Every Day Low Crime

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Abstract

We study the effect of job opportunities on incarceration using nationwide data on applicants to Walmart's hourly positions. We employ a regression discontinuity design using scores from a hiring assessment: three distinct score thresholds give applicants above each cutoff a discontinuously greater chance of hire. Those marginally hired at Walmart are 70% less likely to go to jail in the year following their assessment, dropping incarceration rates to near zero. This effect is driven by applicants at the lowest score threshold and by reductions in violent crimes. Incapacitation due to time employed accounts for at most one-third of the observed effect. Owing to the large decrease in violent crimes, each marginal hire saves about $7,200 in costs to victims, offenders, and the criminal justice system. These findings demonstrate that access to employment significantly reduces criminal activity among those seeking work, and especially for the most marginal job applicants.

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1 Introduction

The connection between crime and economic conditions is a central question in social science. In 2008, violent crime rates were about 12 times higher among cities in the highest decile of poverty rates compared to those in the lowest decile (Lofstrom and Raphael, 2016). Even within cities, the highest crime rates are concentrated in the poorest neighborhoods (Sampson et al., 2002; Lofstrom and Raphael, 2016; Wilson, 1987). A major aspect of this poverty is joblessness: criminal defendants have scant legal earnings both before and after periods of incarceration. Over one in three prisoners in 2016 were not employed in the month prior to their arrest, and those previously incarcerated were even less likely to be employed (Maruschak and Snell, 2023). A large portion of defendants do not even have documented W-2 earnings before their cases (Garin et al., 2025). Labor market outcomes for the formerly incarcerated are also strikingly abysmal: unemployment rates for ex-prisoners are an order of magnitude higher than the national average (Carson et al., 2021). Such weak formal labor market attachment suggests that joblessness is a key driver of crime. Would crime fall if this population had better access to jobs?

Credible causal evidence answering this question is harder to find. Standard OLS techniques face an endogeneity problem, as, for example, incarceration itself may reduce chances of employment and lead to simultaneity bias.[1] Potentially confounding variables, like human capital or socioemotional skills, that affect both crime and employment outcomes also present identification issues (Heckman et al., 2013; Deming, 2017). Early estimates of this relationship using 2SLS methods found an elasticity between unemployment rate and property crime of about one, but did not find a meaningful relationship with respect to violent crime (Levitt, 2001; Gould et al., 2002; Raphael and Winter-Ebmer, 2001). This conclusion has largely not changed since. Ludwig and Schnepel (2025) relay the current consensus: job and transfer programs generally lead to reductions in property crime, but violent crime is

[1] Raphael and Winter-Ebmer (2001) give a few examples of omitted variables bias: alcohol consumption, drug use, and gun ownership may vary procyclically and also affect crime rates.

much more resistant to such policies. Given that violent crimes account for the bulk of the social costs of crime, the benefits of these programs resulting from crime reductions seem disappointingly low.

This paper leverages nationwide data on about 2.4 million unique applicants to Walmart between 2022 and 2025 to study the effects of employment opportunity on criminal behavior. Walmart data allows us to detect effects on crime in a population that is more representative of the median entry-level worker seeking low-skill jobs.[2] Specifically, we exploit a feature in Walmart's application process that exogenously gives certain applicants a greater chance of hire. All applicants must take an assessment prior to submitting applications to hourly in-store positions, which produces a score that is used to categorize applicants. This mechanism creates three discontinuities in hire probability as applicants achieve scores to surpass category thresholds. We use this score in a fuzzy regression discontinuity design.

To observe future criminal outcomes, we merge applicant data with jail population data collected by the NYU Jail Data Initiative (JDI), which contains jail roster data for facilities from around 1,300 counties. The JDI data contains individual-level demographic information, jail admission dates, inferred jail time, and the charges associated with each booking. We link Walmart data to these jail rosters using name and date-of-birth (DOB).

We present four key findings. First, employment opportunity at Walmart substantially cuts incarceration rates. Crossing each score threshold immediately gives applicants a discontinuously greater chance of hire at Walmart. These discontinuities in hire rates range between 5 and 7 percentage points (p.p.); employment differences endure for at least two years. At the same time, jail bookings drop by 0.6 p.p. for the threshold consisting of the lowest-scoring applicants, and by lesser amounts (not statistically significant) for the other two thresholds. Pooled across all discontinuities, receiving a job at Walmart reduces the likelihood of ever being in jail within a year of the assessment by 3.0 p.p. among the com-

[2]The average Walmart applicant is less criminally active than those studied in job program RCTs and is older than those studied in SYEPs. The median age of new Walmart hires from 2022 to 2025 was about 23 years. The average age was about 30 years.

pliers (about 70 percent of the control complier mean). Though the standard error for this estimate is large, we can reject effects smaller than 0.7 p.p. at the 5 percent level. There are similar reductions in the number of jail bookings and the number of days spent in jail in the year following the assessment.

Second, there is important heterogeneity across worker quality, as measured by the different effects across discontinuities. The pooled effect is concentrated on the lowest-scored threshold, implying that employment has stronger effects for workers that are less likely to receive jobs at Walmart. The effect on jail bookings is large and significant at the 1 percent level for the lowest-scoring discontinuity, while the other two discontinuities have estimates closer to zero. Responses to application questions reveal that this low-scoring population is much less likely to have finished high school or college. Such results are consistent with studies that find that the benefits of employment and education opportunities are concentrated in the highest-risk subgroups (Deming, 2011; Kessler et al., 2022).

Third, we find significant decreases to violent crimes. Across all discontinuities, the number of violent crimes decreases by about 77%, accounting for about 40% of the total effect on jail spells. In addition, the number of crimes with an unknown charge type decreases by about 76%. Though we do not know the nature of these crimes, they likely consist of more severe, violent crimes, as their average incarceration length is even greater than that of violent crimes (52 days vs. 43 days). Together, these more severe crime types account for about two-thirds of the total effect. That violent and serious crimes fall greatly with employment opportunities at Walmart, let alone constitute the majority of the entire effect on total criminal activity, marks a sharp contrast with much of the literature.

Lastly, we find that "staying busy" explanations are unlikely to fully explain the observed effects. The reduction in crime could be due to "incapacitation," in that people employed do not commit crime as they stay busy at their jobs. In this case, people do not respond to economic incentives, but rather mechanically reduce criminal activity with non-working less time. A calculation using days employed at Walmart demonstrates that incapacitation

while working only explains about one-third of the observed effect, suggesting that jobs may affect crime through mechanisms that last even after employment spells. Such mechanisms may include future incapacitation channels (more time worked at other jobs due to Walmart employment), earnings channels (higher-paying jobs due to Walmart employment), or human capital channels (through improved socioemotional skills), though our data limits us from distinguishing these. Changes in behavior by police (i.e., issuing a citation instead of formally booking into jail) also do not explain our results.

These results contribute to a longstanding literature on the effects of employment on crime. We provide evidence using individual-level data that job opportunities reduce crime from low-skill job seekers. Studies of job training, targeted job assistance, and subsidized jobs programs that use randomization as a source of exogenous variation find mixed results, though many report reductions to property crime (Bloom et al., 1997; Bloom, 2010; Millenky et al., 2011; Uggen, 2000; Redcross et al., 2010, 2012; Bollinger and Yelowitz, 2021). Generally, only the more expensive job training programs, like the intensive Job Corps program, led to better employment and criminal outcomes (Schochet et al., 2008).

When studies do find effects on crime, they are limited to improvements in property crime (Ludwig and Schnepel, 2025). Aside from direct changes to employment, evidence also suggests that access to more formal income reduces illicit income-generating activities among disadvantaged populations. Agan and Makowsky (2023) find reduced recidivism among ex-offenders following minimum wage increases, concentrated on decreases in property and drug crimes. Studies using regression discontinuity designs also report that a reduction in benefit receipt results in an increase in illicit income-generating activities, providing evidence that crime is responsive to economic incentives (Deshpande and Mueller-Smith, 2022; Khanna et al., 2023). Other research on cash transfer and in-kind benefit programs similarly conclude that property crimes briefly decline following cash transfers and tend to increase with the days since SNAP disbursement (Watson et al., 2020; Foley, 2011).

One exception to this conclusion arises from summer youth employment programs (SYEPs).

SYEPs are social service programs offered in various cities that provide (usually minimum wage) employment to high-school youth during the summer, an age group with a relatively high crime rate during a period with extra idle time (Heller, 2014; Davis and Heller, 2020; Modestino, 2019; Gelber et al., 2016; Kessler et al., 2022). These programs have been effective at reducing violent crime among disadvantaged youth, though it is unclear how these effects generalize to a larger population seeking entry-level jobs.

The above studies reflect predictions of theories of crime tracing back to Becker (1968), which posit that potential criminal actors respond to the expected costs and benefits of crime in a rational way. In particular, these theories model that people choose between a formal wage and a lottery from criminal activity, which depends on expected gain, chances of apprehension, and expected punishment. Without access to a formal wage, or if this wage is not sufficiently high, people rationally choose crime. Though this reasoning sensibly applies to property crime, the connection to violent crime is less clear. Rather, there is a long history of characterizing violent crimes as "crimes of passion," wherein perpetrators are influenced by strong emotional shocks (e.g., Card and Dahl, 2011; Heller et al., 2017). Programs that reduce violent crime without improving post-program outcomes, like SYEPs or Medicaid[3], complicate this understanding. As our results are driven by reductions in violent crimes, other mechanisms are needed to fully understand why violent crimes fall with employment.

Our results also contribute to the multidisciplinary discussion regarding the ability of local economic opportunity to reduce criminal activity. The extent to which improving employment affects crime depends on whether the beneficiaries are actually marginal in pursuing crime when jobless. Theories on urban crime sparked by sociologist William Julius Wilson argue that the effects of joblessness on crime are mediated by social ties, employment networks, and neighborhood characteristics (Wilson, 1996, 1987; Sampson et al., 1997). Those already embedded in communities with social cohesion and high employment may not

[3]Quasi experimental evidence shows that loss of Medicaid coverage leads to increases in violent and property crimes among men with mental health histories (Jácome, 2024), though experimental evidence finds null effects (Finkelstein et al., 2025).

be pushed toward violent crime without jobs (Wilson, 1996). Though our data speaks less to social conditions, this reasoning could explain why the population studied in this paper realizes such large effects on violent crimes: applicants actively seeking low-skill jobs may best represent this margin of committing violent crime. Those with ample opportunity need not depend on a chance at a Walmart job to avoid crime, while particularly disadvantaged populations from other studies (usually ex-prisoners) may need substantial employment assistance to be deterred from violent crime. In our context, higher levels of firm concentration in retail occupations are related to larger effect sizes, hinting that access to more jobs could reduce the importance of jobs specifically at Walmart. Studies by criminologists also suggest that this margin is represented by job seekers: there are large effects of background check clearance on arrests for those already provisionally hired at low-wage jobs (Denver et al., 2017),[4] but little effects on recently incarcerated participants of transitional jobs programs (Redcross et al., 2010).

Among Walmart applicants, the most marginal workers are those that receive the lowest scores on the assessment, as they face the most difficulty in maintaining employment at large. These applicants have the highest jail rates, highest high school dropout rates, and lowest self-reported rates of holding a job for more than a year. That crime reductions are concentrated on the lowest-scoring applicants supports the hypothesis that job seekers more loosely attached to the formal labor market exhibit greater changes in criminal activity due to employment. As a result, this paper provides evidence that employment could profoundly reduce violent criminal activity among job seekers at the margins of the labor market.

This paper is organized as follows. Section 2 describes the background regarding the application process and assessment for Walmart positions. Section 3 describes the data and merge process. Section 4 presents the identification strategy and main results. Section 5 discusses potential mechanisms and concludes.

[4]On average, the most recent arrest for participants was eight years prior to their provisional hire.

2 Background

Walmart operates more than 4,600 stores and employs about 1.5 million workers throughout the United States. The vast majority (1.2 million) of these workers are hourly, in-store employees. For these hourly positions, Walmart receives applications from several million candidates per year. This section describes the application process for hourly, in-store positions and the source of variation for hire probability across applicants.

2.1 Application Process

Every applicant to Walmart applies through the Walmart Careers website. There, the website prompts new applicants to select roles that interest them, complete their profile, and subsequently submit applications to positions. After completing their profile and before submitting applications, applicants must complete a roughly 50 question assessment that evaluates their quality as a candidate, which outputs a quantitative score not observed by the applicant or hiring manager. This score is then used to categorize applicants into quality groups, separated by threshold values. As applicants must complete their profile prior to completing the assessment, we obtain information (name, location, and date of birth) on all applicants prior to their interaction with the assessment, mitigating selection concerns.

In more detail, all applicants complete the following steps:

1. The applicant can either create their account or begin their application and defer profile creation to later in the process. The applicant submits their location and views a list of nearby stores. After selecting one or more stores, the applicant is prompted to select job postings at the selected store(s), which gives more detail on each job's title, pay range, employment type (full-time vs. part-time), and available shifts.

2. After selecting one or more jobs, the applicant is asked about their desired employment type and preferred availability.

3. The site prompts the applicant to create an account. They are asked for their name, email, zip code, birth month and day, last six digits of their SSN, and phone number. The applicant answers several more questions including: student status, minor status, visa status, military service, voluntary questions about race and gender, prior job experience, and receipt of government assistance (TANF/SNAP).

4. At this point, the applicant profile is complete. Before submitting applications to any jobs, applicants must take an assessment that asks questions about their background, work history, and preferred work style. This assessment is immediately evaluated with a numeric score that is not observed by the applicant.

An applicant may fail this assessment if their score is below a particular threshold. If this happens, they will be notified that they may not apply to jobs until they retake the assessment with a passing score, which can only occur again after a thirty-day waiting period.[5] If the applicant passes, they are allowed to submit applications to available jobs.

From July 2022 to January 2025, about 80% of assessments resulted in a passing score. 84% of those who passed applied to at least one job within a day of taking the assessment, and 90% applied to at least one job within 30 days of the assessment. The median applicant applied to seven jobs within 30 days of the assessment, though many continue applying to positions months after taking the assessment.

2.2 The Assessment

Walmart has long used application assessments to evaluate the performance of potential hires. In July 2018, Walmart debuted a new assessment for its hourly, in-store positions created in partnership with Modern Hire Inc. (which has since been acquired by HireVue). This assessment is forty-six questions, divided into four sections, that records the applicant's work history and preferences. These four sections prompt the applicant to do the following:

[5]The assessment does not change for those who retake the test.

(1) select their most and least likely responses to scenarios that may arise on the job, (2) complete a work simulation in which they validate numbers on a few product labels, (3) provide information on their past jobs and performance at those jobs, and (4) select which of two contrasting personality traits or work styles they most agree with. The responses to these questions are mapped to quantitative values using a machine learning model that optimizes over historical worker quality, after which they are aggregated in a weighted average, where these weights are determined by business need. The resulting output is an approximately normally distributed score (called a "z-score") that ranges from -4 to 4.[6] The score is intended to represent the quality of an applicant: generally, a higher score is related to both greater performance and a lesser tendency to separate from the job. Each applicant is assigned a "fit category," depending on their score, S.

$$\text{Fit Category} = \begin{cases} \text{Failed} & S < -0.83 \\ \text{Poor Fit} & -0.83 \leq S < -0.25 \\ \text{Good Fit} & -0.25 \leq S < 0.26 \\ \text{Excellent Fit} & 0.26 \leq S \end{cases}$$

Neither the hiring manager nor the applicant can directly observe this score. Only the hiring manager can observe the category for each applicant. An applicant with a score below -0.83 fails the test and cannot apply for any jobs unless they retake the assessment and achieve a passing score.[7]

This information is used on the hiring platform to sort the list of applicants by estimated quality. For each job posting, applicants are lexicographically sorted using the following features, from highest to lowest priority: (1) veteran status, (2) assessment category, and

[6] From July 2022 through January 2025, the z-score had mean 0.03 and standard deviation 1.01.

[7] As mentioned above, applicants are notified that they failed and can retake the assessment after thirty days. Assessment scores are also not permanent for those who pass. Applicants achieving scores in either the "poor fit" or "good fit" categories must retake the assessment after six months if they wish to continue applying for jobs at Walmart. Applicants achieving a score in the "excellent fit" category must retake the assessment after two years to apply to more positions.

(3) a secondary "hiring helper" score.[8] In addition to being given preference in the sorting mechanism, applicants with better assessment categories (i.e., representative of higher scores) are also given a visual distinction highlighting their category. Excellent, good, and poor fit applicants will have green, yellow, or orange signals on their profile, respectively, along with text denoting the category. During a COVID-era period when those who failed could apply to positions, these applicants were designated with a red signal.[9] Consequently, we will refer to the three assessment thresholds at -0.83, -0.25, and 0.26 as the "red-orange," "orange-yellow," and "yellow-green" cutoffs.

This environment provides an ideal setup for a fuzzy regression discontinuity design with three thresholds for a few reasons. Panel A of Figure 1 shows the clear discontinuities in the rates of being hired after the assessment. Importantly, hiring managers cannot directly observe the assessment score, contributing to the existence of discontinuities in hire rates. Applicants within each category are about equally likely to be hired across the range of scores, which confirms that hiring managers do not implicitly approximate the underlying score from observable characteristics. As the assessment score is effectively double-blind and the function mapping responses to scores is not known to applicants, it is harder to believe that applicants can specifically manipulate their responses to achieve a certain score above a threshold. We test for this in Section 4.

3 Data

We use data from two sources: applicant data from Walmart and jail roster data from NYU's Public Safety Lab Jail Data Initiative.[10] This section describes the datasets used in

[8]The hiring helper score is a score specific to a store and job posting that is used to assess candidate quality and is determined from other characteristics, including prior job history at Walmart, experience at other jobs, time in previous roles, shift availability, and store turnover. The sets of application responses used to determine the assessment score and the hiring helper score are mutually exclusive, though these scores are correlated. The hiring helper score is not directly observed by any person, as it is only used on the back-end to sort applicants within the same assessment category.

[9]For a period of time during the COVID-19 pandemic, Walmart allowed those who failed the assessment to apply for jobs, even without retaking it. This policy ended in August 2022.

[10]Found at `jaildatainitiative.org`.

this paper and the resulting merging process.

3.1 Walmart Applicant Data

Data on Walmart assessments spans from July 2018 (the beginning of the HireVue partnership) to January 2025. Over this time period, there have been about 30 million assessments completed, taken by about 25 million unique applicants.

For the regression discontinuity design, we are only able to use assessments data beginning July 2022. Assessment scores prior to July 2022 face significant measurement error, to the point that there are no detectable discontinuities in hire rates across the thresholds. Prior to this month, scores were determined using a previous version of the assessment model. For our use, these scores were recalculated on the true responses using a later version of the model: we received a score that was correlated with, but not exactly equal to, the true score of each assessment. This measurement issue resulted in the data having essentially no discontinuous changes in hire rates across the assessment thresholds.[11] So, we will only consider assessments taken beginning July 2022 for our main analysis.

Panel A of Table 1 provides demographic information on the pool of applicants starting July 2022. In total, about 13.4 million applicants took 14.8 million assessments from July 2022 through January 2025. Test-takers were about as likely to be male as they were to be female. About one-third each of applicants were non-Hispanic white (34%) and black (30%), and about one-fourth of applicants identified as Hispanic. 20% of assessments resulted in failure, and 18%, 20%, and 42% resulted in poor fit, good fit, and excellent fit designations.

3.2 JDI Data

The JDI data consists of individual-level records from county- and city-level jail populations, starting in January 2020. Being in jail, individuals identified in this dataset may already be

[11] The first stages, or differences in hire rates across the assessment thresholds, are close to zero for this time period and are not statistically significant. The discontinuities in hire rates range from 0.003 to 0.007, over an order of magnitude less than the same for the time period when scores were not mismeasured.

convicted and serving short sentences or are not yet convicted and awaiting trial. In total, there are about 14.5 million observations from 1,200 facilities, comprising 1,259 counties across 40 states.[12] The data contains observations from daily scrapes of publicly available jail populations, with information on name, date of birth, race, sex, age, charge type, state of facility, county of facility, and, if available, arrest date and booking date. The JDI data also records variables relating to the data collection process: a scrape date value records the first day that an individual appears in a jail roster for a given incarceration spell, and a "length of stay" value records the days in between the first and last dates an individual appeared in a jail roster for an incarceration spell. We will use both of these values to measure our primary outcomes.

For each observation, the JDI lists the charges associated with each jail booking and also identifies the "top charge" by using the Criminal Justice Administrative Records System (CJARS) text-based offense classification (Choi et al., 2023) to identify the most "severe" charge.[13] For example, if a particular jail observation recorded a list of charges that comprised a violent crime and a property crime, the "top charge" recorded would be a violent crime. This classification allows us to decompose the effects into constituent crime types and severity. We are interested in both the number of jail bookings associated with each "top charge" and also the number of bookings associated with each crime type.[14]

One drawback of this data is that the JDI scraping procedure will fail to record arrests that do not result in a formal jail booking (i.e., a citation issuing a summons to court). So,

[12]There are facilities whose jurisdiction covers multiple counties.

[13]Charges are ranked, from most to least severe, in the following order of broad crime types: violent, property, drug, public order, DUI offense, criminal traffic. Violent crimes include murder, sexual assault, and aggravated assault. Property crimes include burglary, forgery, and trespassing. Drug crimes include distribution and possession. Public order crimes include parole/probation violations, disorderly conduct, and obstruction. DUI offenses encompass all types of driving under the influence. Criminal traffic crimes include reckless driving and driving with a suspended license.

[14]Using the top charge as an outcome will allow us to determine the proportion of the total effect on jail bookings that are attributable to violent crimes. As non-violent crimes are commonly charged concurrently with violent crimes, this will not necessarily obtain the same result as using the quantity of jail bookings associated with each crime type. For example, many public order offenses (i.e., probation violations or obstruction charges) occur concurrently with violent crimes. Though a reduction in violent crimes may lead to a reduction in public order crimes as well, the effect on the number of bookings with a public order offense being the top charge may be smaller.

our results will be conditional on being booked into jail. The effect of employment on jail bookings may be different than the effect on being arrested, as having a job may influence the police's decision to issue a citation in lieu of a jail booking.

3.3 Merging the Datasets

We merge Walmart applicants to the jail data using name and date of birth. Walmart requires applicants to submit their birth month and day, but not the birth year. However, DOB information is only available for a subset of counties in the JDI data. The distribution of DOB availability within jail records is generally bimodal: 289 county jails have over 90% of observations containing non-null DOB information, while 849 county jails have less than a 5% non-null rate. Panel A of Appendix Figure A1 shows the distribution of counties with any JDI data, and Panel B shows the proportion of observations in each county with DOB information. To maximize the chances of matching applications to the jail data, we restrict our pool of Walmart applicants to only those that live in the 289 counties with mostly full DOB information in the jail data.[15] However, our findings are proportionally similar when using all counties or different DOB availability thresholds (0%–75%). We apply three more filters to this dataset: (1) we truncate our sample to end on October 16, 2024 to ensure that we have a one-year lookout window for each applicant, (2) we remove all prior employees at Walmart, and (3) we only consider the first assessment taken by each applicant.[16]

Panel B of Table 1 shows the demographics for this subset. This subset consists of about 2.4 million applicants, is more likely to be black or Hispanic, is less likely to be white, and is

[15]Across all matches from the Walmart applicant dataset to the JDI dataset (by first name, last name, birth month, and birth day), 69% occur in the same county of the zip code that the applicant provided in their profile. So, a large majority of recorded jail observations occur within the county that the applicant provided.

[16]There are discontinuities in the distribution of scores among prior employees around the assessment thresholds. As prior employees would have already taken the assessment before a previous hire, and as there are discontinuities in hire rates across the thresholds, there will mechanically be discontinuities in the distribution of scores among applicants who were previous employees, especially if their previous scores were still active during their application. For this reason, we remove these applicants from the analysis. Also, just using the first assessment completed by each applicant allows us to focus on a person's first interaction with Walmart's hiring platform in its current form. However, including all assessments for an applicant and clustering standard errors by applicant does not meaningfully change our results.

slightly more likely to be a student and under 18. This sample also does not have much prior contact with the criminal justice system, as observed by jail records. Among this sample, 1.2% of applicants had ever been in jail in the year prior to the assessment. Owing to these relatively low rates of crime, this sample is more comparable to populations from studies on less criminally involved populations than those with higher crime rates, like ex-offenders or disadvantaged youth.

4 Results

The discontinuous nature of the assessment thresholds creates exogenous variation in job opportunities at Walmart. Importantly, we do not observe employment at any other company, so we cannot identify the effect of any employment on crime. Regardless, this variation will allow us to identify the effects of job opportunities at Walmart on future jail incidence using a fuzzy RD strategy. In this section, we outline the identification strategy and report the main results.

4.1 Empirical strategy

Surpassing each score threshold discontinuously increases an applicant's chance of hire. Row 1 of Figure 2 illustrates this by plotting the proportion of applicants that are ever hired at Walmart within a year of their assessment. In addition, hiring is a continuous process: applicants can continue applying to jobs indefinitely. Row 1 of Figure 3 shows how this effect evolves differently for each threshold.[17] As it is possible that an assessment result

[17] As green scores on the assessment last for two years, applicants who receive this score have a lasting advantage over those who initially received a yellow score on their assessment. Over time, they will have an increasingly larger cumulative chance to be hired over those who almost received a green score. This growth in the hire rate differential plateaus at around six months after the assessment for the orange-yellow cutoff, as both sets of applicants must retake the assessment after six months. On the other hand, the difference in hire rates for applicants around the red-orange threshold is almost completely explained by the first two months following the assessment. Another reason the differential hire rate might change over time is the following: those who barely achieved a score greater than the threshold would have been more likely to be hired earlier and would be more likely to still be employed at certain times after the assessment. As a larger proportion of these applicants are still employed, they clearly also cannot be hired at these times. In certain

may still discontinuously affect hiring outcomes in the year following, we must be careful to avoid exclusion issues when calculating the fuzzy RD estimates. For this reason, we use an indicator on whether an applicant was ever hired at Walmart over the year following the assessment as the relevant endogenous variable, though we also check the robustness of our results to using different endogenous variables, like getting hired within 90 days of the assessment. It may also be true that the relevant treatment is not hire, but receipt of a job offer, which occurs potentially weeks before the first day on the job.[18]

Our regression discontinuity strategy relies on the assumption that untreated crime outcomes change continuously across the assessment thresholds. To test for this, we conduct a series of validity checks on our data. Panel B of Figure 1 shows the distribution of assessment scores and the three assessment thresholds—visual inspection does not show evidence of manipulation by applicants to achieve scores surpassing the threshold. More formally, we check for density discontinuities and cannot reject the null that the density is continuous across the thresholds (Cattaneo et al., 2018).[19] We also test for discontinuous changes in covariates across genders, races, ages, student types, prior criminal history, and hiring helper score. For each threshold, we cannot reject the null hypothesis on the joint F-test that the discontinuities for the covariates are zero. See Appendix Table A1.

To estimate the effect of job opportunity on jail rates, we first estimate the following reduced form local linear specification:

$$Y_i = \alpha^c + \beta^c Z_{i,c} + \gamma^c S_{i,c} + \delta^c Z_{i,c} S_{i,c} + \Gamma^c X_i + \varepsilon_i \tag{1}$$

for assessment/applicant i, assessment cutoff $c \in C = \{\text{red-orange, orange-yellow, yellow-green}\}$, assessment score $S_{i,c}$ (centered on the cutoff value for each threshold), $Z_{i,c}$ an indicator on

time periods, this effect may negate or attenuate the discontinuously greater chance those who surpass a threshold have at getting hired than those who barely miss it.

[18] Appendix Figure A2 shows that it may take several weeks between job offer and hire. This would be important if receiving a job offer affects behavior immediately, as workers might respond to the opportunity cost of losing the job offer. We test for this possibility as well.

[19] The p-values for the discontinuities are 0.30, 0.44, and 0.21 for the red-orange, orange-yellow, and yellow-green thresholds.

achieving a score at least as large as the given threshold ($S_{i,c} \geq 0$), and X_i a vector of controls that includes zip code, gender, race, and information from application questions.[20] For our main results, we use a triangular kernel and a bandwidth of 0.2 around each threshold, as 0.2 was approximately the midpoint of the MSE-optimal bandwidths from Calonico et al. (2014) across outcomes.[21] Our outcome of interest is β^c for each cutoff c.

To determine the effect of those given job opportunities (job offer or hire) on crime outcomes, we use following two-stage least squares local linear specification:

$$D_i = \alpha_1^c + \beta_1^c Z_{i,c} + \gamma_1^c S_{i,c} + \delta_1^c Z_{i,c} S_{i,c} + \Gamma_1^c X_i + \varepsilon_i^c \tag{2}$$

$$Y_i = \alpha_2^c + \beta_2^c D_i + \gamma_2^c S_{i,c} + \delta_2^c Z_{i,c} S_{i,c} + \Gamma_2^c X_i + \nu_i^c \tag{3}$$

for D_i an indicator on job opportunity at Walmart (e.g., being hired within one year). Our outcome of interest is β_2^c for each cutoff c.

In addition, we calculate an aggregate estimate by stacking the datasets corresponding to the different thresholds. Using a bandwidth of 0.2 and a triangular kernel, we also calculate the pooled estimate:

$$D_i = \sum_{c \in C} \left[\alpha_1^p G_{i,c} + \beta_{1,c}^p G_{i,c} Z_{i,c} + \gamma_1^p G_{i,c} S_{i,c} + \delta_1^p G_{i,c} Z_{i,c} S_{i,c} \right] + \Gamma_1^p X_i + \varepsilon_i^p \tag{4}$$

$$Y_{i,c} = \beta_2^p D_{i,c} + \sum_{c \in C} \left[\alpha_c^p G_{i,c} + \gamma_2^p G_{i,c} S_{i,c} + \delta_2^p G_{i,c} Z_{i,c} S_{i,c} \right] + \Gamma_2^p X_i + \nu_i^p \tag{5}$$

where $G_{i,c}$ is an indicator on belonging to the dataset corresponding to the cutoff c. So, the model allows for different local linear fits for each set of applicants around the thresholds. Here, our outcome of interest is β_2^p, or the pooled effect of employment at Walmart on crime outcomes among the compliers of each threshold.

[20] These include: night-shift availability, age group, sponsorship requirements, right to work status, full-time preference, student status, retail experience, grocery experience, warehouse experience, and manufacturing experience.

[21] We also show that our results are similar across different kernels and bandwidths, as well as when using the bias-corrected methods with robust standard errors from Calonico et al. (2014).

We focus on three primary Walmart employment outcomes: whether an applicant was ever hired within one year, the number of days employed at Walmart over one year, and whether an applicant was still employed at Walmart after one year. In addition, we measure three primary jail outcomes: whether an applicant was ever on a jail roster, the number of unique times an applicant was booked into jail, and the total number of days that an applicant spent in jail.

4.2 Employment outcomes

Surpassing an assessment threshold gives an applicant a discontinuously larger chance of being employed at Walmart almost immediately. Panel A of Table 2 details these discontinuities in hire rates, as applicants who barely achieved an orange score are 6.6 percentage points more likely to have been hired within a year of the assessment. This is a large jump, representing a 160% increase in hire rate over those who barely failed the assessment. The discontinuities in hire rates for the higher-scored thresholds are a bit smaller, at around 4.8 p.p. each, representing increases in hire rates of about 42% and 29% for the orange-yellow and yellow-green thresholds, respectively. The F-statistics for these discontinuities are 3413, 789, and 456, respectively, showing that the instrument on the red-orange threshold is our strongest.

Relative to applicants around the red-orange threshold, the applicants marginally hired around the orange-yellow and yellow-green thresholds are more likely to retain employment at Walmart conditional on hire, which is expected as these score categories capture higher quality applicants. These results show that employment spells at Walmart among the compliers are fairly short, lasting about four to five months in the year following the assessment, though there are many who maintain longer term employment.

Figure 3 shows the lasting effect of these thresholds on employment outcomes over time. The majority of differences in hiring activity can be explained by the first two to three months following the assessment, as applicants receive and accept job offers to applications

made shortly after completing the assessment.[22] Even though applicants who failed the assessment (red score) can reapply after thirty days, this option does not meaningfully reduce the differential hire rates between those who failed and passed.

As expected, the difference in cumulative days employed at Walmart increases over time, while that in sustained employment decreases over time as hires separate from the company. Even more so, Row 1 of Appendix Figure A3 shows that these discontinuities exist for at least two years for all thresholds. As a result, the assessment threshold structure creates long-term, lasting differences in employment at Walmart.

4.3 Jail outcomes

Similarly, jail outcomes drop discontinuously for Walmart applicants. Rows 2–4 of Figure 2 illustrate the changes in jail bookings across each threshold, using a bandwidth of 0.5 around each threshold (the largest bandwidth we can commonly use without crossing over another assessment threshold). The figure generates two main observations. First, there are clear discontinuities in the jail outcomes for applicants around the red-orange threshold. Second, there is not obvious evidence of the same for the orange-yellow or yellow-green thresholds.

The results from our reduced form and two-stage least squares regressions confirm these visual observations. These are shown in Panel B of Table 2. The reduced form estimates on jail outcomes are large and statistically significant for the red-orange threshold: there is a decrease in ever being in jail by 0.43 p.p., in the number of jail bookings by 0.64 p.p., and in the number of days in jail by 0.42. By using hire at Walmart within a year of the assessment as the endogenous variable in our two-stage least squares estimation, we find that these estimates are also very large compared to baseline complier jail rates. Among the compliers of the red-orange threshold, jail bookings decrease by 0.096 (87%) and time spent in jail decreases by 6.3 days (92%). On the other hand, though the point estimates on

[22] The times between application and job offer, and between job offer and hire date, are potentially lengthy. However, essentially all hire dates occur within 50 days of the relevant applications. See Appendix Figure A2.

jail outcomes are negative for the orange-yellow and yellow-green thresholds, they are not statistically significant at conventional levels. Pooling the data from all of the thresholds together, we find that the proportion of compliers experiencing any jail time within the year after the assessment decreases by 3.0 p.p. (70%), the number of jail bookings decreases by 0.041 (72%), and the time spent in jail decreases by 2.5 days (88%), as detailed in Column 8 of Table 2.[23]

What is causing this difference in jail outcomes across the different thresholds? One explanation is that the marginal applicants who are most affected by job opportunities are those that are less qualified for jobs elsewhere. The ability to be hired at Walmart might be one of the best (or only) opportunities to find legal sources of income for some applicants. Evidence from the responses to the assessment support this hypothesis.[24] Appendix Table A2 shows the characteristics of the complier groups for each threshold. Specifically, the compliers around the red-orange thresholds are much more likely to either have not finished high school or have their highest level education be a high school diploma. At the same time, red-orange applicants are less able to hold down jobs. 61% of red-orange compliers reported that they have held a job for more than one year, while 75% of yellow-green compliers reported the same.[25]

The time paths of the IV estimates over time, as shown in Figure 4, lend more credibility to the observed effects. Though the estimates for the red-orange threshold are not statistically significant at conventional levels over the first 8 months following the assessment, the gradual increase in effect size through month 12 suggests that this is due to insufficient power, rather than null effects. This visible increase in the effect size over time is, on the

[23]The pooled estimate on days in jail is only significant at the 10 percent level ($p = 0.06$).

[24]For a subset of applicants, we have access to responses for most of the assessment questions, which include questions on the level of education and work experience for each applicant. This subset includes those applicants that completed the assessment beginning in February 2024.

[25]This reasoning generally comports with findings from Kessler et al. (2022), where the aggregate effects on arrests are explained by crime reductions from at-risk youth, who might have worse employment outcomes. However, estimates on applicants with no observed prior jail time are still large and significant for the red-orange threshold (ever in jail within one year: -0.051; SE: 0.018; $p = 0.004$), indicating that effects are not restricted to those with prior criminal justice contact, unlike what is observed in Kessler et al. (2022).

other hand, not apparent for the orange-yellow threshold and not as perceptible for the yellow-green threshold.

Just as the assessment structure creates lasting employment differentials across each threshold, the effects on jail outcomes for the red-orange threshold last as well. Appendix Figure A3 graphs these estimates over a two-year horizon for the subset of applicants who took the assessment prior to October 2023. The figure shows that the point estimates on the jail outcomes generally plateau at around one year after the assessment, but last for at least two years. This suggests that the vast majority of crime reductions resulting from employment at Walmart are restricted to the first year after taking the assessment, and that there is not evidence of intertemporal substitution in criminal behavior, which would be consistent with the effect size decreasing over time as hires separate from Walmart.[26]

These findings hold across battery of robustness tests. Table 3 presents our IV estimates when using different kernels, when removing controls, and when using the robust bias-corrected methods from Calonico et al. (2014). The results are generally similar across specifications, in that the effects on the red-orange applicants are statistically significant at conventional levels and drive the pooled effects. Figure 5 also plots how the main results change with respect to bandwidth, ranging from 0.1 to 0.5 (the largest possible bandwidth without crossing into other thresholds). The estimates for the red-orange threshold generally decrease in magnitude with greater bandwidths, though remain statistically significant, while the estimates for the other two thresholds generally are near zero.

We chose 90% as a threshold value for including applicants from counties with at least this proportion of non-null DOB values in the jail data. The intent of this filtering was to ensure that we could observe as many jail bookings as possible for each applicant, but our results hold across a variety of such threshold values and even when including applicants

[26]With respect to longer-term effects, it is possible that getting hired and maintaining employment at Walmart prevents workers from seeking other jobs (Jäger et al., 2024). In this case, hires at Walmart might face worse longer-term employment outcomes if they lose out on finding better jobs, leading to potentially higher crime rates. The consistency of the estimates on jail outcomes after one year shows that there is no evidence for this hypothesis, as that those initially hired at Walmart do not have worse crime outcomes between one and two years after the assessment.

from all counties (threshold values of 0% through 75%). As shown in Appendix Table A3, the levels of the estimates differ across subsets, as would be expected with lesser likelihoods of identifying true jail bookings. However, estimates across thresholds are proportionally similar to (as a percent of the control means) and are about as statistically significant as the main results.

As mentioned above, the relevant endogenous variable that affects crime outcomes may not be hire, but rather the receipt of a job offer. Job offers may affect criminal behavior by increasing the opportunity cost of incarceration even before hire. On average, about three-fourths of all job offers result in a hire, with the large majority of non-conversions due to voluntary action (or inaction) by the applicant (e.g., nonresponse, declining the offer), as opposed to an involuntary rejection (e.g., failure of the background check). Many rejections of job offers may be due to acceptance of another job, which may provide a better estimate of the effect of *any* employment on crime. If we hypothesize that employment reduces crime, and assume that the nature of employment at Walmart versus other employer choices does not affect crime outcomes differently, then using job offer as the endogenous variable gets us closer to estimating the effect of any employment on crime. Appendix Table A4 shows how estimates change when considering job offer as the treatment and also how estimates change with using different employment windows (e.g., hired within thirty days vs. one year).

There is not evidence that the differences in jail outcomes are caused by discontinuities in existing tendencies to commit crime, as measured by historical jail records. We check for jail outcomes in the year preceding the assessment as a placebo test. A violation of continuity may suggest latent differences in criminal activity across the discontinuities. Of our nine jail outcomes across the three thresholds, one is significant at the 5 percent or 10 percent level: ever being in jail in the year prior to the assessment for the yellow-green threshold ($p = 0.04$). However, we cannot reject the null hypothesis of the joint F-test that all nine discontinuities are zero ($p = 0.406$).

Another opportunity for a placebo test uses the mis-measured data prior to July 2022

which does not have observable discontinuities in hire rate, so we should not expect to detect changes in jail outcomes. To allow for a complete one year lookout period in the JDI data, we use only those applicants who took the assessment beginning January 1, 2020 to align with the start of the jail data. We then use the same procedure as the main analysis to obtain reduced form estimates for each of the nine outcomes. None of the nine outcomes are significant at the 5% level, and one (number of jail bookings for the orange-yellow threshold) is significant at the 10% level.

4.4 Effects by charge type

These effects on future bookings are driven by violent offenses and offenses with a yet unknown crime type, which are likely to be representative of more severe charges. We observe the most severe charge type for each offense in the JDI data, with about 30% of observations either having a null value or a value of "TBD." Table 4 shows the results by crime type for each threshold, where the outcome is the number of jail bookings within the year. Though the individual estimates are imprecise, they broadly show large proportional decreases in crimes across all charge types. For the red-orange threshold, the effect on violent crimes and crimes with an unknown charge type are both statistically significant at conventional levels. Pooled across all thresholds, the effect on jail bookings headed by violent crimes, -1.6 p.p., is about three-quarters of the complier control mean and is statistically significant ($p = 0.04$).

The effect on jail bookings with an unknown charge type accounts for almost half of the aggregate effect for the red-orange threshold. Though we do not have much insight into the specific charges involved in these jail observations, average jail time for each observation of this type suggests that these comprise more severe crimes. For example, jail records with a violent crime as the top charge last about 43 days on average. On the other hand, jail bookings headed by property, drug, and public order charges last between 25 and 30 days on average. The average incarceration length of crimes with unknown charge types is 52 days—even longer than that of violent crimes, suggesting that these are more likely to

represent violent crimes or more severe charges within other crime types. About two-thirds of reduction in jail incidences, then, is driven by drops in severe crimes (violent and unknown charges).

As violent crimes and crimes of unknown charge type result in the longest incarceration times, we should expect the main results are not totally driven by decreases in lesser crimes, and that more severe (i.e., longer) incarceration spells would also be deterred by employment. We test this by splitting incarceration spells into severe charges (at least thirty days) and non-severe (less than thirty days).[27] Appendix Table A5 shows these results. Among the red-orange compliers, the proportion of ever being in jail for a severe charge decreases by 3.1 p.p., or almost the entire control complier mean. The estimate on ever being in jail for a non-severe charge decreases by 5.2 p.p. (81%). However, the same is not true for the pooled estimate: only short sentences decrease significantly (3.0 p.p.).

We also estimate effects on jail admissions for each crime type separately, documented in Appendix Table A6. Because many crime types co-occur in a single jail booking, these estimates will not sum to the aggregate estimate on jail admissions. As would be expected if there was substantial overlap across crime types, these estimates are generally larger than those in Table 4. The estimate on public order offenses is especially greater due to co-occurrences with violent crimes—70% of the effect on public order offenses for the red-orange threshold is explained by charges concurrent with violent offenses.

Using more granular charge classifications reveals that point estimates are negative across violent crime subtypes, with the largest estimates corresponding to assault, robbery, and blackmail/extortion. Pooled across thresholds, the estimates for robberies, other violent offenses, auto-related property offenses, and disorderly conduct are significant at the 5% level.[28] See Appendix Table A7.

[27] Thirty days is approximately the 75th percentile of jail times.

[28] Auto-related property offenses comprise auto theft, hit and run causing property damage, and unauthorized use of vehicle. Disorderly conduct offenses comprise drunkenness/vagrancy/disorderly conduct, liquor law violations, family related offenses, commercialized vice, morals/decency - offense, invasion of privacy, contributing to the delinquency of a minor, habitual offender, and rioting.

4.5 Heterogeneity

Estimates across genders, races, student statuses, and age groups are mostly negative and large compared to complier control means. However, we can only provide credible evidence of reductions in crime for men, black applicants, adults, and those in college or not a student. Throughout this section, we report estimates on different populations around the red-orange threshold. Appendix Table A8 shows these results.

The effect of being hired at Walmart on any jail time within one year for men is large, both in levels and in percentage terms. Men who are marginally hired are in jail anytime within the next year 9.2 p.p. less (89%), which is statistically significant at the 1 percent level ($p = 0.006$). On the other hand, the point estimates for female applicants are not different from zero and have mixed signs. Estimates are negative across races, but the only estimates that are statistically significant are those on black applicants. There are also large but imprecise point estimates across age and student groups. Adults (those at least 18 years old) who are hired are 6.8 p.p. ($p = 0.002$) less likely to go to jail after the assessment, or 86%. But, we cannot detect a significant effect on minors who are hired ($p = 0.84$), despite their proportionally large point estimates. In addition, compliers in college are 79% less likely to ever be in jail in the year after the assessment. This is 91% for those who are not a student. Both of these estimates are significant at the 5 percent level.

In general, though many of the estimates across genders, races, ages, and student statuses are imprecise, the fact that most subgroups of applicants exhibit negative point estimates suggests that employment could be an important deterrent of criminality across all types of applicants.

4.6 Social Savings

Given the substantial effects on violent crimes, employment opportunities have the potential to greatly reduce the social costs of crime. Most directly, we can calculate the savings resulting from lower rates of incarceration using our estimate on total days spent in jail.

This generates about $290 in savings per marginal hire attributable only to fewer jail days, not including other aspects of the criminal justice system (investigations, court proceedings, prison time).[29] Importantly, this calculation does not include the costs of victimization caused by violent crimes, which account for the bulk of the social costs of criminal activity.

To quantify a more comprehensive set of costs, we use estimates of crime costs calculated in Cohen and Piquero (2009). They offer two sets of cost estimates—a "bottom-up" approach that calculates the victimization costs, criminal justice costs, and costs from lost offender productivity from incarceration for several crime types, and a willingness-to-pay (WTP) approach that aims to estimate intangible costs to society from survey-based methods. We estimate costs using both methods.

We estimate costs in three steps: (1) we relate crime categories from the CJARS classification system to those from Cohen and Piquero (2009), (2) we calculate the average cost of each crime type (i.e., for violent and property crimes separately) using the observed distribution of crime subtypes associated with the jail bookings of Walmart applicants, and (3) we multiply these average costs by the estimates by crime type from Table 4. Appendix Table A9 details this process. We also assume that crime reductions for applicants are limited to the first year after the assessment.

For the first step, as violent and property crimes have multiple subclassifications for charges that have large variation in estimated cost (e.g., murder vs. simple assault), we first correspond CJARS subclassifications to crime types for which we have cost estimates. When we do not have a straightforward correspondence, we refer to estimates from Miller et al. (1996), who calculate victimization costs using methods that are generally more conservative that those in Cohen and Piquero (2009), as the former does not include criminal justice costs that are not police or fire services and also does not include lost wages to offenders due to

[29]This assumes there are not effects on jail days more than one year after taking the assessment and that marginal jail cost equals average jail cost. We multiply the days in jail estimate, 2.482, by the average cost of jail, $118, to obtain $293. Average cost of jail determined from (Henrichson et al., 2015) by dividing the total spent on jails in 2011 ($22.2 billion) by inmate population on each day (735,601) and converting to 2025 dollars.

incarceration.

Once we have estimates of crime costs for each subclassification, we calculate the distribution of these subtypes among the jail bookings for Walmart applicants that occur within one year of the assessment. This distribution reflects that crimes conditional on being booked into jail are more severe than crimes at-large, as about 60% of violent offenses in our sample relate to aggravated assault and 8% of offenses relate to simple assault or assaulting an officer, even though simple assaults occurred about three times more often than aggravated assaults in the 2020s (Tapp and Coen, 2025).[30] For the other crime types, we refer to the "other offenses" estimate from Cohen and Piquero (2009) and the "drunk driving, no injury" estimate from Miller et al. (1996). And, for the unknown crime type, we take the average of costs from the other crime types, weighted by their frequency among applicants in the JDI data.

Finally, we multiply these crime cost estimates with the IV estimates from Table 4. Though this will underestimate total reductions in crime (as shown in Appendix Table A6), we use the estimates on bookings with each crime as the "top charge" to avoid double-counting crimes, which could be caused if, for example, committing a violent crime causes a probation violation. However, using the estimates from Appendix Table A6 does not change the cost estimate much, given that the pooled effect on violent crimes does not change. Note that while only the pooled estimate on violent crimes is significant at the 5% level, we use the point estimates for all crime types.

Ultimately, we obtain that each marginal hire at Walmart generated social savings of about $7,200 (bottom-up) to $15,100 (WTP), mostly attributable to reductions in violent crimes. However, as less than half of violent crimes are reported to the police and, of those,

[30]For this reason, our estimate of the average violent crime "bottom-up" cost associated with jail bookings is about $350,000 (in 2025 dollars), versus about $160,000 using estimates from (Ludwig and Schnepel, 2025). This is almost fully explained by a greater rate of murder and inclusion of manslaughter charges in the JDI data. This paper provides estimates for murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft. If violent crimes consist of only murder, rape, robbery, and assault, then the average cost of violent crime would be the weighted average of these crime types, with weights equal to the crime rate per 100,000 people. This equates to about $159,000.

less than half are cleared with an arrest, it is likely that true savings are multiple times greater (Tapp and Coen, 2025; CJISD, 2025).

These savings to society constitute a substantial fraction of the costs of employing the marginal hire at Walmart. Walmart pays each marginal hire $9,237 on average through the year following the assessment,[31] so even the more conservative estimate of $7,200 in crime savings represents about 78% of total pre-tax earnings from Walmart. Estimates of savings using WTP methods even exceed the cost of employing these applicants. So, the positive externality of employing such workers is sizable—on the order of their cost to the company.

5 Discussion

5.1 Potential Mechanisms

There is no consensus on how the employment would reduce criminal activity, and specifically, violent crime. Of course, economic incentives may deter financially motivated crimes both due to the greater return of legal work and may deter all crimes due to the greater cost of incarceration. Or, the observed effects may be explained by workers staying busy at their jobs, independent of economic incentives. Other proposed hypotheses on crime-reducing mechanisms include increased prosocial attitudes (Modestino, 2019) and "automaticity" (Heller et al., 2017). Effects may also be explained by changes in behavior by the criminal justice system: issuing citations instead of sending people to jail, setting more lenient release conditions (through lower bail amounts or higher rates of non-bail release), or administering lesser sentences. We present results below that find that incapacitation does not explain the majority of the observed effects within one year of the assessment. We also argue that our results, especially those on violent crimes, are not fully explained by greater rates of citation issuance/immediate release.

In addition, a limitation of our data is that we do not observe employment at any busi-

[31] This estimate is pooled across all discontinuities.

nesses other than Walmart. It may be the case that applicants who are not hired by Walmart can easily find employment elsewhere, but employment at Walmart provides some idiosyncratic crime-deterring effect that is not experienced at other employers.[32] We cannot directly separate these narratives. However, descriptive evidence suggests that difficulty finding jobs could be a determinant of crime, as areas with higher firm concentration in retail occupations exhibit larger treatment effects. If this was representative of an underlying causal relationship, Walmart-specific job features would be unlikely to wholly explain the crime reduction.

Incapacitation One hypothesis of how employment reduces crime is that it keeps people busy. Employment, then, directly reduces crimes as it decreases the amount of available time someone has to commit crime, as long as employment does not increase access to more potential victims (Davis and Heller, 2020; Cook, 1986).

That there is a growing effect on jail outcomes over time for the red-orange threshold, while there is a decreasing effect on employment (Row 3 of Figure 4), suggests that just incapacitation is not enough to explain the observed effect size. Here, we define incapacitation as a phenomenon wherein a person does not commit any crime while they are employed, but commits crime at a constant rate otherwise.

As an exercise in measuring the role of incapacitation in explaining the treatment effect, we perform the following procedure.

1. For every month in the year following the assessment, we calculate the control complier mean for jail bookings.

2. For every applicant, we impute a level of crime that depends entirely on their length of employment at Walmart. If an applicant was employed at Walmart during the entire month, we assign a value of zero as their number of jail bookings. If an applicant

[32]For example, workers may interact with different peer groups, work different shifts, receive different amounts of hours, or receive different pay at Walmart.

was not employed at Walmart during the entire month, we assign a value equal to the control complier mean for that month. For applicants employed at Walmart for a part of the month, we linearly impute a jail booking value.

3. Using these imputed values, we estimate the IV effect. This is our estimated incapacitation effect, if applicants participate in no crime while employed at Walmart but are jailed at the mean rates while not employed at Walmart.

Figure 6 shows the result of this exercise. For months directly after the assessment, the estimated incapacitation effect is about 60%. About one year after the assessment, the incapacitation effect is around one-third of the observed treatment effect. This evidence points to the existence of other mechanisms driving the bulk of the effect.

However, this calculation assumes that Walmart employees exhibit the same crime rates before and after their employment spell. The interpretation of our results would change if there were evidence that, for example, employment at Walmart improved chances of employment in the future. If this were the case, increasing reductions to crime rates outside of Walmart may be more explained by incapacitation effects.

Criminal Justice System Behavior As the JDI dataset only records those who were booked into jail, it misses any arrest that did not result in jail time. If, for example, maintaining employment caused police to evaluate that an individual posed a lesser flight risk or threat to the community, they may instead issue a citation to marginal applicants rather than send them to jail for formal booking. It may then be the case that the effect on jail bookings is explained by changes in behavior by the criminal justice system (police or judges), rather than changes in behavior by job applicants. Here, employment would have little bite in reducing the social harms caused by crime, other than its effects on applicants due to reducing jail time (which may lead to lower conviction rates and higher rates of formal employment, though may not change future crime rates; Dobbie et al., 2018).

However, cite-and-release programs are generally directed to non-violent offenses, giving

police less discretion to issue a citation for violent crimes. Though data on citation rates as a percent of arrests are not available on a national-level, evidence from California shows that about 75% of arrests in 2012 led to formal bookings for those at least 18 years old (Raphael and Rozo, 2019). Further, Raphael and Rozo (2019) report that almost all felony arrests, especially those that involved crimes against a person, led to a formal booking for those over 18 years old. So, discretion by the police is most likely to be explained by responses to misdemeanor offenses.

Changes in citation issuance are unlikely to explain the observed effects for violent offenses, as most are classified as felonies across jurisdictions. If effects were explained by changes in citations, we would expect a larger reduction in violent crimes that could be classified as misdemeanors (simple assault, sexual assault, and hit-and-run injury) compared to those classified as felonies (murder, robbery, aggravated assault, etc.). However, this is not the case: the pooled point estimate on jail bookings for violent crimes that could be classified as misdemeanors is near zero (0.05 p.p.) and negligible relative to the total effect, 1.6 p.p. So, there is not evidence that police discretion in giving citations explains our results.

Employment Access There is a large literature using state- or county-level panel data linking unemployment to increases in property crime, but less is known from individual-level studies (Chalfin and McCrary, 2017). We show that employment availability, as measured by the concentration of the labor market for retail-related occupations, is a descriptive moderator for the effect on crime. Specifically, we employ data from Choi and Marinescu (2024), who calculate labor market concentration by developing a Herfindahl–Hirschman Index (HHI) for each quarter from 2007 to 2021, six-digit SOC, and commuting zone (CZ). This dataset uses job posting information collected by Lightcast that "constitutes most of

the US job vacancies posted online"[33] and calculates HHI as:

$$\text{HHI}_{m,t} = \sum_{j=1}^{J} s_{j,m,t}^2 \tag{6}$$

for $s_{j,m,t}$ the share of total job postings on Lightcast by a company j for market m at time t, where a market is a commuting zone $\times$ SOC. In addition, we use information from the Bureau of Labor Statistics' Occupational Employment and Wage Statistics to calculate the share of employment each SOC takes for NAICS 455000 (General Merchandise Retailers). Roughly two-thirds of employees in this industry are cashiers (41-2011), retail salespersons (41-2031), or stockers (53-7065). Then, for each applicant, we find the relevant HHI for their CZ and repeat the fuzzy RD procedure from equations (1) and (2) while including an interaction term of the HHI (standardized) with the discontinuity.[34]

The results of this exercise on the three occupations with the largest share of employees in the GM retailer industry suggest that more concentrated labor markets are related to larger effects. A one standard deviation increase in cashier HHI is related to a larger treatment effect on ever being in jail within one year of 0.046 (SE: 0.027, $p = 0.09$). Similarly, a one standard deviation increase in retail salesperson HHI is related to a larger treatment effect of 0.068 (SE: 0.027, $p = 0.01$). This relationship is closer to zero when using stocker HHIs, however: -0.018 (SE: 0.027, $p = 0.50$). Though correlational, these results are generally consistent with the hypothesis that limited employment access could increase treatment effects, if higher labor market concentration decreases employment opportunities.[35]

[33] As described in Choi and Marinescu (2024).

[34] So, there are two excluded instruments, $Z_{i,c}$ and $Z_{i,c}H_i$ for two endogenous variables, $D_{i,c}$ and $D_{i,c}H_i$, where H_i is the standardized HHI.

[35] Of course, it is possible that there are important confounders between labor market concentration and treatment effects. In the case that these confounders are common across occupations, we can check if we observe a similar relationship between HHIs from unrelated occupations (i.e., consisting of a small or negligible percent of total GM retailer employment) and larger treatment effects. Repeating this procedure for the 831 occupations that each consist of less than 5% of total GM retailer employment, we find an average interaction estimate of -0.0005, compared to an average estimate of -0.0185 for the related occupations (consisting of at least 5% GM retailer employment). The extent to which labor market concentration acts as a descriptive moderator is related to the extent to which a given occupation constitutes total retail employment. Again, this agrees with the employment access hypothesis, as occupations unrelated to the

5.2 Conclusion

The costs of crime are vast: estimates of incarceration and quality of life costs range in the trillions of dollars per year (e.g., Miller et al., 2021). Though violent crimes constitute the vast majority of such costs, the best available evidence has suggested that the providing employment opportunities to jobseekers has little potential to mitigate this burden: reductions in violent crimes, whenever found, are limited to disadvantaged high school students or those on Medicaid. In contrast to previous studies, this paper finds such an effect on violent crimes for a large population of applicants to low-skilled jobs that mostly do not have criminal backgrounds. For this group, we find that an exogenous increase to job opportunities at Walmart decreases future jail bookings by about 70% and that this crime reduction is driven by the applicants who may face the most obstacles to employment. As a result, improving employment opportunities for those seeking low-skilled jobs may greatly reduce the social costs of crime to society.

One explanation of the discrepancies across these studies is that they consider meaningfully different populations. Much of the best evidence on jobs programs involves former prisoners, whereas SYEPs are directed at (often disadvantaged) high school-aged youth. Recently incarcerated participants may not be marginally deterred from violent crime due to employment and instead may require more extensive programs to discourage criminal activities. That the population studied in this paper is naturally searching for jobs in the formal labor market could account for the substantial differences in outcomes: the margin of violent crime vis-à-vis employment at least consists of those seeking formal jobs but loosely attached to the labor market (elevated high school dropout rates, difficulty maintaining employment).

As we find that employment reduces violent crime, and not just property crime, theories of crime based on economic incentives may not fully be able to explain how crime falls. We also demonstrate that incapacitation is unlikely to explain the full effect. On the other hand,

retail industry may not be desirable or accessible to Walmart applicants, independent of assessment score. On the other hand, it may be the case that relevant confounders are only related to retail occupations.

lack of employment opportunities (as proxied by labor market concentration) is related to larger treatment effects. Employment access could be an important mediator, as those with outside options may not face long periods without legal income, which is captured by the smaller effects on the higher-scoring applicants who may more readily obtain employment. One limitation of our data is that we do not observe employment outcomes at businesses other than Walmart. Consequently, we cannot identify the extent to which employment, as a whole, is affected by our instrument, and we also cannot observe downstream employment outcomes resulting from Walmart hiring. More research into the mechanisms reducing crime could help explain why different contexts exhibit such varied responses to employment.

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Table 1: Demographics of Walmart applicants

	A	B
	All Assessments (Jul 2022 - Jan 2025)	Main Sample (Jul 2022 - Oct 2024)
Gender		
Male	0.493	0.502
Female	0.498	0.490
Race		
White	0.341	0.239
Black	0.302	0.321
Hispanic	0.245	0.329
Asian	0.027	0.030
American Indian	0.014	0.011
Native Hawaiian / Alaskan	0.004	0.003
Two or more races	0.045	0.043
Student Status		
High school student	0.170	0.216
College or trade student	0.135	0.139
Not a student	0.693	0.644
Age		
Less than 16	0.006	0.008
Between 16 and 17	0.148	0.194
At least 18	0.846	0.798
Assessment Category		
Failed	0.196	0.172
Poor Fit	0.184	0.167
Good Fit	0.201	0.193
Excellent Fit	0.420	0.467
Crime		
Ever in jail in year prior to assessment		0.012
Times in jail in year prior to assessment		0.015
Days in jail in year prior to assessment		0.374
Number of assessments	14,829,485	2,359,505
Number of applicants	13,420,534	2,359,505

Notes: The proportions are calculated as the total number of assessments belonging to an applicant with a particular characteristic, divided by the total number of assessments. As a result, it is possible for one person to be counted multiple times in Panel A, in the case that they completed multiple assessments. Percentages among categories may not exactly sum to one, as some demographic information is not available for a subset of applicants. In Panel B, the main sample includes the assessments that were from applicants located in counties with mostly complete DOB information in the JDI data, were not taken by prior Walmart workers, were taken between July 2022 and October 2024, and were the first assessment taken by an applicant.

Table 2: Outcomes across discontinuities

	A: Walmart employment outcomes				B: Jail outcomes			
	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)	Combined (4)	Red-Orange (5)	Orange-Yellow (6)	Yellow-Green (7)	Combined (8)
Hired within one year of assessment					**Ever in jail within one year**			
ITT	0.0660***	0.0479***	0.0475***		-0.0043***	-0.0002	-0.0011	
	(0.0024)	(0.0026)	(0.0027)		(0.0014)	(0.0010)	(0.0009)	
IV	1.000***	1.000***	1.000***	1.000***	-0.065***	-0.004	-0.023	-0.030***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.021)	(0.021)	(0.018)	(0.012)
$Y(0)$ mean	0.000	0.000	0.000	0.000	0.079	0.028	0.032	0.043
	(0.000)	(0.000)	(0.000)	(0.000)	(0.021)	(0.026)	(0.017)	(0.011)
Days employed at Walmart within one year					**Times in jail within one year**			
ITT	9.108***	6.731***	6.640***		-0.0064***	-0.0002	-0.0010	
	(0.433)	(0.499)	(0.556)		(0.0021)	(0.0014)	(0.0012)	
IV	138.083***	140.656***	139.918***	139.565***	-0.096***	-0.005	-0.022	-0.041**
	(4.528)	(7.229)	(8.195)	(4.098)	(0.031)	(0.030)	(0.025)	(0.017)
$Y(0)$ mean	0.000	0.000	0.000	0.000	0.110	0.023	0.039	0.057
	(0.000)	(0.000)	(0.000)	(0.000)	(0.031)	(0.028)	(0.024)	(0.016)
Employed at Walmart one year later					**Days in jail within one year**			
ITT	0.012***	0.010***	0.014***		-0.417**	-0.020	-0.033	
	(0.001)	(0.001)	(0.002)		(0.171)	(0.105)	(0.088)	
IV	0.183***	0.209***	0.288***	0.230***	-6.325**	-0.409	-0.693	-2.482*
	(0.018)	(0.027)	(0.030)	(0.015)	(2.609)	(2.194)	(1.854)	(1.308)
$Y(0)$ mean	0.000	0.000	0.000	0.000	6.842	0.981	0.324	2.809
	(0.000)	(0.000)	(0.000)	(0.000)	(2.531)	(2.148)	(1.686)	(1.252)
N	215,298	318,157	359,587	894,511	215,298	318,157	359,587	894,511
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bandwidth	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20

Notes: Table shows estimates from local linear regressions using a triangular kernel. Controls include zip code fixed effects, gender, race, and information from application questions (night-shift availability, age group, sponsorship requirements, right to work status, full-time preference, student status, retail experience, grocery experience, warehouse experience, and manufacturing experience). For the IV estimates in Panel B, the first outcome in Panel A is used as the endogenous variable. $Y(0)$ means were calculated by employing the same regression using $Y(1-D)$ and $(1-D)$ as the outcome and endogenous variables, for Y the jail outcome and D the endogenous hire variable. The combined regressions (columns 4, 8) stack the data corresponding to columns 1–3 or 5–7 and use three instruments for the discontinuities. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Table 3: Robustness checks for IV estimates

	Local linear fits				Bias-corrected with robust standard errors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Red-Orange								
Ever in jail	-0.065***	-0.052**	-0.039**	-0.058***	-0.058***	-0.054**	-0.050**	-0.037*
	(0.021)	(0.021)	(0.019)	(0.020)	(0.022)	(0.022)	(0.023)	(0.020)
Times in jail	-0.096***	-0.082***	-0.064**	-0.087***	-0.105***	-0.098***	-0.097***	-0.099***
	(0.031)	(0.031)	(0.028)	(0.030)	(0.037)	(0.037)	(0.034)	(0.035)
Days in jail	-6.325**	-5.686**	-5.705**	-5.779**	-6.367**	-6.143**	-5.092*	-5.600**
	(2.609)	(2.544)	(2.266)	(2.443)	(2.679)	(2.671)	(2.736)	(2.435)
Orange-Yellow								
Ever in jail	-0.004	-0.006	0.005	-0.004	-0.006	-0.006	-0.005	-0.007
	(0.021)	(0.021)	(0.019)	(0.020)	(0.023)	(0.023)	(0.023)	(0.023)
Times in jail	-0.005	-0.006	0.006	-0.004	-0.017	-0.017	-0.029	-0.025
	(0.030)	(0.029)	(0.026)	(0.028)	(0.039)	(0.039)	(0.044)	(0.040)
Days in jail	-0.409	-0.339	0.523	-0.115	-0.185	-0.198	0.221	0.116
	(2.194)	(2.108)	(1.974)	(2.094)	(2.386)	(2.405)	(2.345)	(2.343)
Yellow-Green								
Ever in jail	-0.023	-0.019	-0.012	-0.021	-0.029	-0.031	-0.030	-0.029
	(0.018)	(0.018)	(0.017)	(0.018)	(0.023)	(0.024)	(0.022)	(0.023)
Times in jail	-0.022	-0.017	-0.011	-0.019	-0.025	-0.027	-0.029	-0.024
	(0.025)	(0.025)	(0.023)	(0.024)	(0.031)	(0.031)	(0.033)	(0.030)
Days in jail	-0.693	-0.474	-0.848	-0.704	-0.557	-0.634	-0.582	-0.623
	(1.854)	(1.809)	(1.727)	(1.775)	(2.083)	(2.097)	(2.053)	(1.986)
Combined								
Ever in jail	-0.030***	-0.027**	-0.017	-0.027**	-0.039**	-0.038**	-0.028**	-0.042***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.016)	(0.016)	(0.013)	(0.016)
Times in jail	-0.041**	-0.038**	-0.025*	-0.037**	-0.057**	-0.056**	-0.048**	-0.057**
	(0.017)	(0.017)	(0.015)	(0.016)	(0.025)	(0.025)	(0.020)	(0.025)
Days in jail	-2.482*	-2.358*	-2.102*	-2.216*	-2.215	-2.184	-2.626*	-3.078*
	(1.308)	(1.280)	(1.166)	(1.235)	(1.376)	(1.378)	(1.527)	(1.712)
Bandwidth	0.20	0.20	0.20	0.20	MSE-optimal	MSE-optimal	MSE-optimal	MSE-optimal
Controls	Y	N	Y	Y	Y	N	Y	Y
Kernel	Triangular	Triangular	Uniform	Epanechnikov	Triangular	Triangular	Uniform	Epanechnikov

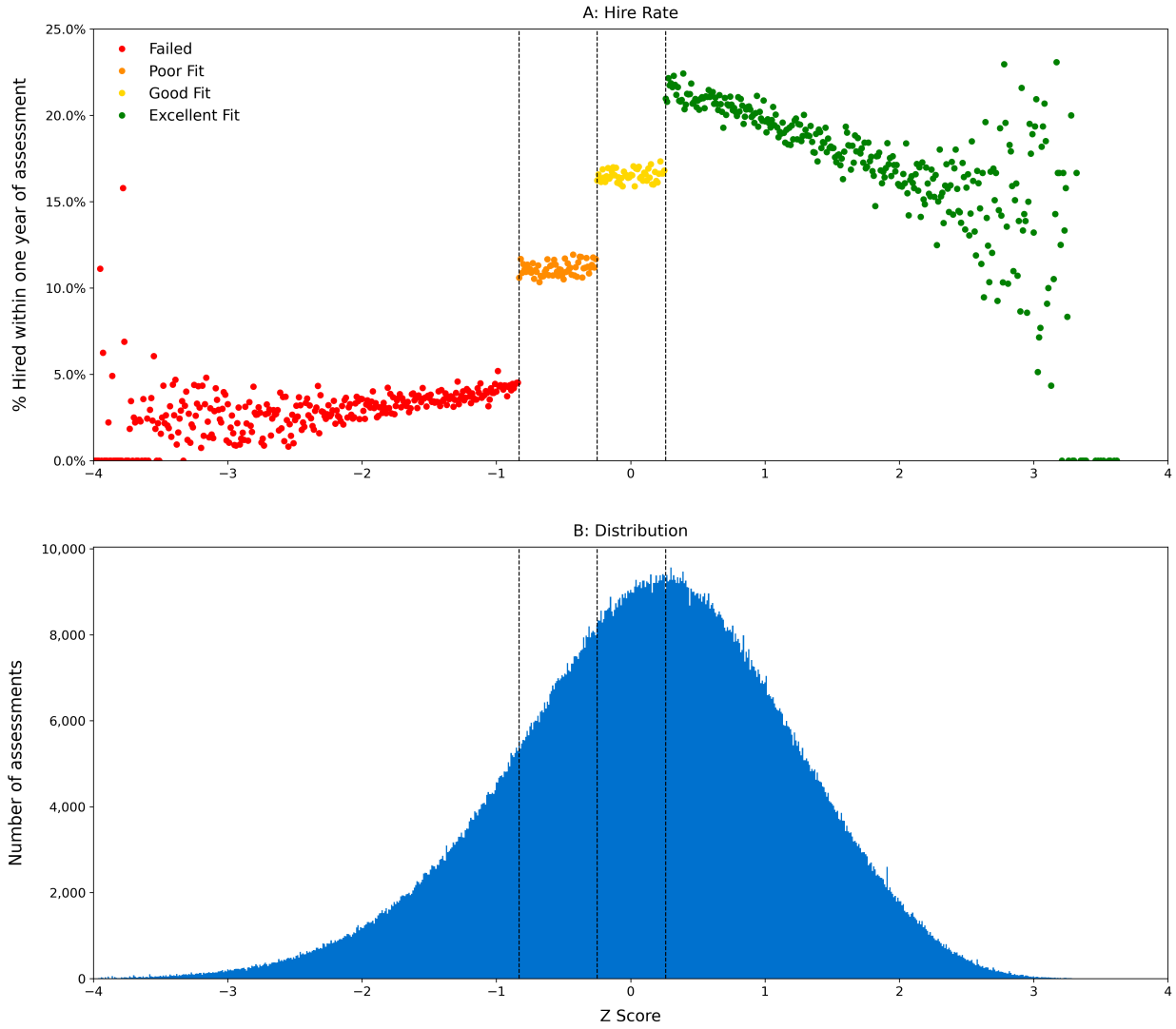
Notes: Estimates in columns 1–4 were calculating using a bandwidth of 0.2 and a local linear fit around the threshold. Column 1 contains the baseline estimates shown in Table 2. Controls include zip code fixed effects, gender, race, and information from application questions (night-shift availability, age group, sponsorship requirements, right to work status, full-time preference, student status, retail experience, grocery experience, warehouse experience, and manufacturing experience). Estimates in columns 5–8 were calculated using the MSE-optimal bandwidth and bias-correction methods from Calonico et al. (2014). (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Table 4: IV estimates across top charge classifications (jail bookings within one year)

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)	Combined (4)	Avg. Jail Days (5)	Prop. of Obs. (6)
Violent	-0.027**	0.000	-0.022*	-0.016**	42.6	0.227
	(0.013)	(0.013)	(0.012)	(0.007)		
$Y(0)$ mean	0.028	0.002	0.030	0.019		
	(0.013)	(0.012)	(0.011)	(0.007)		
Property	-0.008	-0.005	0.004	-0.004	30.2	0.151
	(0.011)	(0.012)	(0.010)	(0.006)		
$Y(0)$ mean	0.009	0.008	-0.003	0.005		
	(0.011)	(0.012)	(0.009)	(0.006)		
Drug	-0.000	0.006	-0.001	0.001	27.2	0.078
	(0.007)	(0.008)	(0.006)	(0.004)		
$Y(0)$ mean	0.002	-0.003	0.003	0.001		
	(0.007)	(0.007)	(0.005)	(0.004)		
Public order	-0.015	-0.005	-0.008	-0.007	24.6	0.193
	(0.012)	(0.012)	(0.010)	(0.007)		
$Y(0)$ mean	0.019	0.009	0.009	0.011		
	(0.012)	(0.012)	(0.010)	(0.006)		
DUI	-0.003	-0.000	0.006*	0.001	11.8	0.025
	(0.004)	(0.004)	(0.004)	(0.002)		
$Y(0)$ mean	0.004	-0.002	-0.003	-0.001		
	(0.004)	(0.003)	(0.003)	(0.002)		
Criminal traffic	-0.005	-0.003	0.003	-0.001	10.2	0.024
	(0.004)	(0.004)	(0.003)	(0.002)		
$Y(0)$ mean	0.005	0.003	-0.001	0.002		
	(0.004)	(0.004)	(0.003)	(0.002)		
Unknown	-0.039**	0.003	-0.003	-0.014	51.5	0.302
	(0.018)	(0.015)	(0.013)	(0.009)		
$Y(0)$ mean	0.042	0.006	0.003	0.015		
	(0.018)	(0.014)	(0.012)	(0.008)		
N	215,298	318,157	359,587	894,511	46,064	46,064
Bandwidth	0.20	0.20	0.20	0.20		

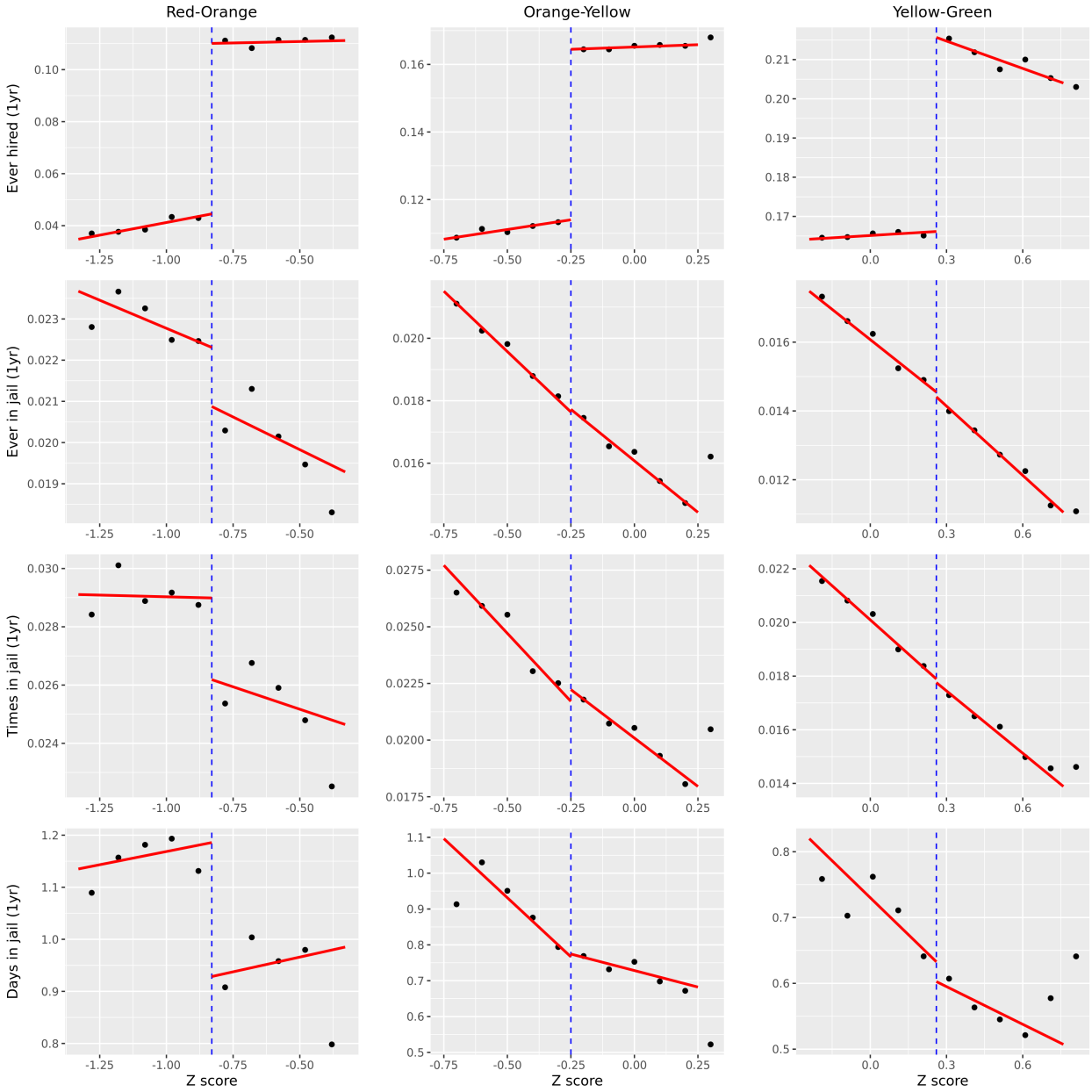
Notes: For each jail observation, the JDI uses the CJARS text-based classification to record the most severe charge. Charges are ranked, from most to least severe, in the following order: violent, property, drug, public order, DUI offense, criminal traffic. Violent crimes include murder, sexual assault, and aggravated assault. Property crimes include burglary, forgery, and trespassing. Drug crimes include distribution and possession. Public order crimes include parole/probation violations, disorderly conduct, and obstruction. DUI offenses encompass all types of driving under the influence. Criminal traffic crimes include reckless driving and driving with a suspended license. Columns 5–6 show the average length of stay and distribution of top charge types from jail observations relating to all applicants in the main sample within one year of the assessment. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Figure 1: Hires rates and score density



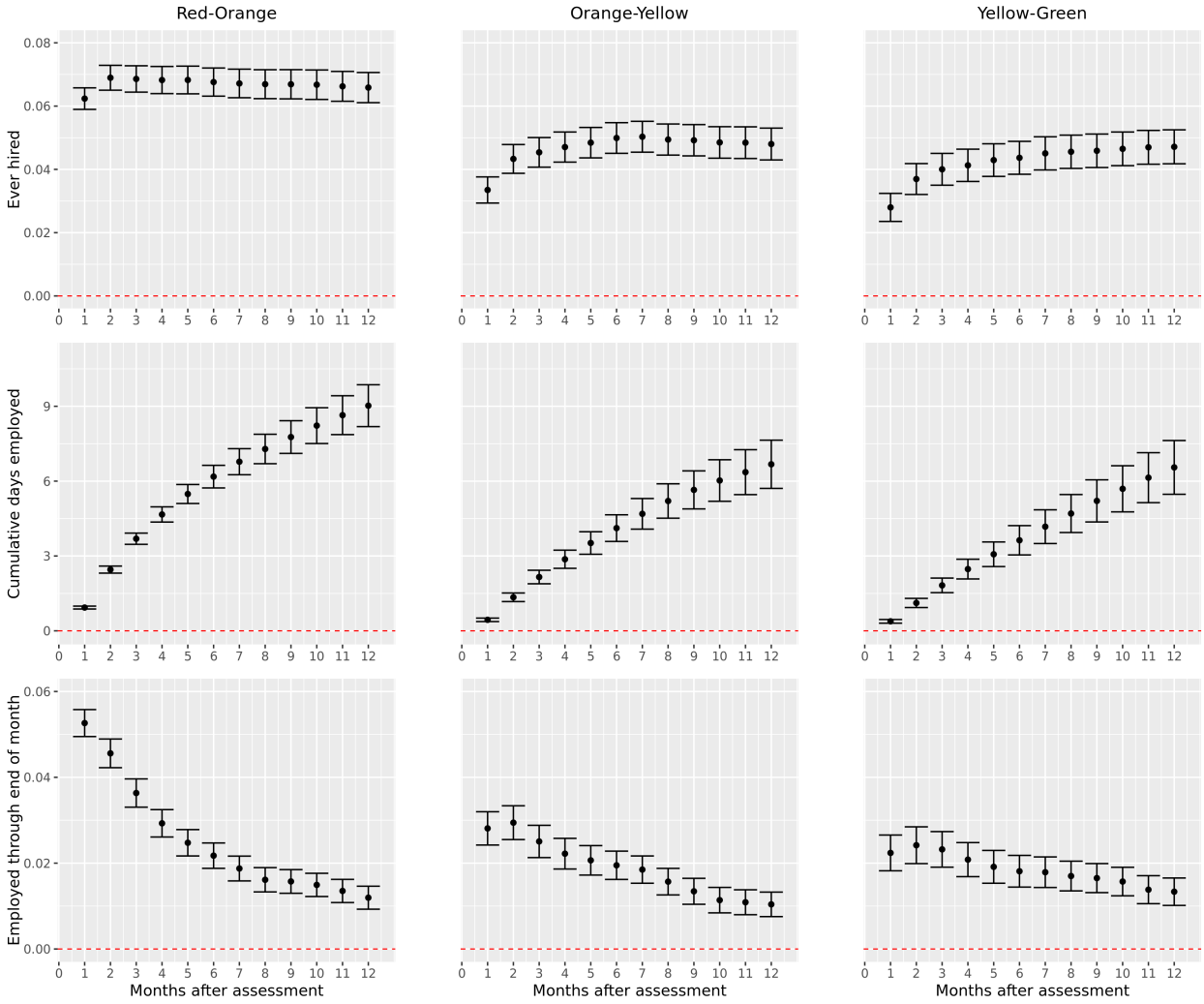
Notes: Panel A shows the percentage of applicants hired at Walmart for each z-score. Panel B shows the score density. Z scores are calculated to the hundredths place. Overlaid are dashed lines indicating the threshold values for each discontinuity.

Figure 2: RD plots for Walmart and jail outcomes



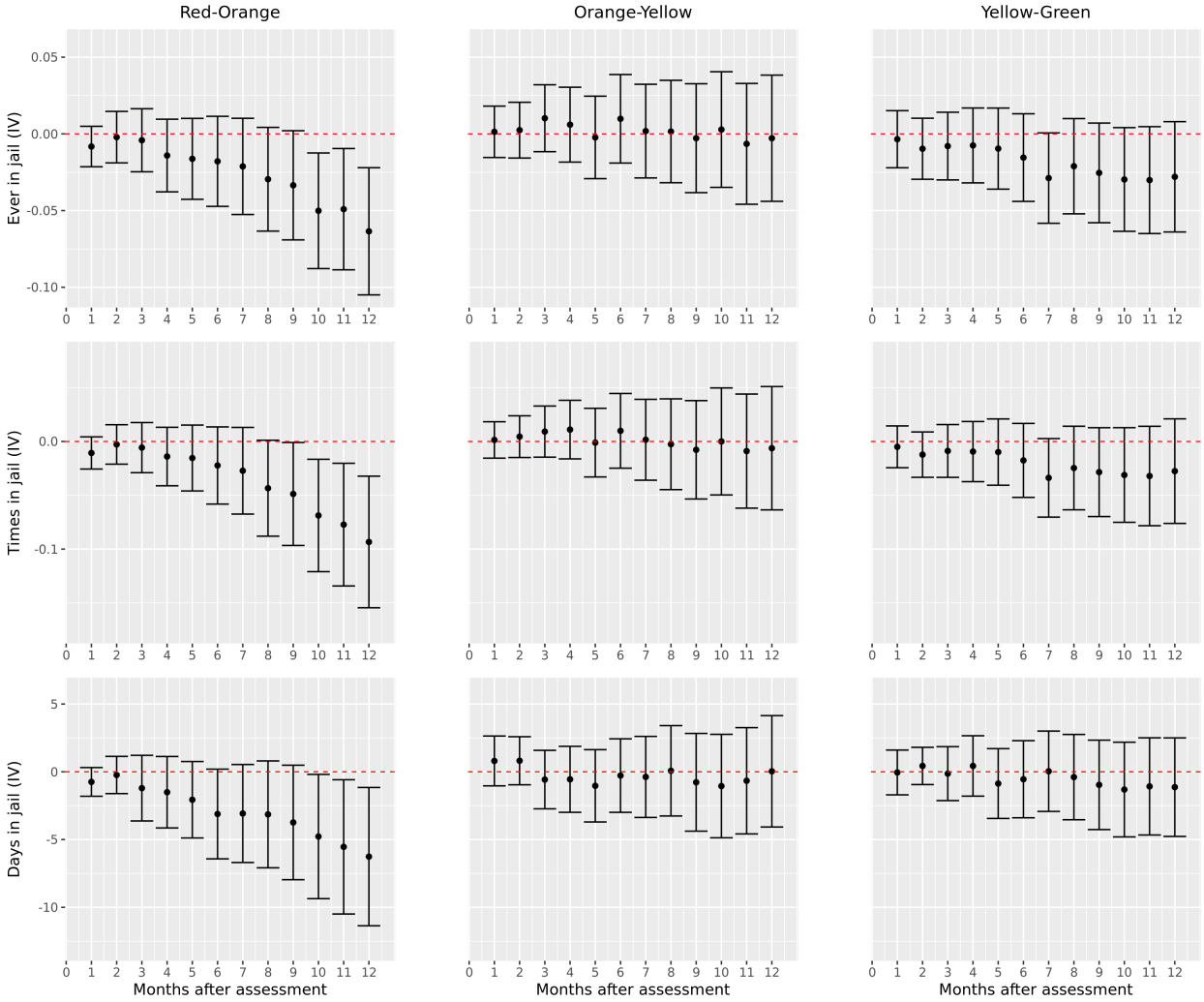
Notes: Figures show averages of bins within 0.5 of each threshold for each of the hiring and jail outcomes, with a bin size of 0.1. Fit lines are overlaid on these points and were determined using a local linear regression with a bandwidth of 0.5 and a triangular kernel, without controls.

Figure 3: Effects on Walmart outcomes over time (one-year horizon)



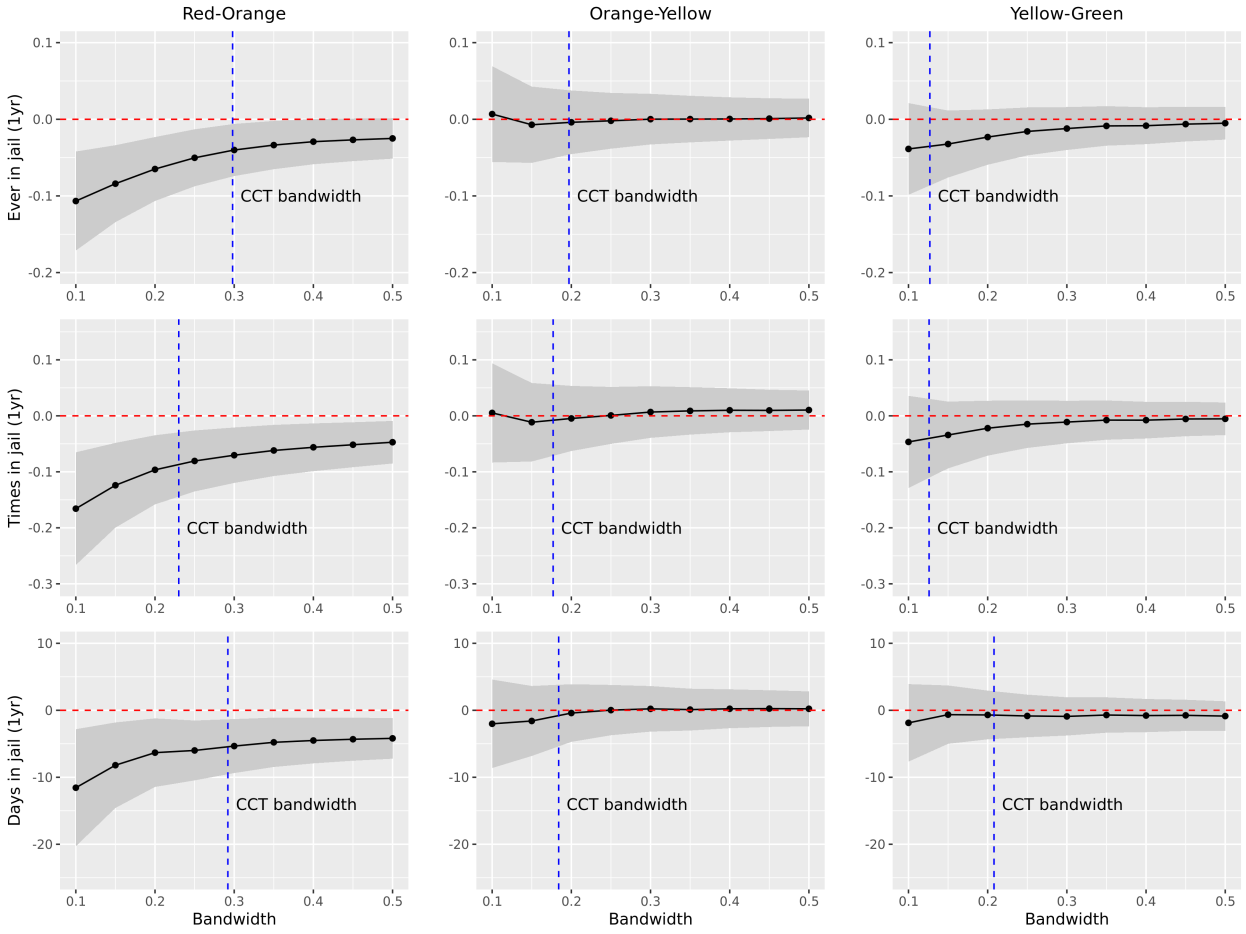
Notes: Figures show estimates at month-long intervals after the assessment for the three thresholds, where one month is represented by thirty days. Being "employed through the end of month" captures if an applicant is employed at Walmart at the end of each 30-day interval and does not require continuous employment beforehand. Error bars are 95% confidence intervals. Each regression used a bandwidth of 0.2 and a triangular kernel.

Figure 4: Effects on jail outcomes over time (one-year horizon)



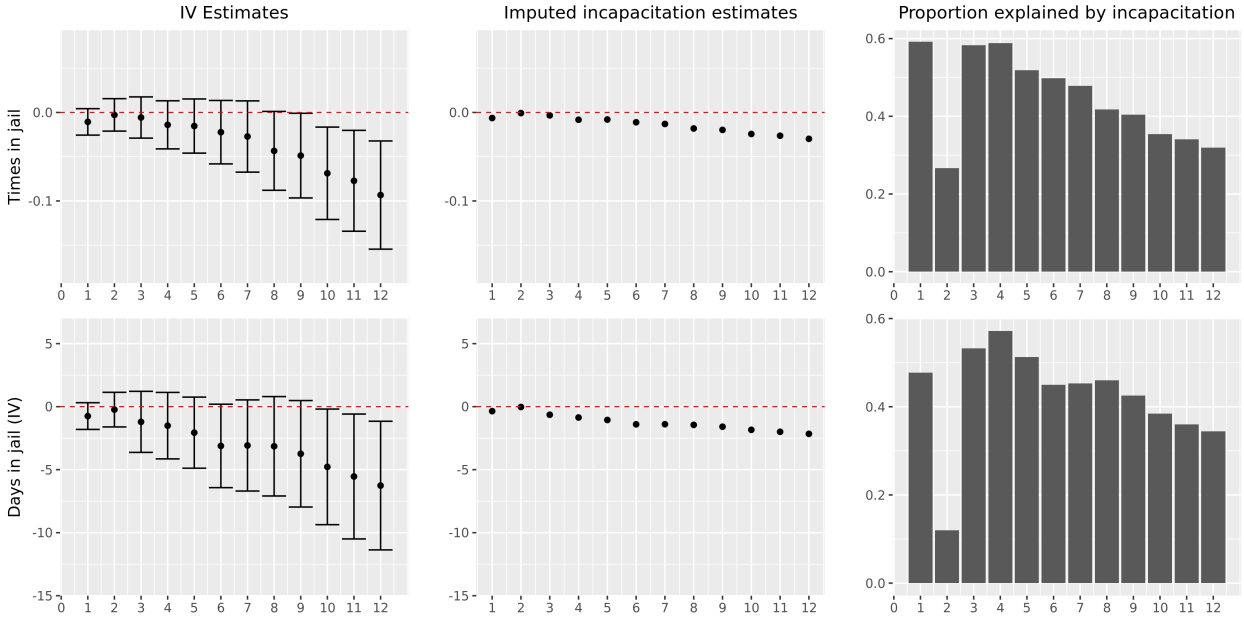
Notes: Figures show estimates at month-long intervals after the assessment for the three thresholds, where one month is represented by thirty days. Figures show the cumulative IV estimates on the three main jail outcomes by each month following the assessment. Error bars are 95% confidence intervals. Each regression used a bandwidth of 0.2 and a triangular kernel.

Figure 5: Bandwidth sensitivity



Notes: Figures show estimates from local linear regressions using a triangular kernel across bandwidths ranging from 0.1 to 0.5, at intervals of 0.05. Vertical dashed lines indicate the MSE-optimal bandwidth determined from Calonico et al. (2014). Error bars represent 95% confidence intervals.

Figure 6: Proportion of effect explained by incapacitation



Notes: Figures show the results of an exercise estimating the proportion of the treatment effect attributable to incapacitation. The first column shows estimates from Figure 4. The second column shows the estimates from using the same specification on imputed jail outcomes determined from control complier means for each month. The third column divides the point estimate from the first column by the point estimate from the second column.

Appendix

Table A1: Continuity tests across thresholds

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)
Male	0.003	-0.001	0.003
	(0.005)	(0.004)	(0.004)
Female	-0.003	0.001	-0.003
	(0.005)	(0.004)	(0.004)
White	0.006	-0.003	-0.001
	(0.004)	(0.003)	(0.003)
Black	-0.003	0.003	0.003
	(0.005)	(0.004)	(0.003)
Hispanic	0.0002	0.0012	0.0008
	(0.0044)	(0.0036)	(0.0034)
Asian	0.0025	0.0028**	-0.0003
	(0.0015)	(0.0012)	(0.0012)
At least 18	0.0048	0.0003	-0.0023
	(0.0030)	(0.0027)	(0.0028)
High school student	-0.003	0.001	0.002
	(0.004)	(0.003)	(0.003)
College or trade student	-0.007**	0.004	0.001
	(0.003)	(0.003)	(0.003)
Not a student	0.010**	-0.004	-0.003
	(0.004)	(0.004)	(0.003)
Any jail time in year prior to assessment	0.0002	-0.0003	-0.0016**
	(0.0012)	(0.0009)	(0.0008)
Times in jail in year prior to assessment	0.0004	-0.0002	-0.0015
	(0.0016)	(0.0012)	(0.0011)
Days in jail in year prior to assessment	-0.026	0.033	0.032
	(0.071)	(0.057)	(0.052)
Hiring helper score	-0.029	-0.044	-0.102
	(0.084)	(0.070)	(0.068)
p-value for joint F-test	0.239	0.980	0.359
N	216,023	318,898	360,299
Bandwidth	0.20	0.20	0.20

Notes: Table reports the estimates from local linear regressions on covariates using a triangular kernel and a bandwidth of 0.2. The joint F-test is on the null hypothesis that all discontinuities across each threshold on the reported covariates are zero. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Table A2: Complier means

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)
Male	0.544	0.517	0.488
	(0.019)	(0.029)	(0.033)
Female	0.440	0.472	0.504
	(0.019)	(0.029)	(0.033)
White	0.317	0.302	0.259
	(0.017)	(0.026)	(0.030)
Black	0.304	0.364	0.330
	(0.018)	(0.027)	(0.031)
Hispanic	0.264	0.226	0.266
	(0.017)	(0.025)	(0.029)
Asian	0.021	0.027	0.043
	(0.006)	(0.009)	(0.010)
Adult	0.946	0.932	0.922
	(0.010)	(0.016)	(0.019)
Did not complete high school	0.086	0.058	0.034
	(0.023)	(0.020)	(0.017)
In high school	0.058	0.069	0.068
	(0.021)	(0.020)	(0.025)
In high school, plans for college	0.075	0.120	0.081
	(0.023)	(0.021)	(0.022)
High school graduate	0.607	0.558	0.472
	(0.040)	(0.037)	(0.042)
In college	0.105	0.125	0.179
	(0.027)	(0.025)	(0.030)
Associate's degree	0.024	0.026	0.083
	(0.014)	(0.016)	(0.020)
Bachelor's degree	0.033	0.036	0.085
	(0.013)	(0.014)	(0.019)
Advanced degree	0.011	0.008	-0.002
	(0.009)	(0.006)	(0.008)
Any retail experience	0.797	0.688	0.741
	(0.035)	(0.033)	(0.038)
Longest employment stint $\geq$ one year	0.611	0.623	0.746
	(0.040)	(0.036)	(0.040)
Most important factor for job: work schedule	0.114	0.159	0.163
	(0.031)	(0.027)	(0.031)
Most important factor for job: how quickly I can get hired	0.403	0.321	0.273
	(0.038)	(0.034)	(0.037)
Most important factor for job: opportunity for long-term career	0.226	0.361	0.356
	(0.036)	(0.034)	(0.039)
Bandwidth	0.20	0.20	0.20

Notes: Table reports the complier means for the listed covariates. Means were calculated by employing an IV regression using YD and D as the outcome and endogenous variables, for Y the covariate and D an indicator on hire within one year of the assessment. Data from assessment responses was only available for applicants who took the assessment beginning February 2024. These responses include all variables below "Adult."

Table A3: Estimates across county thresholds

	Ever in jail (1yr)		Times in jail (1yr)		Days in jail (1yr)	
	IV	Y(0) Mean	IV	Y(0) Mean	IV	Y(0) Mean
	(1)	(2)	(3)	(4)	(5)	(6)
0 percent						
Red-Orange	-0.028***	0.034	-0.040***	0.047	-2.508***	2.637
	(0.008)	(0.008)	(0.011)	(0.011)	(0.884)	(0.862)
Orange-Yellow	-0.004	0.015	-0.005	0.017	-0.137	0.440
	(0.009)	(0.008)	(0.012)	(0.012)	(0.866)	(0.848)
Yellow-Green	-0.007	0.015	-0.008	0.019	-0.533	0.484
	(0.008)	(0.008)	(0.011)	(0.011)	(0.780)	(0.720)
Combined	-0.015***	0.023	-0.021***	0.030	-1.260**	1.419
	(0.005)	(0.005)	(0.007)	(0.007)	(0.507)	(0.490)
25 percent						
Red-Orange	-0.065***	0.074	-0.093***	0.105	-6.097***	6.419
	(0.018)	(0.018)	(0.026)	(0.026)	(2.057)	(2.002)
Orange-Yellow	-0.009	0.026	-0.014	0.032	0.035	0.527
	(0.018)	(0.018)	(0.026)	(0.025)	(1.799)	(1.759)
Yellow-Green	-0.013	0.027	-0.018	0.038	-0.829	0.717
	(0.017)	(0.015)	(0.023)	(0.021)	(1.577)	(1.441)
Combined	-0.030***	0.043	-0.044***	0.060	-2.532**	2.864
	(0.010)	(0.010)	(0.015)	(0.014)	(1.081)	(1.040)
50 percent						
Red-Orange	-0.070***	0.079	-0.099***	0.111	-6.505***	6.822
	(0.019)	(0.018)	(0.027)	(0.027)	(2.169)	(2.110)
Orange-Yellow	-0.009	0.029	-0.014	0.035	-0.026	0.586
	(0.019)	(0.018)	(0.027)	(0.026)	(1.900)	(1.858)
Yellow-Green	-0.014	0.028	-0.019	0.039	-0.859	0.731
	(0.017)	(0.016)	(0.023)	(0.022)	(1.618)	(1.478)
Combined	-0.031***	0.045	-0.045***	0.062	-2.636**	2.959
	(0.011)	(0.010)	(0.015)	(0.015)	(1.125)	(1.080)
75 percent						
Red-Orange	-0.070***	0.080	-0.104***	0.117	-6.446***	6.805
	(0.020)	(0.020)	(0.030)	(0.029)	(2.373)	(2.305)
Orange-Yellow	-0.015	0.033	-0.022	0.039	-0.346	0.959
	(0.020)	(0.019)	(0.028)	(0.027)	(2.045)	(1.999)
Yellow-Green	-0.016	0.027	-0.016	0.035	-0.959	0.761
	(0.018)	(0.017)	(0.025)	(0.023)	(1.753)	(1.600)
Combined	-0.034***	0.046	-0.048***	0.064	-2.688**	3.029
	(0.011)	(0.011)	(0.016)	(0.016)	(1.216)	(1.167)
Bandwidth	0.20	0.20	0.20	0.20	0.20	0.20

Notes: Table reports the IV estimates from using different subsets of applicants from the data. For example, the "25 percent" estimates show the results using all applicants located in counties that have jail data with at least 25% non-null values for DOB. The "0 percent" estimates include applicants from all counties. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$. Stars not reported for $Y(0)$ means.

Table A4: IV estimate by endogenous variable (ever in jail within one year)

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)	Combined (4)
Hire variables				
Hired within one year	-0.065***	-0.004	-0.023	-0.030***
	(0.021)	(0.021)	(0.018)	(0.012)
$Y(0)$ mean	0.079	0.028	0.032	0.043
	(0.021)	(0.026)	(0.017)	(0.011)
Hired within 90 days	-0.062***	-0.004	-0.028	-0.029**
	(0.020)	(0.022)	(0.022)	(0.012)
$Y(0)$ mean	0.076	0.023	0.034	0.042
	(0.020)	(0.021)	(0.020)	(0.012)
Hired within 30 days	-0.069***	-0.005	-0.040	-0.038**
	(0.023)	(0.031)	(0.032)	(0.016)
$Y(0)$ mean	0.086	0.033	0.052	0.057
	(0.022)	(0.030)	(0.030)	(0.015)
Job offer variables				
Job offer within one year	-0.050***	-0.003	-0.019	-0.022**
	(0.016)	(0.017)	(0.015)	(0.009)
$Y(0)$ mean	0.070	0.031	0.024	0.039
	(0.016)	(0.015)	(0.013)	(0.008)
Job offer within 90 days	-0.047***	-0.003	-0.022	-0.023**
	(0.016)	(0.017)	(0.017)	(0.010)
$Y(0)$ mean	0.068	0.032	0.025	0.040
	(0.015)	(0.015)	(0.015)	(0.009)
Job offer within 30 days	-0.047***	-0.003	-0.026	-0.025**
	(0.015)	(0.020)	(0.021)	(0.011)
$Y(0)$ mean	0.069	0.037	0.036	0.047
	(0.015)	(0.018)	(0.018)	(0.010)
N	215,298	318,157	359,587	894,511
Controls	Y	Y	Y	Y
Bandwidth	0.20	0.20	0.20	0.20

Notes: Table reports the IV estimates from using different endogenous variables for D in the main specifications (2)-(5). Controls include zip code fixed effects, gender, race, and information from application questions (night-shift availability, age group, sponsorship requirements, right to work status, full-time preference, student status, retail experience, grocery experience, warehouse experience, and manufacturing experience). $Y(0)$ means were calculated by employing the same regression using $Y(1-D)$ and $(1-D)$ as the outcome and endogenous variables, for Y the jail outcome and D the different endogenous hire variables. The combined regression (column 4) stacks the data corresponding to columns 1–3 uses three instruments for the discontinuities. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Table A5: IV estimates by sentence severity

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)	Combined (4)
Incarceration length < 30 days				
Ever in jail within one year	-0.052***	-0.021	-0.022	-0.030***
	(0.019)	(0.019)	(0.017)	(0.010)
$Y(0)$ mean	0.062	0.046	0.031	0.041
	(0.018)	(0.023)	(0.015)	(0.010)
Times in jail within one year	-0.064**	-0.027	-0.015	-0.032**
	(0.025)	(0.024)	(0.021)	(0.014)
$Y(0)$ mean	0.077	0.051	0.032	0.046
	(0.025)	(0.029)	(0.020)	(0.013)
Days in jail within one year	-0.144	-0.117	-0.084	-0.112
	(0.204)	(0.248)	(0.158)	(0.106)
$Y(0)$ mean	0.194	0.241	0.120	0.173
	(0.201)	(0.242)	(0.148)	(0.102)
Incarceration length ≥ 30 days				
Ever in jail within one year	-0.031**	0.017	-0.008	-0.006
	(0.012)	(0.012)	(0.010)	(0.006)
$Y(0)$ mean	0.029	-0.013	0.008	0.007
	(0.012)	(0.012)	(0.009)	(0.006)
Times in jail within one year	-0.033**	0.022	-0.007	-0.004
	(0.015)	(0.014)	(0.011)	(0.007)
$Y(0)$ mean	0.033	-0.018	0.007	0.006
	(0.015)	(0.013)	(0.010)	(0.007)
Days in jail within one year	-6.170**	-0.388	-0.634	-1.991
	(2.583)	(2.723)	(1.841)	(1.246)
$Y(0)$ mean	6.636	0.855	0.218	2.237
	(2.506)	(2.670)	(1.672)	(1.187)
N	215,298	318,157	359,587	894,511
Controls	Y	Y	Y	Y
Bandwidth	0.20	0.20	0.20	0.20

Notes: Table reports the IV estimates on jail outcomes with lengths of stay below or above thirty days. Controls include zip code fixed effects, gender, race, and information from application questions (night-shift availability, age group, sponsorship requirements, right to work status, full-time preference, student status, retail experience, grocery experience, warehouse experience, and manufacturing experience). $Y(0)$ means were calculated by employing the same regression using $Y(1-D)$ and $(1-D)$ as the outcome and endogenous variables, for Y the jail outcome and D the different endogenous hire variables. The combined regression (column 4) stacks the data corresponding to columns 1–3 uses three instruments for the discontinuities. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

Table A6: IV estimates across charge classifications (times in jail within one year)

	Red-Orange (1)	Orange-Yellow (2)	Yellow-Green (3)	Combined (4)	Avg. Jail Days (5)	Prop. of Obs. (6)
Violent	-0.031**	0.000	-0.021*	-0.016**	51.0	0.233
	(0.013)	(0.013)	(0.012)	(0.007)		
$Y(0)$ mean	0.033	0.003	0.029	0.020		
	(0.013)	(0.012)	(0.011)	(0.007)		
Property	-0.017	-0.007	-0.004	-0.009	41.9	0.196
	(0.012)	(0.014)	(0.011)	(0.007)		
$Y(0)$ mean	0.016	0.010	0.007	0.011		
	(0.012)	(0.014)	(0.011)	(0.007)		
Drug	-0.004	0.008	0.007	0.004	39.4	0.130
	(0.010)	(0.011)	(0.008)	(0.006)		
$Y(0)$ mean	0.007	-0.003	-0.001	0.001		
	(0.010)	(0.010)	(0.007)	(0.005)		
Public order	-0.035**	-0.008	-0.015	-0.016*	42.1	0.350
	(0.017)	(0.018)	(0.014)	(0.009)		
$Y(0)$ mean	0.039	0.015	0.024	0.023		
	(0.017)	(0.017)	(0.013)	(0.009)		
DUI	-0.010*	-0.008	0.009*	-0.002	21.4	0.043
	(0.006)	(0.006)	(0.005)	(0.003)		
$Y(0)$ mean	0.011	0.007	-0.004	0.004		
	(0.005)	(0.005)	(0.005)	(0.003)		
Criminal traffic	-0.011	-0.011	0.007	-0.004	33.4	0.089
	(0.007)	(0.009)	(0.007)	(0.004)		
$Y(0)$ mean	0.014	0.012	0.000	0.008		
	(0.007)	(0.008)	(0.006)	(0.004)		
Unknown	-0.037**	0.004	-0.003	-0.011	40.5	0.309
	(0.018)	(0.014)	(0.013)	(0.009)		
$Y(0)$ mean	0.040	0.003	0.004	0.014		
	(0.018)	(0.014)	(0.012)	(0.008)		
N	215,298	318,157	359,587	894,511	46,064	46,064
Bandwidth	0.20	0.20	0.20	0.20		

Notes: For each jail observation, potentially several charges are observed. Constituent estimates may not equal the aggregate estimate, as multiple charge types may be observed for a single jail booking. Column (5) calculates the average jail time for all bookings associated with the corresponding charge of each row. For this reason, this column is representative of jail lengths for both each charge type and also the charges co-occurring with each charge type. Column (6) calculates the proportion of all jail bookings within one year of the assessment that contain the charge corresponding to each row, and a booking is recorded as having an unknown charge type only if there are no charges associated. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$. Stars not reported for $Y(0)$ means.

Table A7: Estimates across detailed charge classifications (all charges)

	Red-Orange			Stacked		
	IV	Y(0) Mean	% of Mean	IV	Y(0) Mean	% of Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Violent						
Assault	-0.011	0.014	-77.1%	-0.007	0.011	-61.0%
	(0.011)	(0.011)		(0.006)	(0.006)	
Blackmail	-0.008*	0.006	-130.0%	-0.003	0.003	-96.5%
	(0.005)	(0.004)		(0.002)	(0.002)	
Robbery	-0.010***	0.009	-118.3%	-0.006**	0.005	-104.3%
	(0.004)	(0.004)		(0.002)	(0.002)	
Child-related	-0.006	0.006	-102.0%	-0.001	0.002	-47.6%
	(0.004)	(0.004)		(0.002)	(0.002)	
Murder	-0.004	0.004	-102.5%	-0.000	0.000	-104.9%
	(0.003)	(0.003)		(0.001)	(0.001)	
Sexual assault	-0.002	0.002	-100.8%	-0.001	0.001	-100.5%
	(0.001)	(0.001)		(0.001)	(0.001)	
Trafficking/kidnapping	-0.001	0.001	-54.1%	-0.001	0.002	-42.6%
	(0.003)	(0.003)		(0.002)	(0.001)	
Other	-0.003	0.003	-96.4%	-0.002**	0.002	-99.6%
	(0.002)	(0.002)		(0.001)	(0.001)	
Property						
Fraud	0.006	-0.005	-118.2%	0.003	-0.002	-193.6%
	(0.006)	(0.006)		(0.004)	(0.004)	
Theft	-0.002	0.002	-95.0%	-0.002	0.002	-103.6%
	(0.008)	(0.007)		(0.004)	(0.004)	
Burglary/trespassing	-0.009	0.009	-101.7%	-0.006	0.005	-104.0%
	(0.006)	(0.006)		(0.004)	(0.004)	
Destruction/stolen	-0.003	0.005	-74.8%	-0.001	0.002	-48.3%
	(0.005)	(0.005)		(0.003)	(0.003)	
Auto	-0.010**	0.008	-118.2%	-0.005**	0.005	-108.1%
	(0.004)	(0.004)		(0.002)	(0.002)	
Other	-0.002	0.002	-100.0%	0.000	-0.000	-98.1%
	(0.002)	(0.002)		(0.001)	(0.001)	
Drug						
Possession	-0.008	0.010	-79.0%	-0.004	0.008	-50.1%
	(0.008)	(0.008)		(0.005)	(0.004)	
Distribution	0.003	-0.002	-159.2%	0.002	-0.002	-102.4%
	(0.004)	(0.004)		(0.002)	(0.002)	
Other	-0.003	0.003	-108.5%	0.003	-0.002	-162.4%
	(0.006)	(0.006)		(0.003)	(0.003)	
Public order						
Court offenses	0.000	0.000	2.3%	-0.011*	0.011	-95.3%
	(0.011)	(0.011)		(0.006)	(0.005)	
Obstruction/escape	-0.012	0.012	-94.2%	0.005	-0.003	-177.5%
	(0.008)	(0.008)		(0.005)	(0.004)	
Probation/parole violations	-0.008	0.009	-87.8%	-0.001	0.001	-86.6%
	(0.007)	(0.007)		(0.004)	(0.004)	
Weapon offenses	-0.013**	0.013	-99.0%	-0.005	0.007	-76.1%
	(0.006)	(0.006)		(0.003)	(0.003)	
Disorderly conduct	-0.013**	0.014	-91.2%	-0.009**	0.011	-80.8%
	(0.006)	(0.006)		(0.004)	(0.004)	
Other	-0.000	0.002	-12.2%	-0.002	0.003	-82.1%
	(0.004)	(0.003)		(0.002)	(0.002)	

Notes: Table reports the IV estimates on jail admissions using detailed charge classifications from the CJARS text-based classifier. Constituent estimates in a given charge class (e.g., violent crimes) may not add to the aggregate estimate from Table 5, as one jail admission may incorporate multiple offense types. $Y(0)$ means were calculated by employing the same regression using $Y(1-D)$ and $(1-D)$ as the outcome and endogenous variables, for Y the jail outcome and D the different endogenous hire variables. There are not meaningfully more detailed subcategories for DUI or criminal traffic offenses. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$. Stars not reported for $Y(0)$ means.

Table A8: IV estimate heterogeneity (red-orange threshold)

	Ever in jail (1yr)		Times in jail (1yr)		Days in jail (1yr)	
	IV	Y(0) Mean	IV	Y(0) Mean	IV	Y(0) Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Gender						
Male	-0.092***	0.104	-0.137***	0.154	-11.606**	12.110
	(0.034)	(0.033)	(0.050)	(0.049)	(4.766)	(4.636)
Female	-0.024	0.034	-0.040	0.049	0.579	-0.059
	(0.026)	(0.026)	(0.039)	(0.038)	(2.005)	(1.940)
Race						
White	-0.042	0.049	-0.102*	0.110	-1.926	2.696
	(0.036)	(0.035)	(0.060)	(0.059)	(3.640)	(3.543)
Black	-0.102**	0.111	-0.095	0.107	-13.580*	14.305
	(0.047)	(0.046)	(0.062)	(0.060)	(7.127)	(6.868)
Hispanic	-0.042	0.050	-0.082*	0.093	-4.963*	5.531
	(0.032)	(0.032)	(0.045)	(0.044)	(2.663)	(2.624)
Asian	-0.269	0.133	-0.246	0.110	4.303	-5.528
	(0.327)	(0.237)	(0.335)	(0.257)	(17.525)	(17.709)
Age						
At least 18	-0.068***	0.079	-0.101***	0.115	-6.703**	7.239
	(0.022)	(0.022)	(0.033)	(0.032)	(2.751)	(2.665)
Under 18	-0.013	0.014	-0.026	0.028	-0.470	0.480
	(0.067)	(0.067)	(0.073)	(0.073)	(5.042)	(5.042)
Student status						
High school	-0.052	0.064	-0.078	0.089	-4.158	4.178
	(0.087)	(0.086)	(0.110)	(0.109)	(6.920)	(6.919)
College or trade	-0.107**	0.135	-0.233***	0.265	-11.943	13.924
	(0.048)	(0.047)	(0.087)	(0.087)	(7.722)	(7.640)
Not a student	-0.057**	0.063	-0.077**	0.085	-5.055*	5.306
	(0.025)	(0.024)	(0.036)	(0.036)	(2.968)	(2.862)
Controls	N	N	N	N	N	N
Bandwidth	0.20	0.20	0.20	0.20	0.20	0.20

Notes: Table reports the IV estimates on jail outcomes for different subsets of the data across the red-orange threshold. $Y(0)$ means were calculated by employing the same regression using $Y(1-D)$ and $(1-D)$ as the outcome and endogenous variables, for Y the jail outcome and D the different endogenous hire variables. (*) $p < 0.10$ (**) $p < 0.05$ (***) $p < 0.01$.

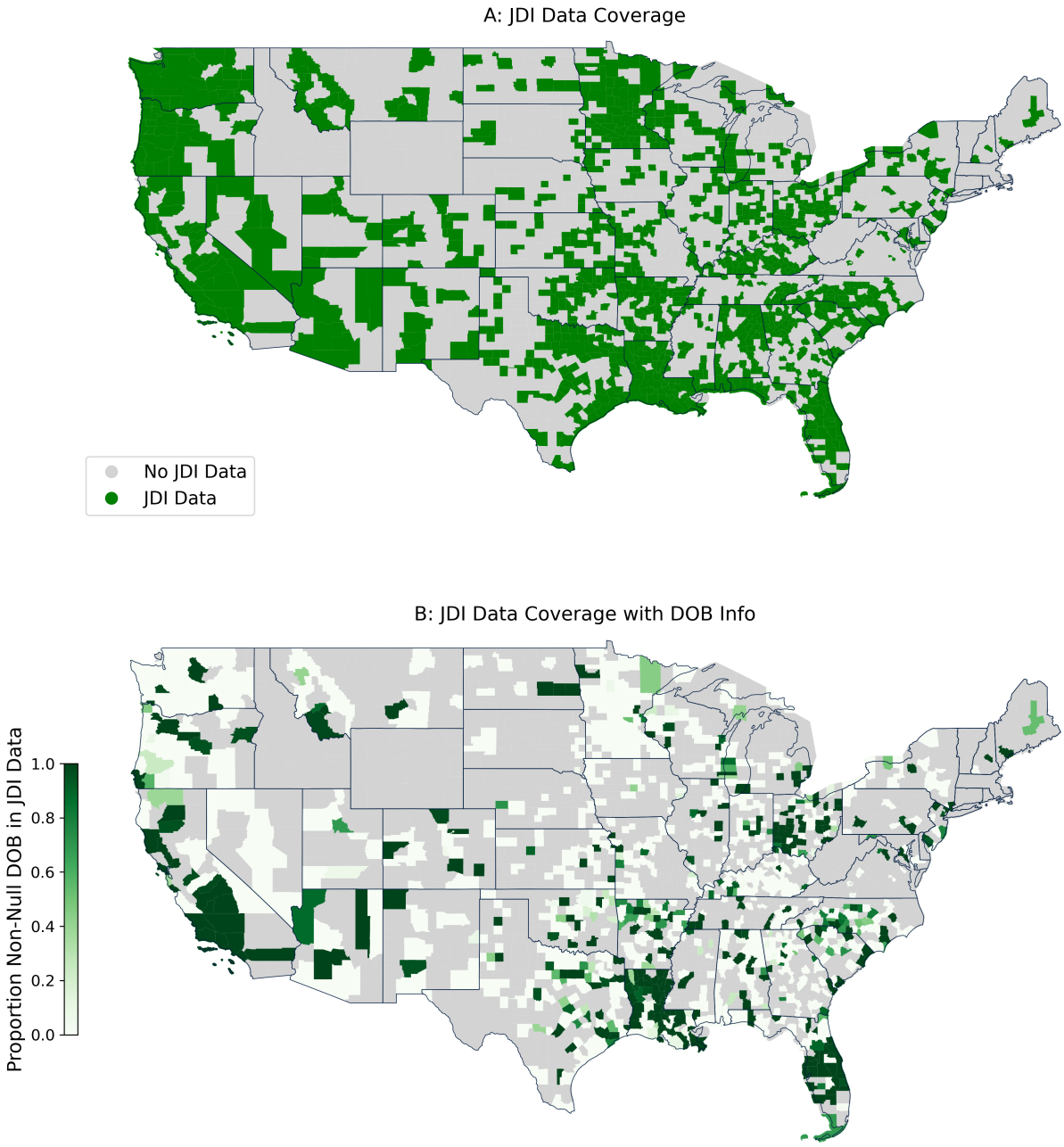
Table A9: Cost savings from crime reductions

Charge type (CJARS)	Source	Cohen and Piquero (2007 $)			Miller (1993 $)	Bottom-up costs	WTP costs	% of
		Victim. costs	CJ costs	Offender costs	Total costs	(2025 $)	(2025 $)	offenses
Violent crimes								
Aggravated assault	CP: aggravated assault	$37,000	$13,500	$6,400		$88,354	$131,988	59.5%
Simple assault	CP: simple assault	$4,500	$5,000	$1,300		$16,770	$29,503	4.4%
Assaulting public officer	CP: simple assault	$4,500	$5,000	$1,300		$16,770	$29,503	3.5%
Blackmail/extortion/intimidation	CP: other offenses	$0	$500	$0		$776	$1,553	10.1%
Armed robbery	CP: armed robbery	$29,000	$14,700	$8,000		$80,280	$434,784	6.1%
Unarmed robbery	CP: robbery	$12,000	$7,400	$4,000		$36,336	$60,559	0.8%
Child abuse	Miller: child abuse				$60,000	$133,656	$133,656	3.5%
Lewd act with children	Miller: child abuse				$99,000	$220,532	$220,532	1.3%
Rape - statutory - no force	Miller: child sexual abuse				$99,000	$220,532	$220,532	0.3%
Murder	CP: murder	$4,600,000	$300,000	$140,000		$7,826,112	$18,323,040	3.1%
Rape - force	CP: rape	$135,000	$8,300	$4,500		$229,504	$450,312	1.0%
Sexual assault - other	CP: rape	$135,000	$8,300	$4,500		$229,504	$450,312	0.7%
Kidnapping	Miller: other assault or attempt				$9,400	$20,939	$20,939	3.2%
Human Trafficking	Miller: other assault or attempt				$9,400	$20,939	$20,939	0.1%
Violent offenses - other	Miller: other assault or attempt				$9,400	$20,939	$20,939	1.4%
Hit and run driving - injury	CP: drunk driving crash	$28,000	$1,700	$700		$47,205	$93,168	0.4%
Voluntary/nonnegligent manslaughter	Miller: arson deaths				$2,740,000	$6,103,624	$6,103,624	0.3%
Manslaughter - non-vehicular	Miller: arson deaths				$2,740,000	$6,103,624	$6,103,624	0.2%
Unspecified homicide	Miller: arson deaths				$2,740,000	$6,103,624	$6,103,624	0.0%
Average cost of a violent crime						$351,422	$733,854	
IV estimate on violent crimes						-0.016		
Change in costs per marginal hire						-$5,485	-$11,454	
Property crimes								
Forgery/fraud	CP: fraud	$1,100	$1,700	$700		$5,435	$8,540	22.0%
Financial Crimes	CP: fraud	$1,100	$1,700	$700		$5,435	$8,540	0.0%
Larceny/theft - value unknown	CP: larceny	$450	$1,700	$700		$4,425	$6,211	24.0%
Grand larceny - theft over $500	CP: larceny	$450	$1,700	$700		$4,425	$6,211	6.6%
Petty larceny - theft equal or under $500	CP: larceny	$450	$1,700	$700		$4,425	$6,211	3.9%
Trespassing	CP: other offenses	$0	$500	$0		$776	$1,553	11.6%
Burglary	CP: burglary	$2,000	$2,300	$1,000		$8,230	$54,348	11.3%
Destruction of property	CP: vandalism	$370	$630	$0		$1,553	$3,106	8.0%
Stolen property - receiving	CP: other offenses	$0	$500	$0		$776	$1,553	4.8%
Stolen property - trafficking	CP: other offenses	$0	$500	$0		$776	$1,553	0.0%
Auto theft	CP: motor vehicle theft	$5,500	$2,900	$1,000		$14,596	$26,398	0.0%
Hit and run driving - property damage	Miller: drunk driving, no injury				$2,700	$6,015	$6,015	2.1%
Unauthorized use of vehicle	CP: other offenses	$0	$500	$0		$776	$1,553	3.8%
Property offenses - other	CP: other offenses	$0	$500	$0		$776	$1,553	1.0%
Arson	CP: arson	$57,000	$1,700	$700		$92,236	$178,572	0.7%
Average cost of a property crime						$4,989	$12,214	
IV estimate on property crimes						-0.004		
Change in costs per marginal hire						-$18	-$45	

Charge type (CJARS)	Source	Cohen and Piquero (2007 $)			Miller (1993 $)	Bottom-up costs	WTP costs	% of
		Victim. costs	CJ costs	Offender costs	Total costs	(2025 $)	(2025 $)	offenses
Drug crimes								
Possession/distribution	CP: other offenses	$0	$500	$0		$776	$1,553	100.0%
Average cost of a drug crime						$776	$1,553	
IV estimate on drug crimes						0.001		
Change in costs per marginal hire						$1	$2	
Public order crimes								
Public order	CP: other offenses	$0	$500	$0		$776	$1,553	100.0%
Average cost of a public order crime						$776	$1,553	
IV estimate on public order crimes						-0.007		
Change in costs per marginal hire						-$6	-$12	
DUI crimes								
DUI	MCW: drunk driving, no injury				$2,700	$6,015	$6,015	100.0%
Average cost of a DUI crime						$6,015	$6,015	
IV estimate on DUI crimes						0.001		
Change in costs per marginal hire						$6	$6	
Criminal traffic crimes								
Criminal traffic	CP: other offenses	$0	$500	$0		$776	$1,553	100.0%
Average cost of a criminal traffic crime						$776	$1,553	
IV estimate on criminal traffic crimes						-0.001		
Change in costs per marginal hire						-$1	-$2	
Unknown charge type								
Average cost of unknown crime type						$122,583	$256,307	
IV estimate on unknown crimes						-0.014		
Change in costs per marginal hire						-$1,725	-$3,606	
All crimes								
Total change in costs per marginal hire						-$7,225	-$15,106	

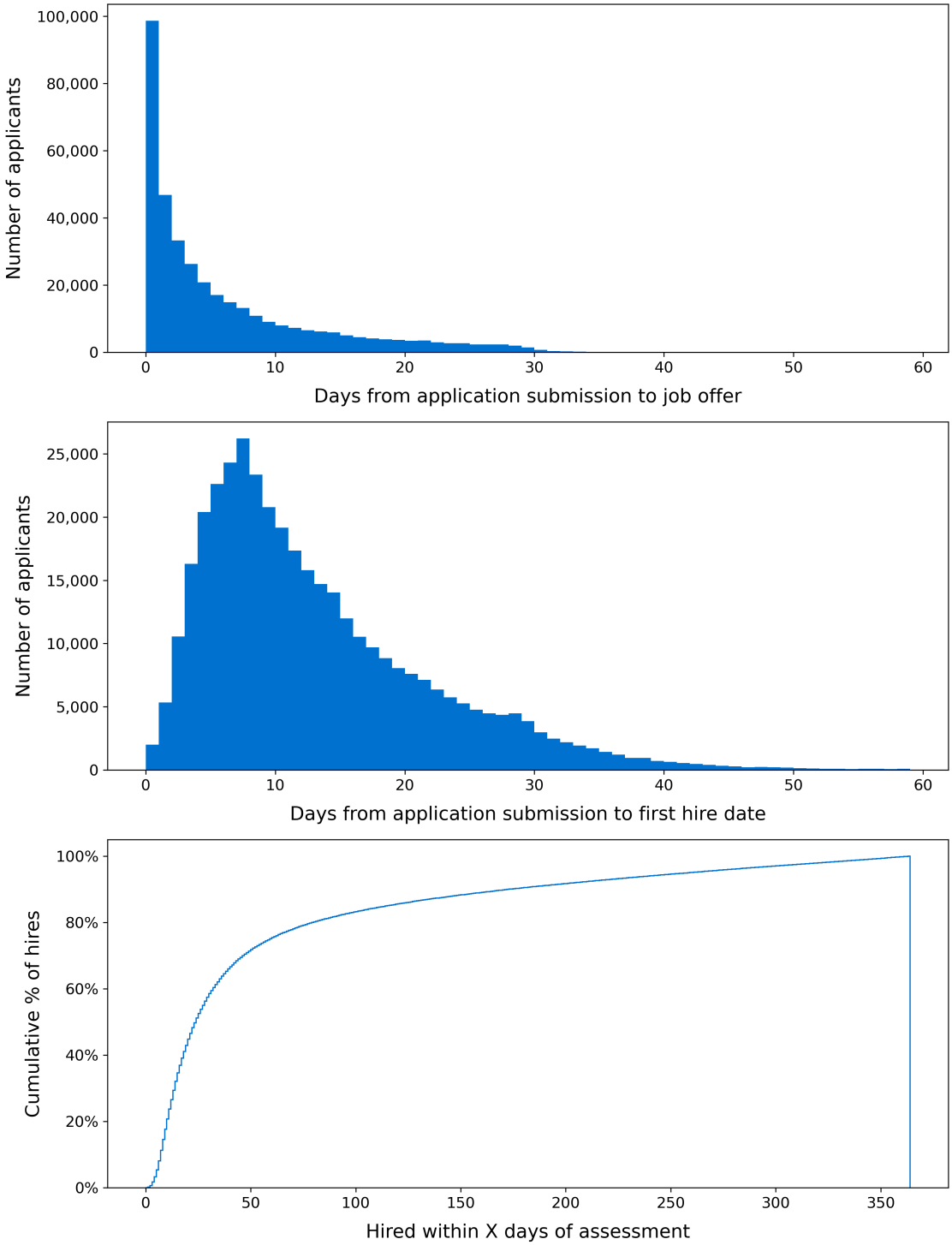
Notes: The JDI data gives more detailed subclassifications for each crime type, shown under the violent and property crime sections. For each of these charge types, we refer to Cohen and Piquero (2009) for the costs of victimization, criminal justice proceedings and incarceration, and lost offender productivity. When a suitable match is not found between the CJARS charge subclassifications and the classes from Cohen and Piquero (2009), we use the total victimization cost from Miller et al. (1996) instead. We refer to each study as CP or MCW. In the absence of data on manslaughter, we use the least costly item under the "Fatal Crime" section from Miller et al. (1996), which is for arson deaths. Costs are then converted to 2025 dollars. When WTP estimates are not available, we use victimization cost estimates from Miller et al. (1996). Using the jail data matches from the main sample, we find the proportion of crimes explained by each subclassification, and calculate the average cost of crime for the violent and property crimes in our sample. We then multiply this cost by the estimates from Table 4.

Figure A1: JDI coverage



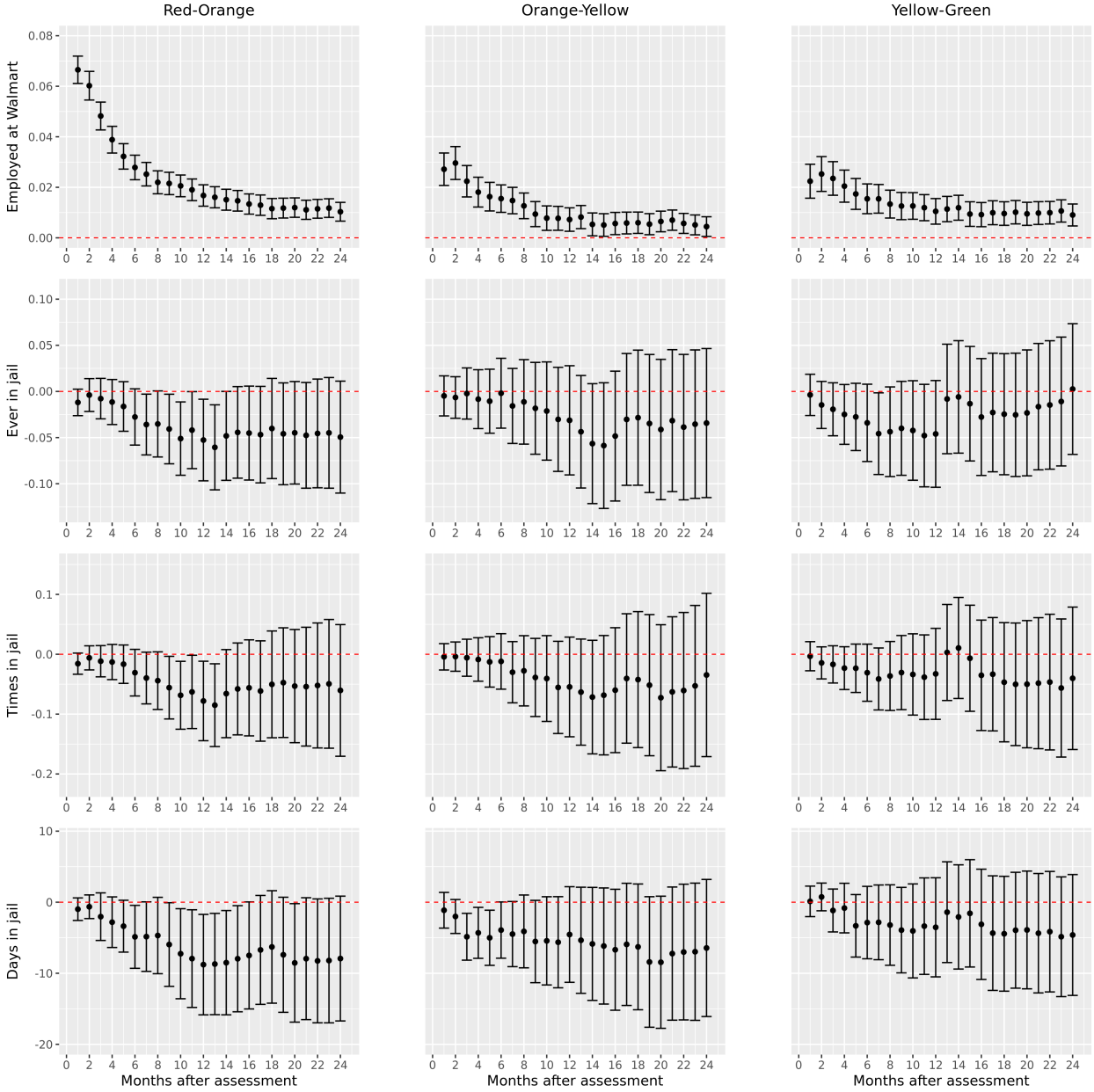
Notes: Figures show jail data coverage at the county-level. For our analysis, we only considered county jails. Data from city jails, for example, would not indicate coverage for its corresponding county.

Figure A2: Time between application, job offer, and hire date



Notes: Figures show the distribution of the number of days between application submission and job offer for that application, as well as days between application submission and eventual hire, among those hired. The bottom figure plots the cumulative distribution of the days between assessment date and earliest hire date, among those hired within one year of the assessment.

Figure A3: Effects over time (two-year horizon)



Notes: Figures show estimates at month-long intervals after the assessment for the three thresholds, where one month is represented by thirty days. These estimates were calculated only using the applicants in our main sample that took the assessment at least two years prior to the last observed date in the JDI data, or October 16, 2023. Figures show the cumulative IV estimates on the three main jail outcomes by each month following the assessment. Error bars are 95% confidence intervals. Each regression used a bandwidth of 0.2 and a triangular kernel.