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Joseph J. Sabia
Taylor Mackay
Thanh Tam Nguyen
Dhaval M. Dave

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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, are intended to increase employment opportunities and reduce economic incentives for crime. This study is the first to comprehensively explore the relationship between state and local BTB laws and criminal arrests among racial/ethnic minorities. Using agency-by-month data from the National Incident-Based Reporting System (NIBRS), we find that BTB laws are associated with a 10 percent increase in criminal incidents involving Hispanic male arrestees. This finding is supported by parallel analysis using data from the National Longitudinal Survey of Youth 1997 (NLSY97) and is consistent with BTB law-induced job loss due to employer-based statistical discrimination. We find no evidence that BTB laws increase property crime among African American men despite their also facing statistical discrimination. Supplemental analyses from the American Community Survey (ACS) suggest that barriers to welfare participation among Hispanic men may explain this result. Our estimates suggest that BTB laws generate $401 million in annual crime costs.

Joseph J. Sabia
San Diego State University
Department of Economics
Center for Health Economics &
Policy Studies
5500 Campanile Drive
San Diego, CA 92182
and IZA & ESSPRI
jsabia@sdsu.edu

Taylor Mackay
University of California, Irvine
Department of Economics
Social Science Plaza
Irvine, CA 92697-5100
tmackay@uci.edu

Thanh Tam Nguyen
University of New Hampshire
Department of Economics
Peter T Paul College of Business & Economics
Durham, NH 03824
tn1028@wildcats.unh.edu

Dhaval M. Dave
Bentley University
Department of Economics
175 Forest Street, AAC 195
Waltham, MA 02452-4705
and IZA
and also NBER
ddave@bentley.edu

1. Introduction

More than 2.2 million Americans are 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).

The recidivism rate among ex-offenders is 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, 33 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, forthcoming; 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, particularly for economically motivated crimes. On the other hand, if BTB laws induce statistical discrimination against racial or ethnic minorities with higher perceived risks of criminal activity, 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.[3]

Despite policymakers' hope that BTB laws would reduce economic incentives for crime, very little is known about their impacts on criminal activity. Given emerging evidence of unintended labor market consequences of BTB laws, exploring this question is particularly

[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.

[3]However, if individuals are forward looking and anticipate statistical discrimination as a response to BTB laws, then this belief may induce more current crime commission.

important. Using data from the National Incident Based Reporting System (NIBRS) from 2004 to 2014, we find that BTB laws are associated with a 10 percent increase in property crime offenses committed by working age Hispanic men. This result is also observed in parallel analysis using data from the National Longitudinal Survey of Youth (1997). This finding is consistent with BTB law-induced statistical discrimination reducing employment for low-skilled Hispanic men. However, we find no evidence that BTB laws increased property crime among working age African American men, a population that also faces job loss due to statistical discrimination. Supplemental analysis using the American Community Survey (ACS) suggests a possible explanation for this race/ethnicity-specific difference in crime response. We observe differential take-up of means-tested public assistance programs in response to BTB-induced employment reductions. While low-skilled African American men are more likely to participate in welfare programs, principally the Supplemental Nutrition Assistance Program (SNAP), following BTB enactment, low-skilled Hispanic men are not. This may be due to differential barriers to participation such as language difficulties, immigration status fears, or cultural/family factors. Together, the findings from this study add to growing evidence on the unintended consequences of BTB laws. Our estimates suggest that BTB laws induce approximately 70,000 additional property crimes, generating $401 million (in 2018 dollars) in social costs (McCollister et al. 2010).

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 of their 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).[4,5] 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 records to be 10 to

[4] Several studies show that employers may overestimate the magnitudes of these associations (Roberts et al. 2007; Blumstein and Nakamura 2009).

[5] Insuring against employee misconduct and malfeasance through "fidelity bonds" is often not possible if the employee has a criminal record (Stafford 2006).

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.

Criminal behavior may also respond to economic conditions (Becker 1968). Local labor market opportunities, including employment (Bushway, Cook, and Phillips 2010; Levitt 2004; Lin 2008; Machin and Meghir 2004; Öster and Agell 2007; Raphael and Winter-Ebmer 2001; Schnepel 2018) and wages (Agan and Makowsky 2018; Fone, Sabia and Cesur 2019; Gould et al. 2002; Yang 2017) have been found to be negatively related to crime.

Studies specifically examining the effect of labor market opportunities on recidivism have reached a similar conclusion. Using data on 1.7 million offenders released from California prisons from 1993 to 2008, Schnepel (2018) finds that construction and manufacturing job availability is negatively related to recidivism rates. Similarly, Yang (2017) utilizes administrative data on more than 4 million ex-offenders in 43 states between 2000 and 2013 and finds that offenders released to counties with higher low-skilled wages are less likely to recidivate. Finally, Wang et al. (2010) pool data on more than 40,000 male offenders released from Florida prisons from 2000 to 2001 and find 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, 33 U.S. states and the District of Columbia have adopted "ban the box" laws, which seek to strategically withhold information on job applicants' criminal histories from prospective employers at first interview.[6] 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 shown that the conviction bears a "rational relationship" to the duties and responsibilities of the position.

Following the adoption of the Hawaii statute, 32 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 the U.S. population, live in a jurisdiction covered by a BTB law (National Employment Law Project 2017).[7]

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[6]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.

[7] Eleven states, the District of Columbia, and 17 localities have adopted BTB laws that apply to both private and public employers. Twenty-two (22) states have adopted BTB laws that apply only to public employers or to private employers with government contracts. However, as discussed below, public BTB laws may have important effects in the private sector (see footnote 19). 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).

Several studies in the economics literature have examined the labor market effects of ban the box laws.[8] There is both experimental and quasi-experimental evidence that BTB laws reduce employment among male racial/ethnic minorities, a consequence of statistical discrimination by employers. Agan and Starr (2018) carry out a large-scale experiment 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 43 percent. This suggests that BTB laws induce employers without information on applicants' criminal histories to statistically discriminate against demographic groups with a higher perceived probabilities of having criminal records.[9]

Doleac and Hansen (forthcoming) reach a similar conclusion using quasi-experimental methods. Drawing CPS data from 2004 to 2014 and exploiting temporal variation in BTB enactment across jurisdictions, the authors find that BTB laws decrease the employment of less-educated (no college degree) Hispanic men ages 25-to-34 by approximately 3 to 4 percent and African American men ages 15-to-34 by 5 percent.

[8] See also Mungan (2017) for a theoretical discussion.

[9] 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)

On the other hand, 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 those who more likely to have criminal records. 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. However, these authors do not explore whether criminal behavior itself could be affected by BTB laws.

Finally, three studies directly explore the relationship between BTB laws and employment of those with criminal records. Craigie (2017) uses individual-level self-reported data from the National Longitudinal Survey of Youth 1997 to estimate the relationship between public sector BTB laws and public sector employment among those with criminal convictions. Craigie (2017) finds that public BTB laws are associated with an increase in public sector employment among convicts. However, pre-treatment employment trends may not be parallel between BTB and non-BTB jurisdictions (see Figures 2 and 3, pp. 28-29); thus it is not clear whether the estimated policy impact should be causally interpreted.[10]

Two other studies use administrative data to overcome concerns with measurement error in self-reported crime data. Rose (2018) examines a Seattle BTB law and, using other cities in Washington as a counterfactual, finds that employment among those with criminal records did not change after the policy was enacted. Jackson and Zhao (2017a) study a BTB-inclusive

[10] In addition, public sector BTB laws may affect private sector employment (see Doleac and Hansen, Forthcoming and footnote 19), as well as criminal behavior.

reform in Massachusetts, the Criminal Offender Record Information (CORI), and find that this policy reduced employment for those who were ex-offenders prior to the reform.

Only two studies of which we are aware have examined the relationship between BTB laws and crime. However, both are case studies of particular state reforms and their findings are inconclusive. 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. Jackson and Zhao (2017b) extend their work on the impacts of the Massachusetts CORI reform to recidivism, and find evidence of a small decline in recidivism, though this effect appears to be driven by labor supply choices rather than labor demand responses.

3. Data

Our analyses use both administrative and survey data from two main sources — the National Incident-Based Reporting System (NIBRS) 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, and 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 (such as agencies that reported in at least half the years covering 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 race/ethnicity.[11]

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 report all of their crime data through the NIBRS (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. However, examining the effect of BTB laws on net crime seems a more relevant policy parameter given that crime committed by first-time offenders may be affected via statistical discrimination-induced employment reductions.

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

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

times higher for Hispanic and African Americans relative to non-Hispanic whites.[12] Across race/ethnicity crime rates are also higher for younger as compared to older individuals.

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-to-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 from self-reported criminal histories (i) may contribute to imprecision in estimates, and (ii) capture changes social stigma for criminal behavior, which could be affected by BTB laws (Doleac 2017). In addition, there is

[12] 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.

evidence that measurement error in crime in the NLSY is related to race/ethnicity (Kirk and Wakefield 2018).

Additionally, owing to the longitudinal cohort design, the age range in the NLSY97 does not perfectly coincide with the 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 definition also restricts the analysis period; hence, differential effects across the age distribution may also reflect heterogeneity over time and/or differences due to 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[13], (ii) *Minor Theft*, set equal to 1 if the respondent stole something worth less than $50 and 0 otherwise, (iii) *Major Theft*, set equal to 1 if the respondent stole something worth $50 or more and 0 otherwise, (iv) *Other Property Crime*, set equal to 1 if the respondent had fenced, possessed, received or sold stolen property, and 0 otherwise, (v) *Destroy Property*, set equal to 1 if the respondent "purposely damaged or destroyed property not belonging to [him/her]" and 0 otherwise, and (vi) *Assault*, set equal to 1 if the respondent had attacked or assaulted someone.

[13] 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.

We also construct a composite measure of *Any Property Crime*, capturing criminal activities (ii) through (iv).[14]

In Table 1B, we present weighted means for the variables from the NLSY97 for our analysis sample. Consistent with the patterns found in the NIBRS, we find that arrest rates are consistently highest among 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.[15,16]

Finally, to explore mechanisms through which BTB laws may affect crime, we draw data on less-skilled working age males with a high school diploma or less using repeated cross-sections of the American Community Survey (ACS) between 2005 and 2015 (corresponding to calendar years 2004 to 2014).[17] The ACS data, administered by the U.S. Census Bureau, are designed to provide estimates for smaller geographic units than states, including counties and

[14] 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.

[15] 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 1B), arrest 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).

[16] Note that arrest rates derived from the NLSY97 are an order of magnitude lower than those derived from the FBI's Uniform Crime Reports. This may reflect underreporting and measurement error as well as the NLSY sample being representative of the non-institutionalized population, excluding those who are under detention or incarcerated.

[17] We use the ACS data from the Integrated Public Use Microdata Series (http://usa.ipums.org/usa/).

census tracts. These data include information on several channels of interest, including employment and means-tested public program participation. Specifically, we measure whether the respondent was employed year round in the prior year (*Year-Round Employ*), weeks of employment in the prior year (*Weeks*), and, conditional on employment, the natural log of annual hours worked (*Annual Hours*). We also measure whether the respondent participated in means-tested public assistance programs (Supplemental Security Income, General Assistance, Supplemental Nutrition Assistance Program, or Temporary Assistance for Needy Families/Aid to Families with Dependent Children), with particular attention to the Supplemental Nutrition Assistance Program (SNAP) program. [18] Finally, we explore selective outmigration of Hispanic immigrants in response to BTB laws, using county-by-year data on the number of male Hispanic immigrants (from Mexico or Central America) with a high school diploma or less per 1,000 working-age population. The means of each of these potential channels are shown in Table 1C.

IV. Methods

[18] *Year-Round Employment* is a dichotomous variable set equal to 1 if the respondent worked for at least 50 weeks in the previous year and 0 otherwise. *Weeks* measures the total number of weeks the respondent worked in the previous year. Starting in 2008, the ACS reports weeks worked in intervals of 0 week, 1-13 weeks, 14-26 weeks, 27-39 weeks, 40-47 weeks, 48-49 weeks, and 50-52 weeks. For consistency, we assign the interval means for all the respondents. *Ln(Annual Hours)* is the natural log of the product of weeks worked in the previous year and usual weekly hours of work. *Public Program* is a dichotomous indicator set equal to 1 if (i) the respondent received income from Supplemental Security Income (SSI), Aid to Families with Dependent Children (AFDC), or General Assistance (GA) or (ii) any member of the respondent's household received benefits from Food Stamp or Supplemental Nutrition Assistance Program (SNAP) in the past year, and 0 otherwise. (Medicaid receipt is only available in the ACS data beginning in 2008. In alternate analyses over the period from 2008 through 2014, we find a pattern of results similar to that reported here.) In our sample, the average program participation rates are 26.7, 18.6 and 11.7 percent for working age African American men, Hispanic men and white men, respectively (Table 1C). We also focus on *SNAP*, set equal to 1 if the respondent or any member of his household received SNAP in the previous year, and 0 otherwise.

We begin by using agency-level month-specific crime data from the NIBRS from 2004 to 2014, focus on criminal incidents involving Hispanic or African American male arrestees, and estimate a Poisson model of the following form[19]:

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

where C_{jcst} is the crime count among males in agency *j* located in county *c* in state *s* at month-by-year *t* (in months from 1 to 132). Exposure for each unit is represented by E_{jcst}, which can be proxied by the estimated population served by the reporting agency. 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.[20] The vector $\mathbf{X}_{cst}$ includes county-level controls, including the share of population that was African American and Hispanic, the average age of the population, and the natural log of personal per capita income; the vector $\mathbf{Z}_{st}$ is a vector of state-level observables,

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[19] The Poisson model accommodates fixed effects well and does not suffer from the incidental parameters, in contrast to the negative binomial model. 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.

[20] 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. Our primary measure of a BTB law is whether any law was enacted, whether applying to the public or private sectors. As Doleac and Hansen (forthcoming, pp. 11-12) note:

> "Public BTB laws can affect both public and private sector employment. These policies were typically implemented due to public campaigns aimed at convincing employers to give ex-offenders a second chance. Public BTB policies were intended in part to model the best practice in hiring, and there is anecdotal evidence that this model – in combination with public pressure – pushed private firms to adopt BTB even before they were legally required to. Several national private firms such as Wal-Mart, Target, and Koch Industries, voluntarily "banned the box" on their employment applications during this period, in response to the BTB social movement….Public BTB laws might also affect private sector employment because workers are mobile between the two sectors, and likely sort themselves based on where they feel most welcome."

Twenty (20) of 111 BTB laws enacted between 2004 and 2014 bind for both public and private employers while the vast majority only bind for public employers. Results that separate out effects of each type of law are discussed below.

including the percent of the population ages 25 and over with a Bachelor degree, the natural log of per-capita police expenditures, the natural log of per capita police employees, right-to-carry laws, the minimum wage, the state EITC refundable credit rate, an indicator for Supplemental Nutrition Assistance Program (SNAP) all vehicle exemption, and a set of immigration policies (E-Verify mandates, 287(g) agreements, Secure Communities).[21] In addition, we also experiment with adding a control for the aggregate county-level unemployment rate. However, we are careful with this control given that it may partially capture a mechanism through which BTB laws may affect criminal behavior. Finally, α_j is a vector of agency fixed effects and τ_t is a set of month-by-year fixed effects. 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.

Next, we turn to self-reported longitudinal data from the NLSY97, and in our most saturated specification, estimate an individual fixed effects model of the following form:

$$C_{icst} = \beta_0 + \beta_1 BTB_{cst} + \mathbf{X}_{icst}\,\beta_2 + \mathbf{P}_{st}\,\beta_3 + \alpha_c + \nu_i + \tau_t + \varepsilon_{ist} \qquad (2)$$

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, and $\mathbf{X}_{icst}$ 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

[21] 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 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.

comparison with the NIBRS analysis, 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.[22] 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.

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. Figure 1 shows the counties in which a BTB law was enacted (either at the state, city, or county-level) from 2004 to 2014. Effective dates for these laws as well as the sources of identifying variation across our datasets is available in Appendix Table 2. In our NIBRS analysis sample, 69 counties contribute identifying variation; in the ACS, 157 counties contribute to identifying variation.[23]

The credibility of our identification strategy rests on the parallel trends assumption. We take a number of tacks to test the credibility of this assumption. First, we conduct an event study analysis where we examine if crime was trending differently in treatment versus comparison jurisdictions prior to the enactment of BTB laws:

$$C_{jcst} = E_{jcst}\,[exp(\gamma_0 + \sum_{j=-4, j\neq -1}^{3} \gamma_j\, BTB_{cst}^{j} + \mathbf{X}_{cst}\,\gamma_4 + \mathbf{Z}_{st}\,\gamma_5 + \alpha_j + \tau_t + \varepsilon_{jcst})] \qquad (3)$$

[22] The person fixed effects can also account for any (stable) reporting errors or biases across individuals.
[23] The data use agreement for the restricted NLSY97 data does not permit similar reporting of representation for individual counties. The identifying variation in the NLSY97 is on par with the ACS.

where $\text{BTB}_{\text{cst}}^{j}$ is a set of mutually exclusive indicators set equal to 1 if county *c* implemented a BTB law *j* years from year *t*. $\text{BTB}_{\text{cst}}^{-4}$ is an indicator for four or more years prior to BTB enactment, and $\text{BTB}_{\text{cst}}^{3}$ analogously accounts for three or more years following enactment.

Second, we test the sensitivity of estimates of β_1 to the inclusion of controls for county-specific quadratic time trends to disentangle the effects of BTB laws from unobservables trending at the county level:

$$C_{jcst} = E_{jcst}\,[exp(\beta_0 + \beta_1 BTB_{cst} + \mathbf{X}_{cst}\beta_2 + \mathbf{Z}_{st}\beta_3 + \alpha_j + \tau_t + \eta_c{*}t + \eta_c{*}t^2 + \varepsilon_{jcst})] \qquad (4)$$

Finally, we estimate placebo-type tests by focusing on populations whose criminal behavior should be less, or at least differently, affected by BTB laws: non-Hispanic white males and females. While these demographic groups might be affected by BTB laws through labor-labor substitution by employers, and could be impacted through moral hazard-related channels, the effects of BTB laws on crime for these groups are second-order, and hence should be smaller or of the opposite sign.

IV. Results

Tables 2 through 8 show our main results. We focus on estimates of β_1 in these tables. Parameter estimates on the control variables are available upon request. For all regressions, reported standard errors are clustered at the state-level (Bertrand et al. 2004; Doleac and Hansen, forthcoming). However, given the relatively small number of clusters in our NIBRS-based analysis (26 to 39 states), we also calculated p-values using a wild cluster bootstrap standard error approach (Cameron et al. 2008; Cameron and Miller 2015). The results of this exercise did not qualitatively change our findings (see footnote 23).

4.1 Main Results

Table 2 shows findings from our fixed effects Poisson regression. Panel I shows estimates of β_1 including only agency fixed effects, month-by-year fixed effects and county demographic characteristics as controls; Panel II adds county- and state-specific economic and crime policy variables to the right hand side of the regression; and Panel III adds county- and state-specific social welfare and immigration policy controls. Across each of the panels, we find that the magnitudes of estimates of β_1 are similar, supportive of the validity of the research design and providing suggestive evidence that BTB laws are enacted exogenously to male criminal arrests.

Focusing on our preferred specification in Panel III, we find that BTB laws are associated with an 8.1 percent [$e^{(0.078)} - 1$] increase in the total number of criminal incidents (property plus violent crimes) involving Hispanic male arrestees (column 1, row 1). This effect is driven by property crimes (row 2), where we find a 12.0 percent increase in such incidents. An examination of the age distribution of Hispanic male arrestees reflects that the property crime increases are largest for working age individuals over the age of 25 (Panel III, columns 3 and 4), those who have been found to suffer employment loss in response to BTB laws (Doleac and Hansen, forthcoming).[24] These results are consistent with the hypothesis that BTB laws induce statistical discrimination by potential employers that reduce employment among low-skilled racial and ethnic minorities (Agan and Starr 2018; Doleac and Hansen, forthcoming).

[24] Our estimates of wild cluster bootstrapped standard errors (Cameron et al. 2008; Cameron and Miller 2015) in columns (3) and (4) generated p-values of 0.089 and 0.090, respectively.

These findings could also conceivably reflect moral hazard by reducing the expected future costs of current criminal engagement. For instance, Bamberger and Donohue (1999) find that workplace discipline practices involving "last chance agreements," which govern discharged employees' reinstatement and reduce their costs of wrongdoing, can, via moral hazard, lead to more wrongdoing and discharges. Moreover, empirical evidence in other contexts, including workers' compensation (Fortin and Lanoie 2000), health insurance (Dave and Kaestner 2009; Dave et al. 2019) and automobile insurance (Cohen and Dehejia 2004), is generally supportive of moral hazard effects.

In contrast to our finding that BTB laws are associated with an increase in property crime for Hispanic men, we find little evidence that BTB laws affect violent criminal incidents, where effects are uniformly smaller. This result suggests that economically motivated crimes are most affected by BTB laws, consistent with an employment related channel.

The absence of property (and violent) crime arrest effects for non-Hispanic white males (columns 9 through 12), a population that does not suffer adverse employment effects from BTB laws (Doleac and Hansen, forthcoming), suggests that the crime effects we observe for Hispanic males are not simply capturing jurisdiction-specific time-varying unobservables. Moreover, this finding also persists after controlling for county-level unemployment rates (see Appendix Table 3).

Interestingly, however, when we examine African American men (columns 5 through 8), we find no evidence that BTB laws affect property or violent crime arrests. This finding persists even among those ages 25-to-34, a population for whom Doleac and Hansen (forthcoming) found evidence of statistical discrimination-induced employment declines. This raises an important question: Why might there be differential criminal responses to BTB laws for Hispanic as compared to African American men?

One explanation may be that less educated Hispanics denied job opportunities due to BTB laws are more likely to be on the margin of crime commission than are similarly situated African American men. This could be due to differences in familial or personal circumstances that affect the need for economic resources. For example, according to the 2015 American Community Survey, the average household size for less educated (high school diploma or less) Hispanic men ages 25 and older is 4.0 compared to 2.5 for similarly aged and educated African American men. Moreover, there are twice as many children under age 18 residing in households with less-skilled Hispanic men as compared to less-skilled African American men (1.1 versus 0.5). On the other hand, the poverty rate among less-educated Hispanic men is substantially lower than for less-educated African American men (18 versus 25 percent).

In addition, barriers to participation in means-tested public assistance programs may deepen the negative economic effects of BTB laws. For instance, there is evidence that language barriers may be an important deterrent to take-up of means-tested public assistance programs among Hispanics (Dillender 2017). Federal law requires that individuals must be US citizens or lawful permanent residents to qualify for public assistance programs such as the Supplemental Nutrition Assistance Program (SNAP), Supplemental Security Income (SSI), Temporary Assistance for Needy Families (TANF), and Medicaid (Siskin 2016).[25] In addition, eligible immigrant families may be deterred from benefits due to fears of disqualification for citizenship or deportation (Kaushal et al. 2014, Perreira et al. 2012). For instance, Kaushal et al. (2014) document higher prevalence of food insecurity and lower rates of SNAP participation among

[25] An exception to this immigration status rule is emergency medical service for unauthorized aliens.

eligible mixed-status Mexican families with both citizen and non-citizen members than eligible Mexican families with all noncitizen members, a result consistent with the hypothesis of immigration fears.[26]

In Table 3, we use the ACS to examine the impact of BTB laws on employment and means-tested public assistance receipt. In the first three panels, we confirm the CPS-based findings of Doleac and Hansen (forthcoming) using the ACS. We find that BTB laws are associated with a 2 to 6 percent reduction in the probability of prior year full-year employment, annual weeks of work, and annual work hours among less educated Hispanic men ages 25 and older and less educated African Americans ages 25-to-34.

Reintegration after leaving prison is difficult for many reasons, notably low income levels, greater material hardship, and lower levels of physical and mental health immediately following incarceration. Studies have highlighted the important role played by SNAP benefits (and other safety net programs) in reducing material hardship and supporting reentry among formerly incarcerated individuals.[27] Our estimates in Table 3 indicate that while less educated African American males are more likely to participate in means-tested public assistance programs, primarily the SNAP program, following the enactment of BTB laws, less educated Hispanic men are not. These findings could suggest that barriers to program participation, including language differences and immigration fears, deepen the negative economic consequences of BTB laws, leading to more property crime among Hispanic relative to African American men.

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[26] In addition, Kaushal et al. (2014) find that the 2004 U.S. Department of Agriculture outreach to inform Mexican immigrants of their SNAP eligibility and benefit expansion under the American Recovery and Reinvestment Act improve SNAP enrollment among mixed-status Mexican families.

[27] See, for instance, McKernan et al. (2018) and Western (2018). Also see: https://www.cbpp.org/sites/default/files/atoms/files/3-6-18fa.pdf and https://thehill.com/opinion/criminal-justice/395405-strong-safety-net-is-crucial-to-americans-in-life-after-prison.

BTB laws could also change arrest rates among Hispanic men if they induce out-migration (or in-migration) of Hispanic men, or if they affect the immigrant-native composition of Hispanic men. Prior research finds that likely undocumented Hispanic immigrants' mobility decisions are more elastic with respect to local labor market conditions than are mobility decisions of low-skilled natives (Cadena and Kovak 2016). Moreover, there is evidence that likely undocumented immigrants are less likely to commit crime than natives (Ewing et al. 2015; Chaflin 2015; Butcher and Piehl 2008). In the final row of Table 3, we examine the impact of BTB laws on the share of the working age population that are less-educated Hispanic males (columns 1 through 4), less-educated male Hispanic immigrants (columns 5 through 8), and less-educated male Hispanic immigrants from Mexico and Central America (columns 9 through 12). Consistent with Doleac and Hansen's (2017) findings, we find no evidence that BTB laws affect the share of the county population that are Hispanic males. While BTB laws are negatively related to the share of Hispanic immigrants in the state (columns 5 through 12), these estimates are not significantly different from zero at conventional levels.

4.2 Plausibility of Magnitudes of Crime Effects

Our estimates thus far suggest that BTB laws led to a significant increase in criminal incidents involving working-age Hispanic male arrestees. This is driven primarily by property crimes, with the effect magnitudes specifically indicating an approximately 10 to 15 percent increase among Hispanic males ages 25-34. How plausible is such an effect size? We explore this question in two ways. First, Doleac and Hansen (forthcoming) use the CPS and estimate that BTB laws reduced employment among low-educated Hispanic males ages 25-34 by 2 to 5 percentage-points (or between 3-6 percent); an effect size we confirm with data from the ACS.

In addition, we also detect effects on the weeks of work margin. Lin (2008) finds that a 1 percentage-point increase in unemployment increases property crime by up to 4 percent. In this context, a BTB-induced increase in property crime of approximately 10 percent among Hispanic men is not implausible. Second, a 3 to 4 percent decline in employment would imply that about 98,000 to 131,000 fewer Hispanic males find jobs. Our estimates on crime for this population imply an additional 1,600 to 2,400 property crime arrests.[28] Assuming a stable population base, we can impute the marginal effect of unemployment on property crime arrests, which is 0.017, indicating that for about every additional 60 low-educated males who remain unemployed there is one additional property crime arrest.[29] This compares to an average probability of an arrest among Hispanic males relative to being unemployed, of about 0.097.[30] Hence, the marginal probability (0.017) implied by our estimates is reasonable given that one would expect it to be lower than the average if the crime production function is concave.[31]

4.3 Event Study Analyses

Next, we explore whether the criminal arrest effects of BTB laws are masking heterogeneous short- and longer-run effects. In Table 4, we present coefficients on the year of BTB enactment, each of the two years following the law's enactment, and three or more years following the law's enactment. For Hispanic men ages 25 and older, we find that property crime

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[28] In 2014, there were 4.022 million Hispanic males between the ages of 25-34 with less than a college degree (based on data from the ACS), with 3.272 million of them employed. In the same year, there were a total of 198,718 property crime male arrestees. Our data indicate that about 8.1% of property crime arrestees are Hispanics, implying a total of 16,096 such arrests among Hispanic males ages 25-34.

[29] Taking the midpoint of the range of estimates, the marginal effect of being unemployed (employed) on property crime arrest is: (2000/115000) = 0.017 (-0.0174).

[30] Our data from the NLSY97 indicate that 9.7 percent (12.2 percent) of low-educated Hispanic males reported being arrested for any offense (engaging in any property crime) over the past 1-2 years.

[31] Corman et al. (2014) exploit welfare reform to impute a similar marginal effect of employment on property crime for low-educated women, and find this magnitude to be 0.03.

arrests increase starting one year after the adoption of a BTB law, with larger magnitudes in the shorter-run (one and two years after the law change). Note, however, that because different jurisdictions identify long-run and short-run coefficients, these differences could also be explained by heterogeneous treatment effects. For African American and non-Hispanic white males, there is little evidence that BTB laws are associated with increases in property or violent crimes in both the short- and long-run.

In Figure 2, we present event study analysis of the effect of BTB laws on property crime arrests for Hispanic men and non-Hispanic white men, with one year prior to the enactment of the BTB law as the reference period. We focus on criminal incidents involving male arrestees ages 25 and older, the age group for whom we find evidence of BTB-induced increases in property crime.[32] Our findings in Figure 2 show no evidence that property or violent crime arrests among Hispanic and non-Hispanic white males were trending differently in treatment and comparison jurisdictions prior to the enactment of BTB laws. Consistent with the findings in Table 4, the event study in Panel A shows that BTB laws are associated with increases in property crimes for Hispanic males following the law's enactment, a pattern of findings consistent with a causal impact of BTB laws. For Non-Hispanic white men (Figure 2, Panel B), our results suggest no evidence of an increase in property crime following the implementation of BTB laws.

In Panel C of Figure 2, we present an event study analysis from a difference-in-difference-in-differences-type model that examines the effect of BTB laws on property crimes committed by Hispanic males relative to non-Hispanic white males. This approach will control for unmeasured jurisdiction-level heterogeneity common across race/ethnicity groups. The event

[32] Separate event studies for 25-to-34 and 35-to-64 year-olds show a similar pattern of results and are available upon request.

study in Panel C continues to show increases in property crime following the enactment of BTB laws for Hispanic males ages 25 and older relative to similarly aged non-Hispanic white men.

In Figure 3, we show comparable event studies for violent crime. We continue to find little evidence that violent crimes were trending differently in treatment and comparison jurisdictions prior to the adoption of BTB laws. We find no evidence that violent criminal incidents involving Hispanic or non-Hispanic white males changed following the enactment of BTB laws. Finally, in Figure 4, we present event studies for African American men. Consistent with our results in Table 4, we find little evidence of significant changes in property crime or violent crime involving African American male arrestees following the enactment of BTB laws, though the effects are imprecisely estimated, especially in the longer-run.

4.4 Sensitivity Tests

In Table 5, we conduct a number of robustness checks of the above findings. In Panel I, we use a larger sample of agencies that report crime data for more than five years during the sample period between 2004 to 2014. Consistent with our findings in Table 2, our estimates suggest BTB laws are associated with a 12 to 14 percent increase in arrests among Hispanic men ages 25 and older, driven by property crime arrests.

In Panel II of Table 5, we restrict the sample to those agencies reporting at least one crime per period, effectively focusing on locales with relatively higher shares of each demographic group. Using ordinary least squares (OLS) models with the dependent variable redefined as the natural log of the crime rate per 1,000 population, we find a qualitatively similar pattern of results to those presented above. In Panel III of Table 5, we explore the sensitivity of our estimates to aggregating our balanced panel to the agency-by-year level. Unsurprisingly, the results are very similar to those using monthly agency-level crime data in Table 2.

In the final panel of Table 5 (Panel IV) we explore the robustness of our findings to the inclusion of controls for county-specific time trends in our two-way fixed effects specification. The results continue to point to evidence that BTB laws are associated with an increase in property crime among Hispanic men ages 25 and older. Event study analyses based on this more saturated specification, shown in Figure 5, are consistent with a causal interpretation of results.

Next, we explore whether the property crime effects of BTB laws extend to females, a population for whom there is less evidence of statistical discrimination (Doleac and Hansen, forthcoming), but for whom moral hazard or labor-labor substitution is still possible. In Figure 6, we present event study analyses for females. The results from these event studies show no evidence that BTB laws are associated with changes in property crime for Hispanic, African American, or non-Hispanic white females.[33]

Together, the NIBRS-based results show that BTB laws have the unintended consequences of increasing property crimes committed by Hispanic males ages 25 and older. Table 6 examines the specific crimes driving this result. We find BTB laws are associated with a 15 to 18 percent increase in larcenies among working age individuals ages 25 and older, and a 22 percent increase in burglaries for those ages 25 to 34. Only for Hispanic men under age 25 is there evidence of BTB-induced increases in motor vehicle theft. For African American and non-

[33] Event study analyses for violent crime, available upon request, show little evidence that BTB laws significantly affected female violent crime.

Hispanic white males, we find little evidence of changes in specific crimes in response to BTB laws. [34, 35]

4.5 NLSY97 Results

In our final section, we explore the effects of BTB laws on crime using individual-level data from the NLSY97. In Table 7, we examine the impact of BTB laws on self-reported arrests, with odd-numbered columns showing results from individual fixed effects models and even-numbered columns showing results from models that include both individual and county fixed effects.[36] We find that BTB laws are associated with an increase in the probability of being arrested, on the order of about one to two percentage points, though these estimates are very imprecise for the broader age group of 19-to-34. In the main, our findings are largely consistent with our NIBRS-based findings and suggest that BTB laws have unintended consequences that increase criminal activity among working age Hispanic males.

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

[34] In results available upon request, we also examine whether the crime effects of BTB laws differ by the breadth of their statutory coverage. During our sample period, the vast majority of enacted BTB laws applied only to public employers. However, these laws may also affect private firms by causing them to voluntarily "ban the box" or through worker mobility between public and private sectors (Doleac and Hansen, forthcoming). Consistent with their results, we find that the property crime effect for Hispanic men is driven by BTB laws applying to public employers.

[35] Because of the limited coverage of the NIBRS across the United States, we also explored data from the Uniform Crime Reports (UCR) over the period from 2004 to 2014. 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. While the UCR would permit greater policy variation for identification, the chief disadvantage of the UCR is that these data do not allow for disaggregation of adult crimes by age, gender and race/ethnicity. Because the impacts of BTB laws differ across these dimensions (Doleac and Hansen, forthcoming) and do not measure ethnicity (e.g. Hispanic identification) of arrestees, we relegate this analysis to Appendix Table 4.

[36] The latter controls account for both spatial and individual (time-invariant) heterogeneity, including any fixed factors that affect locational choices and sorting.

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). While models in Panel II of Table 7 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 both 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 8, we estimate the effects of these laws on active criminal participation based on reported engagement 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.[37] Among Hispanic males, there is a suggestive increase in property crime on the order of about 0.6 (ages 19-to-34) to 1.8 (ages 19-to-26) percentage points, an approximately 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 1.1 percentage-points), but increased major theft (defined in the

[37] As noted above, this is due to the change beginning in Round 8 in the universe of respondents who report on their criminal behaviors.

NLSY97 as stealing anything valued at $50 or greater) and other property crime (receiving and selling stolen merchandise, for instance), 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.[38]

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 no evidence that BTB is associated with an increase in the probability of attacking or assaulting someone for younger Hispanic males.

There is less evidence of statistical discrimination among female minorities in the literature (Doleac and Hansen, forthcoming), 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 6 generally confirm this prediction across all measures of criminal behavior from the NLSY97.

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

[38] In Appendix Table 5, we present results with controls for county fixed effects at the level of the policy variation (county of residence). These models conserve degrees of freedom, and are very similar in terms of both patterns and magnitudes, and in some cases a bit more precise over the main models that control for both person and county fixed effects.

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.[39] One might expect larger crime effects for less educated individuals for whom Doleac and Hansen (forthcoming) 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 all of the effect is driven by less-than-high school educated males, consistent with the hypothesis that statistical discrimination may be an important mechanism at work.[40,41]

V. Conclusions

Advocates for BTB laws argue that such laws may serve to increase labor market opportunities for ex-offenders and reduce incentives for criminal activity. However, several

[39] 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.

[40] 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.

[41] 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 pct. points decline in private sector employment (p-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.

recent high-quality studies suggest that these laws may have an important unintended consequence of statistical discrimination in employment against male racial and ethnic minorities. This study comprehensively examines the effect of state and local BTB laws on criminal incidents involving racial and ethnic minorities.

Using data drawn from the National Incident-Based Reporting System, we find that BTB laws are associated with a 10 percent increase in criminal incidents involving working-age Hispanic men, driven by property crimes. This result is consistent with economically motivated crimes due to statistical discrimination-driven diminished employment opportunities and, perhaps, moral hazard. However, we find no evidence of BTB-induced increases in property crime involving working age African American men, a population that also faces diminished job opportunities due to statistical discrimination. Supplemental analysis using the American Community Survey (ACS) suggest a possible explanation for this race/ethnicity-specific difference in crime response: barriers to public assistance receipt among Hispanics.

The magnitudes of our effects are plausible given employment and moral hazard-related channels and suggest that BTB laws are associated with an important unintended consequence that may generate important social costs. Using per-offense social cost of property crime from McCollister et al. (2010), we obtain back-of-the-envelope additional costs of $401 million (2018 dollars) for property crime involving Hispanic men ages 25 and older.[42] The findings from this

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[42] Data on property crimes committed over the 2004-2014 period are obtained using the FBI's *Crime in the United States* reports (available from: https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-1). We then use the 2004-2014 UCR's *Arrests by Age, Sex, and Race* files to calculate the share of property crime arrests involving men ages 25-to-64. To generate an estimate of the number of crimes committed by men ages 25-to-64, we multiply the crime counts in the 2004-2014 period from the FBI's *Crime in the United States* report with the share of property crime arrests involving men ages 25-to-64 from the UCR's *Arrests by Age, Sex, and Race* files. Next, we estimate the number of crimes committed by Hispanic men ages 25-to-64 by multiplying the estimated crimes committed by men ages 25-to-64 with the percent of arrests involving Hispanic male adults (available at https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-43). Using our findings from Table 2 (Panel III, columns 3 and 4) where we find BTB is associated with a 15.0 to 15.8 percent increase in property crimes among Hispanic males ages 25-to-64, we estimate 69,790 additional property crimes

study add to some emerging evidence that BTB laws, while well-intentioned, may harm certain individuals that they are intended to benefit by further perpetuating the cycle of criminality.

following the enactment of BTB laws. Finally, we use the per crime cost of a property offense of $5,739 (in 2018 dollars) from McCollister et al. (2010) to obtain the total BTB-induced property crime cost of $401 million.

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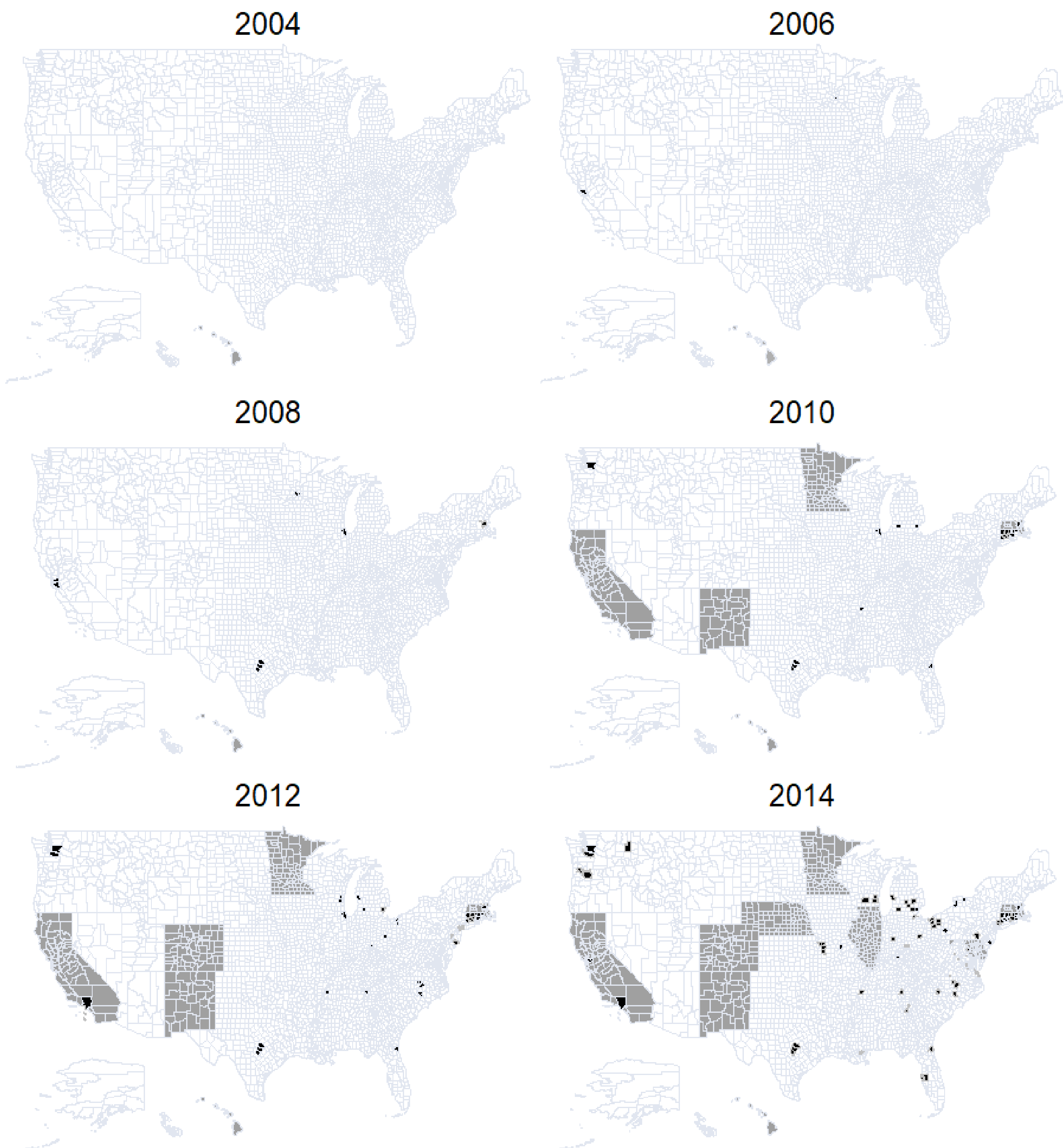
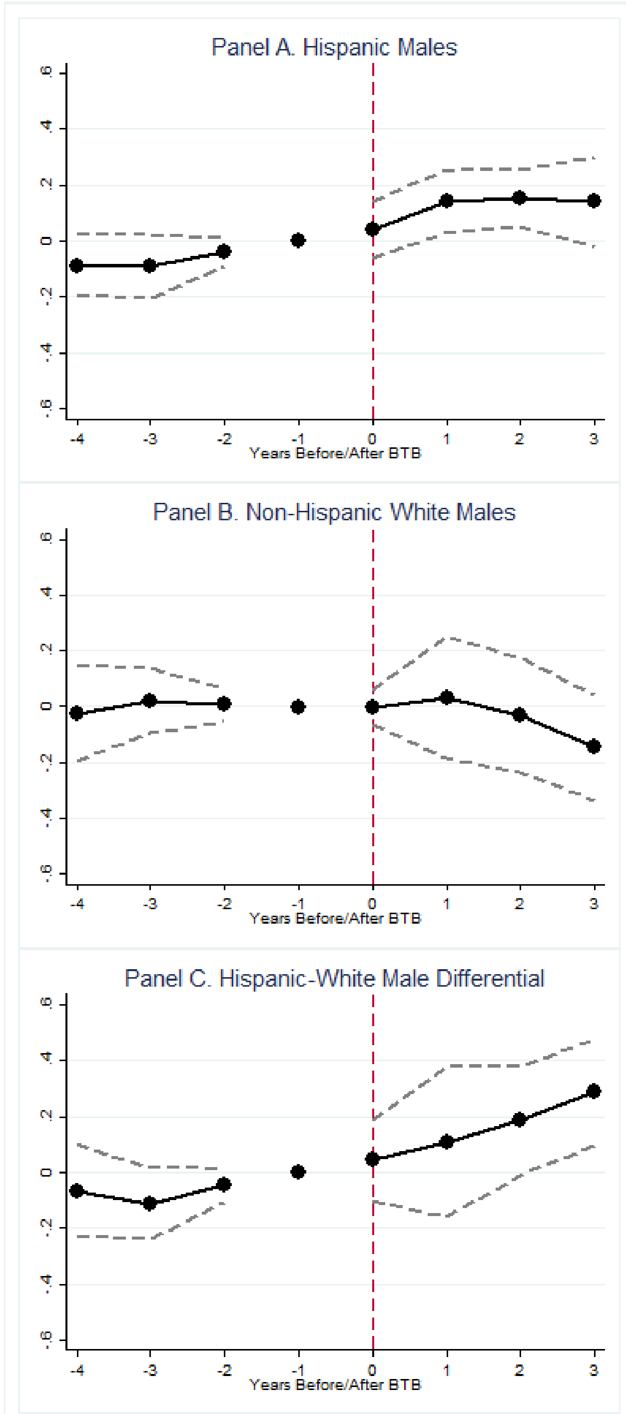
Figure 1. Enactment of Ban the Box Laws, 2004-2014



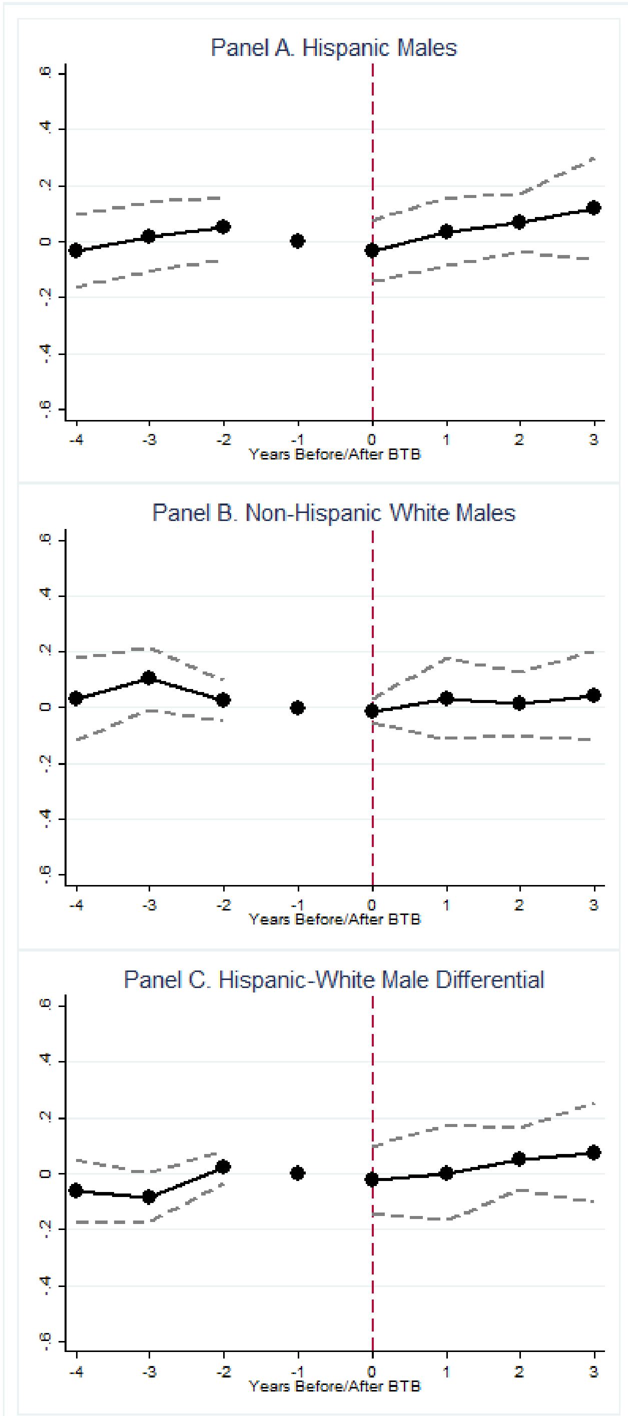
Notes: Enactment years obtained from Doleac and Hansen (Forthcoming). State BTB laws are shaded in gray. County and city BTB laws are shaded in black.

Figure 2. Event Study Analysis of BTB Laws and Property Crime Arrests for Hispanic and Non-Hispanic White Men, Ages 25-to-64



Notes: Estimates are generated using data from the 2004 to 2014 National Incident-Based Reporting System. Dash lines are 95 percent confidence intervals. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level.

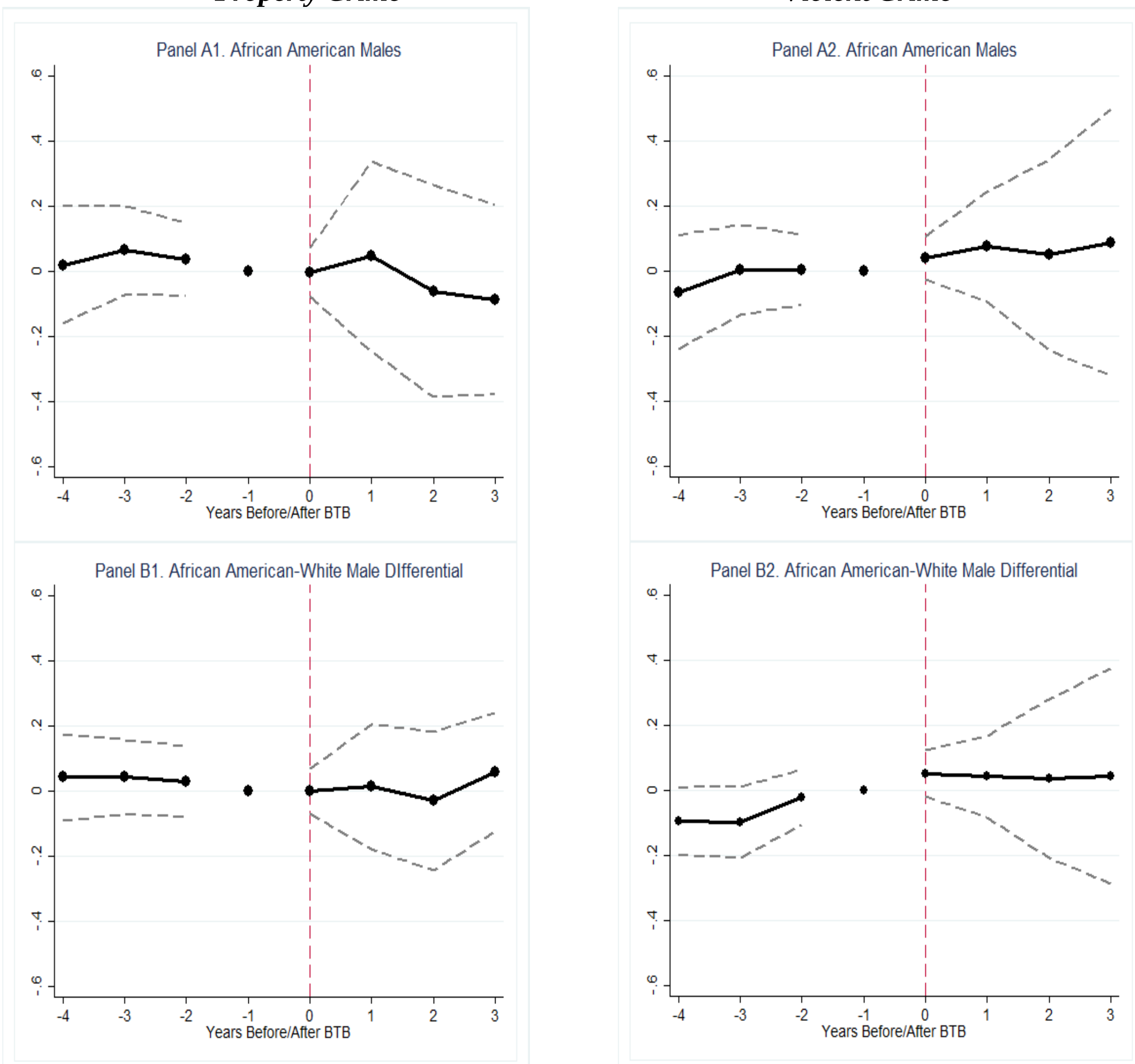
Figure 3. Event Study Analysis of BTB Laws and Violent Crime Arrests for Hispanic and Non-Hispanic White Men, Ages 25-to-64



Notes: Estimates are generated using data from the 2004 to 2014 National Incident-Based Reporting System. Dash lines are 95 percent confidence intervals. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level.

Figure 4. Event Study Analysis of BTB Laws and Arrests for African American Men, Ages 25-to-64

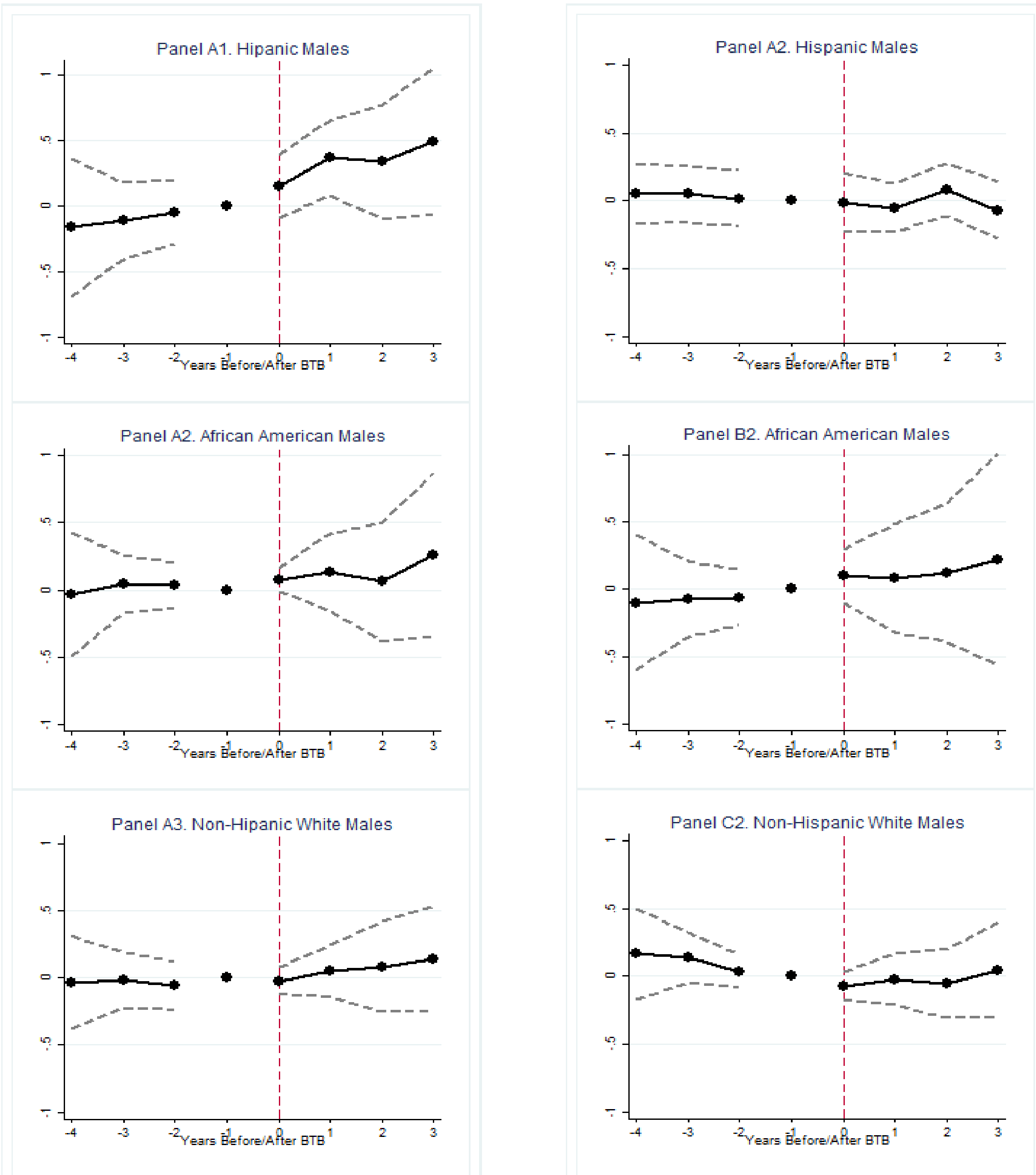
Property Crime ***Violent Crime***



Notes: Estimates are generated using data from the 2004 to 2014 National Incident-Based Reporting System. Dash lines are 95 percent confidence intervals. Each regression has controls for agency fixed effects, time fixed effects, a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level. The imprecise estimates of the lagged effects of BTB laws for African American men are due to the small number of counties (18 to 36 counties or 93 to 114 agencies) that contribute to the identification.

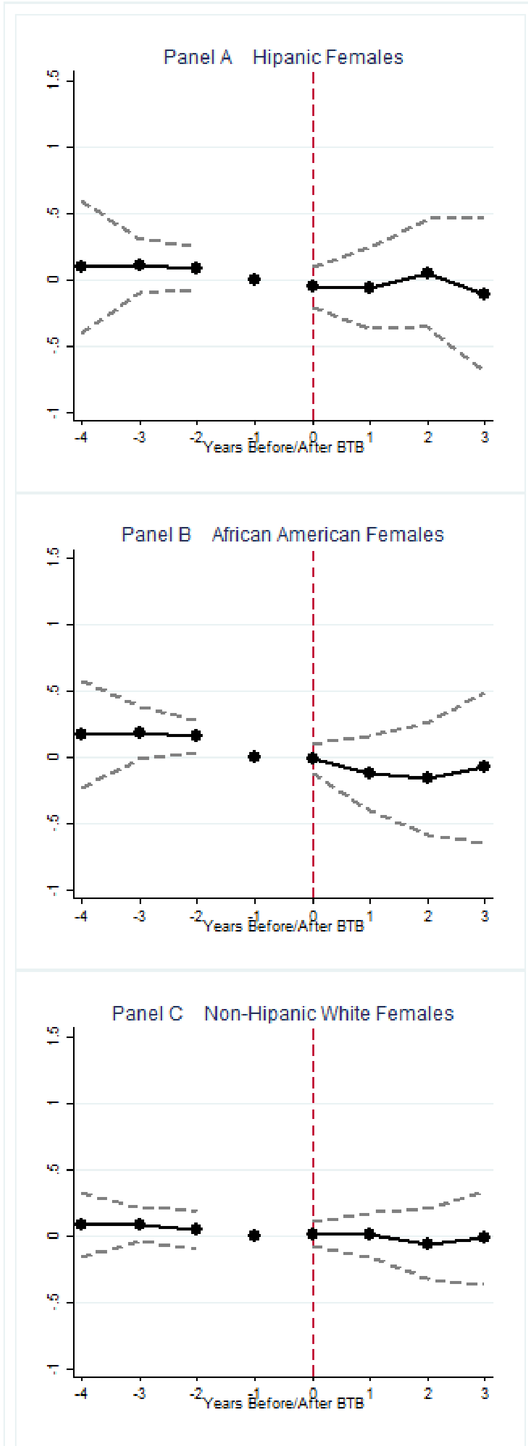
Figure 5. Event Study Analysis of BTB Laws and Male Arrests with Controls for County-Specific Time Trends, Ages 25-to-64

Property Crime ***Violent Crime***



Notes: Estimates are generated using annual agency-level data from the 2004 to 2014 National Incident-Based Reporting System. Dash lines are 95 percent confidence intervals. Each regression has controls for agency fixed effects, year fixed effects, county-specific quadratic time trends, a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level. The imprecise estimates of the lagged effects of BTB laws for African American men are due to the small number of counties (18 to 36 counties or 93 to 114 agencies) that contribute to the identification.

Figure 6. Event Study Analysis of BTB Laws and Female Property Crime Arrests, Ages 25-to-64



Notes: Estimates are generated using annual agency-level data from the 2004 to 2014 National Incident-Based Reporting System. Dash lines are 95 percent confidence intervals. Each regression has controls for agency fixed effects, year fixed effects, county-specific quadratic time trends, a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities).

Standards errors are clustered at the state level. The imprecise estimates of the lagged effects of BTB laws for African American men are due to the small number of counties (18 to 36 counties or 93 to 114 agencies) that contribute to the identification.

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

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*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)
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. Because each incident includes information on up to 3 arrestees of potentially the same race/ethnicity that may differ by age, the sum of crime counts for each individual age group may exceed the count for the pooled sample.

Table 1B. Crime Rates and Selected Characteristics, NLSY97

Sample	*Hispanic Males*			*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*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
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	0.025	0.029	0.010	0.025	0.030	0.008	0.017	0.020	0.005
crime	(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	0.186	0.207	0.144	0.205	0.237	0.140	0.096	0.106	0.074
school	(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 1C. Labor Market and Demographic Outcomes, ACS

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Annual Employment	0.612	0.418	0.669	0.658	0.419	0.240	0.453	0.479	0.572	0.362	0.633	0.617
	(0.487)	(0.493)	(0.47)	(0.474)	(0.493)	(0.427)	(0.498)	(0.5)	(0.495)	(0.481)	(0.482)	(0.486)
Weeks	37.666	28.818	41.013	39.371	26.586	18.689	29.12	28.817	35.333	28.021	39.142	36.439
	(20.141)	(22.372)	(17.687)	(19.45)	(23.605)	(21.579)	(23.11)	(23.878)	(21.4)	(21.525)	(19.135)	(21.468)
Annual Hours	1843.71	1489.43	1893.08	1938.68	1670.88	1159.30	1692.89	1840.90	1875.35	1295.36	1938.25	2026.75
	(707.21)	(825.39)	(658.66)	(649.01)	(823.21)	(839.47)	(792.4)	(753.28)	(817.91)	(890.01)	(761.57)	(732.78)
Public Program	0.186	0.192	0.201	0.176	0.269	0.292	0.29	0.252	0.123	0.111	0.168	0.114
	(0.389)	(0.394)	(0.401)	(0.381)	(0.443)	(0.455)	(0.454)	(0.434)	(0.328)	(0.314)	(0.374)	(0.317)
SNAP	0.184	0.191	0.199	0.173	0.263	0.289	0.284	0.244	0.118	0.109	0.163	0.108
	(0.387)	(0.393)	(0.399)	(0.378)	(0.44)	(0.453)	(0.451)	(0.43)	(0.323)	(0.311)	(0.369)	(0.311)
N	*745,604*	*157,438*	*189,023*	*399,143*	*440,432*	*93,191*	*83,177*	*264,064*	*1,856,073*	*327,979*	*290,116*	*1,237,978*

	Low-Skilled Hispanic Men				*Low-Skilled Hispanic Immigrant Men*				*Low-Skilled Mexican and Central American Immigrant Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Hispanic Men per 1,000	64.53	13.35	18.40	32.78	30.52	4.54	10.33	15.64	27.78	4.14	9.67	13.97
Population Ages 18-64	(58.11)	(11.92)	(15.87)	(31.23)	(28.08)	(4.31)	(9.41)	(15.59)	(27.65)	(4.21)	(9.28)	(15.37)
	4,095	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*

Notes: Weighted means are generated using data drawn from the 2005 to 2015 American Community Survey (ACS). Standard deviations are in parentheses.

Table 2. Estimated Relationship Between Ban-the-Box Laws and Arrests, NIBRS

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel I: Baseline Estimates with Demographic Controls												
Total crime	0.089*	0.044	0.126**	0.116*	0.006	0.018	0.002	0.004	0.025	0.038	0.069	-0.036
	(0.052)	(0.055)	(0.061)	(0.062)	(0.114)	(0.093)	(0.123)	(0.131)	(0.058)	(0.053)	(0.064)	(0.064)
Property crime	0.116**	0.053	0.152**	0.179***	-0.049	-0.025	-0.059	-0.053	0.038	0.052	0.087	-0.043
	(0.056)	(0.059)	(0.070)	(0.068)	(0.120)	(0.094)	(0.127)	(0.138)	(0.069)	(0.061)	(0.076)	(0.079)
Violent crime	0.041	0.061	0.081	-0.020	0.119	0.117	0.111	0.137	0.020	0.032	0.055	0.007
	(0.060)	(0.067)	(0.066)	(0.065)	(0.121)	(0.113)	(0.130)	(0.125)	(0.049)	(0.048)	(0.050)	(0.060)
Panel II: Panel I + Crime & Economic Controls												
Total crime	0.079	0.044	0.115*	0.086	0.008	0.023	-0.007	0.009	0.006	0.038	0.034	-0.063
	(0.051)	(0.051)	(0.060)	(0.056)	(0.119)	(0.101)	(0.122)	(0.135)	(0.070)	(0.057)	(0.081)	(0.079)
Property crime	0.117**	0.060	0.153**	0.163***	-0.033	-0.005	-0.052	-0.038	0.025	0.057	0.058	-0.066
	(0.054)	(0.051)	(0.068)	(0.063)	(0.127)	(0.103)	(0.131)	(0.143)	(0.080)	(0.062)	(0.093)	(0.096)
Violent crime	0.003	0.028	0.050	-0.074	0.076	0.074	0.061	0.108	-0.023	-0.006	-0.002	-0.037
	(0.064)	(0.076)	(0.064)	(0.064)	(0.110)	(0.106)	(0.115)	(0.115)	(0.052)	(0.055)	(0.054)	(0.055)
Panel III: Panel II + Social Welfare & Immigration Policy Controls												
Total crime	0.078	0.050	0.112*	0.076	0.014	0.028	-0.000	0.013	-0.004	0.023	0.020	-0.064
	(0.052)	(0.052)	(0.062)	(0.057)	(0.113)	(0.094)	(0.117)	(0.131)	(0.066)	(0.054)	(0.073)	(0.075)
Property crime	0.113**	0.066	0.140**	0.147**	-0.020	0.006	-0.037	-0.029	0.011	0.041	0.037	-0.067
	(0.055)	(0.052)	(0.067)	(0.065)	(0.121)	(0.097)	(0.124)	(0.140)	(0.075)	(0.058)	(0.084)	(0.091)
Violent crime	0.009	0.028	0.067	-0.073	0.068	0.065	0.055	0.101	-0.021	-0.016	0.011	-0.035
	(0.064)	(0.077)	(0.066)	(0.062)	(0.106)	(0.100)	(0.113)	(0.111)	(0.052)	(0.056)	(0.052)	(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*

*** 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. Demographic controls include the percentage of county population that are male, African American, Hispanic, county-level average age, and share of state population ages 25+ with a bachelor degree. Economic controls include the log of state personal per capita income, minimum wages, and refundable EITC rates. Crime policy controls include logs of nominal police expenditure per capita, police employment per capita, and shall issue laws. Social welfare & immigration policy controls include SNAP vehicle exemption, E-verify, 287(g) program, and Secure Communities. Standards errors are clustered at the state level.

Table 3. Ban-the-Box Laws, Labor Market Outcomes and Immigrant Mobility, ACS

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Annual Employment	-0.009	-0.015*	-0.015*	-0.007	-0.005	0.005	-0.024**	-0.003	0.002	-0.004	0.012**	-0.001
	(0.006)	(0.008)	(0.008)	(0.007)	(0.004)	(0.008)	(0.010)	(0.006)	(0.003)	(0.008)	(0.005)	(0.003)
Weeks	-0.160	-0.893**	-0.241	-0.083	-0.256	0.156	-1.441***	0.009	0.073	-0.227	0.357	0.026
	(0.257)	(0.350)	(0.353)	(0.278)	(0.244)	(0.549)	(0.476)	(0.235)	(0.170)	(0.502)	(0.230)	(0.120)
Ln(Annual Hours)	-0.019*	-0.048*	-0.010	-0.019**	-0.016	-0.009	-0.042	-0.009	-0.006	-0.020	0.012	-0.007
	(0.010)	(0.025)	(0.010)	(0.008)	(0.016)	(0.034)	(0.028)	(0.012)	(0.006)	(0.017)	(0.010)	(0.004)
Public Program	0.010	0.007	0.006	0.012	0.022***	0.008	0.028**	0.024***	0.001	-0.006	-0.000	0.002
	(0.007)	(0.012)	(0.007)	(0.008)	(0.006)	(0.010)	(0.011)	(0.006)	(0.004)	(0.006)	(0.006)	(0.004)
SNAP	0.010	0.006	0.006	0.012	0.020***	0.010	0.024**	0.021***	0.001	-0.005	0.001	0.002
	(0.007)	(0.012)	(0.007)	(0.007)	(0.006)	(0.010)	(0.010)	(0.006)	(0.004)	(0.006)	(0.006)	(0.004)
N	*745,604*	*157,438*	*189,023*	*399,143*	*440,432*	*93,191*	*83,177*	*264,064*	*1,856,073*	*327,979*	*290,116*	*1,237,978*

	Low-Skilled Hispanic Men				*Low-Skilled Hispanic Immigrant Men*				*Low-Skilled Mexican and Central American Immigrant Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Men per 1,000	-0.253	-0.231	-0.062	0.040	-0.947	-0.437	-0.448	-0.061	-0.704	-0.412	-0.329	0.037
Population Ages 18-64	(0.951)	(0.277)	(0.648)	(0.210)	(1.036)	(0.327)	(0.629)	(0.276)	(1.012)	(0.329)	(0.613)	(0.248)
	4,095	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*	*4,095*

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

Notes: Weighted OLS estimates are generated using data drawn from the 2005 to 2015 American Community. Each regression has controls for county fixed effects, time fixed effects, a set of individual-level controls (age, marital status, and number of children in the household), a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level.

Table 4. Lagged Effects of BTB Laws on Arrests, NIBRS

	Total Crime				*Property Crime*				*Violent Crime*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel I: Hispanic Men												
Year of law change	0.041	0.062	0.071	0.007	0.076	0.088*	0.114	0.054	-0.034	0.017	0.008	-0.114*
	(0.051)	(0.046)	(0.069)	(0.054)	(0.052)	(0.048)	(0.074)	(0.063)	(0.071)	(0.079)	(0.083)	(0.068)
1 year after	0.083	0.000	0.121*	0.129**	0.127**	0.025	0.132*	0.239***	-0.002	-0.041	0.106*	-0.098
	(0.053)	(0.056)	(0.062)	(0.063)	(0.059)	(0.061)	(0.070)	(0.072)	(0.062)	(0.072)	(0.063)	(0.075)
2 year after	0.128**	0.097	0.165***	0.107*	0.164***	0.114	0.212***	0.165**	0.060	0.070	0.080	0.008
	(0.057)	(0.069)	(0.062)	(0.063)	(0.060)	(0.070)	(0.071)	(0.070)	(0.064)	(0.088)	(0.063)	(0.078)
3 plus years after	0.122*	0.073	0.153*	0.127	0.127*	0.028	0.151*	0.190*	0.115	0.185*	0.151	0.022
	(0.071)	(0.068)	(0.086)	(0.094)	(0.074)	(0.067)	(0.089)	(0.107)	(0.081)	(0.112)	(0.097)	(0.096)
Panel II: African American Men												
Year of law change	-0.002	0.005	0.009	-0.002	-0.018	0.015	-0.016	-0.037	0.022	-0.018	0.041	0.080
	(0.061)	(0.048)	(0.063)	(0.072)	(0.067)	(0.055)	(0.069)	(0.074)	(0.056)	(0.047)	(0.064)	(0.070)
1 year after	0.052	0.057	0.027	0.065	0.028	0.040	0.008	0.033	0.092	0.091	0.070	0.120
	(0.135)	(0.116)	(0.134)	(0.155)	(0.154)	(0.121)	(0.154)	(0.182)	(0.112)	(0.121)	(0.112)	(0.102)
2 year after	-0.006	0.016	-0.035	-0.020	-0.069	-0.057	-0.104	-0.064	0.096	0.141	0.064	0.070
	(0.169)	(0.147)	(0.180)	(0.189)	(0.178)	(0.151)	(0.180)	(0.200)	(0.167)	(0.160)	(0.191)	(0.165)
3 plus years after	0.002	0.054	-0.051	-0.020	-0.071	-0.021	-0.118	-0.098	0.138	0.206	0.052	0.162
	(0.181)	(0.161)	(0.188)	(0.207)	(0.160)	(0.142)	(0.168)	(0.180)	(0.228)	(0.212)	(0.223)	(0.254)
Panel III: Non-Hispanic White Men												
Year of law change	0.001	0.032	0.029	-0.054	0.019	0.049	0.051	-0.050	-0.045	-0.028	-0.017	-0.061
	(0.039)	(0.034)	(0.044)	(0.045)	(0.045)	(0.037)	(0.051)	(0.053)	(0.035)	(0.038)	(0.044)	(0.041)
1 year after	0.030	0.048	0.059	-0.024	0.057	0.076	0.078	-0.007	-0.013	-0.020	0.047	-0.033
	(0.091)	(0.080)	(0.107)	(0.096)	(0.100)	(0.086)	(0.118)	(0.113)	(0.070)	(0.068)	(0.080)	(0.065)
2 year after	-0.015	0.012	0.018	-0.090	-0.003	0.026	0.039	-0.100	-0.002	0.015	0.001	-0.028
	(0.086)	(0.073)	(0.093)	(0.097)	(0.100)	(0.074)	(0.107)	(0.122)	(0.067)	(0.083)	(0.070)	(0.069)
3 plus years after	-0.077	-0.044	-0.087	-0.136	-0.089	-0.044	-0.101	-0.189	-0.002	-0.017	0.017	0.016
	(0.087)	(0.076)	(0.085)	(0.109)	(0.098)	(0.082)	(0.096)	(0.127)	(0.087)	(0.091)	(0.076)	(0.103)
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 share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level.

Table 5. Sensitivity of Relationship Between Ban-the-Box Laws and Arrests, NIRBS

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel I: Restricting to Agencies Reporting in at Least Half the Sample Period (6 Years)												
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)
	527,357	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*	*527,357*
Panel II: Positive Crime Estimates Using OLS Specification												
Property crime	0.078***	0.046	0.071*	0.088**	-0.012	-0.024	-0.021	-0.025	-0.032	-0.006	-0.006	-0.037
	(0.028)	(0.028)	(0.036)	(0.031)	(0.026)	(0.029)	(0.040)	(0.034)	(0.022)	(0.018)	(0.025)	(0.023)
N	*35,337*	*23,451*	*17,915*	*13,470*	*62,660*	*43,050*	*31,282*	*37,634*	*132,808*	*98,055*	*83,151*	*78,472*
Violent crime	-0.000	-0.026	0.023	-0.009	0.031	0.005	0.025	0.050	-0.013	-0.024	0.001	-0.008
	(0.026)	(0.037)	(0.038)	(0.041)	(0.036)	(0.034)	(0.036)	(0.033)	(0.018)	(0.018)	(0.015)	(0.019)
N	*22,448*	*12,385*	*12,182*	*8,337*	*44,707*	*27,941*	*24,257*	*23,989*	*87,523*	*45,245*	*45,640*	*51,706*
Panel III: Annual Agency-Level Male Crime												
Property crime	0.121*	0.055	0.151**	0.177**	-0.030	-0.006	-0.055	-0.037	0.005	0.035	0.030	-0.079
	(0.067)	(0.066)	(0.073)	(0.079)	(0.143)	(0.114)	(0.148)	(0.165)	(0.086)	(0.065)	(0.098)	(0.106)
Violent crime	0.000	0.008	0.060	-0.074	0.083	0.087	0.072	0.106	-0.025	-0.023	0.006	-0.038
	(0.070)	(0.079)	(0.078)	(0.075)	(0.123)	(0.116)	(0.131)	(0.130)	(0.061)	(0.066)	(0.063)	(0.064)
N	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*
Panel IV: Annual Agency-Level Male Crime with County-Specific Time Trends												
Property crime	0.119*	0.107	0.182**	0.109	-0.003	0.002	-0.013	0.012	0.008	0.001	0.034	-0.38
	(0.072)	(0.074)	(0.091)	(0.107)	(0.052)	(0.059)	(0.077)	(0.070)	(0.036)	(0.024)	(0.067)	(0.045)
Violent crime	-0.016	-0.059	0.060	-0.066	0.023	-0.040	0.042	0.082	-0.030	-0.072	-0.023	-0.072
	(0.079)	(0.068)	(0.097)	(0.104)	(0.068)	(0.066)	(0.073)	(0.095)	(0.056)	(0.071)	(0.054)	(0.071)
N	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*	*20,284*

*** 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 share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level.

Table 6. Estimates of Relationship Between Ban the Box Laws and Specific Property Crime Arrests, NIBRS

	Hispanic Men				*African American Men*				*Non-Hispanic White Men*			
Ages	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*	*All*	*18-24*	*25-34*	*35-64*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Larceny-theft	0.130**	0.079	0.139*	0.167**	-0.046	-0.001	-0.076	-0.054	0.015	0.047	0.047	-0.075
	(0.063)	(0.059)	(0.072)	(0.079)	(0.123)	(0.090)	(0.130)	(0.143)	(0.076)	(0.058)	(0.087)	(0.094)
	[0.336]	[0.168]	[0.107]	[0.081]	[1.063]	[0.435]	[0.235]	[0.440]	[2.293]	[1.008]	[0.742]	[0.684]
Motor vehicle theft	0.085	0.197***	0.117	-0.114	0.022	0.090	-0.069	-0.077	0.003	-0.002	0.012	-0.016
	(0.057)	(0.076)	(0.075)	(0.113)	(0.194)	(0.202)	(0.207)	(0.152)	(0.104)	(0.066)	(0.131)	(0.141)
	[0.030]	[0.018]	[0.010]	[0.005]	[0.119]	[0.061]	[0.032]	[0.033]	[0.201]	[0.086]	[0.071]	[0.058]
Burglary	0.052	-0.024	0.197**	0.066	0.088	0.075	0.129	0.113	0.008	0.050	0.023	-0.069
	(0.062)	(0.073)	(0.094)	(0.071)	(0.105)	(0.091)	(0.094)	(0.132)	(0.080)	(0.072)	(0.092)	(0.090)
	[0.079]	[0.043]	[0.027]	[0.015]	[0.364]	[0.194]	[0.097]	[0.100]	[0.641]	[0.302]	[0.229]	[0.169]
Arson	0.025	0.097	-0.370	0.142	0.130	0.159	-0.040	0.232*	0.021	0.060	-0.170	0.103
	(0.148)	(0.190)	(0.305)	(0.396)	(0.161)	(0.146)	(0.209)	(0.132)	(0.130)	(0.166)	(0.264)	(0.081)
	[0.003]	[0.001]	[0.001]	[0.001]	[0.009]	[0.004]	[0.003]	[0.003]	[0.027]	[0.013]	[0.007]	[0.009]
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 share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level. Means of dependent variables are in brackets.

Table 7. Estimates of Relationship between Ban-the-Box Laws and Probability of Arrest, NLSY97

	Hispanic Men		***African American Men***		***Non-Hispanic White Men***	
			Panel I: Ages 19-34			
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	0.020	0.011	-0.003	-0.008	-0.011*	-0.003
	(0.014)	(0.013)	(0.014)	(0.015)	(0.006)	(0.006)
N	*7114*	*7114*	*8621*	*8621*	*16736*	*16736*
			Panel II: Ages 19-26			
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	0.053**	0.053**	-0.021	-0.028	-0.007	0.004
	(0.025)	(0.022)	(0.024)	(0.028)	(0.018)	(0.022)
N	*4188*	*4188*	*5055*	*5055*	*10034*	*10034*
			Panel III: Ages 27-34			
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	0.001	-0.003	-0.003	0.006	-0.012	-0.008
	(0.019)	(0.020)	(0.018)	(0.021)	(0.011)	(0.013)
N	*2926*	*2926*	*3566*	*3566*	*6702*	*6702*
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	No	Yes	No	Yes	No	Yes

*** 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 share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level and reported in parentheses.

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

	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	0.006	0.018	0.043	0.0003	-0.033	-0.010	0.005	0.024	-0.033
	(0.017)	(0.071)	(0.035)	(0.014)	(0.027)	(0.024)	(0.021)	(0.045)	(0.028)
Minor Theft	-0.011	-0.033	0.0001	0.001	-0.002	-0.015	0.005	0.038	-0.031
	(0.012)	(0.042)	(0.021)	(0.0103)	(0.022)	(0.017)	(0.013)	(0.036)	(0.022)
Major Theft	0.008	0.018	0.010	0.0004	0.004	0.008	0.006	-0.008	0.016
	(0.011)	(0.053)	(0.023)	(0.008)	(0.017)	(0.019)	(0.012)	(0.030)	(0.020)
Other Property Crime	0.019	0.032	0.010	-0.004	-0.016	-0.014	-0.006	0.011	-0.014
	(0.021)	(0.050)	(0.033)	(0.014)	(0.034)	(0.018)	(0.006)	(0.021)	(0.015)
Destroy Property	-0.006	-0.015	0.036	-0.001	-0.018	0.015	0.009	-0.017	0.040
	(0.014)	(0.040)	(0.045)	(0.020)	(0.036)	(0.024)	(0.021)	(0.026)	(0.039)
Assault	0.005	-0.001	0.041	-0.011	-0.053	-0.050	-0.012	-0.031	0.030
	(0.022)	(0.043)	(0.072)	(0.027)	(0.058)	(0.044)	(0.015)	(0.034)	(0.021)
N	*3090*	*1769*	*1321*	*4120*	*2332*	*1788*	*6806*	*4131*	*2675*

*** 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 wave fixed effects, county fixed effects, individual fixed effects, indicators for age, educational attainment (high school graduate, some college, college graduate or above), a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communitie

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

	NIBRS	***NLSY97***	***ACS***
	(1)	(2)	(3)
BTB	0.080 (0.271)	0.113 (0.317)	0.193 (0.383)
Police expenditure per capita	241.17 (39.95)	280.16 (83.56)	308.07 (81.74)
Police employment per capita (in 1,000)	2.197 (0.434)	2.293 (0.628)	2.289 (0.620)
Shall-issue law	0.818 (0.386)	0.679 (0.467)	0.664 (0.472)
College degree	0.299 (0.061)	0.295 (0.050)	0.273 (0.961)
Share of male	0.493 (0.013)	0.492 (0.012)	0.490 (0.009)
Share of African American	0.100 (0.132)	0.135 (0.141)	0.126 (0.111)
Share of Hispanic	0.060 (0.067)	0.140 (0.149)	0.149 (0.136)
Average age	38.39 (2.766)	36.857 (2.373)	37.308 (2.624)
State personal per capita income	39,448.44 (7,284.40)	39,568.15 (6,183.47)	42,639.66 (11,274.86)
Minimum wage	6.712 (1.002)	6.678 (1.010)	7.166 (0.937)
EITC	0.050 (0.801)	0.065 (0.114)	0.065 (0.110)
E-verify	0.183 (0.377)	0.160 (0.360)	0.217 (0.408)
287(g) program	0.011 (0.103)	0.052 (0.213)	0.061 (0.230)
Secure Communities	0.323 (0.468)	0.237 (0.410)	0.468 (0.485)
SNAP Vehicle exemption	0.824 (0.374)	0.724 (0.439)	0.790 (0.402)
N	*243,804*	*68,951*	*1,392,610*

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

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

Source: Doleac and Hansen (2020).

Appendix Table 3. Sensitivity of Estimates to Added Control for County 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)
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 4. Estimates of Relationship Between Ban-the-Box Laws and Arrests, UCR

	African American Adults Ages 18+	*White Adults Ages 18+*
	(1)	(2)
Total crime	-0.019	-0.022
	(0.070)	(0.032)
	[39.60]	[51.95]
Property crime	-0.017	-0.022
	(0.080)	(0.037)
	[25.48]	[33.35]
Violent crime	-0.021	-0.013
	(0.056)	(0.027)
	[14.12]	[18.60]
N	*188,848*	*188,848*

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

Notes: Poisson estimates are generated using a balanced panel of agencies and months in jurisdictions with population exceeding 25,000 from the 2004-2014 Uniform Crime Reports (Anderson 2014). Each regression has controls for agency fixed effects, time fixed effects, county-specific quadratic time trends, and a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the number of reporting agencies, the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level. Means of the dependent variable are in brackets.

Appendix Table 5. Sensitivity Checks of Relationship between Ban-the-Box Laws and Arrests with only County FE, NLSY97

	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	0.010	0.006	0.040*	0.002	-0.0001	0.003	-0.014	-0.011	-0.011
	(0.026)	(0.083)	(0.023)	(0.010)	(0.015)	(0.015)	(0.013)	(0.032)	(0.013)
Minor Theft	-0.009	-0.025	0.002	0.002	-0.0001	0.003	-0.014	-0.011	-0.011
	(0.010)	(0.040)	(0.015)	(0.010)	(0.015)	(0.015)	(0.013)	(0.032)	(0.013)
Major Theft	0.011	0.036	0.005	-0.001	0.005	0.006	-0.003	-0.015	0.009
	(0.010)	(0.041)	(0.015)	(0.007)	(0.009)	(0.015)	(0.009)	(0.025)	(0.014)
Other Property Crime	0.030*	0.024	0.002	0.006	0.016	-0.009	-0.011	0.001	-0.010
	(0.018)	(0.037)	(0.022)	(0.010)	(0.011)	(0.013)	(0.011)	(0.012)	(0.013)
Destroy Property	-0.003	0.003	0.033	-0.005	-0.021	0.009	-0.004	-0.023	0.025
	(0.023)	(0.039)	(0.035)	(0.016)	(0.022)	(0.016)	(0.013)	(0.022)	(0.026)
Assault	0.002	0.009	0.006	-0.018	-0.034	-0.018	-0.014	-0.036**	0.011
	(0.025)	(0.032)	(0.043)	(0.029)	(0.048)	(0.028)	(0.016)	(0.017)	(0.014)
N	*3090*	*1769*	*1321*	*4120*	*2332*	*1788*	*6806*	*4131*	*2675*

*** 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 wave fixed effects, county fixed effects, indicators for age, educational attainment (high school graduate, some college, college graduate or above), a set of state-level controls (including share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). Standards errors are clustered at the state level and reported in parentheses.

Appendix Table 6. Estimates of Relationship between Ban-the-Box Laws and Criminal Behaviors among Females, NLSY97

	Females	
Ages	*19-34*	*19-26*
	(1)	(2)
Arrested	-0.005	-0.014
	(0.004)	(0.009)
Any Property crime	-0.002	-0.003
	(0.011)	(0.023)
Minor Theft	0.002	0.010
	(0.008)	(0.023)
Major Theft	0.008	0.017
	(0.007)	(0.017)
Other Property Crime	0.0002	0.002
	(0.003)	(0.005)
Destroy Property	-0.004	-0.009
	(0.010)	(0.013)
Assault	-0.002	0.0004
	(0.010)	(0.025)

*** 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 share of state population ages 25+ with a bachelor degree, nominal minimum wages, refundable EITC rates, nominal police expenditure per capita, police employment per capita, Shall issue laws, SNAP vehicle exemption, and E-verify), and county-level controls (the percentage of the population that are male, African American, Hispanic, average age, nominal personal per capita income, 287(g) program, and Secure Communities). 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.