Miller et al. (1996).

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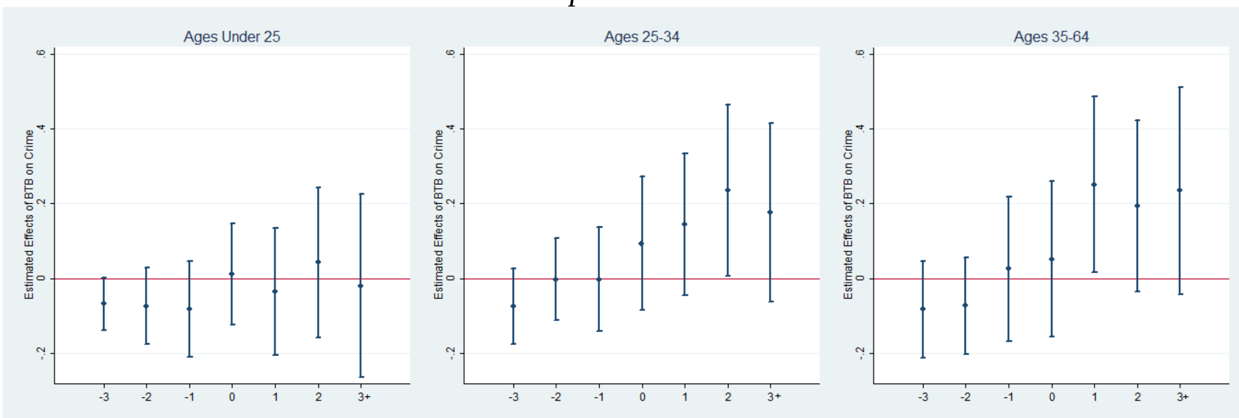
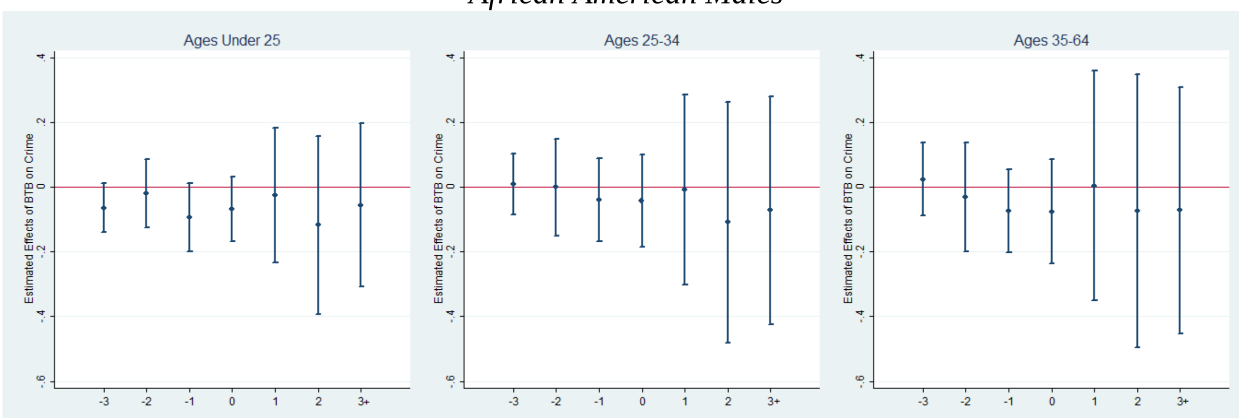
Figure 1. Event Study for Property Crime, NIBRS, 2004-2014

Hispanic Males



African American Males



Non-Hispanic White Males

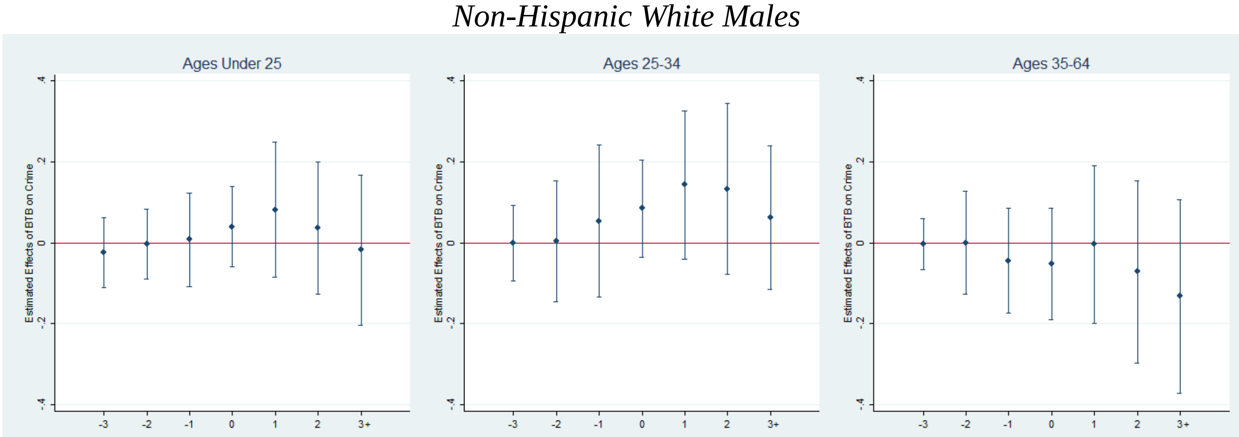
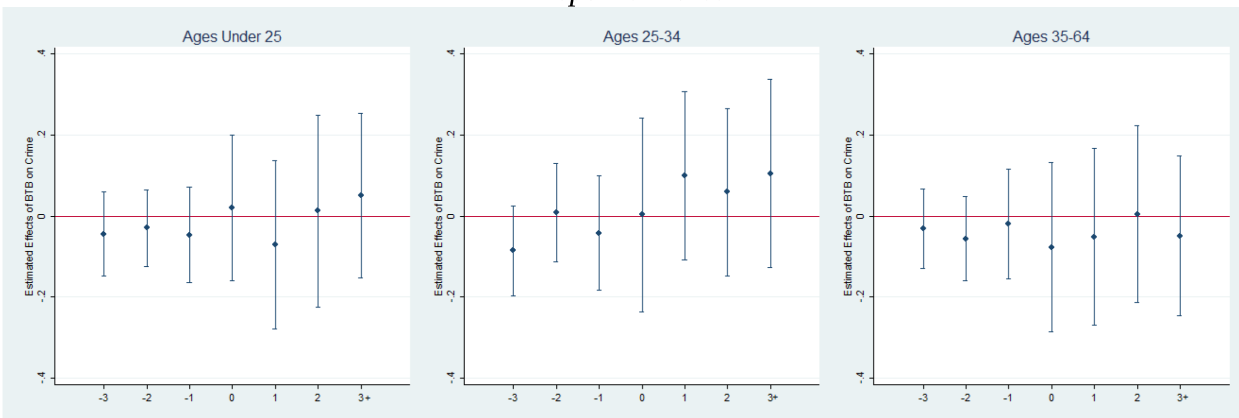
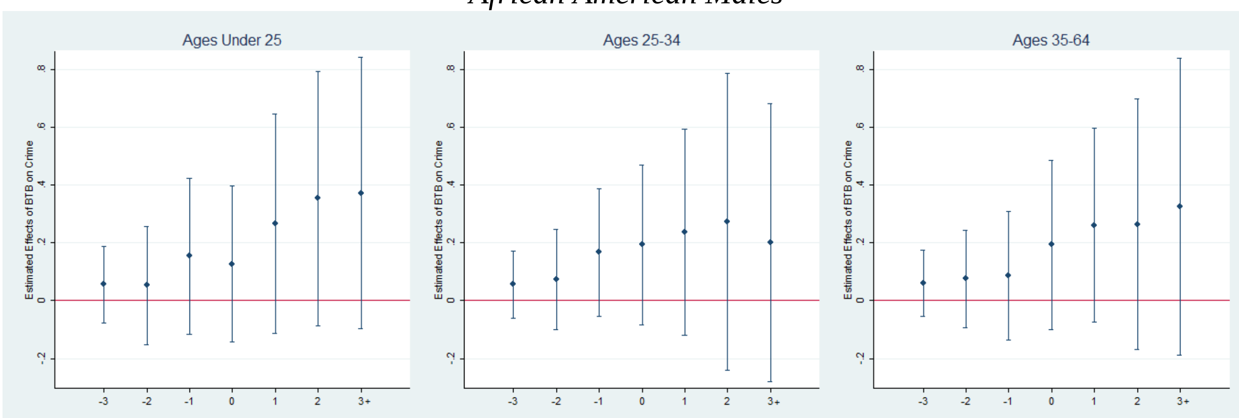


Figure 2. Event Study for Violent Crime, NIBRS, 2004-2014

Hispanic Males



African American Males



Non-Hispanic White Males

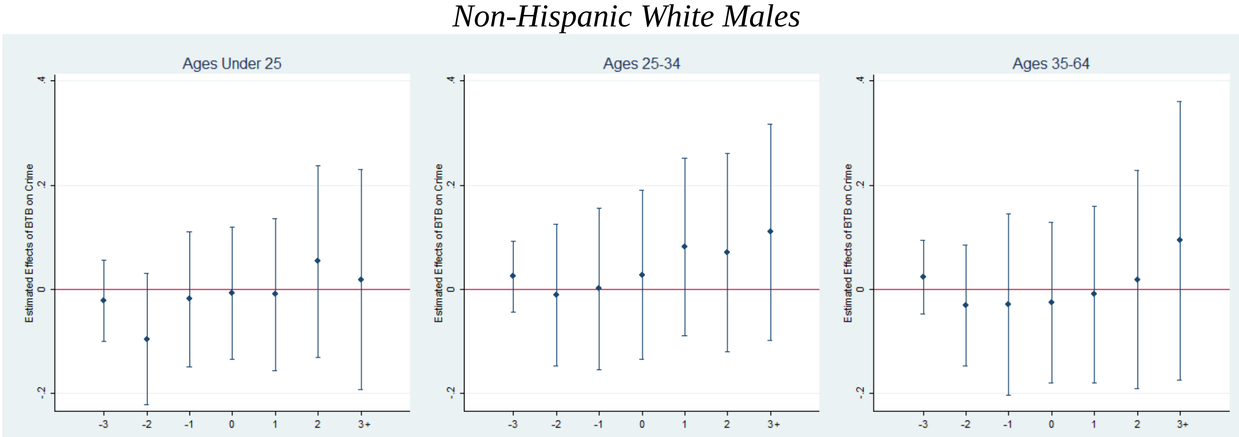


Table 1A. Means of Agency-by-Month Crime Counts, NIBRS, 2004-2014

	Hispanic Men				***African American Men***				***White Men***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Total crime	0.632	0.307	0.219	0.148	2.261	1.003	0.591	0.799	3.996	1.660	1.323	1.275
	(2.775)	(1.399)	(1.041)	(0.790)	(12.076)	(5.466)	(3.226)	(4.520)	(9.082)	(3.817)	(3.376)	(3.274)
Property crime	0.439	0.224	0.142	0.100	1.525	0.677	0.359	0.568	3.073	1.364	1.017	0.898
	(2.011)	(1.075)	(0.731)	(0.587)	(8.040)	(3.518)	(1.993)	(3.368)	(7.295)	(3.274)	(2.779)	(2.508)
Violent crime	0.202	0.087	0.082	0.050	0.774	0.343	0.246	0.242	0.965	0.311	0.322	0.391
	(1.031)	(0.503)	(0.466)	(0.327)	(4.636)	(2.290)	(1.489)	(1.422)	(2.409)	(0.887)	(0.947)	(1.124)
Larceny-theft	0.336	0.168	0.107	0.081	1.063	0.435	0.235	0.440	2.293	1.008	0.742	0.684
	(1.612)	(0.852)	(0.585)	(0.505)	(5.398)	(2.121)	(1.290)	(2.598)	(5.698)	(2.587)	(2.134)	(1.992)
Motor vehicle theft	0.030	0.018	0.010	0.005	0.119	0.061	0.032	0.033	0.201	0.086	0.071	0.058
	(0.237)	(0.167)	(0.115)	(0.072)	(0.886)	(0.535)	(0.286)	(0.284)	(0.706)	(0.376)	(0.354)	(0.299)
Burglary	0.079	0.043	0.027	0.015	0.364	0.194	0.097	0.100	0.641	0.302	0.229	0.169
	(0.466)	(0.300)	(0.218)	(0.153)	(2.265)	(1.320)	(0.659)	(0.727)	(1.825)	(0.988)	(0.879)	(0.662)
Robbery	0.029	0.017	0.010	0.005	0.207	0.132	0.055	0.040	0.111	0.049	0.042	0.030
	(0.258)	(0.177)	(0.123)	(0.079)	(1.655)	(1.174)	(0.473)	(0.330)	(0.498)	(0.275)	(0.263)	(0.210)
Aggravated assault	0.154	0.062	0.064	0.040	0.516	0.190	0.173	0.186	0.791	0.238	0.262	0.337
	(0.799)	(0.377)	(0.382)	(0.275)	(2.857)	(1.114)	(1.017)	(1.098)	(2.001)	(0.716)	(0.793)	(0.993)
Murder	0.003	0.002	0.001	0.001	0.023	0.013	0.009	0.005	0.013	0.004	0.004	0.006
	(0.062)	(0.046)	(0.036)	(0.027)	(0.235)	(0.150)	(0.119)	(0.076)	(0.118)	(0.064)	(0.067)	(0.077)
Arson	0.003	0.001	0.001	0.001	0.009	0.004	0.003	0.003	0.027	0.013	0.007	0.009
	(0.053)	(0.040)	(0.028)	(0.023)	(0.111)	(0.067)	(0.056)	(0.059)	(0.193)	(0.129)	(0.094)	(0.104)
Stolen property	0.036	0.019	0.013	0.007	0.117	0.060	0.034	0.032	0.177	0.071	0.064	0.057
	(0.298)	(0.191)	(0.138)	(0.097)	(0.804)	(0.536)	(0.252)	(0.255)	(0.684)	(0.336)	(0.342)	(0.314)
Weapon law violation	0.087	0.050	0.031	0.014	0.449	0.250	0.163	0.084	0.412	0.171	0.130	0.140
	(0.524)	(0.344)	(0.234)	(0.138)	(3.196)	(1.821)	(1.253)	(0.645)	(1.236)	(0.627)	(0.508)	(0.511)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

Notes: Means of crime counts are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Standard deviations are in parentheses.

Table 1B. Means of County-by-Month Criminal Arrests, UCR, 2004-2014

	African American Adults	***White Adults***
	(1)	(2)
	Panel I: Arrest Counts	
Total crime	220.96 (709.55)	312.64 (713.26)
Property crime	145.32 (519.75)	207.41 (423.40)
Violent crime	75.640 (203.25)	105.23 (307.29)
Larceny-theft	102.13 (361.88)	143.18 (269.37)
Motor vehicle theft	16.128 (106.10)	19.249 (63.091)
Burglary	26.405 (66.031)	43.948 (113.61)
Robbery	20.886 (62.603)	16.184 (46.978)
Aggravated assault	49.746 (123.986)	84.279 (250.04)
Murder	2.5995 (11.428)	1.9363 (6.1776)
Arson	0.6663 (2.1203)	1.0361 (4.3698)
Stolen property	9.5227 (20.817)	16.687 (41.303)
Weapon law violation	26.405 (66.031)	43.948 (113.31)
	Panel II: Arrest Rates per 100,000 county population	
Total crime	141.52 (449.49)	47.783 (125.77)
Property crime	95.041 (283.09)	35.651 (111.26)
Violent crime	46.483 (205.77)	11.841 (56.017)
Larceny-theft	71.653 (238.08)	27.286 (110.22)
Motor vehicle theft	6.1253 (35.897)	1.9505 (3.0721)
Burglary	16.792 (41.018)	6.1922 (6.8926)
Robbery	11.818 (70.299)	1.6088 (2.4280)
Aggravated assault	31.670 (121.51)	9.5219 (55.642)
Murder	1.4474 (8.1300)	0.2174 (0.7349)
Arson	0.4701 (4.1257)	0.2227 (1.1243)
Stolen property	7.7946 (15.977)	2.4792 (48.467)
Weapon law violation	16.792 (41.018)	6.1922 (6.8926)
N	369,438	369,438

Notes: Weighted means are generated using county-level data drawn from the 2004 to 2014 Uniform Crime Reports data. Standard deviations are in parentheses.

Table 1C. Crime Rates and Selected Characteristics, NLSY97, 1997-2014

Sample	Hispanic Males			Non-Hispanic African American Males			Non-Hispanic White Males		
	Ages 19-34	Ages 19-26	Ages 27-34	Ages 19-34	Ages 19-26	Ages 27-34	Ages 19-34	Ages 19-26	Ages 27-34
Arrested	0.064	0.073	0.045	0.101	0.113	0.077	0.061	0.069	0.043
	(0.245)	(0.261)	(0.207)	(0.301)	(0.316)	(0.266)	(0.238)	(0.254)	(0.202)
Any property crime	0.082	0.097	0.039	0.075	0.091	0.034	0.077	0.089	0.040
	(0.275)	(0.297)	(0.193)	(0.264)	(0.287)	(0.181)	(0.267)	(0.285)	(0.195)
Steal < $50	0.043	0.051	0.022	0.033	0.040	0.016	0.040	0.045	0.025
	(0.204)	(0.220)	(0.148)	(0.180)	(0.196)	(0.125)	(0.197)	(0.207)	(0.157)
Steal ≥ $50	0.026	0.030	0.015	0.021	0.025	0.010	0.019	0.021	0.012
	(0.160)	(0.171)	(0.121)	(0.144)	(0.157)	(0.100)	(0.135)	(0.142)	(0.109)
Destroy property	0.039	0.046	0.014	0.036	0.043	0.014	0.041	0.047	0.014
	(0.194)	(0.209)	(0.118)	(0.188)	(0.202)	(0.117)	(0.198)	(0.211)	(0.118)
Other property crime	0.025	0.029	0.010	0.025	0.030	0.008	0.017	0.020	0.005
	(0.156)	(0.167)	(0.100)	(0.157)	(0.171)	(0.091)	(0.131)	(0.141)	(0.072)
Assault	0.080	0.088	0.049	0.079	0.086	0.053	0.058	0.066	0.022
	(0.271)	(0.283)	(0.216)	(0.270)	(0.281)	(0.223)	(0.234)	(0.249)	(0.147)
Age	24.658	22.526	29.035	24.635	22.527	29.018	24.621	22.515	29.001
	(3.723)	(2.284)	(1.765)	(3.713)	(2.288)	(1.764)	(3.708)	(2.284)	(1.757)
Married	0.217	0.153	0.346	0.126	0.068	0.246	0.236	0.152	0.410
	(0.412)	(0.360)	(0.476)	(0.332)	(0.252)	(0.431)	(0.425)	(0.360)	(0.492)
Less than high school	0.186	0.207	0.144	0.205	0.237	0.140	0.096	0.106	0.074
	(0.389)	(0.405)	(0.352)	(0.404)	(0.425)	(0.347)	(0.295)	(0.308)	(0.262)
High School	0.699	0.717	0.663	0.693	0.695	0.689	0.676	0.730	0.564
	(0.459)	(0.451)	(0.473)	(0.461)	(0.460)	(0.463)	(0.468)	(0.444)	(0.496)
Some College	0.040	0.029	0.062	0.030	0.025	0.041	0.047	0.039	0.064
	(0.196)	(0.168)	(0.241)	(0.171)	(0.156)	(0.197)	(0.211)	(0.193)	(0.244)
College	0.075	0.048	0.130	0.071	0.043	0.131	0.181	0.125	0.298
	(0.263)	(0.213)	(0.337)	(0.257)	(0.202)	(0.337)	(0.385)	(0.331)	(0.457)
Observations	*9367*	*6374*	*2993*	*11555*	*7801*	*3754*	*21898*	*15035*	*6863*

Notes: Weighted means are reported from Rounds 1-16 (1997-98 through 2013-14) of the NLYS97. Standard deviations are in parentheses. Observations reported represent the maximum sample size. Sample size is smaller for some variables due to missing information (see text).

Table 2. Poisson Estimates of Relationship Between Ban-the-Box Laws and Crime, NIBRS, 2004-2014

	Hispanic Men				***African American Men***				***White Men***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel I: Full Panel												
Total crime	0.069	0.028	0.111**	0.086	0.003	0.013	-0.011	0.007	0.007	0.036	0.036	-0.061
	(0.048)	(0.048)	(0.056)	(0.056)	(0.118)	(0.099)	(0.122)	(0.135)	(0.069)	(0.057)	(0.080)	(0.078)
Property crime	0.111**	0.050	0.153**	0.161**	-0.041	-0.019	-0.058	-0.043	0.025	0.054	0.059	-0.063
	(0.052)	(0.051)	(0.062)	(0.064)	(0.125)	(0.100)	(0.130)	(0.142)	(0.079)	(0.062)	(0.092)	(0.095)
Violent crime	-0.005	0.003	0.049	-0.061	0.082	0.076	0.068	0.115	-0.019	-0.001	0.003	-0.034
	(0.065)	(0.073)	(0.067)	(0.066)	(0.112)	(0.107)	(0.119)	(0.117)	(0.052)	(0.056)	(0.054)	(0.056)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*
Panel II: Agencies with Positive Crimes												
Total crime	0.045	-0.004	0.062	0.062	-0.058	-0.047	-0.055	-0.072	-0.048	-0.017	-0.020	-0.089*
	(0.039)	(0.039)	(0.042)	(0.046)	(0.056)	(0.034)	(0.069)	(0.061)	(0.045)	(0.036)	(0.055)	(0.048)
N	*44,398*	*28,695*	*24,287*	*18,074*	*73,904*	*51,757*	*40,964*	*45,785*	*148,078*	*109,460*	*97,694*	*97,532*
Property crime	0.093**	0.025	0.083**	0.097**	-0.106*	-0.065	-0.093	-0.136***	-0.038	0.005	-0.007	-0.096
	(0.041)	(0.043)	(0.042)	(0.042)	(0.058)	(0.040)	(0.066)	(0.052)	(0.056)	(0.041)	(0.071)	(0.060)
N	*35,403*	*23,407*	*17,797*	*13,306*	*62,697*	*42,979*	*31,146*	*37,539*	*133,113*	*98,232*	*83,324*	*78,664*
Violent crime	-0.041	-0.059	-0.003	0.002	0.019	-0.020	0.016	0.053	-0.050**	-0.047**	-0.030*	-0.045*
	(0.048)	(0.046)	(0.045)	(0.050)	(0.063)	(0.041)	(0.077)	(0.065)	(0.024)	(0.020)	(0.017)	(0.025)
N	*22,455*	*12,302*	*12,061*	*8,160*	*44,705*	*27,813*	*24,129*	*23,848*	*87,756*	*45,303*	*45,725*	*51,819*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 3. Heterogeneity in Effects of BTB Laws by Partial or Comprehensive Coverage, NIBRS, 2004-2014

	Hispanic Men				*African American Men*				*White Men*			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
					Panel I: Total Crime							
BTB	0.069	0.028	0.109*	0.088	0.007	0.018	-0.008	0.011	0.007	0.037	0.038	-0.063
	(0.048)	(0.048)	(0.056)	(0.056)	(0.119)	(0.100)	(0.122)	(0.135)	(0.070)	(0.059)	(0.082)	(0.078)
BTB*Private	-0.003	-0.021	0.102	-0.091	-0.290	-0.287	-0.247	-0.328	-0.003	-0.021	-0.044	0.049
	(0.098)	(0.114)	(0.084)	(0.100)	(0.448)	(0.466)	(0.366)	(0.487)	(0.254)	(0.254)	(0.249)	(0.249)
					Panel II: Property crime							
BTB	0.114**	0.056	0.152**	0.164***	-0.036	-0.012	-0.055	-0.036	0.027	0.056	0.062	-0.062
	(0.052)	(0.051)	(0.062)	(0.064)	(0.126)	(0.100)	(0.130)	(0.143)	(0.080)	(0.063)	(0.094)	(0.096)
BTB*Private	-0.121	-0.177	0.015	-0.162	-0.340	-0.342	-0.249	-0.422	-0.066	-0.064	-0.096	-0.044
	(0.115)	(0.131)	(0.100)	(0.130)	(0.503)	(0.525)	(0.376)	(0.542)	(0.248)	(0.256)	(0.236)	(0.241)
					Panel III: Violent crime							
BTB	-0.010	-0.004	0.043	-0.060	0.084	0.078	0.071	0.116	-0.026	-0.007	-0.002	-0.044
	(0.065)	(0.073)	(0.067)	(0.067)	(0.113)	(0.107)	(0.119)	(0.117)	(0.052)	(0.057)	(0.054)	(0.054)
BTB*Private	0.206***	0.281***	0.277***	-0.023	-0.170	-0.182	-0.265	-0.062	0.154	0.107	0.112	0.221
	(0.066)	(0.067)	(0.063)	(0.065)	(0.312)	(0.308)	(0.346)	(0.348)	(0.253)	(0.227)	(0.274)	(0.248)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 4A. Estimates of Lead and Lagged Effects of BTB Laws on Crime, Hispanic Men, NIBRS, 2004-2014

	Total Crime				***Property Crime***				***Violent Crime***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2 years before BTB	-0.020	-0.047	0.040	-0.040	-0.033	-0.055	0.026	-0.053	0.001	-0.014	0.040	-0.024
	(0.043)	(0.046)	(0.055)	(0.049)	(0.050)	(0.059)	(0.062)	(0.055)	(0.033)	(0.046)	(0.044)	(0.049)
1 year before BTB	-0.023	-0.064	0.003	0.008	-0.014	-0.069	0.025	0.034	-0.059*	-0.067	-0.051	-0.048
	(0.049)	(0.056)	(0.053)	(0.061)	(0.061)	(0.064)	(0.066)	(0.080)	(0.035)	(0.051)	(0.044)	(0.051)
Year of law change	0.017	0.010	0.070	0.002	0.053	0.031	0.122	0.056	-0.061	-0.016	-0.019	-0.126
	(0.063)	(0.060)	(0.076)	(0.078)	(0.067)	(0.065)	(0.081)	(0.086)	(0.078)	(0.083)	(0.093)	(0.086)
1 year after BTB	0.069	-0.045	0.139*	0.140*	0.121	-0.009	0.170**	0.250***	-0.030	-0.098	0.082	-0.082
	(0.067)	(0.075)	(0.073)	(0.085)	(0.075)	(0.081)	(0.081)	(0.096)	(0.073)	(0.091)	(0.076)	(0.106)
2 plus years after BTB	0.098	0.020	0.174*	0.113	0.141	0.029	0.226**	0.197*	0.019	0.032	0.074	-0.042
	(0.080)	(0.086)	(0.090)	(0.099)	(0.089)	(0.097)	(0.103)	(0.112)	(0.078)	(0.097)	(0.084)	(0.094)
χ^2 of $\sum(\beta_{leads})$=0	0.23	1.31	0.18	0.09	0.19	1.19	0.17	0.02	1.03	0.90	0.02	0.90
(p-value)	0.63	0.25	0.67	0.76	0.67	0.28	0.68	0.89	0.31	0.34	0.88	0.34
χ^2 of $\beta_{lead\ 1}$=$\beta_{lead\ 2}$=0	0.23	1.34	1.41	5.37*	0.87	1.22	0.19	8.53**	3.25	1.98	3.25	0.99
(p-value)	0.89	0.51	0.49	0.07	0.54	0.54	0.91	0.01	0.20	0.37	0.20	0.61
χ^2 of $\sum(\beta_{yrchange},\beta_{lags})$=0	0.81	0.00	2.80*	1.01	1.94	0.05	4.19**	3.06*	0.11	0.10	0.33	0.99
(p-value)	0.37	0.95	0.09	0.32	0.16	0.82	0.04	0.08	0.74	0.75	0.57	0.32
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income). Standards errors are clustered at the state level.

Table 4B. Estimates of Lead and Lagged Effects of BTB Laws on Crime, African American Men, NIBRS, 2004-2014

	Total Crime				***Property Crime***				***Violent Crime***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2 years before BTB	0.008	0.018	0.015	-0.002	-0.013	0.014	-0.007	-0.031	0.042	0.021	0.047	0.070
	(0.053)	(0.046)	(0.055)	(0.064)	(0.049)	(0.039)	(0.055)	(0.070)	(0.054)	(0.058)	(0.058)	(0.058)
1 year before BTB	-0.022	-0.019	0.005	-0.042	-0.064	-0.058	-0.049	-0.071	0.051	0.055	0.087	0.029
	(0.058)	(0.058)	(0.063)	(0.058)	(0.053)	(0.053)	(0.059)	(0.054)	(0.066)	(0.066)	(0.070)	(0.077)
Year of law change	-0.024	-0.020	0.001	-0.024	-0.067	-0.033	-0.061	-0.081	0.053	0.006	0.092	0.110
	(0.076)	(0.061)	(0.080)	(0.091)	(0.077)	(0.062)	(0.077)	(0.086)	(0.082)	(0.069)	(0.093)	(0.101)
1 year after BTB	0.034	0.042	0.019	0.041	-0.018	0.003	-0.037	-0.015	0.130	0.126	0.123	0.154
	(0.156)	(0.133)	(0.156)	(0.176)	(0.171)	(0.134)	(0.173)	(0.199)	(0.142)	(0.151)	(0.144)	(0.134)
2 plus years after BTB	-0.007	0.028	-0.035	-0.031	-0.096	-0.061	-0.128	-0.109	0.158	0.207	0.120	0.152
	(0.208)	(0.182)	(0.219)	(0.229)	(0.201)	(0.173)	(0.207)	(0.218)	(0.229)	(0.212)	(0.246)	(0.244)
χ^2 of $\sum(\beta_{leads})=0$	0.02	0.00	0.05	0.15	0.72	0.27	0.54	0.80	0.67	0.44	1.36	0.59
(p-value)	0.88	1.00	0.83	0.70	0.40	0.60	0.46	0.37	0.41	0.51	0.24	0.44
χ^2 of $\beta_{lead\ 1}=\beta_{lead\ 2}=0$	0.46	0.83	0.08	1.11	1.70	2.99	0.73	2.22	0.67	0.84	1.54	2.55
(p-value)	0.80	0.66	0.96	0.57	0.43	0.22	0.69	0.33	0.72	0.66	0.46	0.28
χ^2 of $\sum(\beta_{yrchange}, \beta_{lags})=0$	0.00	0.02	0.00	0.00	0.17	0.06	0.25	0.17	0.60	0.66	0.50	0.81
(p-value)	0.99	0.89	0.97	0.98	0.68	0.80	0.61	0.68	0.44	0.42	0.48	0.37
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 4C. Estimates of Lead and Lagged Effects of BTB Laws on Crime, Non-Hispanic White Men, NIBRS, 2004-2014

	Total Crime				***Property Crime***				***Violent Crime***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2 years before BTB	-0.015	-0.012	-0.008	-0.007	-0.002	0.010	-0.000	0.007	-0.056	-0.092*	-0.027	-0.049
	(0.040)	(0.033)	(0.057)	(0.048)	(0.046)	(0.036)	(0.063)	(0.058)	(0.048)	(0.050)	(0.053)	(0.051)
1 year before BTB	-0.013	0.010	0.013	-0.055	0.002	0.025	0.034	-0.050	-0.060	-0.039	-0.053	-0.080
	(0.053)	(0.046)	(0.064)	(0.055)	(0.060)	(0.053)	(0.074)	(0.059)	(0.049)	(0.039)	(0.054)	(0.063)
Year of law change	-0.008	0.034	0.021	-0.078	0.017	0.057	0.053	-0.071	-0.076	-0.048	-0.053	-0.099*
	(0.052)	(0.043)	(0.058)	(0.062)	(0.061)	(0.047)	(0.069)	(0.074)	(0.050)	(0.047)	(0.059)	(0.057)
1 year after BTB	0.031	0.061	0.073	-0.048	0.069	0.101	0.110	-0.024	-0.053	-0.047	0.004	-0.081
	(0.104)	(0.088)	(0.122)	(0.113)	(0.115)	(0.094)	(0.136)	(0.133)	(0.083)	(0.079)	(0.093)	(0.080)
2 plus years after BTB	-0.015	0.014	0.028	-0.099	-0.003	0.031	0.051	-0.120	-0.018	-0.008	-0.001	-0.028
	(0.105)	(0.085)	(0.110)	(0.126)	(0.118)	(0.088)	(0.126)	(0.149)	(0.094)	(0.099)	(0.093)	(0.105)
χ^2 of $\sum(\beta_{leads})=0$	0.09	0.00	0.00	0.39	0.00	0.18	0.06	0.14	1.57	2.41	0.62	1.47
(p-value)	0.76	0.97	0.97	0.53	1.00	0.67	0.80	0.70	0.21	0.12	0.43	0.22
χ^2 of $\beta_{lead\ 1}=\beta_{lead\ 2}=0$	0.18	0.58	0.56	2.78	0.02	0.25	1.10	3.39	1.58	4.18	1.22	1.62
(p-value)	0.91	0.75	0.76	0.25	0.99	0.88	0.58	0.18	0.45	0.12	0.54	0.45
χ^2 of $\sum(\beta_{yrchange,}\beta_{lags})=0$	0.00	0.29	0.19	0.60	0.09	0.79	0.45	0.39	0.49	0.24	0.05	0.88
(p-value)	0.98	0.59	0.66	0.44	0.77	0.37	0.50	0.53	0.48	0.63	0.82	0.35
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 5. Poisson Estimates of Relationship Between Ban-the-Box Laws and Female Crime, NIBRS, 2004-2014

	Hispanic Women				***African American Women***				***White Women***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Total crime	0.060	0.069	0.045	0.036	-0.017	-0.046	-0.022	0.015	-0.010	-0.020	0.060	-0.069
	(0.065)	(0.075)	(0.063)	(0.067)	(0.104)	(0.100)	(0.089)	(0.121)	(0.062)	(0.056)	(0.080)	(0.053)
Property crime	0.092	0.097	0.084	0.063	-0.070	-0.097	-0.081	-0.031	0.001	-0.022	0.089	-0.061
	(0.067)	(0.074)	(0.068)	(0.074)	(0.097)	(0.087)	(0.082)	(0.123)	(0.067)	(0.060)	(0.085)	(0.060)
Violent crime	-0.054	-0.034	-0.095	-0.089	0.132	0.139	0.128	0.126	-0.008	0.045	-0.030	-0.022
	(0.096)	(0.114)	(0.083)	(0.124)	(0.126)	(0.141)	(0.117)	(0.131)	(0.061)	(0.070)	(0.064)	(0.066)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 6. Heterogeneity in BTB Law Effects by Criminal Offense, NIBRS, 2004-2014

	Hispanic Men				*African American Men*				*White Men*			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Larceny-theft	0.127**	0.061	0.149**	0.180**	-0.065	-0.020	-0.096	-0.068	0.027	0.057	0.064	-0.070
	(0.060)	(0.055)	(0.068)	(0.077)	(0.124)	(0.087)	(0.133)	(0.144)	(0.080)	(0.063)	(0.095)	(0.096)
Motor vehicle theft	0.110*	0.208***	0.169*	-0.123	-0.002	0.070	-0.089	-0.101	0.043	0.030	0.072	-0.002
	(0.063)	(0.073)	(0.096)	(0.120)	(0.213)	(0.230)	(0.219)	(0.155)	(0.117)	(0.066)	(0.152)	(0.153)
Burglary	0.054	-0.039	0.210**	0.104	0.077	0.066	0.117	0.101	0.023	0.061	0.055	-0.064
	(0.066)	(0.080)	(0.086)	(0.075)	(0.113)	(0.097)	(0.103)	(0.137)	(0.082)	(0.072)	(0.095)	(0.096)
Robbery	0.033	-0.002	0.148	-0.061	0.047	0.025	0.084	0.041	0.055	0.091	0.060	-0.027
	(0.104)	(0.099)	(0.129)	(0.154)	(0.124)	(0.112)	(0.137)	(0.159)	(0.132)	(0.128)	(0.153)	(0.141)
Aggravated assault	-0.002	0.002	0.048	-0.042	0.102	0.107	0.073	0.134	-0.034	-0.035	-0.015	-0.029
	(0.062)	(0.074)	(0.064)	(0.067)	(0.110)	(0.101)	(0.115)	(0.119)	(0.047)	(0.050)	(0.045)	(0.054)
Murder	-0.314*	-0.063	-0.084	-0.788***	-0.025	0.048	-0.093	-0.116	0.059	0.358	0.047	-0.212
	(0.168)	(0.200)	(0.295)	(0.246)	(0.114)	(0.136)	(0.126)	(0.149)	(0.171)	(0.218)	(0.262)	(0.240)
Arson	-0.009	0.008	-0.407	0.249	0.096	0.106	-0.029	0.182	0.040	0.082	-0.169	0.130
	(0.140)	(0.198)	(0.343)	(0.313)	(0.163)	(0.152)	(0.218)	(0.129)	(0.126)	(0.153)	(0.272)	(0.080)
Stolen property	-0.051	-0.088	0.015	0.094	-0.083*	-0.118**	-0.033	-0.065	0.030	0.056	0.033	-0.008
	(0.075)	(0.074)	(0.113)	(0.139)	(0.046)	(0.054)	(0.100)	(0.065)	(0.053)	(0.070)	(0.075)	(0.059)
Weapon law violation	0.155***	0.139**	0.209***	0.104**	-0.006	-0.012	0.008	-0.002	0.046	0.031	0.084	0.053
	(0.049)	(0.060)	(0.079)	(0.047)	(0.059)	(0.052)	(0.066)	(0.096)	(0.066)	(0.065)	(0.072)	(0.091)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Table 7A. Poisson Estimates of Relationship Between Ban-the-Box Laws and Crime, UCR, 2004-2014

	African American Adults Ages 18+	*White Adults Ages 18+*
	(1)	(2)
Total crime	-0.028	-0.070**
	(0.037)	(0.029)
Property crime	-0.063	-0.073**
	(0.040)	(0.034)
Violent crime	0.053	-0.015
	(0.040)	(0.028)
Larceny-theft	-0.053	-0.072*
	(0.039)	(0.037)
Motor vehicle theft	-0.089	-0.051
	(0.058)	(0.045)
Burglary	0.029	-0.014
	(0.035)	(0.026)
Robbery	0.003	-0.025
	(0.037)	(0.027)
Aggravated assault	0.076*	-0.020
	(0.046)	(0.031)
Murder	-0.094**	-0.039
	(0.045)	(0.034)
Arson	0.153	0.142
	(0.099)	(0.095)
Stolen property	0.033	0.010
	(0.070)	(0.055)
Weapon law violation	0.029	-0.014
	(0.035)	(0.026)
N	*369,438*	*369,438*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using county-level data drawn from the 2004 to Uniform Crime Reports. Each regression has controls for county fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the number of reporting agencies, the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, and the number of agencies). Standards errors are clustered at the state level.

Table 7B. Heterogeneity in Effects of BTB Laws by Partial or Comprehensive Coverage, UCR, 2004-2014

	African American Adults Ages 18+	*White Adults Ages 18+*
	(1)	(2)
	Panel I: Total crime	
BTB	-0.039	-0.058**
	(0.044)	(0.029)
BTB*Private	0.065	-0.063*
	(0.077)	(0.034)
	Panel II: Property crime	
BTB	-0.078	-0.058*
	(0.048)	(0.035)
BTB*Private	0.091	-0.076**
	(0.091)	(0.038)
	Panel III: Violent crime	
BTB	0.046	-0.013
	(0.044)	(0.027)
BTB*Private	0.040	-0.015
	(0.053)	(0.040)
N	*369,438*	*369,438*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using county-level data drawn from the 2004 to Uniform Crime Reports. Each regression has controls for county fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, and the number of agencies). Standards errors are clustered at the state level.

Table 8A. OLS Estimates of Relationship between Ban-the-Box Laws and Probability of Arrest, NLSY97, 2004-2014

	Hispanic Men			***African American Men***			***White Men***		
				Panel I: Ages 19-34					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB Law	0.011	0.020	0.009	0.003	-0.002	-0.006	-0.006	-0.013	-0.005
	(0.012)	(0.015)	(0.014)	(0.013)	(0.014)	(0.013)	(0.007)	(0.007)	(0.007)
N	*7132*	*7132*	*7132*	*8702*	*8702*	*8702*	*16831*	*16831*	*16831*
				Panel II: Ages 19-26					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB Law	0.057**	0.053*	0.055**	0.005	-0.012	-0.032	0.002	-0.007	0.004
	(0.019)	(0.020)	(0.019)	(0.028)	(0.022)	(0.024)	(0.021)	(0.021)	(0.025)
N	*4200*	*4200*	*4200*	*5098*	*5098*	*5098*	*10088*	*10088*	*10088*
				Panel III: Ages 27-34					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB Law	-0.021	0.007	0.002	0.004	-0.004	0.009	-0.021*	-0.009	-0.007
	(0.015)	(0.021)	(0.022)	(0.017)	(0.018)	(0.022)	(0.009)	(0.010)	(0.013)
County fixed effects	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Person fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	*2932*	*2932*	*2932*	*3604*	*3604*	*3604*	*6743*	*6743*	*6743*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Coefficients from OLS models are reported. Each regression also controls for indicators for age, educational attainment (high school graduate, some college, college graduate or above), wave fixed effects, and a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income). Standards errors are clustered at the state level and reported in parentheses.

Table 8B. OLS Estimates of Relationship between Ban-the-Box Laws and Probability of Property and Violent Crime, NLSY97, 2004-2014

	Hispanic Men			***African American Men***			***Non-Hispanic White Men***		
Ages	*19-34*	*19-26*	*27-34*	*19-34*	*19-26*	*27-34*	*19-34*	*19-26*	*27-34*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any Property crime	0.015	0.018	0.033	0.002	-0.005	0.005	-0.011	0.012	-0.022
	(0.027)	(0.076)	(0.024)	(0.013)	(0.020)	(0.021)	(0.016)	(0.034)	(0.026)
Minor Theft	-0.005	-0.016	-0.007	-0.001	-0.001	0.002	-0.015	-0.010	-0.008
	(0.009)	(0.037)	(0.019)	(0.011)	(0.015)	(0.016)	(0.013)	(0.030)	(0.013)
Major Theft	0.015	0.045	0.003	-0.003	0.0001	0.006	-0.004	-0.017	0.008
	(0.010)	(0.041)	(0.013)	(0.006)	(0.009)	(0.014)	(0.010)	(0.028)	(0.013)
Other Property Crime	0.029	0.032	0.006	0.002	0.013	-0.009	-0.011	0.001	-0.011
	(0.019)	(0.035)	(0.023)	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.010)
Destroy Property	-0.003	0.003	0.028	-0.005	-0.017	0.005	-0.003	-0.020	0.025
	(0.023)	(0.035)	(0.034)	(0.015)	(0.019)	(0.014)	(0.014)	(0.023)	(0.026)
Assault	0.010	0.017	0.001	-0.012	-0.033	-0.012	-0.013	-0.033	0.015
	(0.027)	(0.029)	(0.040)	(0.029)	(0.046)	(0.022)	(0.014)	(0.016)	(0.013)
N	*3100*	*1777*	*1323*	*4173*	*2358*	*1815*	*6844*	*4152*	*2692*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Coefficients from OLS models are reported. Each cell reports the effect of Ban-the-Box laws on the specific crime measure from a separate regression model. Each regression also controls for indicators for age, educational attainment (high school graduate, some college, college graduate or above), wave fixed effects, and a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income). Standards errors are clustered at the state level and reported in parentheses.

Appendix Table 1. Means of Independent Variables, 2004-2014

	NIBRS	*UCR*	*NLSY97*
	(1)	(2)	
BTB	0.080 (0.271)	0.164 (0.370)	0.113 (0.317)
Minimum wage	6.712 (1.002)	6.760 (0.974)	6.678 (1.010)
Police expenditure per capita	241.17 (39.95)	287.05 (97.62)	280.16 (83.56)
Police employment per capita (in 1,000)	2.197 (0.434)	2.446 (0.715)	2.293 (0.628)
Shall-issue law	0.818 (0.386)	0.658 (0.474)	0.679 (0.467)
College degree	0.299 (0.061)	0.297 (0.056)	0.295 (0.050)
Share of male	0.493 (0.013)	0.487 (0.014)	0.492 (0.012)
Share of African American	0.100 (0.132)	0.261 (0.173)	0.135 (0.141)
Share of Hispanic	0.060 (0.067)	0.141 (0.138)	0.140 (0.149)
Average age	38.39 (2.766)	36.504 (2.046)	36.857 (2.373)
Personal per capita income	39,448.44 (7,284.40)	40,495.49 (7,205.79)	39,568.15 (6,183.47)
N	*243,804*	*369,438*	*68,951*

Notes: Unweighted means in column 1 are generated using data drawn from the 2004 to 2014 National Incident-Based Reporting System; and weighted means in columns 2 and 3 are generated using data from the 2004 to 2014 Uniform Crime Reports and the National Longitudinal Survey of Youth 97 Cohort. Standard deviations are in parentheses.

Appendix Table 2. Effective Date of BTB Policies

State	*NIRBS*[a]	*UCR*[b]	*NLSY*[c]	*State/County/City*	*Effective Date*
California	No	Yes	Yes	State	06/2010
	No	Yes	Yes	Alameda	01/2007
	No	Yes	Yes	San Francisco	10/2005
	No	Yes	Yes	Santa Clara	01/2005
Colorado	Yes	Yes	Yes	State	08/2012
Connecticut	Yes	Yes	Yes	State	10/2010
	Yes	Yes	Yes	Fairfield	10/2009
	Yes	Yes	Yes	Hartford	06/2009
	Yes	Yes	Yes	New Haven	02/2009
	Yes	Yes	Yes	New London	12/2008
District of Columbia	Yes	Yes	Yes	Washington	01/2011
Delaware	Yes	Yes	Yes	State	05/2014
	Yes	Yes	Yes	New Castle	12/2012
Florida	No	No	No	Broward	12/2014
	No	No	Yes	Duval	11/2008
	No	No	Yes	Hillsborough	01/2013
Georgia	No	Yes	Yes	Atlanta-Sandy Springs-Roswell	01/2013
Hawaii	No	No	No	State	01/1998
Illinois	Yes	Yes	Yes	State	01/2014
	No	Yes	Yes	Cook, Du Page	06/2007
Indiana	Yes	Yes	Yes	Marion	05/2014
Kentucky	Yes	Yes	Yes	Jefferson	03/2014
Kansas	Yes	Yes	No	Wyandotte	11/2014
Louisiana	No	Yes	Yes	Orleans Parish	01/2014
Maryland	No	Yes	Yes	State	10/2013
	No	Yes	Yes	*Baltimore*	12/2007
Massachusetts	Yes	Yes	Yes	State	08/2010
	Yes	Yes	Yes	Middlesex	05/2007
	Yes	Yes	Yes	Suffolk	07/2006
	Yes	Yes	Yes	Worcester	06/2009
Michigan	Yes	Yes	Yes	Clinton	04/2014
	Yes	Yes	No	Genesee	06/2014
	Yes	Yes	No	Ingham	04/2014
	Yes	Yes	Yes	Kalamazoo	01/2010
	Yes	Yes	Yes	Muskegon	01/2012
	Yes	Yes	No	Washtenaw	05/2014
	Yes	Yes	Yes	Wayne	09/2010
Minnesota	No	Yes	Yes	State	01/2009
	No	Yes	Yes	Hennepin	12/2006
	No	Yes	Yes	Ramsey	12/2006
Missouri	No	Yes	No	Boone	12/2014
	Yes	Yes	Yes	Cass, Clay, Jackson, Platte	04/2013
	No	Yes	Yes	*St. Louis*	10/2014
Nebraska	Yes	Yes	No	State	04/2014
New Jersey	No	Yes	Yes	Atlantic	12/2011

State	*NIRBS*[a]	*UCR*[b]	*NLSY*[c]	*State/County/City*	*Effective Date*
	No	Yes	Yes	Essex	09/2012
New Mexico	No	Yes	Yes	State	03/2010
New York	No	Yes	Yes	Bronx, Queens, Kings, New York, Richmond	03/2010
	No	Yes	Yes	Erie	06/2013
	No	Yes	No	Monroe	05/2014
	No	Yes	No	Ulster	11/2014
	No	Yes	No	Westchester	11/2014
North Carolina	No	Yes	Yes	Cumberland	09/2011
	No	Yes	Yes	Durham, Orange, Wake	02/2011
	No	Yes	Yes	Mecklenburg	02/2014
Ohio	Yes	Yes	Yes	Cuyahoga	09/2011
	Yes	Yes	Yes	Franklin	06/2012
	Yes	Yes	Yes	Hamilton	08/2010
	Yes	Yes	Yes	Lucas	10/2013
	Yes	Yes	Yes	Mahoning, Trumbull	03/2014
	Yes	Yes	Yes	Stark	05/2013
	Yes	Yes	Yes	Summit	09/2012
Oregon	Yes	Yes	No	Clackamas, Washington	07/2014
	Yes	Yes	Yes	Multnomah	10/2007
Pennsylvania	Yes	Yes	Yes	Allegheny	12/2012
	Yes	Yes	No	Lancaster	10/2014
	No	Yes	Yes	Philadelphia	06/2011
Rhode Island	Yes	Yes	Yes	State	07/2013
	No	Yes	Yes	Providence	04/2009
Tennessee	Yes	Yes	Yes	Hamilton	01/2012
	Yes	Yes	Yes	Shelby	07/2010
Texas	Yes	Yes	Yes	Hays, Williamson	10/2008
	Yes	Yes	Yes	Travis	04/2008
Virginia	Yes	Yes	Yes	Alexandria City	03/2014
	Yes	Yes	No	Arlington	11/2014
	Yes	Yes	No	Charlottesville City	03/2014
	Yes	Yes	No	Danville City	06/2014
	Yes	Yes	Yes	Fredericksburg City	01/2014
	Yes	Yes	Yes	Newport News City	10/2012
	Yes	Yes	Yes	Norfolk City	07/2013
	Yes	Yes	Yes	Petersburg City	09/2013
	Yes	Yes	Yes	Portsmouth City	04/2013
	Yes	Yes	Yes	Richmond City	03/2013
	Yes	Yes	Yes	Virginia Beach City	11/2013
	Yes	Yes	Yes	Alexandria City	03/2014
Washington	Yes	Yes	Yes	King	04/2009
	Yes	Yes	Yes	Pierce	01/2012
	Yes	Yes	Yes	Spokane	07/2014
Wisconsin	Yes	Yes	Yes	Dane	02/2014
	Yes	Yes	Yes	Milwaukee, Washington, Waukesha	10/2011

Source: Doleac and Hansen (2017).

Appendix Table 3A. Robustness of Estimates to Restricting to Agencies Reporting in at Least Half the Sample Period (6 Years), NIBRS, 2004-2014

	Hispanic Men				***African American Men***				***White Men***			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Total crime	0.087**	0.042	0.111**	0.129***	0.008	-0.001	0.004	0.023	0.030	0.050	0.063	-0.036
	(0.036)	(0.035)	(0.047)	(0.043)	(0.103)	(0.085)	(0.107)	(0.118)	(0.061)	(0.054)	(0.069)	(0.068)
Property crime	0.118***	0.055	0.131**	0.193***	-0.029	-0.024	-0.043	-0.018	0.052	0.070	0.092	-0.031
	(0.044)	(0.044)	(0.060)	(0.053)	(0.116)	(0.092)	(0.122)	(0.135)	(0.071)	(0.061)	(0.079)	(0.083)
Violent crime	0.035	0.038	0.083	0.005	0.077	0.053	0.083	0.108	-0.004	0.008	0.023	-0.025
	(0.043)	(0.049)	(0.052)	(0.041)	(0.086)	(0.083)	(0.095)	(0.086)	(0.041)	(0.038)	(0.045)	(0.046)
N	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Appendix Table 3B. Robustness of Positive Crime Estimates to OLS Specification, NIBRS, 2004-2014

	Hispanic Men				*African American Men*				*White Men*			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Total crime	0.039*	0.007	0.069**	0.043	0.000	-0.019	-0.019	0.009	-0.034	-0.021	-0.018	-0.043**
	(0.023)	(0.019)	(0.026)	(0.031)	(0.024)	(0.020)	(0.027)	(0.030)	(0.020)	(0.017)	(0.018)	(0.019)
N	*44,398*	*28,695*	*24,287*	*18,074*	*73,904*	*51,757*	*40,964*	*45,785*	*148,078*	*109,460*	*97,694*	*97,532*
Property crime	0.071**	0.029	0.075**	0.078**	-0.020	-0.034	-0.027	-0.029	-0.033	-0.011	-0.009	-0.043*
	(0.026)	(0.024)	(0.030)	(0.031)	(0.025)	(0.027)	(0.037)	(0.034)	(0.022)	(0.017)	(0.024)	(0.023)
N	*35,403*	*23,407*	*17,797*	*13,306*	*62,697*	*42,979*	*31,146*	*37,539*	*133,113*	*98,232*	*83,324*	*78,664*
Violent crime	-0.009	-0.033	0.018	-0.007	0.025	0.006	0.022	0.050	-0.018	-0.025	-0.008	-0.011
	(0.025)	(0.036)	(0.038)	(0.041)	(0.034)	(0.031)	(0.036)	(0.033)	(0.017)	(0.017)	(0.012)	(0.018)
N	*22,455*	*12,302*	*12,061*	*8,160*	*44,705*	*27,813*	*24,129*	*23,848*	*87,756*	*45,303*	*45,725*	*51,819*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates in Panel I and OLS estimates in Panel II are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level.

Appendix Table 4. Sensitivity of Estimates to Added Control for Aggregate Unemployment Rate, NIBRS, 2004-2014

	Hispanic Men				*African American Men*				*White Men*			
Ages	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*	*All*	*Under 25*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Total crime	0.067	0.023	0.109**	0.083	0.003	0.014	-0.013	0.006	0.007	0.036	0.036	-0.061
	(0.047)	(0.046)	(0.054)	(0.055)	(0.119)	(0.101)	(0.119)	(0.136)	(0.070)	(0.058)	(0.081)	(0.079)
Property crime	0.108**	0.046	0.151**	0.156**	-0.041	-0.019	-0.059	-0.043	0.025	0.054	0.059	-0.064
	(0.051)	(0.050)	(0.061)	(0.064)	(0.125)	(0.101)	(0.128)	(0.143)	(0.081)	(0.063)	(0.093)	(0.097)
Violent crime	-0.007	0.001	0.045	-0.060	0.081	0.078	0.062	0.113	-0.019	-0.001	0.003	-0.035
	(0.063)	(0.072)	(0.064)	(0.066)	(0.115)	(0.112)	(0.115)	(0.119)	(0.052)	(0.056)	(0.054)	(0.056)
Larceny-theft	0.124**	0.056	0.148**	0.173**	-0.065	-0.021	-0.096	-0.068	0.027	0.058	0.064	-0.071
	(0.059)	(0.054)	(0.067)	(0.077)	(0.123)	(0.087)	(0.130)	(0.144)	(0.082)	(0.064)	(0.096)	(0.099)
Motor vehicle theft	0.113*	0.209***	0.172*	-0.124	-0.002	0.070	-0.087	-0.101	0.044	0.031	0.073	-0.001
	(0.060)	(0.072)	(0.092)	(0.120)	(0.211)	(0.228)	(0.215)	(0.155)	(0.117)	(0.067)	(0.153)	(0.154)
Burglary	0.053	-0.040	0.209**	0.102	0.079	0.067	0.118	0.105	0.023	0.061	0.054	-0.064
	(0.066)	(0.082)	(0.086)	(0.076)	(0.119)	(0.104)	(0.107)	(0.145)	(0.084)	(0.074)	(0.096)	(0.098)
Robbery	0.029	-0.011	0.143	-0.054	0.050	0.026	0.084	0.048	0.058	0.093	0.063	-0.021
	(0.103)	(0.096)	(0.126)	(0.154)	(0.133)	(0.120)	(0.140)	(0.172)	(0.134)	(0.130)	(0.156)	(0.141)
Murder	-0.318*	-0.053	-0.101	-0.788***	-0.021	0.052	-0.094	-0.118	0.059	0.358*	0.049	-0.215
	(0.167)	(0.198)	(0.298)	(0.246)	(0.124)	(0.148)	(0.133)	(0.150)	(0.172)	(0.216)	(0.262)	(0.240)
Aggravated assault	-0.002	0.001	0.046	-0.041	0.097	0.105	0.063	0.128	-0.035	-0.035	-0.016	-0.030
	(0.060)	(0.073)	(0.061)	(0.067)	(0.107)	(0.101)	(0.107)	(0.116)	(0.047)	(0.050)	(0.045)	(0.053)
Arson	-0.031	-0.022	-0.433	0.260	0.090	0.106	-0.038	0.174	0.045	0.084	-0.162	0.134*
	(0.129)	(0.190)	(0.334)	(0.318)	(0.159)	(0.151)	(0.204)	(0.132)	(0.124)	(0.153)	(0.267)	(0.080)
Stolen property	-0.041	-0.081	0.026	0.104	-0.077*	-0.107*	-0.035	-0.069	0.030	0.056	0.031	-0.010
	(0.069)	(0.071)	(0.102)	(0.132)	(0.046)	(0.062)	(0.096)	(0.056)	(0.055)	(0.071)	(0.078)	(0.061)
Weapon law violatior	0.157***	0.141**	0.209***	0.110**	0.001	-0.002	0.015	-0.002	0.046	0.030	0.083	0.053
	(0.050)	(0.061)	(0.080)	(0.049)	(0.069)	(0.064)	(0.077)	(0.096)	(0.067)	(0.066)	(0.074)	(0.091)
Rape	-0.052	-0.015	-0.070	-0.129	0.195***	0.126	0.211*	0.234**	0.015	0.034	0.148**	-0.090
	(0.106)	(0.231)	(0.160)	(0.102)	(0.062)	(0.144)	(0.120)	(0.095)	(0.058)	(0.122)	(0.070)	(0.090)
N	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*	*243,804*

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Poisson estimates are generated using agency-level data drawn from the 2004 to 2014 National Incident-Based Reporting System. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree, and unemployment rates), county-level controls (the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income), and the natural log of the population served by the agency. Standards errors are clustered at the state level

Appendix Table 5. Estimates of Relationship between Ban-the-Box Laws and Criminal Behaviors among Females, NLSY97, 2004-2014

	Females	
Ages	*19-34*	*19-26*
	(1)	(2)
Arrested	-0.005	-0.015
	(0.004)	(0.008)
Any Property crime	0.003	0.003
	(0.011)	(0.021)
Minor Theft	0.005	0.017
	(0.008)	(0.019)
Major Theft	0.008	0.014
	(0.006)	(0.016)
Other Property Crime	0.001	0.003
	(0.003)	(0.004)
Destroy Property	-0.003	-0.010
	(0.009)	(0.011)
Assault	0.001	-0.001
	(0.009)	(0.022)

*** Significant at 1% level ** at 5% level * at 10% level

Notes: Coefficients from OLS models are reported. Each cell reports the effect of Ban-the-Box laws on the specific crime measure from a separate regression model. Each regression also controls for indicators for age, educational attainment (high school graduate, some college, college graduate or above), wave fixed effects, and a set of state-level controls (including nominal minimum wages, nominal police expenditure per capita, police employment per capita, Shall issue laws, share of state population ages 25+ with a bachelor degree), and county-level controls (gender-/age-/race-specific population, the percentage of the population that are male, African American, Hispanic, average age, and nominal personal per capita income). Standards errors are clustered at the state level and reported in parentheses. Sample sizes for models in column 1 range from 7839 to 33698, and those for models in column 2 range from 5290 to 19881.