Does Unemployment Insurance Reduce Crime?

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Abstract

Does unemployment insurance (UI) reduce criminal behavior for low-wage displaced workers? To answer this question, I examine the effect of UI eligibility on subsequent criminal justice system involvement with linked UI and jail bookings administrative data. I estimate this effect using a regression discontinuity design (RDD), which exploits the minimum earnings requirements for UI. I provide evidence indicating that being barely eligible for UI decreases arrest probability in the short run. A reduction in arrests for assaults and drug crimes drives the overall reduction. The effects are large, although somewhat imprecisely estimated–within one year, UI leads to a 49 percent decrease in arrest probability, a 77 percent decrease in assault arrest probability, and a 71 percent decrease in drug crime arrest probability. Moreover, I consistently find a negative relationship between UI eligibility and arrests across many different specifications, and I can generally rule out large positive effects, which might be a concern if UI benefits prolong spells of nonemployment. A back-of-the-envelope calculation suggests that this crime reduction generates large public benefits approximately equal to the fiscal cost of loosening monetary eligibility requirements.

JEL Classification: H53, I38, J65, K42

Keywords: unemployment insurance, crime, employment, low-wage work

1 Introduction

Studies of optimal unemployment design typically weigh the consumption-smoothing benefit of this social insurance program against the distortionary effects on labor supply (Baily, 1978; Chetty, 2006; Gruber, 1997). This framework fails to capture potential externalities unemployment insurance generates (Collinson & Jenkins, 2024; Hsu et al., 2018). Reduction in criminal justice system involvement is one such expensive externality. Because of the high social costs of crime, in many cases, such as Supplemental Security Income and Medicaid, reductions in criminality can fully or partially offset the fiscal cost of program expansions (Arenberg et al., 2020; Deshpande & Mueller-Smith, 2022; Jácome, 2022; Tuttle, 2019). Given the size of the UI system, serving over five million recipients a year, even small reductions in criminal involvement can create large fiscal savings.[1] Despite this, little is known about how UI impacts criminal involvement.

In this paper, I test whether UI eligibility affects the likelihood of being arrested following a job loss. To determine the effect of UI on crime, I link New York City administrative arrest, means-tested benefits, and homeless shelter records to quarterly earnings and UI data from the New York State Department of Labor. I use a regression discontinuity design (RDD) based on New York State's minimum earnings requirements to credibly estimate the impact of UI eligibility. Two rules create a discontinuity UI in eligibility: 1) claimant's highest-earning quarter in the last five completed quarters to be above a certain threshold, $1700 in 2014; and 2) the sum of earnings in the prior year to be greater than 1.5 times the claimant's highest-earning quarter. I combine these two rules to create a joint threshold determining eligibility. Claimants around the joint threshold are extremely disadvantaged; among claimants near the joint threshold, 14 percent have previously stayed at a homeless shelter, and 10 percent are arrested within three years following job loss. Although the exact structure of UI varies from state to state, every state has a minimum earnings or time worked requirement for UI, many resembling New York's requirement. This is a large and understudied policy-relevant margin affecting millions of Americans. Over a million claims (14 percent) are denied due to insufficient prior earnings or employment each year.[2]

Point estimates suggest that being barely eligible for UI leads to a short-term decrease in arrest probability. UI reduces the probability of arrest within one year by 3.0 percentage points (49 percent). This is driven by a reduction in assault arrests (the second-largest crime category). The probability of assault arrest within one year decreases by 1.7 percentage points of a baseline of 2.3 percentage points. These estimated effects are marginally significant. However, the point estimates are consistently negative and have similar magnitude across a variety of specifications.

I find suggestive evidence that arrests for drug crimes decrease. The probability of a drug arrest falls by

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[1]ETA 5159 report.
[2]ETA 218 report.

1.0 percentage points (71 percent) within three years. Both drug distribution and drug possession arrests decrease, but the reduction is larger in relative terms of drug distribution, an income-generating crime. These estimates are consistent with consistent with additional income from UI crowding out illicit income-generating activity.

While many of these estimated effects are imprecise, I can reject short-term increases in arrest probability greater than 6.6 percent. UI may increase crime by increasing nonemployment duration, thus increasing the time available to engage in criminal activity (Jacob & Lefgren, 2003). At least in the short run, this channel is not strong enough to overcome other mechanisms that cause UI to decrease crime. Although many estimated effects are either insignificant or marginally significant, the large magnitudes of the point estimates are economically meaningful. Additionally, across a variety of specifications, the point estimates for assault, drug crime, drug distribution, and overall arrest probability are consistently negative and have similar magnitude.

UI generates large public benefits through crime reduction. Using a back-of-the-envelope calculation with conservative assumptions, I show that the crime reduction from loosening minimum earnings requirements generates public benefits approximately equal to the fiscal cost of UI. Reductions in assaults generate most of this social benefit.

This paper adds to the growing literature on the effects of social safety net programs on criminal justice involvement. Regarding cash transfers, Deshpande and Mueller-Smith (2022) find that Supplemental Security Income benefit removal generates large increases in crime, especially financially motivated crimes. The increase in crime counteracts any savings to taxpayers from reduced SSI payments. Palmer et al. (2019) find that one-time emergency financial assistance reduces violent crime but increases property crime. This change still generates sizable social benefits due to the high cost of violent crime. Public assistance and the Earned Income Tax Credit reduce recidivism for released ex-offenders (Agan & Makowsky, 2023; Yang, 2017). In addition, in-kind transfers, including housing programs (E. Cohen, 2024; Kling et al., 2005), food stamps (Tuttle, 2019), and Medicaid (Arenberg et al., 2020; He & Barkowski, 2020; Jácome, 2022) have been shown to reduce criminal involvement.

A few studies have looked at the effects of unemployment insurance on crime. This paper contributes to that literature in several ways. Papers examining the effect of UI on crime in the US tend to use county aggregate data and thus implement research designs that require stronger assumptions (Beach & Lopresti, 2019; NoghaniBehambari & Maden, 2021). Using a two-way fixed effects model, these papers exploit variation in UI generosity across states or time. These designs require strong assumptions and generate estimates that are hard to interpret (Callaway et al., 2024). These papers find reductions in property crime rates.

Much of past work has focused on outside the United States. Two papers use linked administrative UI

and crime data. In Brazil, Britto et al. (2022) estimate the effect of job loss on crime using a mass-layoff strategy and then estimate how much UI mitigates the increase in crime using an RDD.[3] Following job loss, they find an increase in both financially motivated crime and violent crime. They find that UI offsets this increase in crime while benefits are being received. Bennett and Ouazad (2020) use a similar mass-layoff design in Denmark. They then use the timing of unemployment benefits reform to identify the impact of UI and find that UI mitigates increases in property crime caused by job loss. Despite being well-identified, it is important to see if these findings generalize to the US.

To my knowledge, this is the first paper to credibly estimate the impact of UI eligibility on criminal involvement using linked individual-level administrative UI and crime data in the United States. The closest related paper estimates the effect of job loss on recidivism using linked earnings and crime data from Washington State (Rose, 2018). Rose finds that job loss increases property crimes and domestic violence. Rose uses a kink around the maximum weekly benefit to estimate the mitigating impact of benefit generosity on recidivism. He finds that increased weekly benefit lowers the probability of arrest for property crimes and domestic violence.

This paper diverges from Rose (2018) in three substantive ways. First, I estimate the impact of UI eligibility rather than a change in the weekly benefit amount. UI eligibility is a policy lever that is different from increasing the weekly benefit amount. Beyond the tangible differences in treatment intensity, the affected populations differ greatly. Those on the edge of eligibility are significantly more disadvantaged than those around the maximum weekly benefit. Individuals around the maximum benefit amount in Washington State have quarterly earnings of around $17,700; individuals around the joint eligibility threshold in my sample have quarterly earnings of around $2,900. Second, I am not estimating the impact of job loss on crime. Unlike Rose, my sample comprises individuals who apply for UI rather than past offenders who lost their jobs due to a mass layoff event. The focus of this paper is exclusively on the impact of UI eligibility. Lastly, unlike Rose, I observe UI receipt and benefit amount directly and do not need to infer it using past earnings.

2   Conceptual Framework

There are several ways in which UI might affect crime. Some mechanisms of how UI may reduce crime include: increased income, a scarcity-behavioral mechanism where reduce financial stress leads to less impulsive decisions, and increasing housing stability. However, UI may lead to an increase in crime through increasing

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[3]They estimate an RD exploiting the feature in the Brazil UI system that layoffs must be longer than 16 months apart to receive UI benefits.

nonemployment duration. If individuals are unemployed for longer, they have more available time and are less incapacitated. This may increase crime.

UI impacts both income and employment. Both employment and income are central components in canonical theoretical models of crime (Becker, 1968; Ehrlich, 1973; Lochner, 2004). In these models, when deciding to commit a crime, economic agents weigh the expected benefit of committing a crime against the non-crime alternative. In this framework, UI can impact criminal involvement through two different channels: an income effect and a labor-supply effect.

First, UI increases income directly. Evidence suggests that criminal involvement is inferior (Deshpande & Mueller-Smith, 2022; Palmer et al., 2019; Tuttle, 2019; Watson et al., 2020). So, as income increases, criminal involvement should decrease. This is the most direct way UI may impact crime. If this is the main mechanism, the crime reduction should primarily be in income-generating crimes such as theft or drug distribution.

Second, UI reduces search effort and increases unemployment duration (J. P. Cohen & Ganong, 2024; Krueger & Meyer, 2002b; Lopes, 2022; Meyer, 2002; Schmieder et al., 2012). Because UI benefits end when individuals find employment, UI can be viewed as a tax on formal employment. This tax may shift individuals to spend more time engaging in the informal and illicit labor markets. Because those eligible for UI are unemployed longer, they have more available time to commit crimes and are less "incapacitated." Jacob and Lefgren (2003) find that juveniles commit less property crimes when school is in session.

A series of older RCTs in the 1970s suggest that this labor-supply incapacitation mechanism might be particularly important. These RCTs provided UI-like benefits to those exiting prison. The first RCT did not reduce benefits when individuals gained employment. The total benefit received remained the same regardless of when an ex-prisoner was reemployed. They found that these benefits reduced arrest for thefts (Mallar & Thornton, 1978). The latter RCT structured the benefits to more closely resemble UI benefits and ended once ex-prisoners gained employment. They find no reduction in arrests across several categories, including property crimes. They additionally find large disemployment effects. The authors postulate that reduced crime from increased income was counteracted by increased crime caused by longer unemployment duration (McGahey, 1982). Ex-ante, it is unclear which direction UI may impact crime.

An additional mechanism to consider is a scarcity-behavioral mechanism. Scarcity can impact cognition and lead to impulsiveness (Mani et al., 2013; Mullainathan & Shafir, 2013). This may increase crime, in particular violent crime. Cognitive behavioral therapy has been shown to generate large reductions in violent crime among high-risk young men in Chicago. The stability of a weekly UI benefit may reduce stress and thus reduce violent crime.

Another way UI might affect crime is through increasing housing security. Individuals around the min-

imum earnings threshold for UI are particularly housing unstable. Before applying for UI, 14 percent of my sample had stayed at a New York City homeless shelter. Using a similar RDD, past work has shown that UI eligibility reduces homelessness (Collinson & Jenkins, 2024). Qualitative work suggests that housing instability may move people to situations where violence is more likely to occur (Desmond, 2016). Additionally, there is increasing empirical causal work documenting the link between housing security and crime (E. Cohen, 2024; Palmer et al., 2019).

3   Institutional Background

The United States unemployment insurance (UI) system provides temporary and partial wage replacements to eligible workers that lose their job. Although federally mandated, the UI system is state-run. States have the power to set program parameters such as benefit amount and eligibility. Although the exact structure of UI varies from state to state, the broad structure remains fairly similar. This section focuses on the UI institutional details of New York state over the relevant period for this study. New York's UI system is largely representative of the US as a whole.[4] In 2014, 27 other states used the same high-quarter and base-period wages system that New York uses to determine eligibility.[5]

A claimant must satisfy both the "monetary" and "non-monetary" eligibility criteria to qualify for UI benefits. The monetary eligibility criteria require that a worker have sufficient labor force attachment prior to the job separation. Specifically, wages in the "base period," either the first four of the last five completed quarters (standard base period) or the four most recently completed quarters (alternative base period), must satisfy two criteria. First, the highest-earning quarter in the base period must be about $1,900, depending on the year.[6] Second, total earnings in the base period must be at least 1.5 times higher than the highest quarter earnings. These requirements are displayed in graphical form in Figure 1. In the main analysis, I combine these two criteria into a single threshold by taking the minimum of dollars above the highest earning quarter threshold and dollars above the base-period threshold. This new joint threshold determines monetary eligibility. Compared to other states, New York has relatively strict monetary eligibility requirements (Leung & O'Leary, 2020). In addition to monetary eligibility, a worker must be non-monetarily eligible. Specifically, the worker must not have quit or been fired for cause.

The weekly benefit amount (WBA) is calculated from a claimant's highest-earning quarter. Figure 2 displays the weekly benefit varies with the highest-earning quarter from 2004 to 2013. For those barely above the minimum earnings threshold, WBA is 1/25 of the highest-earning quarter. This amount is roughly a 50

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[4]A comparison of state UI laws can be found on the United States Department of Labor's website.

[5]These states had different specific dollar thresholds.

[6]The cutoff is indexed to the state's minimum wage. Specifically, it is 221 times the states minimum wage rounded to the lowest $100. The cutoff was nominally $1,600 from 2004 to 2013. Then it gradually increases to $1,900 by 2016.

percent replacement rate. If there is variability in a claimant's quarterly earnings, the replacement would be much higher because it is calculated from the highest-earning quarter, not average quarterly earnings. If the highest-earning quarter is above \$3,575, then the WBA is 1/26 of the highest earnings quarter. This change results in roughly \$6 discontinuity. WBA is capped at roughly \$400, so a highest-earning quarter would have to be around \$11,000 to be above this cutoff.

New York, along with eight other states, is a uniform duration state. This means that workers eligible for UI are entitled to 26 weeks as long as they remain unemployed. My sample period encompasses the Great Recession, where the Extended Benefits (EB) and Emergency Unemployment Compensation (EUC08) programs dramatically expanded UI potential duration. Some claimants were eligible for 73 more weeks, 99 weeks in total. Figure 3 plots the maximum available duration by claim date. The red dashed lines indicate the start and end of the sample used in the main analysis. Evidenced by Figure 3, a large majority of claims in my sample are eligible more than 26 weeks of UI. On average, claims used in the main analysis were eligible to up to 70 weeks of UI benefits. Additionally, the American Recovery and Reinvestment Act of 2009 increased weekly benefit levels by a flat \$25, a 40 percent increase for claimants receiving the minimum WBA, for all recipients from March 2009 through May 2010.

New York State requires individuals to be able and willing to work to continue receiving UI benefits.[7] NYSDOL works to end UI payments going to incarcerated individuals. They receive daily data on NYC jail inmates from NYC DOC. Short stays may not interfere with the ability to work, so an individual may still be eligible for UI payments.

4 Data and Empirical Strategy

4.1 Data

The empirical analysis for this study uses linked UI and jail bookings administrative data. I use New York City Department of Corrections (DOC) jail booking data to measure crime. These are stays in NYC jails. Many of these stays are short. Eight percent of the stays are less than a day, with the median lasting ten days. However, over a quarter of stays last longer than five months. Stays are due to felonies (45 percent), misdemeanors (45 percent), and violations (10 percent).

Figure 4 plots trends of DOC bookings through time broken up into different crime categories. Drug crime is the largest category (25 percent), followed by property crimes (23 percent), violent crimes (22 percent), and then public order crime (17 percent). The number of bookings goes down from 26,000 bookings a

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[7]Article 18, Title 7, Section 591 of New York State Unemployment Insurance Law.

quarter in 2005q2 to 16,000 bookings in 2014q4. Most of this is a reduction in drug crimes that fell by more than 60 percent in that same period. In 2009, New York passed a series of drug crime sentencing reforms. These laws eliminated or shortened mandatory minimum sentences created in 1973 (Rockefeller Drug Laws). Additionally, these laws gave judges more discretion to divert nearly all drug charges [8] to court-mandated treatment (Parsons, 2015). Although not directly related to these legislative reforms, NYPD dramatically reduced drug crime arrests in response to these reforms and other public pressure.[9]

I use a sample of UI claims representative of individuals who have historically received a means tested benefit from New York City's Human Resources Administration.[10] These benefits include Medicaid, Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP; food stamps), or other city-specific cash subsidies between 2004 to 2016.[11] I use claims filed in between 2005Q2 to 2014q4 in the main analysis. The UI claims data includes the date when a claim was made, whether the claim was deemed eligible, the total benefit amount received, the number of weeks receiving UI, and basic demographic information. This data includes both regular state UI and UI paid through EB and EUC08 programs. The claims data does not include the potential maximum benefit duration or the potential maximum benefit amount. I impute these using state rules and prior earnings.

The UI claims are linked to quarterly earnings data from the New York State Department of Labor (NYSDOL) from 2004-2017. These data include detailed industry codes (six-digit NAICS) and cover approximately 97 percent of New York State's non-farm employment. However, these data do not capture private household workers, student workers, the self-employed, or unpaid family workers.

These records are then linked to mean-tested benefits receipt data from New York City's Human Resources Administration (HRA) using social security numbers. The HRA data contains information on benefit receipt,[12] demographics, addresses, and identifiers such as name and date of birth. To gain additional covariates, I link this sample to administrative data covering applications to homeless shelters from the New York City Department of Homeless Services from 2003 to 2017. These data contain information on applications to and stays in New York City homeless shelters.

To estimate the effect of UI on future crime, I link the UI claims data to administrative jail booking data from New York City Department of Corrections (DOC) data. The DOC data only has a social security number for a limited number of observations (35 percent). Because of this, I fuzzy-match UI claims to DOC records using a combination of SSN (if available), name, and date of birth. Name and date of birth are

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[8]Class A felonies could not be diverted to court-mandated treatment.

[9]https://www.vice.com/en/article/how-new-york-quietly-ended-its-street-drug-war/

[10]The composition of our sample is dictated, in part, by the initial use of this data, examining the effects of evictions (Collinson et al., 2024). For more details see Appendix B.

[11]This data does not capture Medicaid clients receiving Medicaid from the state Department of Health.

[12]I do not have information on amount received.

obtained using the means-tested benefits records.

I make some restrictions to construct the main analysis sample. Table A1 lists the restrictions used to construct the main analysis sample and the number of claims excluded with each restriction. First, I exclude claims made within two years of prior claims. This restriction ensures that each UI claim is a new unemployment spell. I observe a mass of claims a year after the initial claim. Since a claim is only valid for one year, a new claim must be filed to continue receiving benefits from the EB or EUC08 programs. Second, I restrict to individuals aged 18 to 65. Since UI may impact the receipt of a means-tested benefit(Leung & O'Leary, 2020), I exclude claimants who do not appear in means-tested benefits data before he or she file a UI claim. I focus exclusively on males because males commit the vast majority of crimes. Males comprise 89.6 percent of all bookings in the DOC records. Lastly, I restrict to claims that are at least within $2,000 of the nearest binding monetary eligibility criteria. After these restrictions, there are around 32,000 remaining UI claims.

Table 1 presents descriptive statistics for new claims without restricting to claims around the cutoff (columns 1) and new claims near the joint threshold (column 2). Since the sample is comprised of UI claims from means-test benefit recipients, the individuals in our sample are relatively disadvantaged, even before restricting to claims around the cutoff. For example, the mean yearly earnings for this population is $26,133, and nearly 10 percent have applied to stay in a homeless shelter prior to filing a UI claim. As a whole, this population is disproportionally Black, Hispanic, and less educated. Nearly 4 percent of these claims have stayed in NYC jail within two years prior to applying for UI.

After restricting to claims within $2,000 to the nearest binding threshold, the sample is even more disadvantaged. The previous year's annual earnings are roughly $8,600. This is well below the federal poverty line. 14 percent of this sample has previously applied to stay in a homeless shelter. A fifth of the sample received TANF within two quarters before the UI claim. A third of the sample has not completed high school. Prior to applying for UI, this sample has significant interaction with the criminal justice system. 5.5 percent has previously been booked by NYC DOC within two years. Nearly two percent of the main sample have been arrested for a violent crime within two years before claiming UI.

In summary, individuals in the main analysis sample are extremely disadvantaged due to three reasons: all individuals in our sample have historically received a means-tested benefit between 2004-2016, all individuals have recently lost employment, and all individuals have earnings that put them within $2,000 of the minimum earnings requirement for UI. The restriction that all individuals in our sample have historically received means-tested benefits from New York City's Human Resources Administration may limit generalizability. However, around the minimum earnings threshold, most individuals should qualify for a means-tested program such as SNAP.

As shown in Table 1, a small difference exists between receipt and eligibility. To appear in our sample, an individual must submit a UI claim. Conditional on applying for UI, our main sample has a 97 percent take-up rate (column 2), calculated by dividing UI receipt by entitled to UI. In our main sample, around a fifth of claimants have previously applied for UI benefits.[13] In the main sample, around 55 percent of claims are deemed eligible.

As stated earlier, the time period in this paper spans the 2008 financial crisis, during which UI duration was dramatically expanded. In normal times, UI in New York lasts for a maximum of 26 weeks; however, the average number of weeks recieveing UI in my sample is 36 weeks. The average weekly benefit amount in the main sample is around $160. Hence, in normal times, the average potential maximum benefit amount for this sample would be around $4,160. Because of the extended UI, the actual average potential benefit amount is just under $11,000. Of this amount, $5,700 is received on average. Similarly, on average, claims in the sample are eligible for nearly 70 weeks of UI benefits but end up staying on UI for 36 weeks.

4.2 Empirical Strategy

To estimate the causal impact of UI, I implement a regression discontinuity (RD) design, exploiting the sufficient earnings history requirement for UI. Leung and O'Leary (2020) use a similar empirical strategy to identify the effect of UI eligibility on labor earnings and means-tested program receipt.[14] As described in Section 3.1, there are two criteria used to establish monetary eligibility in New York: a high quarter criterion and a base period criterion. The high quarter criterion requires that a claimant's highest earning quarter in the last 5 completed quarter is above a certain threshold — $1,600 for most of the sample.[15] The base period criterion requires the sum of earnings in the base period (the first four of the last five completed quarters or the four most recently completed quarters) to be greater than 1.5 times the earnings in the highest earning quarter. A claimant must satisfy both of these requirements to be eligible for UI.

Instead of estimating two different RDs, I combine these criteria into a single RDD by taking the minimum of the two running variables. Specifically, the new running variable is:

$$min\{high\ quarter\ earnings - threshold, base\ period\ earnings - 1.5 \times high\ quarter\ earnings\} \qquad (1)$$

Combining these two criteria increases the effective sample size and increases statistical power. Intuitively,

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[13]As described above, this excludes claims filed within two years of each other.

[14]They find that UI eligibility increases the duration of nonemployment. Additionally, UI eligibility decreases TANF participation in half; SNAP and Medicaid receipt is not affected. Overall eligible workers have 55 percent more income than ineligible workers.

[15]Whether the claimant qualifies using the standard base period or the alternative base period this condition must be satisfied to qualify for UI.

I compare outcomes of individuals with similar earnings history, but one group is barely eligible for UI, and the other group is barely ineligible. Any difference between the two groups in subsequent outcomes can be attributable to UI eligibility.

I estimate the following equation,

$$Y_i = \alpha_0 + \beta_y T_i + \alpha_1(R_i - c_t) + \alpha_2 T_i \times (R_i) + \Phi X_i + \epsilon_i, \tag{2}$$

using observations within h_- below the threshold and h_+ above the threshold. Y_i is an outcome variable of interest such as whether a claimant is booked within 3 years; R_i is the running variable; X_i is a vector of covariates;[16] T_i indicates whether the running variable is above the cutoff. The main estimates presented are reduced-form estimates.

For the main estimates, I use the method proposed by Calonico et al. (2014) to select the bandwidths h_- and h_+.[17] Intuitively, there is a bias-variance trade-off. Selecting a bandwidth too large may yield biased estimates; on the other hand, a bandwidth too small excludes many observations creating imprecise estimates. I allow the bandwidths to vary on different sides of the threshold. I use a triangular kernel. In Table A2 and A3, I show that the main results are consistent across different specifications.

4.3 Validity of Identifying Assumptions

The key identifying assumption is that the conditional expectation function of the outcome variables is continuous around the eligibility threshold for both groups above and below the cutoff. Practically, it means that individuals barely below and above the threshold are not systematically different from another. This is often violated when agents can sort around the threshold. One may be concerned that workers or employers time the jobs separation in order to be barely eligible or ineligible for UI. Alternatively, employers may strategically pay employees just below the minimum earnings thresholds to prevent workers from claiming UI, leading to paying increased taxes.[18]

A common method of assessing the validity of this identifying assumption is to test for changes in density around the threshold. A change in density would suggest that the running variable is being manipulated. Figure 5a displays a histogram of UI claims around the threshold. Visually, there is a noticeable increase in the number of claims just above the threshold. However, this increase does not look dissimilar to other increases across the distribution. I perform a randomization inference following Bertanha et al. (2024) to

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[16]Covariates include demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

[17]I use the "rdrobust" package developed by Calonico et al. (2014)

[18]Unemployment insurance is experience rated. Having former employees receive UI benefits raises the UI taxes paid by the employer.

formally test whether this increase is statistically significant. I estimate the change in density at 115 evenly spaced points (including the true threshold) between -1500 and 10000 (there is no other rule change in this range), and the p-value is the fraction of density estimates with a greater absolute value than the true density estimate. The resulting p-value is 0.112. At conventional confidence levels, I fail to reject the null of no change in density at the threshold.

To further assess the validity of this RDD, I test whether predetermined covariates are smooth around the threshold. Table 2 presents estimates of discontinuities in predetermined covariates around the threshold, such as demographic information, prior earnings, prior homelessness, and prior DOC interaction. Across each covariate, no point estimate is statistically significant at the ten percent confidence level except for past employment. Prior DOC bookings are strongly predictive of future booking probability. I estimate a statistically insignificant and a 1.2 percentage point (17 percent) discontinuity in the probability of a prior booking within two years. In order to summarize these estimates, I predict whether an individual is booked within three years using a probit model with predetermined characteristics. I also linearly predict the nonemployment duration. These measures should be smooth around the threshold if individuals are not systematically different around the threshold. Figure 6 plots binned averages of these predicted outcomes, prior DOC booking, and quarterly earnings two years prior to the UI claim. From visual inspection, they appear smooth around the threshold, and the estimated discontinuities are small and insignificant.

Leung and O'Leary (2020) estimate an RDD using a similar threshold with the universe of Michigan UI claims from 2005-2010. They do not find a jump in density around the threshold. Perhaps, in New York, it is easier to determine whether one is eligible for UI before applying, so fewer individuals apply for UI below the cutoff. Potentially, means-tested benefit recipients are more knowledgeable and more likely to apply for UI when they are just above the threshold. One concern is that we have sample-selection bias, where "knowledgeable" individuals know their eligibility and only claim UI above the threshold. Hence, they only appear in our sample above the threshold. This is a threat to identification if being knowledgeable is correlated with future probability of criminal justice system interaction. The covariate balance provides suggestive evidence that this is not the case.

5   Results

5.1   UI Eligibility and Benefit Amount (First Stage)

Figure 7a plots the probability of whether a claim is deemed eligible around the joint threshold. There is a slightly less than 54 percentage point increase in eligibility at the threshold. Above the threshold, a

claim may still be ineligible for nonmonetary reasons. For example, the claimant may have been fired for cause or quit. Below the cutoff, roughly 14 percent of claims are deemed eligible. Based on New York State's monetary eligibility rules, no claimant below the threshold should qualify for UI. One explanation for this difference is that the claimant's employer inaccurately reported earnings. If a claimant's earnings were underreported, the claimant may request that her earnings be re-verified. Additional earnings would be added, but the quarterly earnings file may be left uncorrected.

Table 3 displays RD estimates of first-stage outcomes such as UI eligibility, receipt, potential benefit amount, benefit amount received, potential maximum duration, and observed duration. Being above the threshold increases UI potential duration by 37 weeks (68 weeks, conditional on eligibility). This is similar to the sample average in Table 1. Of this, 22 weeks are received. In terms of dollar amount, Being above the threshold increases the potential total UI benefit by $4,362 ($8,078 conditional on eligibility). This is much less than the sample mean in Table 1. This is because those barely eligible for UI often receive the minimum weekly benefit amount. Of the maximum $4,362, $2,860 is received. In summary, crossing the threshold increases the probability of treatment (UI eligibility) by 54 percentage points. At the threshold, treatment on average is the offer of $8,078 over 68 weeks ($119 per week). Of this offer, $5,296 is received over 40 weeks.

5.2 The Effects of UI Eligibility on Earnings and Income

Before examining the effect of UI on crime, I first explore how UI eligibility affects earnings in the formal labor market and total income. It is well documented that increases in UI duration and generosity increase unemployment duration (J. P. Cohen & Ganong, 2024; Krueger & Meyer, 2002a; Schmieder & Von Wachter, 2016). Around the joint threshold, Figure 8 plots nonemployment duration (the number of consecutive quarters with zero earnings following a UI claim) and the sum of earnings for the first nine quarters following the UI claim, including the quarter of the UI claim.

Visually, there appears to be an 0.462 quarter (13 percent) increase in nonemployment duration, although the point estimate is not statistically significant (p-value = 0.116). The point estimate may be attenuated towards zero because I can only measure employment at the quarter; any intra-quarter adjustment will not be captured. Under the assumption that around the threshold variation in future earnings is mostly driven by hours rather than wages, earnings may be a more accurate measure of labor supply responses. Similar to nonemployment duration, visual evidence suggests a decrease in earnings in the first nine quarters following a UI claim. I find a $1,807 reduction in earnings off a baseline of $15,133; however, I cannot reject a null effect (p-value = 0.154).

Figure 9a plots the reduced-form estimated effect of UI on earnings for each quarter within three years after the claims and two years before the claim. The dashed line plots the baseline earnings time trend before and after apply for UI. The solid line plots the reduced-form estimated effect added to baseline earnings. Hence, the reduced-form estimated effect is the difference between the solid and dashed lines. The large spike in earnings in the quarter prior to the UI claim is due to the policy variation being exploited. The base-period eligibility threshold is often binding for those who recently found employment and have been employed for a quarter and a half before losing employment. This creates the large increase in earnings for the two quarters before claiming UI. There is no difference in prior earnings between those on either side of the threshold, validating the empirical design. In the following nine quarters after claiming UI, those barely eligible for UI have consistently lower earnings, although statistically insignificant.

Table 4 displays RDD estimates of the reduced-form effect of UI on whether a claimant's nonemployment spell is longer than zero quarters, one quarter, two quarters, all the way up to 11 quarters. Each point estimate is positive, consistent with UI increasing nonemployment duration. UI increases the probability of very long nonemployment spells (greater than eleven quarters) by 5.2 percentage points off a baseline of 14.6 percentage points. This increase is significant at the five percent confidence level.

Relating these estimates to the broader literature poses some difficulty. Recent papers focus on how changes in either maximum duration or replacement rate impact nonemployment duration instead of how UI eligibility impacts nonemployment duration. To translate my estimates into this literature, I consider UI eligibility first as an increase in UI duration from zero to a non-zero amount and second as an increase in the replacement rate from zero to a non-zero replacement rate. With this formulation, I can convert estimates of the impact of UI eligibility on nonemployment duration to estimates of increasing maximum potential duration or replacement rate on nonemployment duration.

Being above the threshold increases the maximum potential duration by 36.71 weeks. This then increases nonemployment duration by .462 quarters (6.02 weeks). Hence, a one-week increase in potential benefit is associated with a 0.16-week increase in nonemployment duration. In terms of replacement rate, UI eligibility increases the replacement rate by 28 percent.[19] Hence, a ten percent increase in replacement rate increases nonemployment duration by 2.15 weeks. Both of these estimates are within the range of what past studies have found but tend to be larger than the median estimate (J. P. Cohen & Ganong, 2024).

Using a similar policy margin in Michigan, Leung and O'Leary (2020) produce similar estimates. They find that one-week increase in potential benefit duration increases nonemployment duration by between 0.09 and 0.16 weeks. In terms of replacement rate, they find that a 10 percentage point increase in replacement

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[19]This is calculated by multiplying the first stage in terms of UI eligibility (0.54) and the replacement rate for those barely eligible for UI (0.522).

rate increase nonemployment duration by between 0.8 and 1.4 weeks. These estimates vary by whether UI potential duration is expanded. In terms of potential duration, my estimates are quite similar. In terms of replacement rate, estimated effect is more than 50 percent greater than Leung and O'Leary's estimates. However, this difference is not statistically significant.

Turning to income, Figure 9b plots the reduced-form estimated effect of UI on income (earnings and UI benefits) for each quarter within three years after the claims and two years before the claim. Differences in income occur in the early periods, from the quarter when a claimant applies for UI and the next three quarters. However, the difference is statistically significant only the quarter after applying for UI. From the fourth quarter after claiming UI to the seventh quarter, UI benefits, on average, counteract lower earnings caused by labor-supply effects. Since nearly all the differences in income appear within a year, any impact on future crime should be expected to appear early on.

5.3 The Effect of UI Eligibility on Arrests

This section presents results for the main outcomes of interest, future criminal involvement measured by DOC interaction. These estimates are displayed in Table 5. Figure 10 plots whether a claimant appears in the DOC records within one and three years after applying for UI. Within one year, being above the threshold decreases the probability of being booked by 3.0 percentage points off a baseline of 6.1 percentage points. This estimate is marginally significant. I can reject increases in the one-year arrest probability greater than 6.6 percent. Table A2 presents the main RDD estimates of the effect of UI eligibility on future arrest using a variety of alternative specifications. Across a these specifications, the point estimate is consistently negative and of similar magnitude. However, excluding controls changes the point estimate from a 3.0 percentage point decrease to a 2.0 percentage point decrease.

Looking at a longer time horizon, Figure 10b plots the three-year DOC booking probability. The point estimate is small and insignificant. Being above the threshold increases the probability of being booked within three years by 0.5 percentage points off a baseline of 9.8 percentage points. This is around a 5 percent increase. Despite the small point estimate, I cannot rule out large increases or decreases in crime caused by UI eligibility. Anything within a 49 percent increase and a 35 percent decrease is within the 95 percent confidence interval.

Due to the wide confidence interval, it is unclear whether UI merely delays crime or generates an economically meaningful permanent decrease in crime. Differences in income between those barely eligible and barely ineligible occur almost entirely in the first year after the UI claim. I may lack power to detect longer run effect as other factors orthogonal to UI eligibility impact overall booking probability, increasing the

standard errors.

Another explanation is that being above the threshold increases unemployment duration. This increase is especially concentrated at the tail end, increasing the probability of a very long nonemployment spell. For example, being barely eligible for UI increases the probability of a nonemployment duration spell lasting longer than 11 quarters by 5.2 percentage points off a baseline of 14.6 percentage points. These nonemployment effects may counteract any income effects occurring in the first year, leading to a null result.

The three most common charges in the DOC data are drug possession charges, assault charges, and drug distribution charges. These comprise 16.7, 10.0, and 8.3 percent of all charges, respectively. Due to the social cost, policymakers pay particular attention to violent crime, which includes assaults. Assaults generate the largest social cost out of all criminal offenses due to the combination of a high unit cost and frequent occurrence (Chalfin, 2015). Table 5 displays the reduce-form estimate of the impact of UI eligibility on being arrested for an assault within one, two, and three years. Figure 11 plots whether a claimant is booked with an assault as the top charge within one year in Panel A and within three years in Panel B. Being above the threshold leads to a 1.7 percentage point (77 percent) decrease in the likelihood of being arrested for an assault charge within one year. The effect is significant at the 10 percent level. Table A2 shows that this estimate is consistent across various specifications.

After three years, UI eligibility leads to a 1.5 percentage point (53 percent) reduction in assault-arrest probability. This is similar to the one-year effect but is no longer statistically significant at any conventional level. This is consistent with UI decreasing assaults only while benefits are being received.

The largest category in the DOC data is drug crime. There are a priori theoretical reasons for UI to either increase or decrease drug crime. In terms of drug use, income support from UI may reduce drug use by mitigating the income shock that leads to substance abuse. Alternatively, the income support could be used directly to purchase illicit drugs. Additionally, 33.2 percent of drug charges in the DOC data are drug distribution charges. This is an income-generating crime. The additional income from UI may crowd out this particular income-generating crime.

Estimates of the effect of UI eligibility on drug crime, drug distribution, and drug distribution arrest probablity are displayed in Table 5. Being above the threshold leads to a 1.0 percentage point (71 percent) reduction in being arrested for a drug crime within one year. After two years, being above the threshold leads to a 1.6 percentage point (55 percent) reduction in being arrested for a drug crime. After three years, the reduced-form impact of UI on any drug crime increases to a 2.3 percentage point (72 percent) reduction. The three-year (two-year) estimated effect is significant at the 5 (10) percent level; however, the one-year estimate is not significant (p-value = 0.15). Figure 12 displays the RDD plots for drug crime arrests. Panel A (C) plots whether a claimant is booked with a drug crime as the top charge in the DOC records within

one year (three years) after the UI claim. Panel B (D) plots whether a claimant is booked with a drug distribution charge as the top charge in the DOC records within one year (three years) after the UI claim.

The reduction in ever being charged with a drug crime occurs through a reduction in distribution and possession charges. Being above the threshold leads to a 1.4 percentage point reduction in being charged with a drug crime within three years. This effect is significant at a 5 percent level. Possession charges decrease by a similar margin, although less precisely estimated.

Table 5 displays the RDD estimates of main DOC interaction outcomes. The probability of being charged with a violent crime decreases by 0.9 percentage points (50 percent) within one year and 0.8 percentage points (24 percent) within three years. These estimates have large standard errors preventing ruling out large increases or large decreases in violent crime as a broad category. UI eligibility appears to increase property crime. Being barely eligible for UI leads to a 2.0 percentage point (80 percent) increase in being charged for a property crime within three years. This increase in property crimes is driven by an increase in larceny charges, as seen in Figure B1.

Given the reduction in other income-generating crimes like drug distribution charges, it seems counterintuitive that larcenies are increasing. This increase in property crimes does not appear in the first year after claiming UI, suggesting that the estimated effect may be spurious. However, other research has found increases in property crimes in response to an income transfer. Palmer et al. (2019) find that receiving emergency financial assistance intended to prevent homelessness increases property crime, particularly larceny charges. Like this study, Palmer et al. find increases in property crimes only after 12 months.

In summary, I present evidence indicating that UI eligibility leads to a short term decrease in crime. Although imprecisely estimated, I find being barely eligible for UI leads to a 49 percent decrease in arrest probability within one year. A decrease in assaults and drug crime drives the decrease. The probability of an assault arrest within one year decreases by 1.7 percentage points from a baseline of 2.2 percentage points. The probability of a drug crime arrest within one year falls by 1.0 percentage points of a baseline of 1.4 percentage points. The reduction in drug crimes occurs both through reductions in drug possession and drug distribution. Across alternative specifications these estimates remain negative and of similar magnitude. Being above the threshold may increase property crimes. However, this effect only occurs a year later.

5.4 Heterogeneity

This section explores how the estimated effects of UI on nonemployment duration and crime vary based on predetermined characteristics. Figure 13 plots RDD point estimates and the bias-corrected confidence intervals for nonemployment duration, being booked within one year, being charged with an assault within

one year, and being charged with a drug crime for various subgroups. The subgroups include those under age 30, those under age 45, claims where the maximum UI duration was the standard 26 weeks,[20] claims where the maximum UI duration was expanded beyond 26 weeks,[21], those with and without a child under 18 present in the household,[22] those who have experienced homelessness in the two years prior and those who have not,[23], those whose predicted nonemployment duration using predetermined covariates is below and above the median, and those whose predicted probability of a booking within three years using predetermined covariates is above and below the median. Heterogeity for three-year arrest outcomes is shown in Figure B2.

Overall, the main estimates of DOC interaction do not vary much across subgroups. This is in contrast with nonemployment duration, which varies widely across subgroups. In particular, the nonemployment effect is much larger in the period when UI duration is expanded and for those whose predicted nonemployment duration is large.

The time period used in this paper includes the 2008 financial crisis, during which UI duration was dramatically expanded. One concern is that the main results of this paper are only relevant to that particular time and cannot be generalized to how UI impacts crime in ordinary times. The concern is that UI eligibility only reduces short-term arrests, assaults, and drug crimes when the potential duration is nearly two years, and the labor market is in poor condition. This does not appear to be the case. The point estimates for booked within a year, charged with an assault within a year, and charged with a drug crime within a year are larger in the period where the maximum duration was the standard 26 weeks than in the period where the maximum duration was expanded. In fact, these estimates are near zero and insignificant in the period where UI duration was expanded.

In the expanded duration period, UI increases nonemployment duration, leading those eligible for UI to be less incapacitated and more likely to engage in criminal behavior. This effect counteracts an income effect, leading to no change in DOC interaction. In the regular duration period, due to a stronger labor market or shorter UI duration, UI eligibility increases nonemployment duration less. Hence, the income effect dominates any labor-supply-incapacitation effect, leading to a reduction in crime.

Across other subgroups, evidence for this labor-supply-incapacitation mechanism is mixed. For example, the impact on nonemployment duration varies widely between those predicted to have a long nonemployment duration and those predicted to have a short nonemployment duration. However, across these two subgroups, the impact on DOC outcomes is similar and exhibits no consistent pattern.

For those who have experienced homelessness in the two years prior to claiming UI, I find a large reduction

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[20]This is the period before 4/25/2006 and after 6/24/2013.
[21]This is the period between 4/25/2006 to 6/24/2013.
[22]This is determined using means-tested benefits case files.
[23]This uses administrative data from NYC Department of Homeless Services.

in the one-year arrest probability. I also find a large reduction in assault arrest probability for this group. However, I do not find any impact on drug crime arrest. Because this group is more housing insecure than the group that has not experienced homelessness in the prior two years, the contrasting estimated effects on short-term arrest and assault-arrest probability is suggestive of UI reducing crime through increasing housing stability.

6   Cost-Benefit Analysis

Standard optimal unemployment insurance design weighs the consumption-smoothing benefit of UI against the distortionary effects on labor supply (Baily, 1978; Chetty, 2006). This framework fails to account for potential externalities generated by UI, specifically crime reductions. A comprehensive cost-benefit analysis of loosening monetary eligibility requirements would require weighing the benefits of UI such as consumption smoothing and crime reduction against the costs including the fiscal cost and the distortionary labor supply effect. To illustrate the potential societal benefit of crime reductions from loosening monetary eligibility requirements, I provide a back-of-the-envelope calculation of the crime-reduction benefit of losing monetary eligibility weighed against the fiscal cost. As discussed in Section 5.2, differences in income and earnings occur primarily in the first year. Hence, I use one-year estimates for this cost-benefit analysis.

Presented in Table 3, the direct fiscal cost of loosening monetary eligibility requirements is $2,860 per UI claimant. In my main analysis, I exclude female claimants. For this cost-benefit calculation, I assume that UI eligibility does not impact future criminal involvement for female claimants. During my sample period, 57 percent of UI claimants are male in New York. Hence, the fiscal cost of loosening monetary eligibility requirements is $5,017 ($2,860/0.57) per male claimant.

Turning first to the benefits to victims from crime reduction, the reduced-form impact of UI eligibility on assault arrest within one year is a 1.7 percentage point reduction. Miller (1996) estimate the victim cost of an assault to be $25,544, adjusted to 2017 using the consumer price index. Other estimates are higher. Chalfin (2015) in review empirical literature on the cost of crime, report a median cost estimate for assaults of $100,022 in 2017 dollars. Some of the studies in this median include criminal justice costs such as incarceration. Using the Miller (1996) cost estimate, this generates $434 ($25,544 × 0.017) in benefits to victims. The Chalfin (2015) estimate yields $1,700 ($100,022 × 0.017) in benefits. This is 34 percent of the fiscal costs.

Since many assaults are not reported and many reported assaults do not lead to arrest, this is an underestimate of the benefit to victims. The Bureau of Justice Statistics National Crime Victimization Survey shows that only 48 percent of assaults are reported to the police (Truman et al., 2015). In 2017,

NYPD cleared 71 percent of reported assaults.[24,25] Thus, approximately 2.93 ($1/0.48 \times 0.77$) assaults occur for every assault arrest. Including these unreported and uncleared assaults increases the benefits to victims of crime to \$1,272 (\$434 × 2.93). Using the larger Chalfin estimate puts the social benefit of this assault reduction at \$4,981 (\$1,700 × 2.93). This is 99 percent of the fiscal cost. I do not include any benefits to victims resulting in reduced drug crime.

In addition to social benefits accrued to potential victims, crime reductions lead to fiscal savings. Figure B3 plots the number of days a claimant spends in DOC custody within a year. Being barely eligible for UI increases the days spent in DOC custody by 1.81. As measured by cost per incarcerated person, NYC jails are amongst the costliest local jail systems in the US (Heller & Harris-Calvin, 2021). During my sample period, the DOC's average daily cost per incarcerated person is approximately \$395.[26] However, the marginal cost is likely much lower. Using \$150 as the marginal cost, loosening monetary requirements generates \$272 (\$150 × 1.81) in fiscal savings through reduced use of NYC jails. I do not include fiscal savings accrued outside DOC, such as incarceration in prisons, to avoid double-counting criminal justice costs.

Under these conservative assumptions, the total benefits and cost-savings from loosening monetary eligibility requirements sum to \$5,253, surpassing the fiscal cost. Even without the primary benefit of UI, consumption smoothing, benefits outweigh the fiscal cost. Based on prior earnings and homelessness history, the population in this sample is likely to have little savings or access to credit. Other work shows that around this margin, UI reduces homelessness (Collinson & Jenkins, 2024). For these reasons, the consumption smoothing benefit around this policy margin is likely quite large.

7   Conclusion

This paper explores whether UI eligibility affects future criminal behavior. In particular, I estimate the impact of UI eligibility using an RDD that exploits UI's minimum earnings requirements. Minimum earnings requirements for UI are a large policy-relevant margin affecting millions of Americans. Over a million claims (14 percent) are denied each year due to insufficient prior earnings or employment. Despite this, only a few papers examine the impact of UI at this margin (Collinson & Jenkins, 2024; Leung & O'Leary, 2020). None of these papers have examined whether UI eligibility reduces crime.

I provide evidence suggesting that UI eligibility leads to a short-term decrease in the probability of arrest. Being barely eligible for UI leads to a 49 percent decrease in arrest probability within one year. A decrease in assaults and drug crimes drives the decrease. The one-year probability of arrest with assault as the top

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[24]This is the clearance rate for aggravated assaults. The clearance rate for assaults, more generally, is not reported.
[25]NYPD Clearance Report.
[26]NYC Comptroller

charge decreases by 77 percent, and the one-year probability of a drug-crime arrest decreases by 71 percent. The reduction in drug crimes occurs both through reductions in drug possession and drug distribution. Many of these estimated effects are imprecise; however, across many alternate specifications these point estimates remain negative and have similar magnitude.

This crime reduction generates large public benefits. Under conservative assumptions, these benefits exceed the direct cost of loosening monetary eligibility requirements. Most of this benefit is driven by a reduction in assaults. This back-of-the-envelope calculation ignores any consumption-smoothing benefit of UI. Due to this population's disadvantage, the consumption-smoothing benefit of UI is likely quite large at this margin.

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Figures

Figure 1: New York State Monetary Eligibility (in 2016)
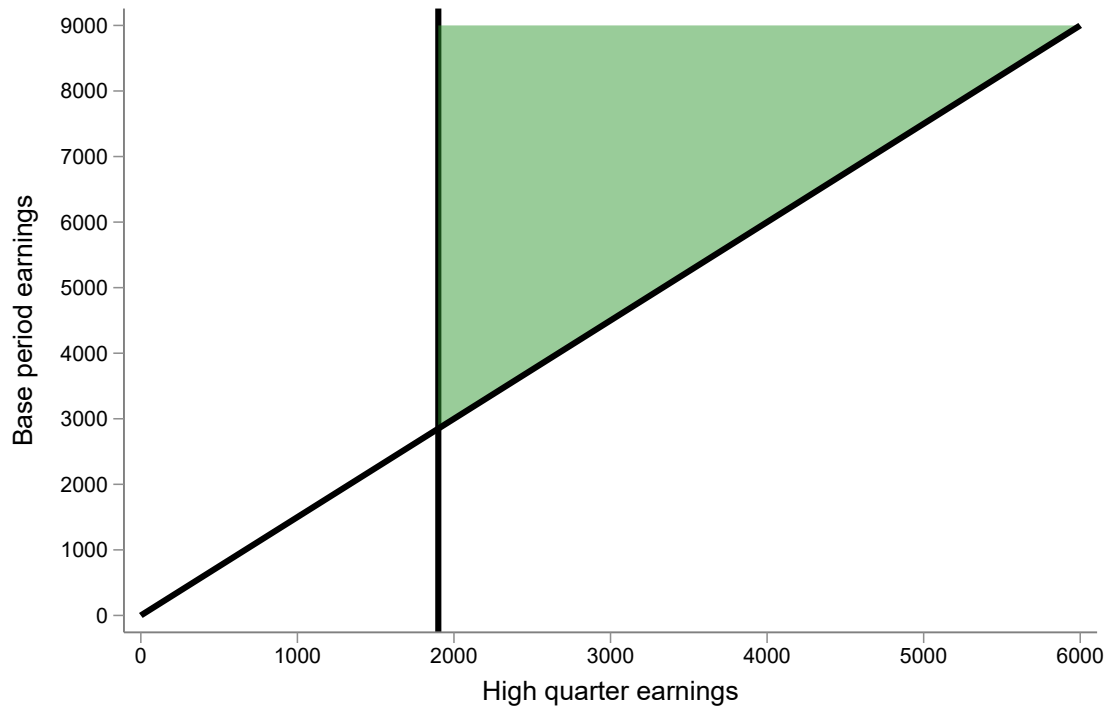
Notes: This figure plots unemployment insurance monetary eligibility requirement rules for New York State in 2016. The earnings from the highest earning quarter out of the last five completed quarters are plotted on the x-axis. Base period earnings (the sum of earnings from either the first four of the last five completed quarters or the sum of earnings from the last four completed quarters) are plotted on the y-axis. The green shading indicates that a claim with that level of high-quarter and base-period earnings would be monetarily eligible for UI. Source: United States Department of Labor https://oui.doleta.gov/unemploy/statelaws.asp

Figure 2: 2004-2013 Weekly Benefit Amount Schedule
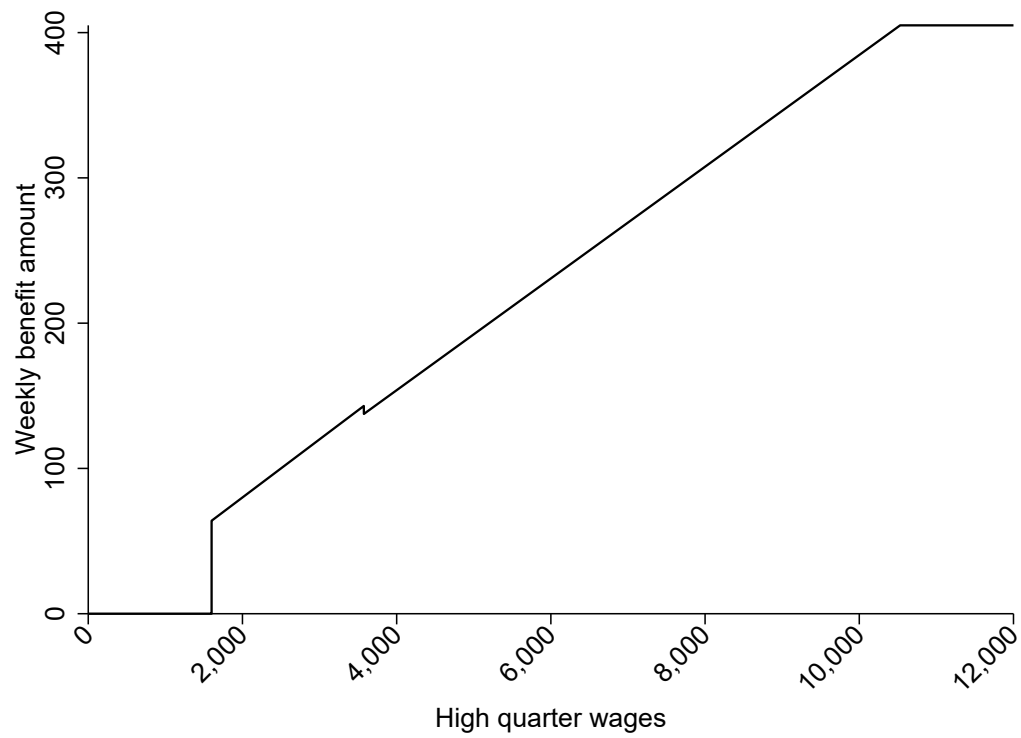
Notes: This figure plots the weekly benefit amount schedule for New York State from 2004-2013. Source: United States Department of Labor https://oui.doleta.gov/unemploy/statelaws.asp

Figure 3: Potential Benefit Duration through Time
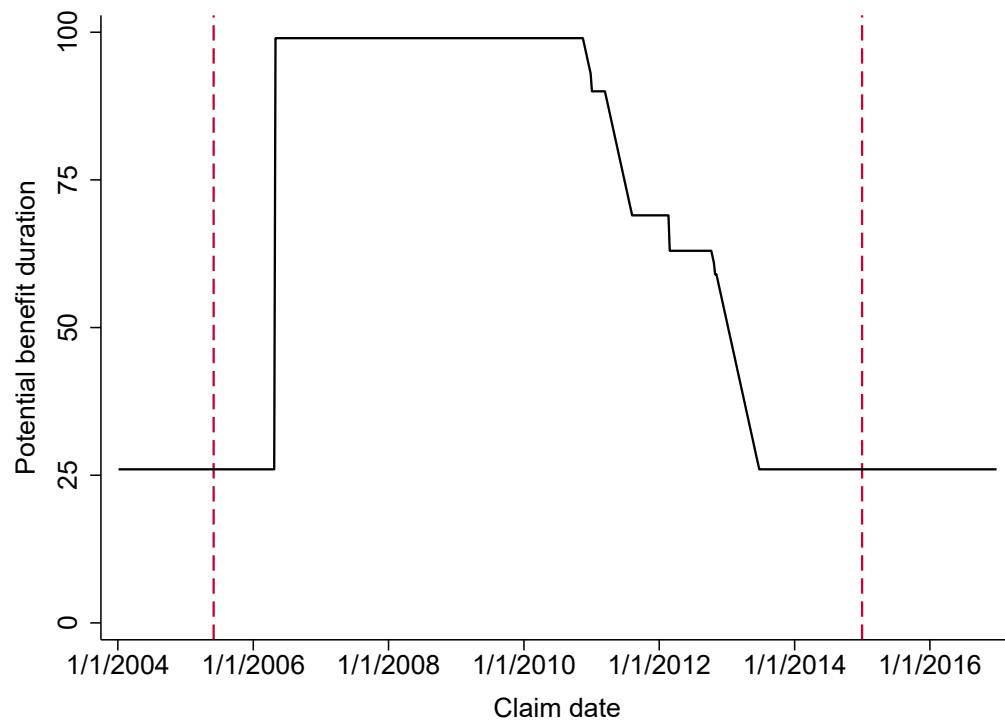
Notes: This figure plots the maximum potential duration (weeks) by UI claim date. The red vertical dashed lines indicate the start and end of the sample period in the main analysis.

Figure 4: NYC Department of Corrections Jail Booking Trends
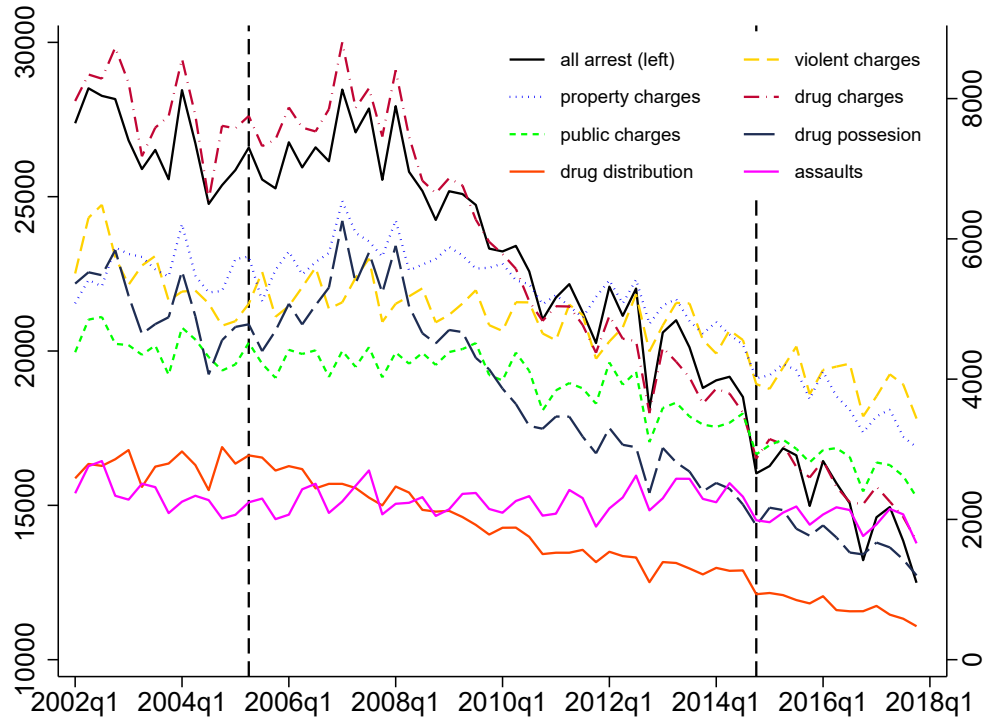
Notes: This figure plots the quarterly totals of stays in NYC Department of Correction Jails, axis on the left. The counts are broken up by broad crime type (violent, property, drug, and public crime) using the CJARS TOC tool, axis on the right. Counts of drug sales, assaults, and drug distribution are shown on the axis on the right. These are the three most common charges. The vertical dashed lines indicate the start and end of the sample period in the main analysis.

Figure 5: Density of Claims around Eligibility Threshold

(a) Histogram

p-value: .112

(b) Randomization Inference
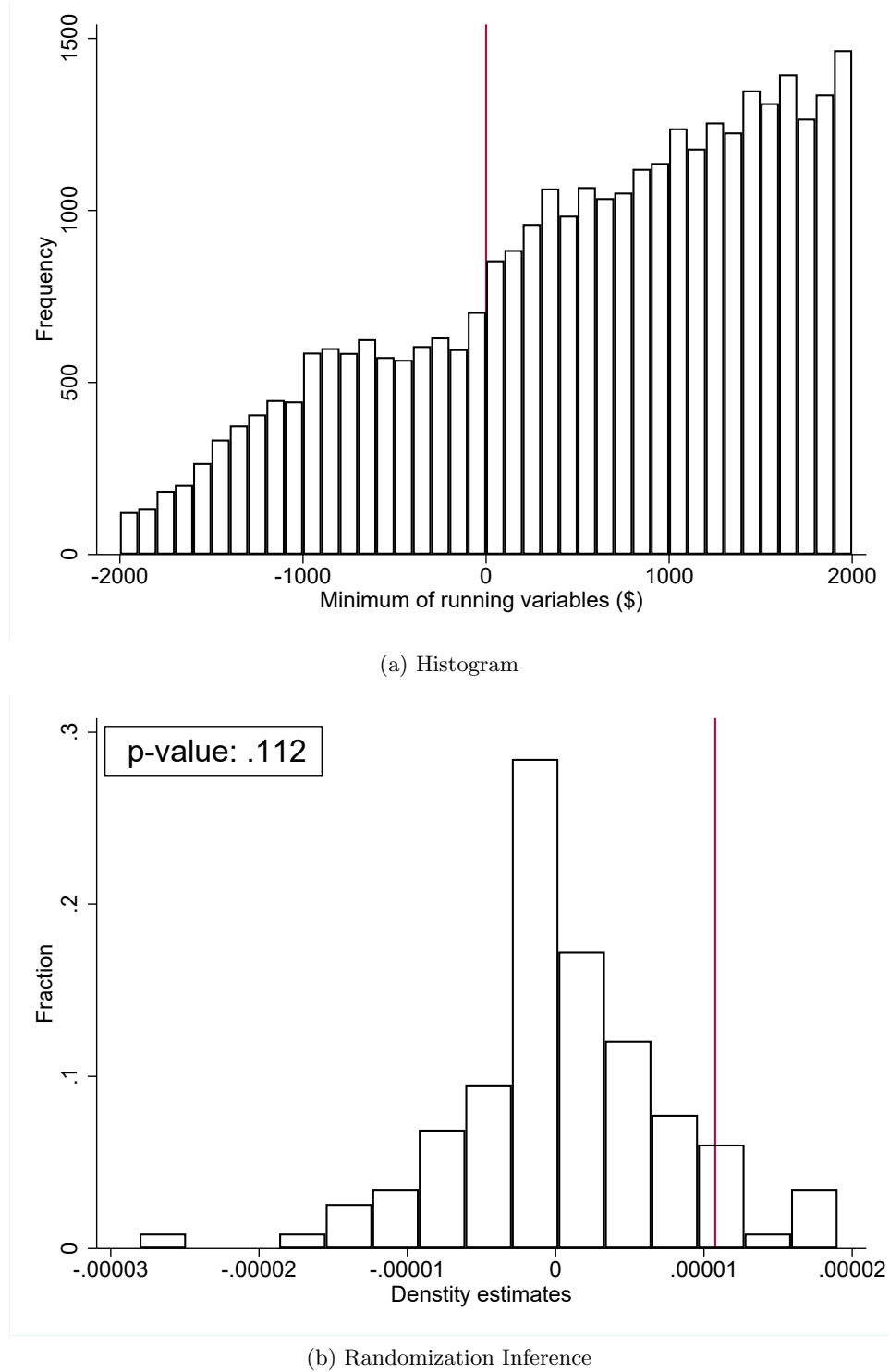
Notes: Panel A plots the number of UI claimants in each nonoverlapping $100 interval of the minimum of the two running variables (high-quarter earnings minus the relevant threshold and base-period earnings minus 1.5 times the high-quarter earnings. The vertical line denotes the minimum earnings threshold. Panel B plots the results from randomization inference of a change in density at the threshold (Bertanha et al., 2024). The change in density is calculated at 115 evenly spaced points (including the true threshold) between -1500 and 10000 (there is no other rule change in this range), and the p-value is the fraction of density estimates with a greater absolute value than the true density estimate.

Figure 6: Covariate Balance Around Eligibility Threshold

(a) Predicted Probability of DOC Booking within 3 Years

(b) Predicted Nonemployment Duration

(c) Average Quarterly Earnings Two Years before Claim

(d) Prior DOC Booking (2 Years)
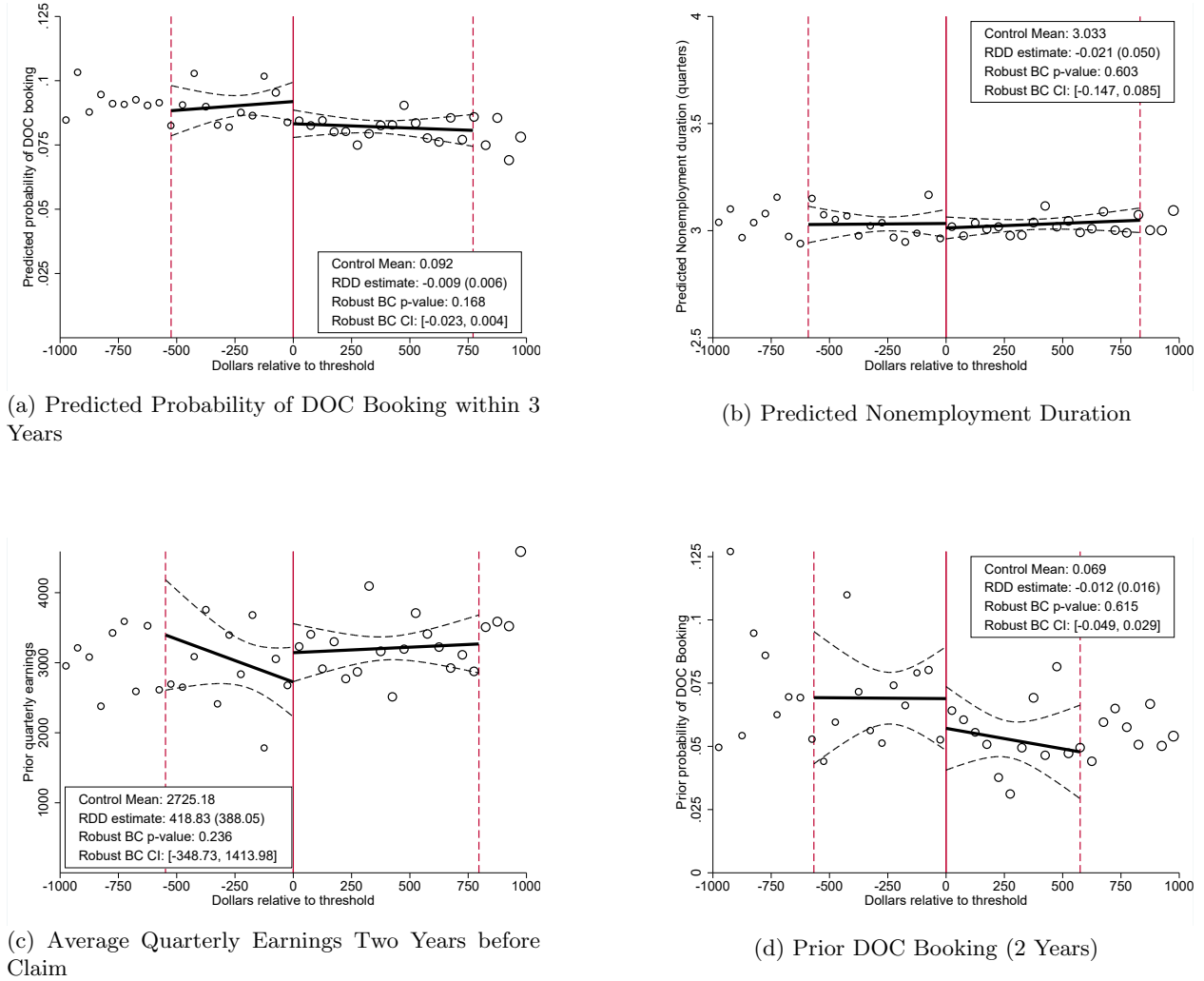
Notes: These figures summarize the relationship between observable predetermined characteristics and the running variable of UI claimants. The summary indexes in panels (A) and (B) are the predicted probability of DOC booking within 36 months and nonemployment duration after a layoff is calculated using a probit model for the probability of DOC booking and a linear regression for the nonemployment duration with demographic information, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction. For each summary index, the mean value is plotted. Panel (C) plots the average quarterly earnings from 6-9 quarters before the UI claim. Panel (D) plots the proportion of claimants appearing in the DOC data before the UI claim. The vertical line denotes the minimum earnings threshold. Each ring is a non-overlapping $50 interval of dollars to the nearest binding monetary eligibility requirement. The size of the ring is proportional to the number of observations in that interval. The dashed vertical lines denote the bandwidths. The thick solid lines within the bandwidths are the local linear fits, and the gap between them at the cutoff date approximates the RDD point estimate. The dashed lines around the local linear fits are 90 percent confidence intervals. In the box, I report the RDD point estimate, the conventional standard in parentheses, and the robust bias-corrected p-value. These RDDs are estimated without controls. All other RDD figures in this paper use the same methodology.

Figure 7: UI Eligibility and Benefit Amount (First Stage)
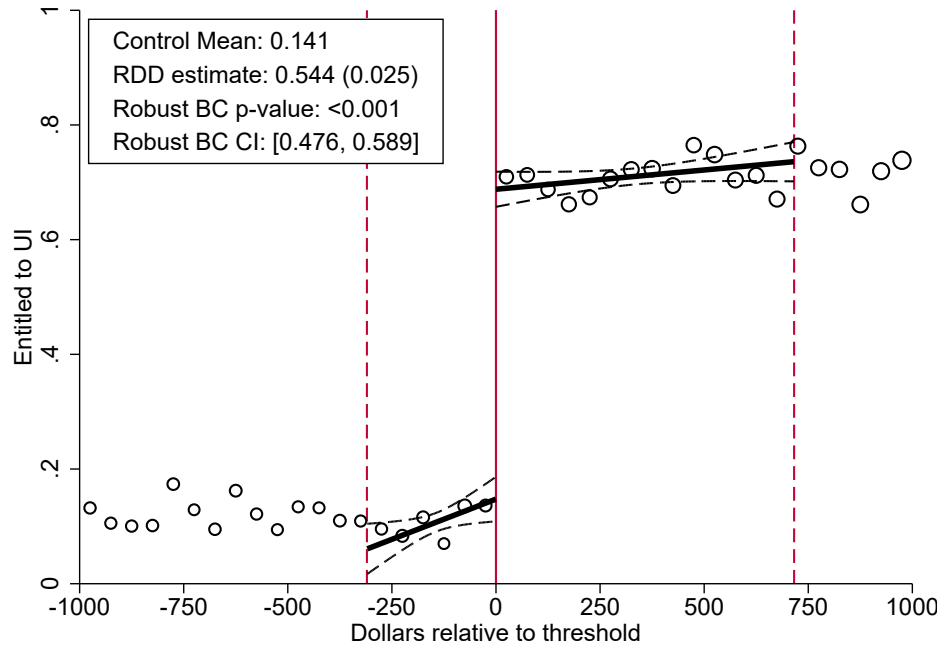
(a) Probability of UI Eligibility
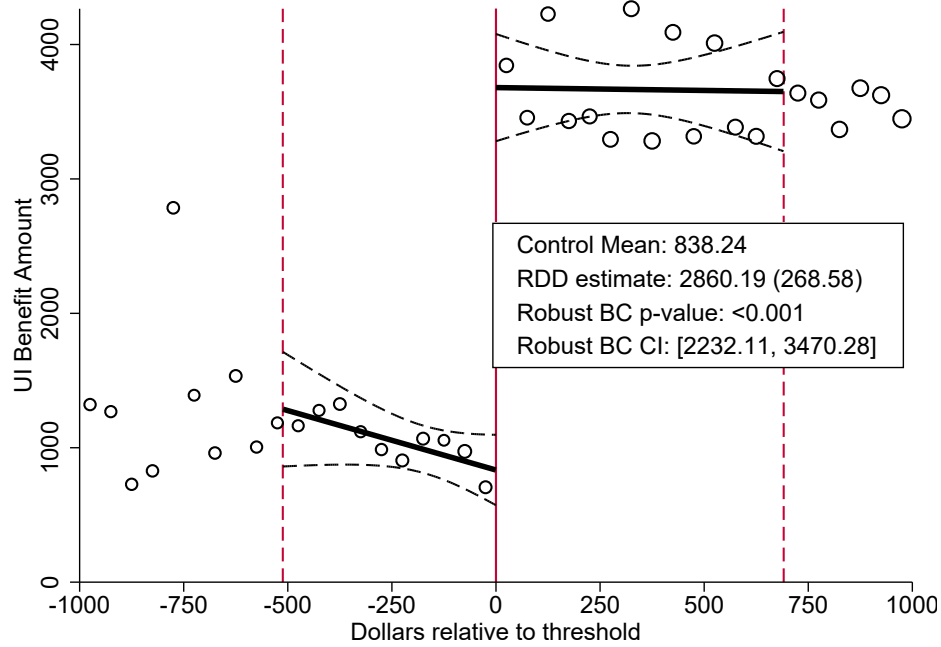
(b) UI Benefit Amount

Notes: Panel a (b) plots the proportion of UI claimants eligible for UI (the mean benefit amount received). See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Figure 8: Labor Supply Response

(a) Nonemployment Duration

Control Mean: 3.501
RDD estimate: 0.462 (0.273)
Robust BC p-value: 0.116
Robust BC CI: [-0.125, 1.139]
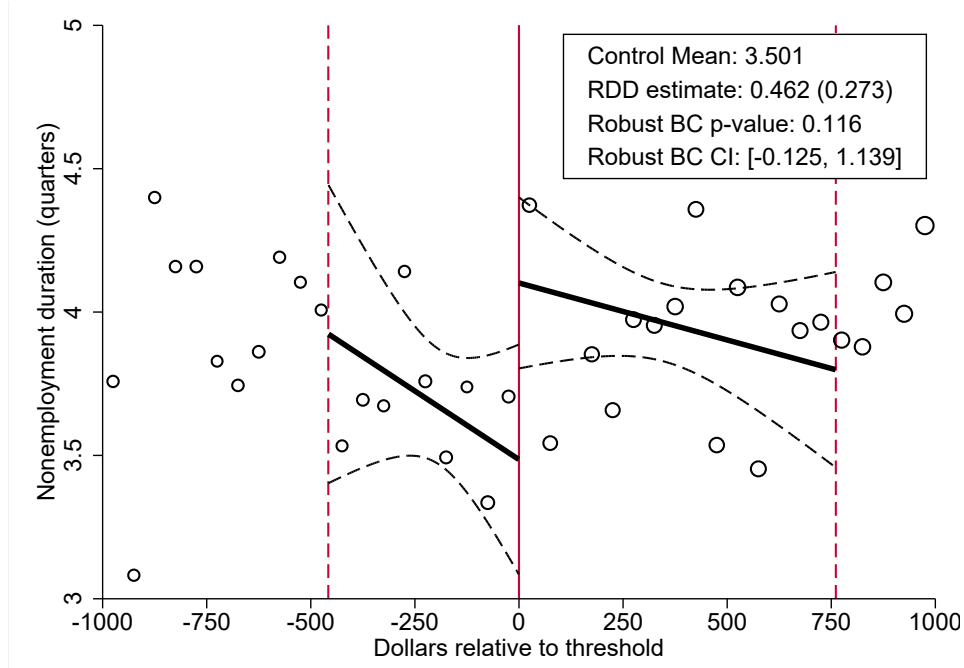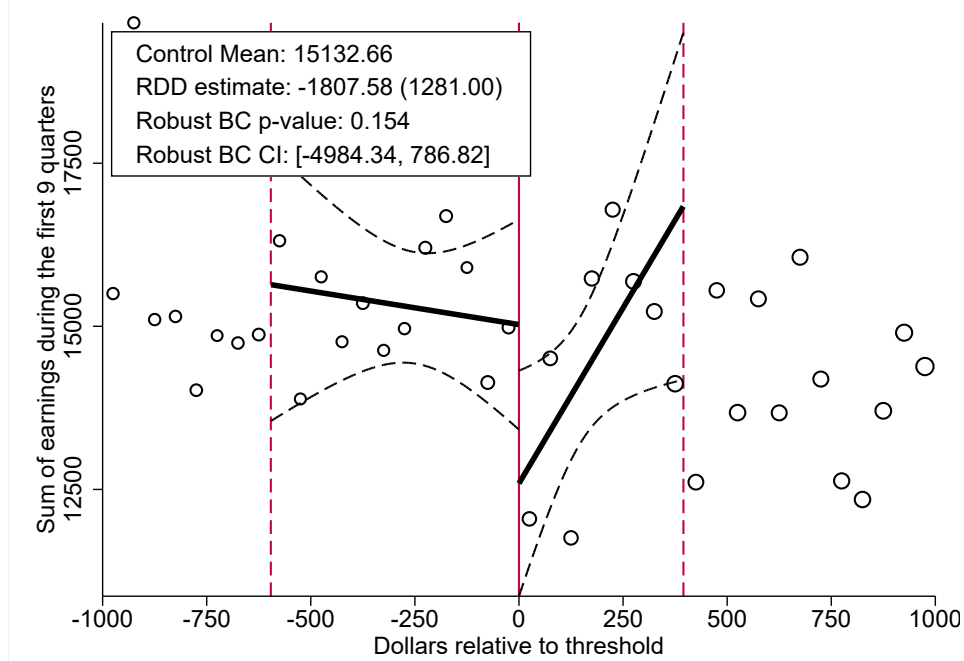
(b) Earnings Following UI Claim

Control Mean: 15132.66
RDD estimate: -1807.58 (1281.00)
Robust BC p-value: 0.154
Robust BC CI: [-4984.34, 786.82]

Notes: Panel A plots the nonemployment duration in quarters following the UI claim. Panel B plots the sum of earnings for the first nine quarters following the UI claim, including the quarter of the UI claim. See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Figure 9: Earnings and Income through Time
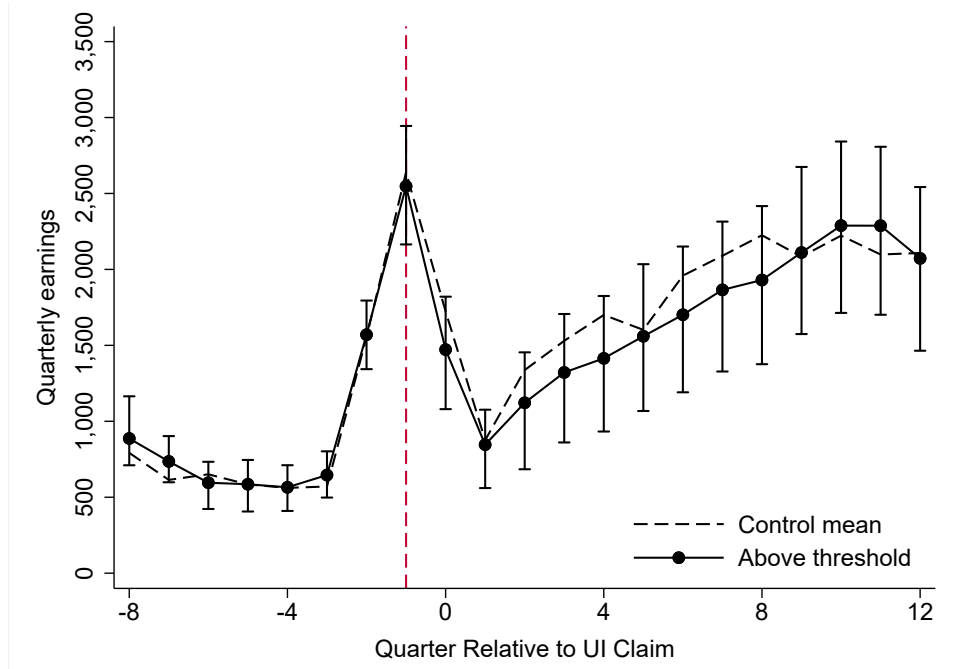
(a) Quarterly Earnings
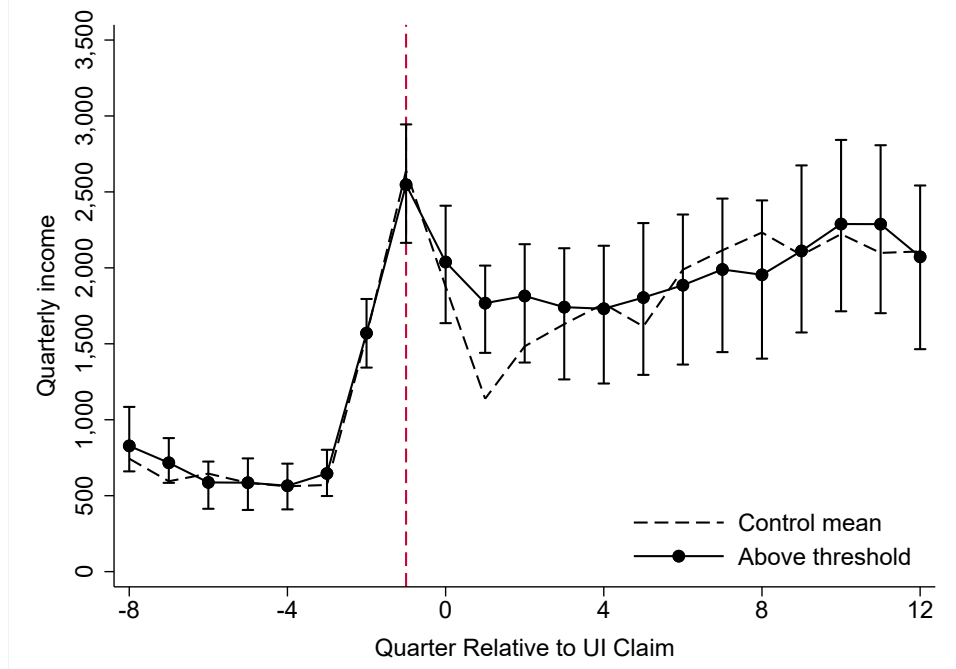
(b) Quarterly Income (Earnings and UI)

Notes: These figures plot RDD estimates of β_Y from equation (1) on earnings (panel A) and income (panel B) for each quarter relative to the claim date using a separate regression discontinuity for each quarter. The dotted line indicates the control mean (the estimated intercept to the left of the threshold). The point estimates and confidence intervals are added to the control mean. The 95 percent confidence intervals are robust bias-corrected. For precision, the RDD estimates include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction. The dashed red vertical line indicates the quarter before the UI claim.

Figure 10: Overall Crime

Control Mean: 0.061
RDD estimate: -0.030 (0.017)
Robust BC p-value: 0.081
Robust BC CI: [-0.070, 0.004]

(a) Booked within 1 Year

Control Mean: 0.098
RDD estimate: 0.005 (0.018)
Robust BC p-value: 0.753
Robust BC CI: [-0.035, 0.048]
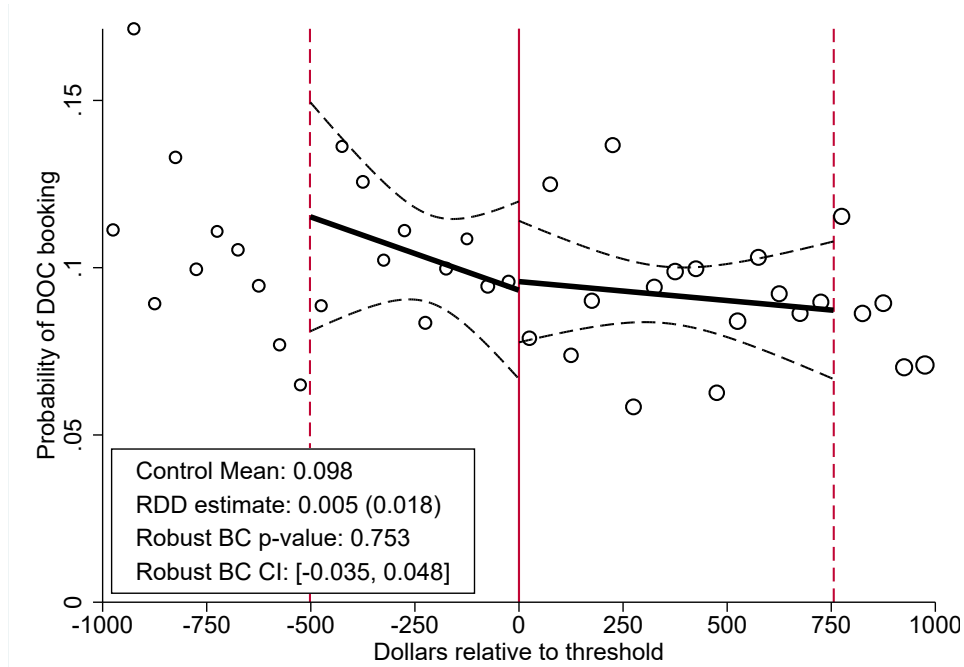
(b) Booked within 3 Years

Notes: Panel A (B) plots whether a claimant appears in the DOC records within one year (three years) after the UI claim. See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Figure 11: Assaults
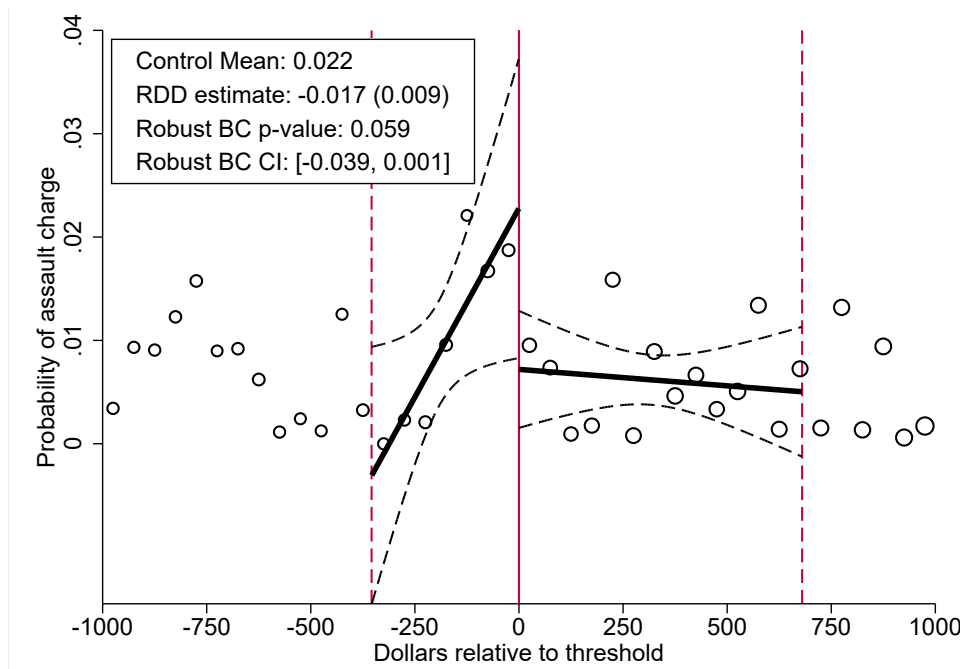
(a) Assault within 1 Year
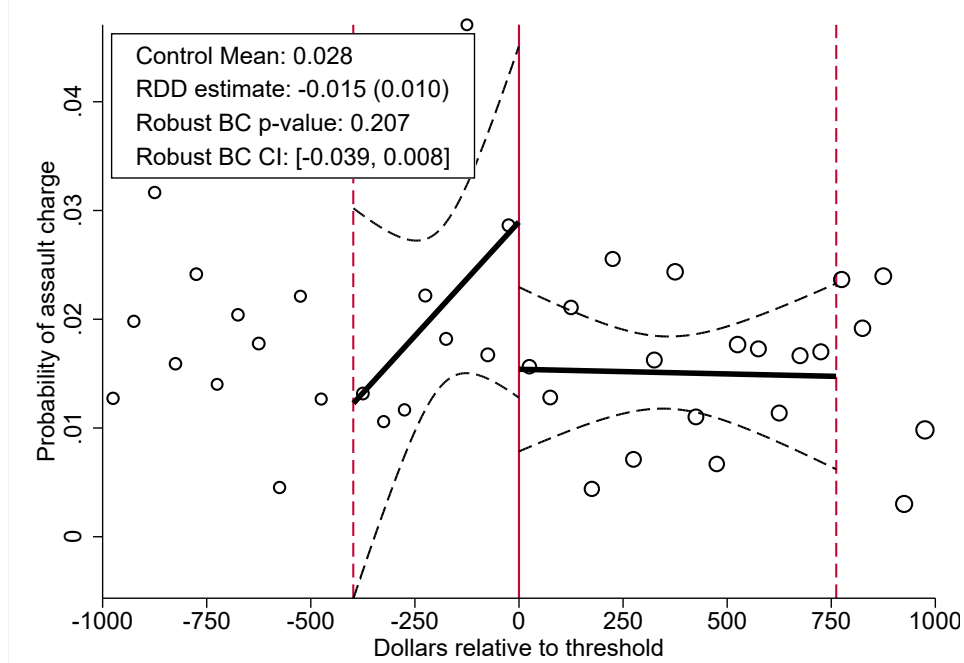
(b) Assault within 3 Years

Notes: Panel A (B) plots whether a claimant is booked with assault as the top charge in the DOC records within one year (three years) after the UI claim. See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Figure 12: Drug Crimes

(a) Drug Crime within 1 Year

(b) Drug Sale within 1 Year

(c) Drug Crime within 3 Years

(d) Drug Sale within 3 Years
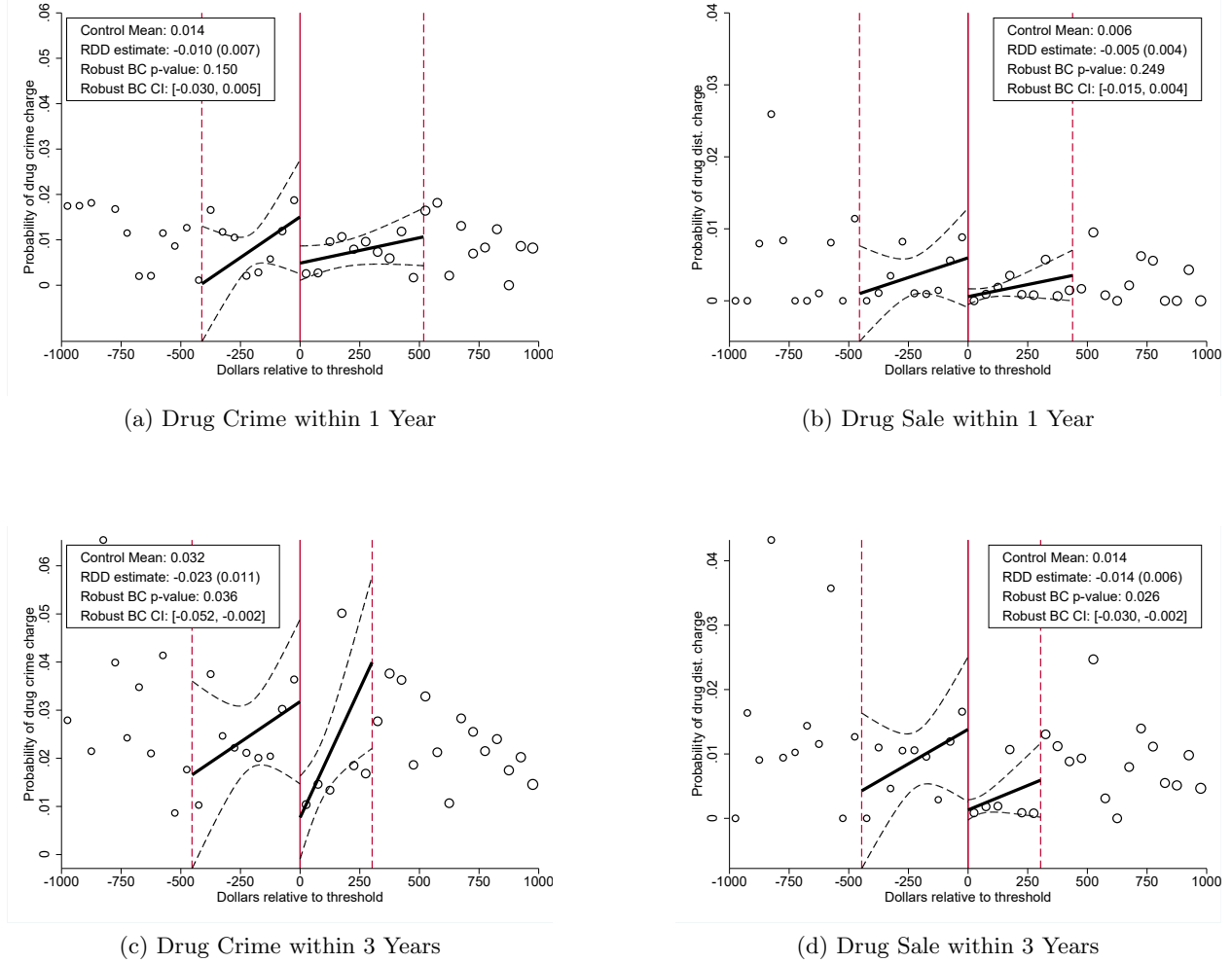
Notes: Panel A (C) plots whether a claimant is booked with a drug crime as the top charge in the DOC records within one year (three years) after the UI claim. Panel B (D) plots whether a claimant is booked with a drug distribution charge as the top charge in the DOC records within one year (three years) after the UI claim. See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.
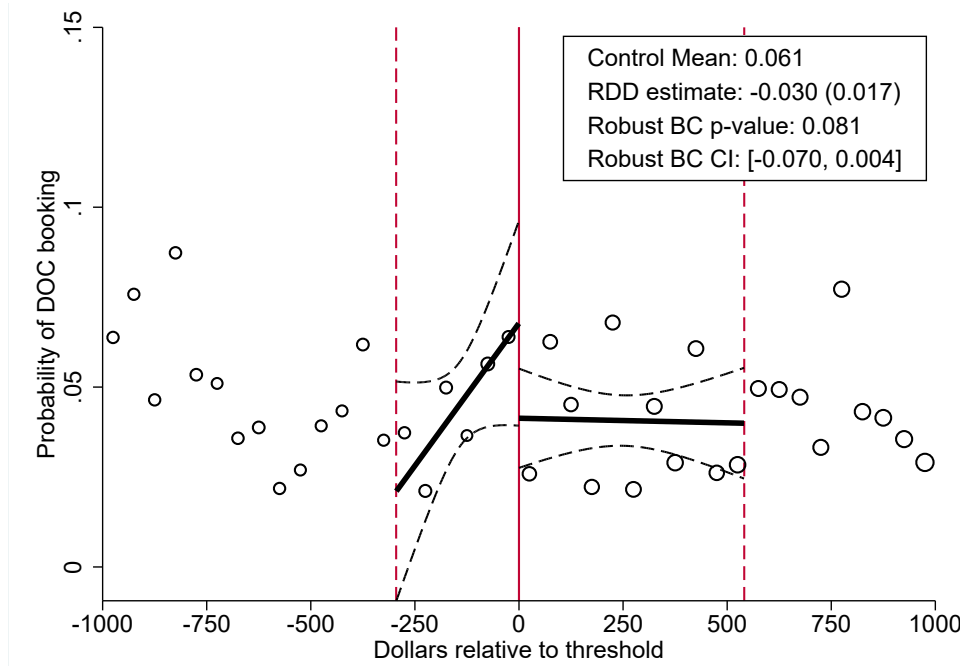
Figure 13: Heterogeneity by Claimant Characteristic
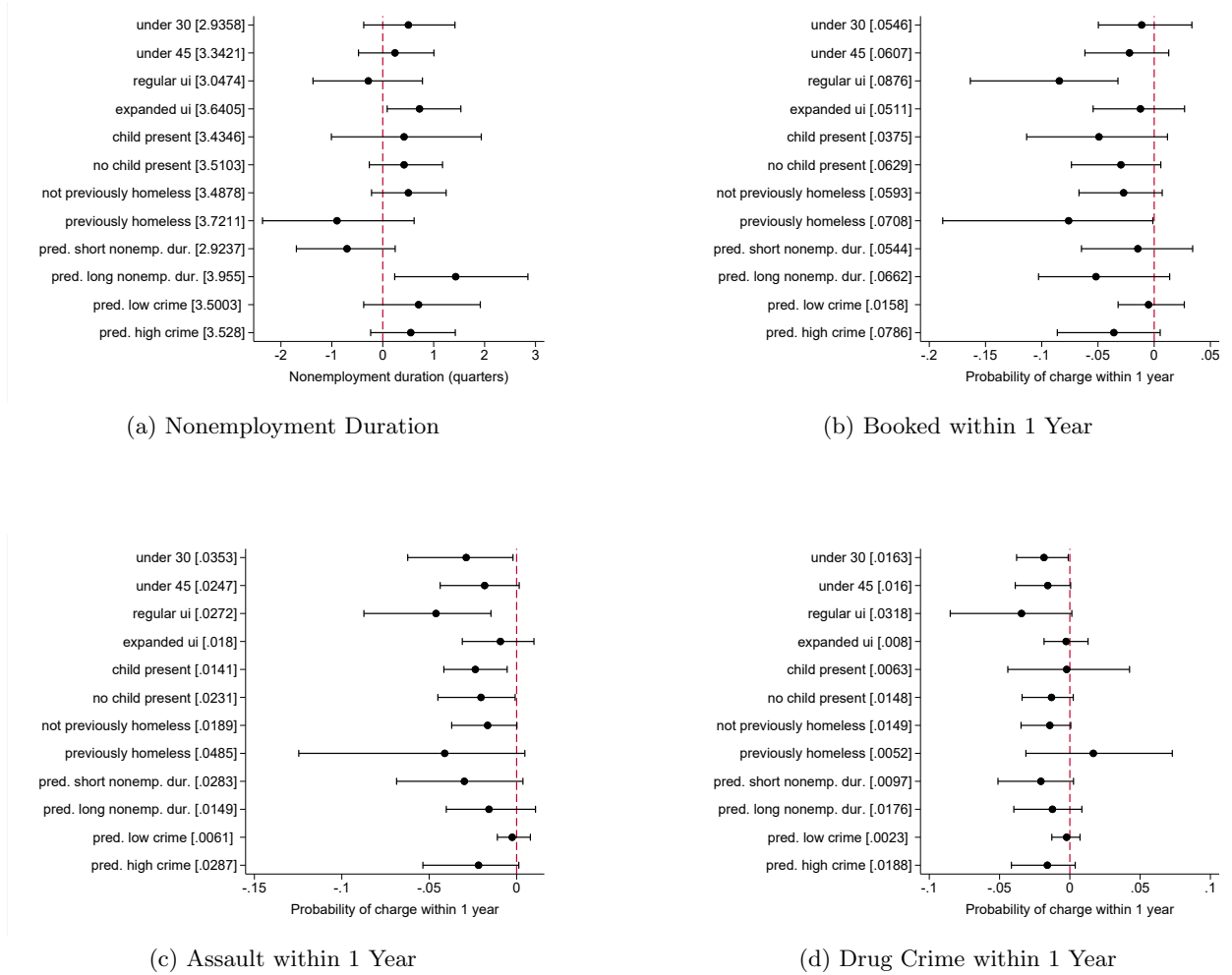
(a) Nonemployment Duration

(b) Booked within 1 Year

(c) Assault within 1 Year

(d) Drug Crime within 1 Year

Notes: This figure plots RDD point estimates and the bias-corrected confidence intervals for nonemployment duration (Panel A), being booked within one year (Panel B), being charged with an assault within one year (Panel C), and being charged with a drug crime (Panel D) for various subgroups. The subgroup is listed on the y-axis. The estimated intercept to the left of the threshold is displayed in brackets. Regular UI refers to the period (before 4/25/2006 and after 6/24/2013) where the maximum available UI duration was 26 weeks. Expanded UI refers to the period (4/25/2006-6/24/2013) where UI duration was expanded beyond 26 weeks. Pred. short (long) nonemp. dur. refers to individuals whose predicted nonemployment duration using predetermined covariates is below (above) the median. Pred. high (low) crime refers to individuals whose predicted probability of booking within three years using predetermined covariates is above (below) the median.

Tables

Table 1: Descriptive Statistics

	All (1)	Near Threshold (2)
Demographic Characteristics		
Age	35.44	32.9
Child in the household (%)	17.0	12.5
Education		
Less than high school (%)	29.2	33.0
High school (%)	32.3	32.7
Some college (%)	28.3	27.0
Bachelor's degree (%)	6.8	4.9
Race and Ethnicity		
White, not hispanic (%)	13.5	11.4
Black (%)	32.5	37.1
Asian (%)	12.4	13.6
Hispanic (%)	35.9	33.6
Employment, program participation,		
and booking history before layoff		
Previous year earnings	26132.85	8589.1
Ever claimed UI (%)	28.9	22.4
Received TANF (%)	13.0	21.7
Applied to shelter (%)	9.6	13.7
Booked within 2 years prior (%)	3.70	5.53
Violent crime within 2 years prior (%)	1.20	1.81
Property crime within 2 years prior (%)	0.76	1.22
Drug crime within 2 years prior (%)	0.80	1.26
Employment and DOC bookings after layoff		
Entitled for UI (%)	72.9	54.5
UI receipt (%)	70.2	52.7
Potential benefit amount (conditional on eligibility)	17926.77	10777.12
Benefit amount (conditional on eligibility)	9574.16	5746.64
Potential weeks on UI (conditional on eligibility)	68.72	69.56
weeks on UI (conditional on eligibility)	36.38	36.08
Employed quarter after UI claim (%)	42.9	39.6
Number of consecutive jobless quarters	2.91	3.19
Booked within 1 year (%)	2.80	3.95
Violent crime within 1 year (%)	0.84	1.16
Property crime within 1 year (%)	0.61	0.89
Drug crime within 1 year (%)	0.64	0.94
Observations	113921	32192

Notes: The sample for column 1 consists of all "new" UI claims (e.i., first claims or claims filed at least two years after a previous claim) from 2005Q2-2014Q4. Columns 2 restrict the sample to contain claims within $2000 of the nearest potentially binding monetary eligibility threshold.

Table 2: Local Linear Estimates of Discontinuities in Predetermined Covariates

Covariate	Pt. Est.	SE	P-Value	Control Mean	Percent Break
Pred. prob. of DOC booking	-0.009	(0.006)	[0.168]	0.092	-9.4
Demographic Characteristics					
Age	0.493	(0.688)	[0.653]	31.773	1.6
Child present	0.007	(0.022)	[0.837]	0.101	6.7
Education					
Less than high school	0.004	(0.030)	[0.873]	0.313	1.1
High school	-0.018	(0.029)	[0.521]	0.354	-5.1
Some college	0.011	(0.027)	[0.864]	0.252	4.5
Bachelor's degree	-0.007	(0.014)	[0.806]	0.059	-11.9
Race and ethnicity					
White, not hispanic	-0.015	(0.021)	[0.424]	0.115	-13.2
Black	0.028	(0.031)	[0.321]	0.387	7.3
Hispanic	0.013	(0.032)	[0.594]	0.315	4.0
Program part. and employment before layoff					
Prior shelter app.	-0.037	(0.023)	[0.124]	0.166	-22.1
Quarterly earnings (two years before)	0.01	(0.03)	[0.236]	2725.182	15.4
Employed (two years before)	0.072	(0.026)	[0.006]	0.298	24.1
Ever Claimed UI	0.014	(0.026)	[0.807]	0.223	6.2
Received TANF	0.001	(0.025)	[0.993]	0.240	0.5
Criminal involvement before layoff					
Prior booking	-0.012	(0.016)	[0.615]	0.069	-17.1
Prior violent charge	-0.005	(0.009)	[0.620]	0.023	-21.5
Prior property charge	-0.005	(0.007)	[0.576]	0.011	-42.7
Prior drug charge	-0.005	(0.009)	[0.737]	0.021	-24.8
Prior drug sale	-0.007	(0.006)	[0.214]	0.014	-52.3

Notes: Each row corresponds to an estimate of the discontinuity using equation (1) with the listed covariate as the outcome variable. Bandwidths are selected using the method proposed by Calonico et al. (2014), allowing bandwidths to vary on either side of the threshold. The Pt. est. reports $\hat{\beta}_y$. The SE column reports the convenctional heteroskedastic-robust standard error. The P-Value column reports the robust bias-corrected p-value. The Percent Break column gives the linear discontinuity estimate as a percent of the control mean (the estimated intercept to the left of the threshold). The predicted probability of DOC booking within 36 months is calculated using a probit model with demographic information, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction. Quarterly earnings (two years before) are the average earnings from 6-9 quarters before the UI claim. Employed (two years before) is the fraction of quarters 6-9 quarters before the UI claim the claimant has positive earnings.

Table 3: First-stage RDD Estimates

Outcome	Pt. Est.	SE	P-Value	Confidence interval	Control Mean	Bandwidths	Effective Obs.
Entitled for UI	0.54	(0.03)	0.000	[0.48, 0.59]	0.14	(311, 716)	8951
UI receipt	0.51	(0.03)	0.000	[0.44, 0.56]	0.14	(299, 615)	7852
Potential benefit amount	4362.33	(419.12)	0.000	[3279.55, 5210.86]	2033.96	(447, 722)	9841
Benefit amount	2860.19	(268.58)	0.000	[2232.11, 3470.28]	838.24	(512, 692)	9878
Potential weeks on UI	36.71	(2.10)	0.000	[31.03, 40.57]	9.55	(405, 597)	8311
Weeks on UI	21.77	(1.45)	0.000	[18.22, 24.85]	4.21	(482, 570)	8461

Notes: Each row presents estimates of β_Y from equation (1) where the outcome variable, Y_i, in row 1 is whether the claim is deemed eligible, in row 2 whether the claimant received UI benefits, in row 3 is the maximum potential benefit available, in row 4 the total benefit amount received, and in row 5 is the maximum potential number of weeks a claimant could receive UI, and in row 6 is the number of weeks a claimant receives UI. Bandwidths are selected using the method proposed by Calonico et al. (2014), allowing bandwidths to vary on either side of the threshold. Conventional heteroskedastic-robust standard errors are included below the point estimate in parentheses. The control mean is the estimated intercept to the left of the threshold. The 95 percent confidence intervals and p-values are robust bias-corrected. Effective Observations list the number of claims that fall into the selected bandwidths. For precision, the RDD estimates include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Table 4: Labor Supply RDD Estimates

Outcome	Pt. Est.	SE	P-Value	Confidence interval	Control Mean	Bandwidths	Effective Obs.
Nonemployment duration (quarters)	0.462	(0.273)	0.116	[-0.125, 1.139]	3.501	(458, 762)	10317
Sum of earnings during the first 9 quarters	-1807.58	(1281.001)	0.154	[-4984.34, 786.818]	15132.66	(597, 396)	7312
Nonemployment duration > 0 quarters	0.019	(0.029)	0.486	[-0.043, 0.090]	0.646	(649, 524)	8968
Nonemployment duration > 1 quarters	0.059	(0.033)	0.073	[-0.007, 0.146]	0.474	(417, 598)	8390
Nonemployment duration > 2 quarters	0.030	(0.032)	0.342	[-0.038, 0.109]	0.399	(412, 610)	8483
Nonemployment duration > 3 quarters	0.020	(0.029)	0.572	[-0.048, 0.087]	0.343	(480, 726)	10070
Nonemployment duration > 4 quarters	0.033	(0.028)	0.308	[-0.031, 0.099]	0.290	(473, 660)	9341
Nonemployment duration > 5 quarters	0.031	(0.027)	0.301	[-0.029, 0.094]	0.259	(503, 773)	10661
Nonemployment duration > 6 quarters	0.049	(0.026)	0.088	[-0.008, 0.112]	0.224	(468, 761)	10366
Nonemployment duration > 7 quarters	0.048	(0.026)	0.082	[-0.007, 0.113]	0.198	(471, 638)	9109
Nonemployment duration > 8 quarters	0.044	(0.027)	0.117	[-0.012, 0.110]	0.187	(472, 533)	8034
Nonemployment duration > 9 quarters	0.043	(0.025)	0.110	[-0.011, 0.104]	0.167	(460, 663)	9312
Nonemployment duration > 10 quarters	0.037	(0.023)	0.171	[-0.016, 0.092]	0.162	(452, 806)	10775
Nonemployment duration > 11 quarters	0.052	(0.023)	0.043	[0.002, 0.109]	0.146	(409, 807)	10551

Notes: Each row presents estimates of β_Y from equation (1) for the outcome variable listed in the first column. Bandwidths are selected using the method proposed by Calonico et al. (2014), allowing bandwidths to vary on either side of the threshold. Conventional heteroskedastic-robust standard errors are included below the point estimate in parentheses. The control mean is the estimated intercept to the left of the threshold. The 95 percent confidence intervals and p-values are robust bias-corrected. Effective observations list the number of claims that fall into the selected bandwidths. For precision, the RDD estimates include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction

Table 5: DOC interaction RDD Estimates

Outcome	Pt. Est.	SE	P-Value	Confidence interval	Control Mean	Bandwidths	Effective Obs.
Booked within 1 year	-0.030	(0.017)	0.081	[-0.070, 0.004]	0.061	(296, 541)	7060
Booked within 2 years	-0.010	(0.017)	0.651	[-0.049, 0.030]	0.081	(460, 698)	9667
Booked within 3 years	0.005	(0.018)	0.753	[-0.035, 0.048]	0.098	(502, 757)	10502
Violent crime charge within 1 year	-0.009	(0.009)	0.364	[-0.029, 0.011]	0.018	(427, 766)	10165
Violent crime charge within 2 years	-0.005	(0.011)	0.686	[-0.031, 0.020]	0.023	(392, 647)	8734
Violent crime charge within 3 years	-0.008	(0.012)	0.589	[-0.034, 0.019]	0.033	(484, 674)	9525
Property crime charge within 1 year	0.004	(0.007)	0.503	[-0.010, 0.020]	0.009	(366, 485)	6880
Property crime charge within 2 years	0.011	(0.009)	0.144	[-0.005, 0.035]	0.016	(436, 454)	7015
Property crime charge within 3 years	0.020	(0.011)	0.051	[0.000, 0.048]	0.025	(471, 419)	6847
Drug crime charge within 1 year	-0.010	(0.007)	0.150	[-0.030, 0.005]	0.014	(413, 518)	7537
Drug crime charge within 2 years	-0.016	(0.010)	0.100	[-0.041, 0.004]	0.029	(508, 560)	8509
Drug crime charge within 3 years	-0.023	(0.011)	0.036	[-0.052, -0.002]	0.032	(453, 303)	5546
Public order crime charge within 1 year	0.005	(0.006)	0.354	[-0.007, 0.020]	0.007	(478, 541)	8136
Public order charge within 2 years	-0.001	(0.007)	0.924	[-0.016, 0.015]	0.015	(624, 668)	10296
Public order charge within 3 years	0.006	(0.009)	0.490	[-0.014, 0.029]	0.023	(586, 687)	10254
Assault charge within 1 year	-0.017	(0.009)	0.059	[-0.039, 0.001]	0.022	(354, 681)	8854
Assault charge within 2 years	-0.015	(0.011)	0.223	[-0.041, 0.009]	0.027	(328, 547)	7310
Assault charge within 3 years	-0.015	(0.010)	0.207	[-0.039, 0.008]	0.028	(399, 762)	9976
Drug distribution charge within 1 year	-0.005	(0.004)	0.249	[-0.015, 0.004]	0.006	(455, 438)	6955
Drug distribution charge within 2 years	-0.010	(0.005)	0.039	[-0.021, -0.001]	0.009	(545, 429)	7359
Drug distribution charge within 3 years	-0.014	(0.006)	0.026	[-0.030, -0.002]	0.014	(446, 304)	5517
Drug possession charge within 1 year	-0.006	(0.006)	0.282	[-0.021, 0.006]	0.008	(482, 464)	7360
Drug possession charge within 2 years	-0.007	(0.009)	0.388	[-0.030, 0.011]	0.020	(464, 570)	8369
Drug possession charge within 3 years	-0.015	(0.010)	0.102	[-0.041, 0.004]	0.023	(445, 304)	5507

Notes: Each row presents estimates of β_Y from equation (1) for the outcome variable listed in the first column. Bandwidths are selected using the method proposed by Calonico et al. (2014), allowing bandwidths to vary on either side of the threshold. Conventional heteroskedastic-robust standard errors are included below the point estimate in parentheses. The control mean is the estimated intercept to the left of the threshold. The 95 percent confidence intervals and p-values are robust bias-corrected. Effective observations list the number of claims that fall into the selected bandwidths. For precision, the RDD estimates include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

A   Additional Tables

Table A1: Sample Construction

Sample Restriction	Number of Claims Dropped	Number of Claims Remaining
All UI claims		951,887
At least 2 years between claims	387,144	564,743
2005Q2-2014Q4	150,281	414,462
Appear in HRA data before UI claim	75,448	339,014
Age between 18-65	4,916	334,098
Positive past earnings	20,195	313,903
Male	199,982	113,921
Near joint threshold	81,729	32,192

Notes: This shows how the main analysis sample is constructed. The sample restriction is listed in column 1. The number of claims this sample restriction drops is listed in column 2. The number of claims remaning after imposing the restriction is listed in column 3. The final analysis sample is comprised of 32,192 UI claims.

Table A2: Alternative Specifications

Outcome	Pt. Est.	SE	P-Value	Confidence interval	Bandwidths	Effective Obs.
UI eligible						
preferred	0.544	(0.025)	0.000	[0.476, 0.589]	(311, 716)	8951
without controls	0.548	(0.027)	0.000	[0.476, 0.598]	(388, 756)	9885
common bandwidth	0.565	(0.026)	0.000	[0.497, 0.616]	(454, 454)	7106
uniform kernel	0.551	(0.024)	0.000	[0.490, 0.599]	(332, 616)	8054
means (p=0)	0.572	(0.019)	0.000	[0.515, 0.607]	(209, 251)	3552
quadratic (p=2)	0.534	(0.034)	0.000	[0.458, 0.605]	(475, 734)	10123
donut	0.551	(0.027)	0.000	[0.479, 0.600]	(335, 662)	8368
Nonemployment duration (quarters)						
preferred	0.330	(0.205)	0.136	[-0.114, 0.835]	(462, 760)	10316
without controls	0.448	(0.211)	0.058	[-0.017, 0.979]	(615, 724)	10885
common bandwidth	0.276	(0.217)	0.265	[-0.218, 0.791]	(499, 499)	7783
uniform kernel	0.449	(0.224)	0.075	[-0.047, 0.962]	(310, 660)	8366
means (p=0)	0.219	(0.140)	0.140	[-0.088, 0.625]	(302, 360)	5275
quadratic (p=2)	0.371	(0.319)	0.290	[-0.323, 1.081]	(415, 605)	8453
donut	0.245	(0.222)	0.326	[-0.257, 0.773]	(474, 641)	8992
Booked within 1 year						
preferred	-0.030	(0.017)	0.081	[-0.070, 0.004]	(296, 541)	7060
without controls	-0.020	(0.016)	0.233	[-0.060, 0.015]	(444, 673)	9363
common bandwidth	-0.035	(0.017)	0.033	[-0.079, -0.003]	(301, 301)	4632
uniform kernel	-0.028	(0.017)	0.121	[-0.068, 0.008]	(244, 433)	5661
means (p=0)	-0.020	(0.012)	0.083	[-0.057, 0.003]	(110, 375)	4259
quadratic (p=2)	-0.040	(0.021)	0.048	[-0.090, 0.000]	(433, 663)	9159
donut	-0.022	(0.017)	0.231	[-0.063, 0.015]	(327, 500)	6625
Booked within 3 years						
preferred	0.005	(0.018)	0.753	[-0.035, 0.048]	(502, 757)	10502
without controls	-0.002	(0.018)	0.860	[-0.046, 0.038]	(635, 858)	12454
common bandwidth	0.001	(0.018)	0.963	[-0.041, 0.043]	(536, 536)	8410
uniform kernel	0.007	(0.020)	0.629	[-0.034, 0.056]	(301, 637)	8097
means (p=0)	0.002	(0.013)	0.810	[-0.029, 0.038]	(227, 437)	5574
quadratic (p=2)	-0.008	(0.029)	0.688	[-0.077, 0.051]	(346, 750)	9497
donut	0.005	(0.019)	0.853	[-0.039, 0.048]	(526, 682)	9701
Assault charge within 1 year						
preferred	-0.017	(0.009)	0.059	[-0.039, 0.001]	(354, 681)	8854
without controls	-0.015	(0.009)	0.089	[-0.037, 0.003]	(436, 799)	10645
common bandwidth	-0.018	(0.009)	0.052	[-0.041, 0.000]	(366, 366)	5692
uniform kernel	-0.018	(0.009)	0.050	[-0.039, 0.000]	(279, 519)	6754
means (p=0)	-0.011	(0.007)	0.088	[-0.031, 0.002]	(131, 397)	4563
quadratic (p=2)	-0.019	(0.011)	0.113	[-0.043, 0.005]	(517, 819)	11269
donut	-0.020	(0.010)	0.066	[-0.046, 0.001]	(343, 777)	9594
Assault charge within 3 years						
preferred	-0.015	(0.010)	0.207	[-0.039, 0.008]	(399, 762)	9976
without controls	-0.012	(0.010)	0.245	[-0.037, 0.009]	(516, 777)	10825
common bandwidth	-0.014	(0.011)	0.238	[-0.039, 0.010]	(440, 440)	6888
uniform kernel	-0.016	(0.010)	0.142	[-0.040, 0.006]	(347, 651)	8503
means (p=0)	-0.009	(0.007)	0.188	[-0.030, 0.006]	(163, 470)	5484
quadratic (p=2)	-0.013	(0.014)	0.442	[-0.044, 0.019]	(499, 656)	9448
donut	-0.017	(0.012)	0.177	[-0.047, 0.009]	(391, 730)	9425

Notes: Each row presents estimates of β_Y from equation (1) for the outcome variable listed in the first column using alternative specifications. The specification is listed in the first column, below the outcome variable. "Preferred" denotes the primary specification used throughout the paper described in Section 4.2. "Without controls" estimates the RDD without using pre-determined covariates as controls. "Common bandwidth" requires the bandwidth on either side of the threshold to be the same. Bandwidths are selected using the method proposed by Calonico et al. (2014), using the "mserd" option. The "uniform kernel" specification using a uniform kernel instead of a triangular kernel. The "means (p=0)" specification calculates means on either side of the threshold and linearly estimates the bias. The "quadratic (p=2)" specification uses a quadratic spline and estimates the bias with a cubic polynomial. The "donut" specification excludes observations within \$10 of the joint threshold. Conventional heteroskedastic-robust standard errors are included below the point estimate in parentheses. The 95 percent confidence intervals and p-values are robust bias-corrected. Effective observations list the number of claims that fall into the selected bandwidths.

Table A3: Alternative Specifications Cont.

Outcome	Pt. Est.	SE	P-Value	Confidence interval	Bandwidths	Effective Obs.
Drug crime charge within 1 year						
preferred	-0.010	(0.007)	0.150	[-0.030, 0.005]	(413, 518)	7537
without controls	-0.009	(0.008)	0.200	[-0.031, 0.006]	(446, 561)	8228
common bandwidth	-0.012	(0.008)	0.102	[-0.032, 0.003]	(393, 393)	6137
uniform kernel	-0.010	(0.008)	0.238	[-0.029, 0.007]	(297, 544)	7092
means (p=0)	-0.004	(0.004)	0.314	[-0.016, 0.005]	(337, 313)	4958
quadratic (p=2)	-0.022	(0.012)	0.062	[-0.052, 0.001]	(356, 443)	6412
donut	-0.010	(0.007)	0.162	[-0.029, 0.005]	(392, 457)	6617
Drug crime charge within 3 years						
preferred	-0.023	(0.011)	0.036	[-0.052, -0.002]	(453, 303)	5546
without controls	-0.023	(0.012)	0.057	[-0.055, 0.001]	(437, 325)	5703
common bandwidth	-0.021	(0.011)	0.036	[-0.049, -0.002]	(442, 442)	6916
uniform kernel	-0.027	(0.012)	0.026	[-0.057, -0.004]	(318, 262)	4331
means (p=0)	-0.014	(0.007)	0.032	[-0.036, -0.002]	(319, 136)	3176
quadratic (p=2)	-0.036	(0.016)	0.026	[-0.076, -0.005]	(410, 509)	7425
donut	-0.025	(0.012)	0.042	[-0.058, -0.001]	(403, 289)	4961
Drug distribution charge within 1 year						
preferred	-0.005	(0.004)	0.249	[-0.015, 0.004]	(455, 438)	6955
without controls	-0.006	(0.004)	0.198	[-0.016, 0.003]	(462, 393)	6586
common bandwidth	-0.005	(0.004)	0.260	[-0.015, 0.004]	(447, 447)	6993
uniform kernel	-0.003	(0.005)	0.565	[-0.015, 0.008]	(279, 367)	5200
means (p=0)	-0.004	(0.002)	0.190	[-0.011, 0.002]	(339, 243)	4261
quadratic (p=2)	-0.006	(0.005)	0.249	[-0.016, 0.004]	(675, 502)	8922
donut	-0.006	(0.005)	0.213	[-0.018, 0.004]	(435, 453)	6823
Drug distribution charge within 3 years						
preferred	-0.014	(0.006)	0.026	[-0.030, -0.002]	(446, 304)	5517
without controls	-0.013	(0.007)	0.072	[-0.030, 0.001]	(437, 301)	5470
common bandwidth	-0.013	(0.006)	0.027	[-0.030, -0.002]	(436, 436)	6822
uniform kernel	-0.017	(0.007)	0.017	[-0.034, -0.003]	(300, 180)	3479
means (p=0)	-0.011	(0.004)	0.009	[-0.021, -0.003]	(371, 145)	3566
quadratic (p=2)	-0.014	(0.010)	0.174	[-0.038, 0.007]	(383, 606)	8270
donut	-0.011	(0.006)	0.068	[-0.025, 0.001]	(395, 304)	5054
Drug possession charge within 1 year						
preferred	-0.006	(0.006)	0.282	[-0.021, 0.006]	(482, 464)	7360
without controls	-0.004	(0.006)	0.463	[-0.020, 0.009]	(519, 591)	8922
common bandwidth	-0.006	(0.006)	0.267	[-0.021, 0.006]	(451, 451)	7070
uniform kernel	-0.002	(0.006)	0.801	[-0.016, 0.012]	(378, 573)	7899
means (p=0)	-0.001	(0.003)	0.731	[-0.011, 0.007]	(288, 357)	5149
quadratic (p=2)	-0.018	(0.011)	0.091	[-0.046, 0.003]	(358, 430)	6309
donut	-0.006	(0.005)	0.272	[-0.019, 0.005]	(432, 337)	5598
Drug possession charge within 3 years						
preferred	-0.015	(0.010)	0.102	[-0.041, 0.004]	(445, 304)	5507
without controls	-0.014	(0.011)	0.156	[-0.043, 0.007]	(436, 377)	6287
common bandwidth	-0.012	(0.009)	0.158	[-0.037, 0.006]	(447, 447)	7003
uniform kernel	-0.007	(0.010)	0.354	[-0.033, 0.012]	(367, 288)	4875
means (p=0)	-0.001	(0.005)	0.680	[-0.017, 0.011]	(369, 259)	4627
quadratic (p=2)	-0.031	(0.015)	0.037	[-0.071, -0.002]	(386, 474)	6904
donut	-0.015	(0.011)	0.140	[-0.043, 0.006]	(396, 292)	4937

*Notes*: Each row presents estimates of β_Y from equation (1) for the outcome variable listed in the first column using alternative specifications. The specification is listed in the first column, below the outcome variable. "Preferred" denotes the primary specification used throughout the paper described in Section 4.2. "Without controls" estimates the RDD without using pre-determined covariates as controls. "Common bandwidth" requires the bandwithd on either side of the threshold to be the same. Bandwidths are selected using the method proposed by Calonico et al. (2014), using the "mserd" option. The "uniform kernel" specification using a uniform kernel instead of a triangular kernel. The "means (p=0)" specification calculates means on either side of the threshold and linearly estimates the bias. The "quadratic (p=2)" specification uses a quadratic spline and estimates the bias with a cubic polynomial. The "donut" specification excludes observations within \$10 of the joint threshold. Conventional heteroskedastic-robust standard errors are included below the point estimate in parentheses. The 95 percent confidence intervals and p-values are robust bias-corrected. Effective observations list the number of claims that fall into the selected bandwidths.

B   Additional Figures

Figure B1: By Detailed Crime Category
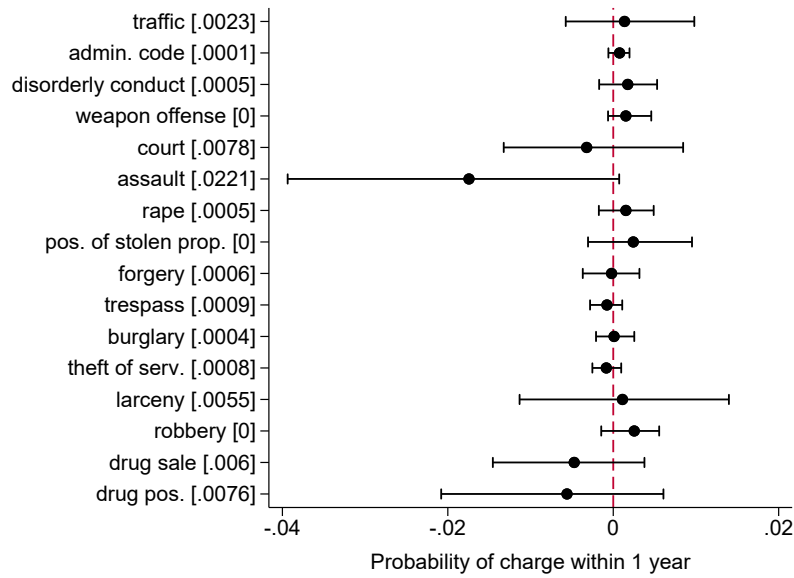
(a) Detailed Crime Categories within 1 Year
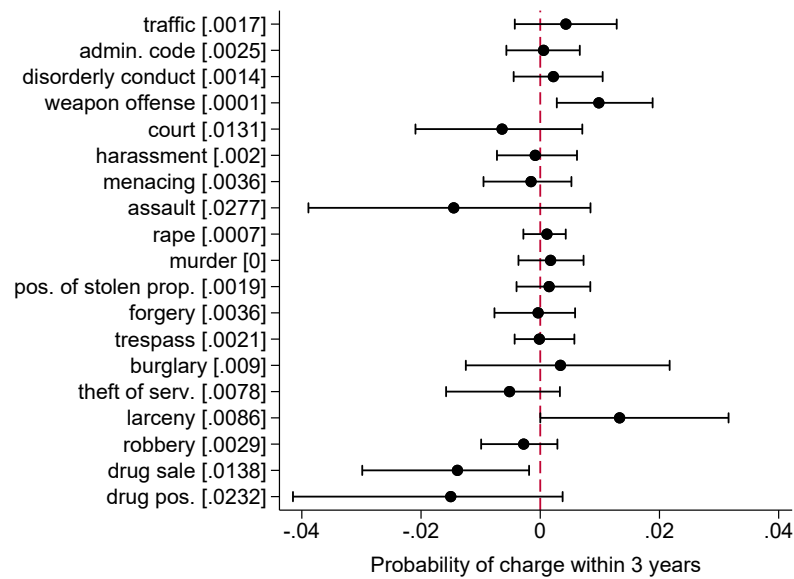

(b) Detailed Crime Categories within 3 Years

Notes: Panel A (B) plots RDD point estimates and the bias-corrected confidence intervals for whether a claimant is charged with a crime type listed on the y-axis within one year (three years). The estimated intercept to the left of the threshold is displayed in brackets. For precision, the RDD estimates include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

Figure B2: Heterogeneity by Claimant Characteristic (3 years)
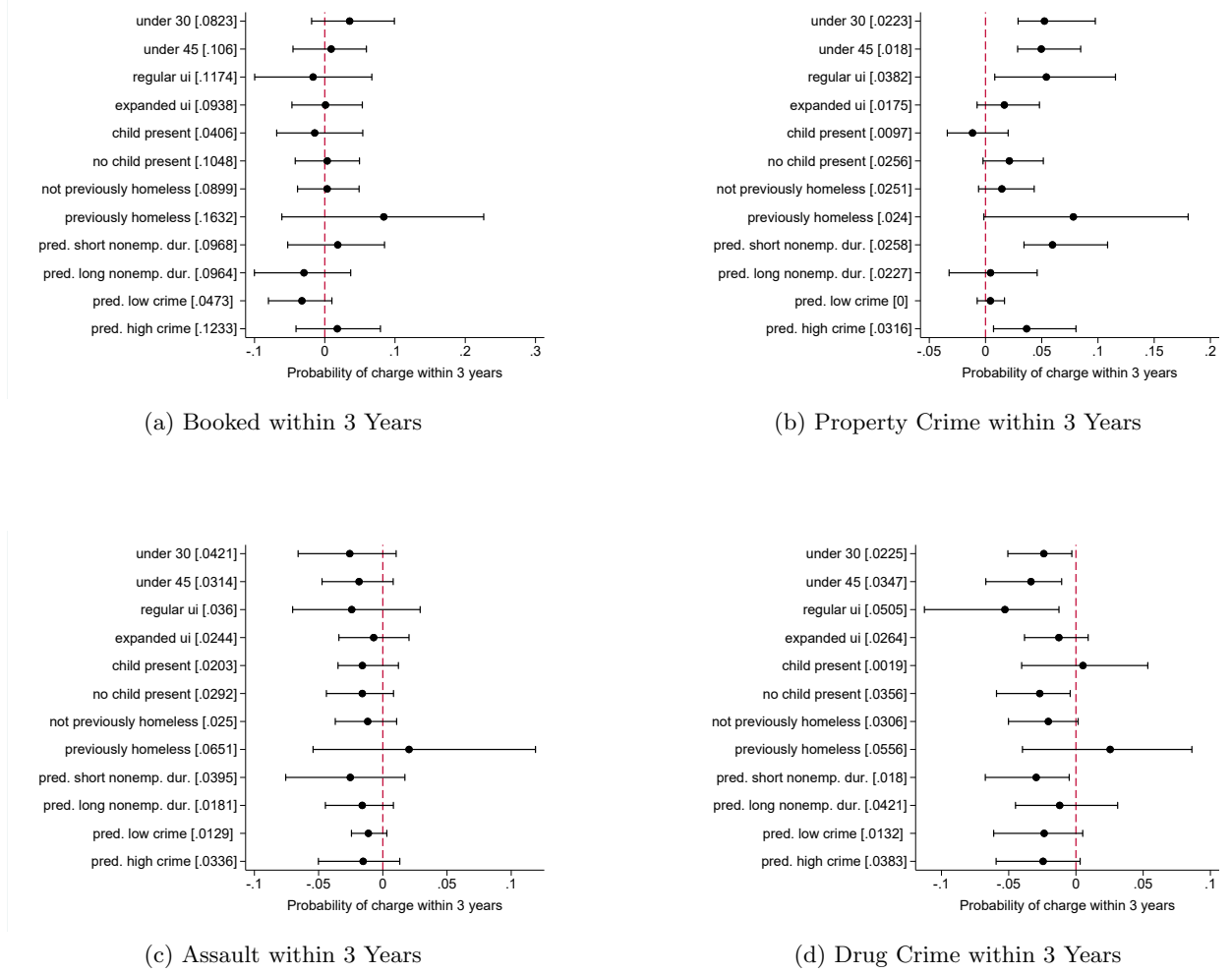
(a) Booked within 3 Years

(b) Property Crime within 3 Years

(c) Assault within 3 Years

(d) Drug Crime within 3 Years

Notes: This figure plots RDD point estimates and the bias-corrected confidence intervals for being booked within three years (Panel A), being charged with a property crime within three years (Panel B), being charged with an assault within three years (Panel C), and being charged with a drug crime (Panel D) for various subgroups. The subgroup is listed on the y-axis. The estimated intercept to the left of the threshold is displayed in brackets. Regular UI refers to the period (before 4/25/2006 and after 6/24/2013) where the maximum available UI duration was 26 weeks. Expanded UI refers to the period (4/25/2006-6/24/2013) where UI duration was expanded beyond 26 weeks. Pred. short (long) nonemp. dur. refers to individuals whose predicted nonemployment duration using predetermined covariates is below (above) the median. Pred. high (low) crime refers to individuals whose predicted probability of booking within three years using predetermined covariates is above (below) the median.

Figure B3: Days Spent in DOC Jails within a Year
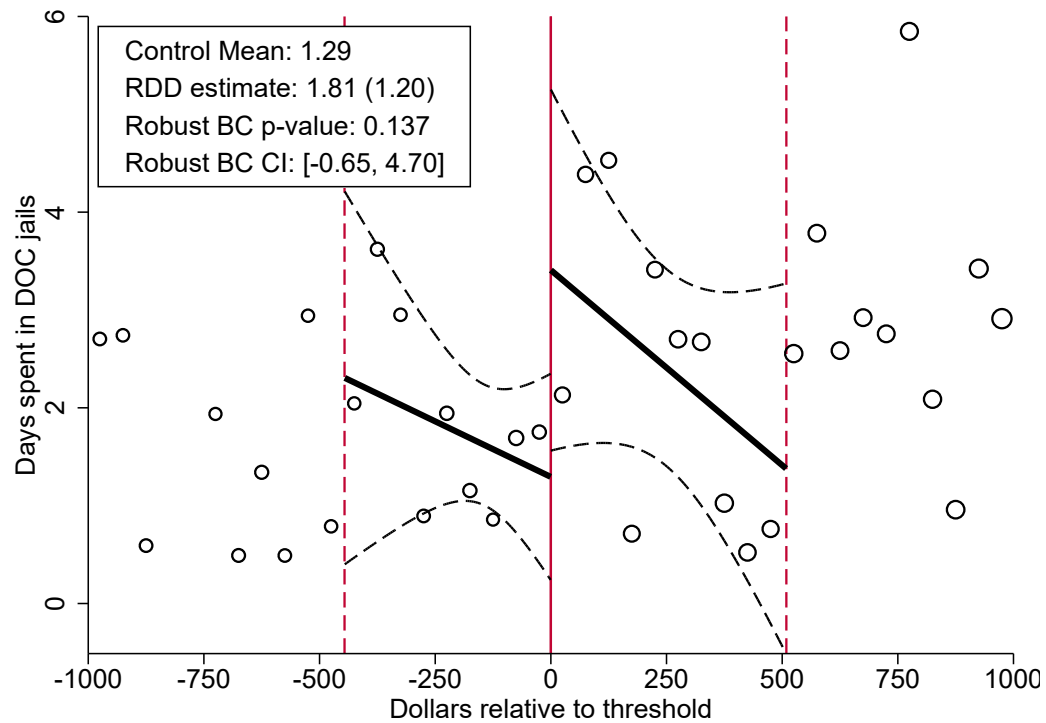
Notes: This figure plots the number of days claimant spends in DOC jails within one year after the UI claim. See Figure 6 for more information on figure construction. For precision, the RDD estimates displayed in the box include controls such as demographic information, education, prior earnings and employment, prior homelessness, zip code, date, and past DOC interaction.

C   Sample Construction

The composition of our sample is dictated, in part, by the initial use of this data, examining the effects of evictions (Collinson et al., 2024). I begin with administrative data on historical benefits receipt from New York City's Human Resources Administration (HRA). These records include individuals who received Medicaid, Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP; food stamps), or other city-specific cash subsidies between 2004 to 2016. This data does not capture Medicaid clients receiving Medicaid from the state Department of Health. From this point, there are two samples constructed. The first sample includes individuals in the HRA records that can be matched to the universe of housing court records in New York City from 2007-2016. These records are mostly eviction filings. Roughly 40 percent of these court records can be matched to HRA benefit receipt data. The second sample is a 14 percent random subsample of individuals who appear in the HRA benefits records but are not linked to housing court records.

By combining the these two samples, I am overweighting individuals who have an eviction filing linked to them. Since having an eviction filing is likely endogenously related to receiving UI, one concern is that we have endogenous selection into our sample. To address these concerns, throughout the paper I upweight the random of individuals who are not linked to the housing court by 1/0.14. This reweighting ensures that the main analysis sample is representative of claims filled by those who have historically received a means-tested benefit from New York City's HRA. Additionally, with this reweighting, being linked to a eviction filling does not impact the probability of being in the final sample.

The sample of UI claims used in this paper is all UI claims filled by this combined sample from 2004-2017. UI claims were linked to this combined sample by New York State Department of Labor using Social Security Numbers. UI claims are fuzzy-matched to the DOC using a combination of SSN (if available), name, and date of birth. Name and date of birth are obtained using the HRA means-tested benefits records. The fuzzy-match is preform with the "fastlink" package in R (Enamorado et al., 2019).