Essays in Law and Economics

by

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ABSTRACT

Chapter 1: This chapter uses theoretical and empirical methods to understand the most common reason employers report reluctance to hire workers with a criminal record: legal liability generated by the tort of negligent hiring. While the purpose of the tort is ostensibly to protect and make whole those harmed when an employee misbehaves in a foreseeable manner, I find that, in practice, the tort generates additional criminal behavior and worsens employment outcomes. I first provide a survey of the current doctrine across the states and trace the origins of the tort through the common law. Using a difference-in-differences strategy, I show that adoption of negligent hiring increased the number of property criminal offenses (7%). Next, I examine state legislation clarifying the negligent hiring standard and reducing the likelihood that an employer will be found liable. I use survey and administrative data from over a dozen states to document that the states that reformed their negligent hiring law increased employment for people with criminal records by 2-5 p.p. (5-9%) and lowered reincarceration for a new criminal offense by 2 p.p. (10%) relative to people in non-reforming states.[1]

Chapter 2: This chapter (with Michael Mueller-Smith and Caroline Walker)studies the impact of adult prosecution on recidivism and employment for adolescent, first-time felony defendants. Regression discontinuity evidence shows that, relative to juvenile prosecution, adult prosecution reduces criminal charges over 5 years by 0.48 felony cases (-20%) while worsening labor market outcomes: $674 fewer earnings (-21%) per year. We develop an econometric framework that incorporates predictive machine learning models to identify mechanism-specific treatment effects, which we use to evaluate four policy counterfactuals: (1) raising the age of majority, (2) increasing adult dismissals rates, (3) eliminating adult incarceration, and (4) expanding juvenile record sealing opportunities to teenage adults.[2]

Chapter 3: This chapter (written with Edward Fox) studies the impact of corporate tax cuts

[1]The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926 and CBDRB-FY23-P2295-R10669).

[2]Any opinions and conclusions expressed herein are those of the authors and do not reflect the views of the U.S. Census Bureau. The U.S. Census Bureau reviewed this data product for unauthorized disclosure of confidential information and approved the disclosure avoidance practices applied to this release (DMS number: 7512453, DRB approval numbers: #CBDRB-FY22-291 & #CBDRB-FY23-088 &CBDRB-FY23-0392, & CBDRB-FY23-0414).

on taxed and non-taxed entities competing in the same industry. The TCJA of 2017 made large changes to the taxation of corporate and pass-through businesses in the U.S. Understanding the effects of these changes is complicated by the difficulty of finding control firms whose taxation was not altered by the Act. We study the effect of the TCJA on small and medium size banks using credit unions—which compete with these banks for deposits and in making loans—as a novel control group. Credit unions were not taxed both before and after the Act. Using a difference-in-differences framework, we find that an important fraction of the incidence of the tax cut goes to depositors. We find little evidence that employees or borrowers from banks receive a share of the tax cut in the form of higher wages or lower interest rates on loans or that banks increase their investment in fixed assets as a result of the Act.

CHAPTER I

Negligent Hiring: Recidivism and Employment with a Criminal Record

1.1 Introduction

There is substantial evidence that employers are less willing to hire applicants with criminal records (Holzer et al., 1999; Society for Human Resource Management, 2018).[1] Research has demonstrated that decreased employment for this population generates worse outcomes for those who do not work, hampers national productivity, exacerbates racial income inequality, increases crime rates, and causes a host of other problems (Mueller-Smith and Schnepel, 2021; Abraham and Kearney, 2020; Schnepel, 2018). However, we know less about precisely why employers are less likely to hire from this pool of potential workers. This lack of knowledge makes improving employment opportunities for this population more difficult. This paper seeks to answer three major questions: 1) how much of the employment declines caused by a person having an observable criminal record are due to employer liability under state tort law for negligent hiring, 2) how did the adoption of tort liability for negligent hiring change criminal behavior, and 3) how did later statutory clarifications that reduced the risk of negligent hiring liability impact criminal and employment behavior? This paper presents evidence that the tort of negligent hiring ultimately results in more criminal behavior and explains a significant portion of the gap in earnings between those with and without a criminal record. I analyze employment and criminal behavioral changes around state-wide tort recognition as well as behavioral changes around a series of statutory changes that limit and clarify negligent hiring liability. It finds that lowered liability increases earnings and

[1]Thanks to Michael Mueller-Smith, Nina Mendelson, Rebecca Eisenberg, J.J. Prescott, Charlie Brown, Brian Jacobs, Hanna Hoover, James Reeves, Nikhil Rao, and participants at the Student Scholarship Workshop and Student Research Roundtable, and labor seminar participants at the University of Michigan for helpful feedback and suggestions. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

reduces recidivism for returning citizens impacted by the reforms.

The footprint the criminal justice system has on the labor market is substantial. Contact with the criminal justice system carries with it a host of collateral consequences beyond the punishment initially assigned by a court. While these consequences are sprawling and varied, this paper will focus on just one: the formal and informal hindrances to labor market opportunities. These consequences are not distributed equally but rather fall predominantly on Black men. In 2017, 622,400 prisoners were released from state and federal prisons (Bronson and Carson, 2019). Many of these returning citizens are of working age. Shannon et al. (2017) estimate that eight percent of all adults and thirty-three percent of the Black adult male population have a felony conviction. Given that similar collateral consequences can extend to individuals who have less severe contact with the criminal justice system (such as an arrest or a conviction without imprisonment), these percentages underestimate the impacted population. There are also likely important intergenerational consequences to having a parent or caregiver who has contact with the criminal justice system. Finlay et al. (2022b) show that nine percent of children with parental prison involvement, eighteen percent with a parent with a felony conviction, and thirty-nine percent with a criminal charge (sixty-two percent for Black children), consequences of the criminal justice system reach a vast proportion of the population.

People who have been criminally convicted are substantially more likely to be unemployed (Couloute and Kopf, 2018). Examining raw correlations between criminal justice exposure and employment outcomes does not tell us whether contact with the criminal justice system causes worse labor market outcomes. While more work needs to be done, there is substantial and growing evidence that criminal records cause a large earnings gap between those with and without criminal justice histories (Pager, 2003a, 2008; Pager et al., 2009; Agan and Starr, 2017; Leasure and Stevens Andersen, 2017; Leasure, 2018; Decker et al., 2015). Many of these studies use individual longitudinal surveys (analysis following this approach is presented in the appendix) to estimate employment, wage, and earnings gaps (Freeman, 1991; Grogger, 1992; Allgood et al., 1999; Western and Beckett, 1999; Western, 2002, 2006; Raphael, 2007; Richey, 2015). Other studies have used different methodologies and more comprehensive administrative data (Waldfogel, 1994; Grogger, 1995; Nagin and Waldfogel, 1998a; Lalonde and Cho, 2008; Kling, 2006; Pettit and Lyons, 2007; Harding et al., 2018; Mueller-Smith and Schnepel, 2021; Manudeep et al., 2020; Dobbie et al., 2018). For instance, Mueller-Smith and Schnepel (2021) find that avoiding a felony conviction causes recidivism rates to be halved, quarterly employment to increase by fifty-three percent (or eighteen percentage points), and quarterly earnings to grow by sixty-four percent. In other words, a person who just avoids a felony conviction (in this case due to a randomly assigned deferred adjudication) works almost two more years over the next ten years and earns about $60,000 more than if they had received a guilty verdict.

There are many reasons to care about the employment prospects for people with criminal histories, but one especially relevant for this paper is that better employment opportunities may lower recidivism (and criminal behavior generally). For instance, Yang (2017) found that a one percent increase in the average wages of non-college educated men in the county of release reduces the quarterly hazard rate by about one-half percent. Similar effects are found for those released into areas with higher employment growth. Together these results suggest that exiting prison in average labor market conditions (instead of a recession) lowers recidivism by almost seven percent. Many researchers have cited employment for people with criminal convictions as a critical component of reentering society, decreasing future offending and reliance on the social safety net (Raphael, 2007; Redcross et al., 2011).

While the evidence that exposure to the criminal justice system hurts employment prospects is substantial, less is known about the precise reasons for these gaps. Understanding what mechanisms are at work is essential to policymakers seeking to implement changes. Potential explanations are plentiful. For instance, employers might be engaging in statistical discrimination, exploiting differences in average productivity, defined in terms of both output and job turn-over, between individuals with and without criminal justice histories; that is, employers might believe workers with felony convictions perform worse on the job. Employers may have an aversion to working with ex-felons, or alternatively framed, a desire to punish individuals with a criminal record more heavily than the criminal justice system has done thus far. Licensing and other legal restrictions may formally prohibit employers from hiring individuals with felony histories for particular categories of work, effectively shutting off certain sectors of the economy to returning citizens. Employers may fear harm to their reputations from subsequent harmful actions by employees with criminal histories. Finally, employers may be reluctant to hire this population for fear of potential liability for an employee's harmful actions under the common law doctrines of *respondeat superior* and negligent hiring, regardless of the applicants' potential productivity.

The best available evidence on the mechanisms driving employer demand for workers with a criminal record is from Cullen et al. (2023) which uses experimental methods in the context of hiring workers with criminal records on a temporary worker staffing platform. This work finds an eleven percentage point increase in businesses willing to work with individuals with a criminal record when businesses are offered crime and safety insurance, a single performance review, wage subsidy, or a limited background check covering just the past year. I build on this work by using quasi-expirementation evidence across a broad array of employment relationships (including both temporary workers hired through application such as the Cullen et al. (2023) setting, but others as well) but focuses on the importance of the under-explored employer liability channel.

While little is known about the relative magnitude of the mechanisms that drive employer reluctance to hire from the returning citizen population, employers do self-report that the chief reason they inquire into applicants' criminal backgrounds is potential liability for employee actions. For instance, survey data shows that most organizations report that reducing legal liability for negligent hiring is the primary reason for running a background check (Society for Human Resource Management, 2018). Follow-up surveys have confirmed that the single most salient concern HR and business managers have about hiring workers with a criminal record is legal liability (Society for Human Resource Management, 2021). In addition, social scientists studying the impact of criminal records on employment have frequently suggested that negligent hiring is likely to reduce employment rates for people with criminal records significantly and is a good target for reform (Agan, 2017; Bushway and Kalra, 2021; McElhattan, 2022).

Unfortunately, little data is available to study the frequency and size of negligent hiring suits, so the employer survey is the best evidence on the subject to date. Studies focused on written opinions available via traditional legal research aggregators like Westlaw or Lexis are unlikely to generate accurate measures of the risk of potential litigation and how it has evolved (Boyd et al., 2020). Using data from aggregators is an especially poor measure in this setting because many service-based companies have varied their policies regarding compelled arbitration and non-disclosures in negligent hiring cases over time.[2] The most cited survey in this area suggest that employers lose 72% of negligent hiring cases with an average settlement of more than $1.6 million dollars; another survey suggests a lower employer loss rate of 66% and damages averaging over $600 thousand (Minor et al., 2018). However, there have been some large and well-publicized judgments, suggesting that an employer's fear of negligent hiring liability may be associated with real costs and generate behavioral changes. For instance, news coverage of Wal-mart's adoption of wide-spread background checks ties the decision to contemperaneous negligent hiring cases against the company (Zimmerman, 2004).

The remainder of the paper studies the relationship between the evolution of negligent hiring doctrine, employment, recidivism, and reported criminal offenses. Section 2 provides relevant background on the tort of negligent hiring. Section 3 frames negligent hiring liability to a stylized contracting model in a limited liability framework. Section 4 presents evidence connecting changes in negligent hiring liability to labor market outcomes and criminal records. Finally, section 5 provides additional context regarding how negligent hiring can relate to other policies and concludes.

1.2 Background on employer liability

What gives rise to employer liability for worker behavior? The two most common sources of employer liability in this context (both of which are common law doctrines and thus vary by state)

[2]For instance, see Mulvaney (2020) which details that Uber "previously ended its mandatory arbitration program for assault victims."

are *respondeat superior* and negligent hiring. The doctrine of negligent hiring seeks to encourage employers to fill job openings with appropriate employees and independent contractors. In contrast, the doctrine of *respondeat superior* imposes liability on the employer for the torts of its employees based on the understanding that the worker acts on behalf of the employer. While *respondeat superior* may be a worry for some employers, it only applies to torts committed 1) by their employees, 2) for actions in the course of employment, and 3) regardless of an employer's fault.[3]

In general, *respondeat superior* imposes vicarious liability on the employer for the torts of its employees without distinguishing between those with and without criminal histories. Thus, this channel of liability will only cause gaps in employment outcomes between those with and without criminal records to the extent that employers believe that criminal history is relevant information about a potential employee's propensity to incur civil liability through actions directly related to their employment. Whether workers with criminal histories are more likely to commit misconduct is an open question, as are employers' perceptions of these risks. Evidence from New Zealand suggests that, at least in that setting, employees with a criminal conviction before entering the workforce were less likely than other workers to fight or steal at work Roberts et al. (2007). Lundquist et al. (2018) looks at the performance of those with felony records and those without, finding that those with felony records in the U.S. military and finds that those with records are more attached to their jobs and perform better on several performance measures. In a study of over 10,000 workers in the U.S., Minor et al. (2018) find workers with criminal convictions in sales jobs had a somewhat elevated risk for job separation due to misconduct. In contrast, those in customer support jobs did not. The empirical results I present do not provide evidence regarding this source of liability.

The same conduct performed by two employees may generate differential employer liability if one employee has a criminal record while the other does not. Negligent hiring establishes direct liability of the employer for a wider array of employment arrangements and worker behavior than *respondeat superior*. For instance, recovery under *respondeat superior* is limited when a worker is an independent contractor, while recovery under negligent hiring is not. Additionally, intentional torts of the employee (such as an assault) are frequently excluded from *respondeat superior* because the employee misconduct was outside the scope of employment. But the employer may still be liable for its own negligence in hiring the employee in such a case, as the employer is deemed to

[3]An example here is helpful. Consider a pizza delivery driver employed full-time by a firm who negligently runs a red light, causing a traffic accident and injuring a bystander. Here, regardless of the driver's previous driving and employment record, the firm will be liable under *respondeat superior* for the injuries caused by the driver's negligence because it occurred in the course of his duties as a driver. Of course, such a firm might avoid hiring drivers with poor driving records to minimize its liability risk for future accidents. Still, the firm's liability for such accidents under *respondeat superior* does not turn on whether the firm behaved unreasonably in hiring its drivers. However, suppose the pizza driver was an independent contractor. Here, there is likely no liability on *respondeat superior*. However, the driver's history may well matter in determining employer liability for negligent hiring. If the driver had a history of DUIs and criminal traffic offenses at the time he was hired, then the defendant could bring this evidence to bear on the issue of whether the firm was negligent in hiring the driver to deliver pizza

have failed to exercise reasonable care towards the victim by hiring an employee who committed a second assault. States have occasionally attempted to place statutory limits on what criminal records a plaintiff can introduce as evidence in negligent hiring cases. These statutes require the records to be of the same type of misconduct as the misconduct in the current case. It is worth noting that this type of legislation, as well as case law making a similar argument, is typically relying on an implicit premise of crime-type specialization (that people who are convicted of a particular type of crime are more likely to repeat the behavior alleged in the crime). While still an open question some of the best evidence suggests limited specialization within type of crime (Shen et al., 2020; Bushway and Kalra, 2021). However, more often than not, the question of how much evidentiary value a specific criminal record has is often left up to the jury.

A recent review of the case law concluded that "[s]tate courts are inconsistent at best in applying these general standards of liability to employers who have hired dangerous employees. . . . [and] the inconsistencies across states are even greater." Succinctly, "the law in this area is not clearly defined and is highly dependent on the individual facts of the case." (Hickox, 2010) Several representative cases are discussed in A.1.1 and additional details on how different courts conceptualize the tort are provided in A.1.2. Additionally, employers have difficulty insuring against negligent hiring liability. There has been substantial ambiguity over whether negligent hiring is covered under general liability insurance, as most policies exclude intentional acts Martin (2002). When negligent hiring is insurable, it tends to be limited, expensive, and infrequently used; it is most commonly excluded from policies (Pager and Western, 2009).[4]

In sum, it is difficult to discern a clear pattern to these decisions beyond the feature that criminal records increase liability risk, and therefore difficult for employers to predict their exposure to negligent hiring liability when they hire employees with criminal records.

1.2.1 Changes to liability

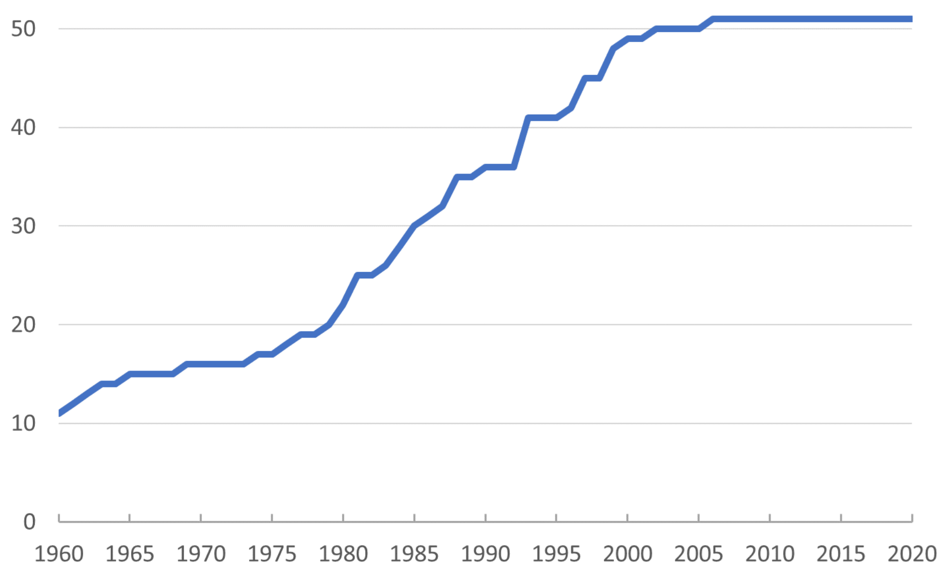
This paper studies two changes to negligent hiring liability: (1) judicial adoption through common law of negligent hiring liability in states ("negligent hiring recognition") and (2) recent laws passed by several state legislatures aimed at limiting employer liability and clarifying what criminal record evidence plaintiffs can bring in negligent hiring claims ("negligent hiring reform").

In general, how courts judge what amounts to negligent hiring varies widely across states, making it a bit challenging to identify precisely when such a cause of action emerged. Negligent hiring has been recognized broadly across the states (Vance, 2014). Table A.1 provides a systematic survey the case-law across the United States, coding when the highest court in that jurisdiction

[4] A representative quote from Pager and Western (2009) illustrates that "The manager of a courier company, discussing his reluctance to hire anyone with a history of violent crime, touched on similar themes, though couched as an insurance concern: It's an insurance problem. I can't, I can't get insurance coverage. . .")

formally recognizes liability for negligent hiring. Figure 1.1 plots the number of states that have recognized the tort by each year.

Figure 1.1: Jurisdictions Recognizing Negligent Hiring Cause of Action



Source: Review of published cases in Westlaw and Lexis.

While some states like Massachusetts and Indiana recognized negligent hiring as a cause of action in the early 1900s, the tort did not emerge to prominence in other states, like South Carolina, Wisconsin, and South Dakota, until nearly a century later. For many states, recognition occurred in the mid-1980s to 1990s, coinciding with the rise in mass incarceration. Between 1980 and 2000, the state and federal prison population increased from 315,974 inmates to 1,331,278, and about half of the states formally recognized the tort of negligent hiring-generating increased liability for the potential employers of the quickly growing released population.

Recent legislation has lowered employer liability and provided guidance for what factors should determine negligent hiring liability. Colorado, Texas, Minnesota, New York, New Jersey, Louisiana, D.C, Indiana, Arizona, and Iowa have all clarified how an employee's criminal record should be considered in assessing negligent hiring liability. Legislation is currently under consideration in Illinois and has been previously proposed in Arkansas. These bills were popular in the state legislatures, generally enjoying near-unanimous support.[5]

While these bills have some variations in their approaches and precise limitations to employer

[5]A list of the bills that states have passed to limit employer liability follows Colorado, 2005 (House Bill 10-1023), Texas, 2013 (H.B. 1188), Minnesota, 2009 (Statute Â§181.981), New York, 2008 (N.Y. EXEC. L. Â§ 296), Louisiana, 2015 (HOUSE BILL NO. 505), D.C. Re-entry Act of 2012, New Jersey, 2015 (A-1999) Indiana, 2017 (Indiana SB 312, signed as Public Law 210), Arizona, 2018 (H.B. 2311), and Iowa 2019 (Iowa HF 650).

liability, they share certain common features. They do not remove employer liability for crimes that are directly associated with previous offenses, but they do restrict the use of criminal histories more generally and/or raise the standard to gross negligence. The statutes set a standard for admissibility of evidence of a criminal record that requires the historical criminal behavior to be more closely related to the offense than was permitted under the common law standard before the legislation. Many states provide more guidance regarding how closely related the previous conviction must have been to the current offense for the harm to be considered foreseeable. Prior convictions that do not meet this standard are then excluded from evidence.

The Texas bill provides a representative example: "House Bill 1188 amends the Civil Practice and Remedies Code to prohibit a cause of action from being brought against an employer, general contractor, premises owner, or other third party solely for negligently hiring or failing to adequately supervise an employee, based on evidence that the employee has been convicted of an offense." The bill provides an exception "when (2) the employee was convicted of: (A) an offense that was committed while performing duties substantially similar to those reasonably expected to be performed in the employment, or under conditions substantially similar to those reasonably expected to be encountered in the employment, taking into consideration the factors listed in Sections 53.022 and 53.023(a) , Occupations Code, without regard to whether the occupation requires a license; (B) an offense listed in Section 3g, Article 42.12, Code of Criminal Procedure; or (C) a sexually violent offense, as defined by Article 62.001, Code of Criminal Procedure." The discussions surrounding these bills explicitly recognize the trade-offs discussed above and indicate that the state legislatures were attempting to lower employer liability. The ultimate goal was to improve access to labor markets for released individuals and decrease recidivism. Consistent with this goal, the bills almost invariably contain language such as "this section does not create a cause of action" and "the protections provided by this bill to employers." These legislative efforts also work to increase certainty.[6]

1.3 Economic model of negligent hiring

There are many overlapping groups potentially impacted by the imposition of the tort: (1) employers and consumers, (2) victims of the harm caused by negligently hiring, (3) victims harmed by people who cannot get jobs because negligent hiring liability limits their employment prospects,

[6]These bills also seek to provide additional guidance regarding when plaintiffs can introduce criminal records as evidence of negligent hiring. For instance, Texas House Bill 1188 Sections 53.022 and 53.023(a) explicitly list the factors relevant to evaluating proximate cause. These factors largely mirror guidance from the EEOC. They include the nature and seriousness of the crime, the extent to which employment might offer an opportunity to engage in further criminal activity of the type previously committed, the extent and nature of the person's past criminal activity, the age of the person when the crime was committed, and the amount of time that has elapsed since the person's last criminal activity.

and (4) job applicants with criminal records. Note that while these are helpful conceptual categories, they are not mutually exclusive.

Additional negligent hiring liability imposes additional costs on employers, including more protracted and costlier searches as employers conduct additional screening and hire a smaller fraction of job applicants. Additionally, employers sued for negligent hiring will bear additional legal costs regardless of whether they win or lose. These costs will ultimately be shared with consumers (although the incidence of this cost will depend on relative supply and demand elasticities).

A marginal expansion of employer liability will have two effects on potential victims. First, it will allow other injured parties compensation for their injuries. They would otherwise not have viable paths to recovery against judgment-proof employees. Second, because employers behave more carefully in their hiring practices, there may be fewer potentially actionable behaviors from employees, thus lowering the number of victims. Whether there will be more or fewer successful negligent hiring claims after expanding employer liability depends on which of these effects is larger.

As discussed above, the lack of employment opportunities causes an increase in criminal activity, especially for those with a history of criminal behavior. An increase in negligent hiring liability makes employers less likely to hire folks with criminal records, thus decreasing the employment opportunities for applicants with criminal records and thereby increasing recidivism and tortious behavior.

Suppose employers cannot perfectly identify the applicants who will commit misconduct for which they will be responsible as de facto insurers via the negligent hiring tort. In that case, a rational employer will form an estimate of their expected liability from hiring the applicant. This estimate will be based on the perceived compounded probabilities that an employee will offend while employed and that the employer will be found liable for the offense, scaled by the average cost of the negligent hiring settlement or judgment. Both of the estimated probabilities will likely increase with the length and severity of an applicant's criminal record since employers believe that juries and courts are more likely to find an employer negligent when the employee has a longer record. As employer estimates of perceived liability or uncertainty of liability increase, employers may be less willing to hire individuals with criminal histories for fear of later being found negligent, even if they personally believe that such a hire was not negligent at the time of hiring.

Translating the impacts across these groups to aggregate behavioral changes is challenging. Additional employer negligent hiring risk may increase or decrease the number of total offenses. Employers will take more precautions when they think they are more likely to be found liable. This suggests there will be fewer offenses by employees. But if the employer chooses not to hire an applicant because of his criminal record, the applicant does not disappear. Instead, the applicant may remain unemployed and thus be at more risk of offending. If the decreased rate of offending related to negligent hiring is less than the increased offending rate because of unemployment, the absolute number of offenses will increase.

Of course, we do not only care about the gross number of offenses but also about the harm they generate. Altering negligent hiring liability may influence the frequency of criminal behavior and the nature of offenses committed. Being employed may change the type of criminal opportunities available to a person. For instance, employment in a customer-facing job may increase the number of person-to-person interactions. Perhaps someone not hired due to an increase in negligent hiring liability would have committed an assault on the job but instead committed a burglary off the job. Certain employment may place workers inside other people's homes or in charge of supervising other people's possession. The subsequent analysis will be able to speak to these changes in behavior. However, other differences are more difficult to measure. For instance, perhaps being harmed by an employee who is implicitly in a position of trust is inherently more damaging than being injured in other contexts. The type of person who is the victim of the offense is also likely to change. Negligent hiring may provide deeper pockets for victims of torts, providing a compensation function.

Employers may provide some benefit to society by reducing criminal offenses generated by unemployment and transferring wealth to victims of offenses who are dealt an unexpected painful life event. This externality could be solved by providing a separate incentive to hire workers with a criminal record so that employers are fully compensated for the benefit they provide in preventing other offenses. Alternatively, the state could accomplish this insurance function through victim funds or transfer programs. State insurance may also have the attractive feature of fully compensating all injured parties (those harmed by unemployed tortfeasors), rather than just those who happened to be harmed by employed tortfeasors.

1.3.1 Limited Liability Framework

In this subsection I present a toy contracting model to help provide more structure and intuition for the problem. I model the problem as a contracting model between the employer and employee, with observable "effort," (where more effort is associated with less criminal behavior) however, the agent has limited liability. This alters and builds on earlier work applying such modeling of vicarious liability and principal-agent contracting Bisso and Choi (2008). I abstract away from formally modeling the value of insurance by simply leaving generic harm functions from each category of offense and leave further exploration this element for later work (one could also justify such an abstraction by noting that if the policy preference is for insuring victims, the current structure could be replaced with a government payout system funded by general taxes that does not generate the behavioral distortions displayed below).

Abstracting to a single agent (worker with a criminal record) and a single employer (principal). Call the worker's utility function $H(w, e) = U(w) - g(e)$, where w is the wage earned, e is the action/effort exerted by the worker, $U(\cdot)$ is a concave utility function, $g(\cdot)$ is a convex effort cost and $g(e_L) = 0$, ψ is the outside option, and π is output/revenue. Output is a function of both effort and a random term, θ, $\pi(e, \theta)$. Applying some additional structure to illustrate the mechanisms at play, let there be two possible effort values $a \in \{e_L, e_H\}$. When the worker chooses e_L, they commit a criminal offense while employed with a higher probability (they spend some of their time shirking in the illicit market). For convenience, assume two possible output values $\pi \in \{\pi_L, \pi_H\}$ where $F(x|e_H) = \pi_H$ with probability p_H and $F(x|e_L) = \pi_H$ with probability p_L and $F(x|e_L) = \pi_L$ with probability $1 - p_L$. Further assume $p_H > p_L$, that is, when the agent exerts more effort at work, they are less likely to offend. $\pi = \pi_H$ represents an employment relationship that ends without a negligent hiring payout, while $\pi = \pi_L$ represents the firm being found liable and can be written as as $\pi_L = \pi_H - N$ where N represents the total cost to the firm of a negligent hiring case. I also assume that the firm can choose a payout of 0 if no contract is formed.

The contract proceeds in the following order. First the firm offers a contract s. The agent accepts or rejects the contract. A rejection gives outside utility $\bar{u}$. If the agent accepts he chooses effort a. Nature draws θ, determining $x(a, \theta)$ and the worker receives payment $s(\pi)$

In this case we are working in a limited liability environment, meaning that the firm is unable to privately punish the worker beyond what the criminal justice and tort system already do. This means that $s(x) \geq L$, where L is the limited liability constraint. In this case we set $s(x) = \{w_H, w_l\}$

$$\begin{aligned}
\max_{z, w_L, w_H} \; & \{z\{p_H(\pi_H - w_H) + (1 - p_H)(\pi_H - N - w_L)\} \\
& +(1 - z)\{p_L(\pi_H - w_H) + (1 - p_L)(\pi_H - N - w_L)\}\} \qquad s.t. \\
& p_H w_H + (1 - p_H)w_L - g(e_H) \geq \psi \qquad \text{(IR)} \\
& p_H w_H + (1 - p_H)w_L - g(e_H) \geq p_L w_H + (1 - p_L)w_L \qquad \text{(IC)} \\
& w_L, w_H, \geq L \qquad \text{(LL)}
\end{aligned}$$

From this contracting problem, we can generate the following conditions (assuming the limit to liability binds). If the firm chooses to hire, but does not attempt to generate high effort ($z = 0$), say by choosing to screen and monitor (i.e. to simply accept liability), the wage offer will be $w_L, w_H = \psi$. The worker will choose any new offenses that comes along such that $\psi \leq j(Y+L)+(1-j)(Y+L-f)$ where j is the probability of offending without being caught, Y is the income from criminal activity, f is the criminally imposed penalty, all of which can be interpreted as functions of θ (drawn by nature), and there is no civil penalty imposed on the worker due to the limited liability constraint. Assuming some arbitrary, unobserved probability distribution over Y generates some probability of offenses which will be deemed o_n. This can then be mapped back into the θ term described above, and the firm will be found liable in $o_n * (1 - j)$ cases where it hires carelessly. Further assume that the harm caused by activity Y to the victim is $H(Y) > Y$ and $H'(Y) > 0$. If the liability system fully compensates victims, $N = H(\cdot)$.

The profit from a potentially negligent hire is $p_L(\pi_H - \psi) + (1 - p_L)(\pi_H - N - \psi)$ which simplifies to $\pi_H - N - \psi + P_L N$. If, however, $\pi_H - N - \psi + P_L N < 0$, then it will be unprofitable for the firm to hire the applicant without care. In this case the offense condition is $0 < j(Y') + (1 - j)(Y' - f)$, where $0 < \psi$. Here the opportunities to offend are allowed to vary based on whether or not the agent has been employed. Again, assuming some arbitrary, unobserved probability distribution over Y' generates some probability of offenses which will be deemed o_u. Here Y' is allowed to follow a different distribution than Y. If, for instance, the crimes available to an employed agent are more profitable than one might impose first order stochastic dominance of Y over Y'. Because little is known about these distributions, they are left in general terms here.

In order for the firm to satisfy the constrained optimization problem above and induce effort, $z = 1$ (i.e., if it wants to supervise/screen its hires), the firm will choose $w_L = L$ and $w_H = L + g(e_H)/(p_H - p_L)$. This means that in expectation the worker receives $p_H w_H + (1 - p_H) w_L = L + p_L g(e_H)/(p_H - p_L)$. In this case the offense condition is $j(L + g(e_H)/(p_H - p_L)) + (1 - j)L < j(Y) + (1 - j)(Y - f)$. For completeness, assume a similar offense function in the monitored problem, with offense $\hat{Y}$ at rate o_s (one could model the supervision as shifting the distribution of $\hat{Y}$ lower or as a shift to o_s).

The firm profit if non-negligent hire is $p_H(\pi_H - L - g(e_H)/(p_H - p_L)) + (1 - p_H)(\pi_H - N - L)$ which simplifies to $g(e_H)/(p_H - p_L) + \pi_H - N - L + P_H N$. Thus the firm will only find it profitable to do so if the following inequality holds: $g(e_H)/(p_H - p_L) + \pi_H - N - L + P_H N > 0$. When considering this option compared to a negligent hire, the following conditions must hold for the firm to prefer to hire more carefully: if $p_H < 1$ and $\frac{-P_H g(e_H)}{(P_H - P_L)(P_L - P_H)} + \frac{\psi - L}{P_L - P_H} \leq N \leq \frac{\pi_H - L}{1 - p_H} - \frac{P_H g(e_H)}{P_H - P_L}$ and if $p_H = 1$, then the same lower bound holds for N but $\pi_H \geq \frac{-g(e_H)}{1 - p_L} + L$. Thus by increasing N, or the liability a firm faces for its employees offending, the firm is more likely to choose the monitoring contract. However, as N increases less contracts are struck since the required revenue generated by a firm match is higher. Assuming match revenue is distributed across randomly across applicants, there will also be some distribution across contract types: label S_u the share who are unemployed, S_n the share in potential negligent matches, and S_s the share in non-negligent matches.

How much harm is being generated by these offenses? In the negligent contract some fraction o_n offend with harm $H(Y)$, so offense harm is $o_n H(Y)$. In the unemployment contract it is $o_n H(Y')$, and in the supervised scenario $o_s H(\hat{Y}) < o_n H(Y')$ (in other words if firms were constrained to hire everyone, imposing negligence liability reduces the harm of offenses). Increasing N or decreasing p_L, increases the proportion of contracts that fall into either the unemployed or supervised contract structure and decreases the number of contracts in the negligent bucket, i.e. $S_s < S_s^*; S_u < S_u^*; S_n > S_n^*$. The change in harm is thus $(S_s - S_s^*) * o_s H(\hat{Y}) + (S_u - S_u^*) o_u H(Y') + (S_n - S_n^*) o_n H(Y)$, or a shift in weighting of the average harm dealt by each category of contract. The first two of these terms are negative, while the last is positive; thus the amount of harm is ambiguously signed and is dependent on the relative shifts in shares and the distribution of Y, $\hat{Y}$ and Y' (something that can be studied by examining the composition of criminal behavior after a change in N). In the simple model a shift into the supervised category unambiguously lowers recidivism rates. However, whether a shift from negligent hiring to unemployment increases recidivism rates depends on whether $o_u > o_n$. If, after N increases recidivism increases, then the share of unemployed must offend more frequently than those who are negligently employed by an amount that is greater than the reduction of recidivism rates driven by the lower recidivism rates from supervised employees.[7]

The next section of this paper will address these empirical question directly by examining the impact on recidivism and employment outcomes in a number of states which have changed the standard for negligent hiring over time. It focuses on reforms reducing p_0 and measures outcomes by looking at responses in offense rates $\sum_{i=u,n,s} O_i$ and attempts to proxy for changes to $\sum H(\cdot)$ by studying changes to the composition of offense type.

1.4 Evidence on the Impact of Changes to Negligent Hiring Liability

The theoretical concerns laid out above suggest that changes to negligent hiring liability will impact the number and type of criminal offenses as well as a host of other labor market outcomes. This section analyzes the impact of statutory changes to negligent hiring. In particular, it will focus on ten states that have passed statutes that limit employer liability for hiring individuals with criminal records. It will also study the impact of tort recognition (increasing risk of liability) on offending.

While these bills all aim to reduce and/or clarify employer liability for the tort of negligent hiring, they do not remove liability entirely (in terms of the model, this is akin to lowering p_L but not setting it to 0). These legislative acts do not remove liability for crimes that are directly associated with previous offenses but do restrict the introduction of criminal records in negligent hiring cases. This restriction is consistent with legislatures wanting to encourage employers to hire workers with criminal records but not to provide these new hires with additional criminal opportunities.

The following subsections suggest that individuals with a criminal record are more likely to be employed in states after they have enacted negligent hiring reform. There are lower new-crime recidivism rates after lowering negligent hiring liability, especially from the groups of released individuals most likely to be impacted by the reforms. There are, if anything, fewer criminal offenses after a state passes negligent hiring reform and more criminal offenses after a state recognizes the tort. Employment opportunities for people with criminal records are a plausible causal channel

[7]Notably absent from this simple model are welfare considerations generated by the transfer payment acting as insurance to harmed third parties. This is akin to assuming risk-neutral third parties, but is an assumption that should be studied further.

through which these reforms lower recidivism.

1.4.1 The Impact of Negligent Hiring Reform on Labor Market Outcomes

Data linking criminal justice exposure and labor market outcomes has been a challenge in this literature, although substantial efforts are being made to improve the state of data availability. Existing studies that study the interplay between labor markets and criminal activity has relied on either 1) administrative records from one or two jurisdictions or 2) survey data with broader geographic coverage but a small sample of respondents with criminal histories. However, improvements to criminal justice data in recent years allow the following analysis to combine administrative court and prison records covering nearly half of the U.S. population with Census survey data.

Criminal histories are measured using the Criminal Justice Administrative Records System (CJARS), which compiles and harmonizes criminal justice records from many jurisdictions and agencies and I match this admistrative criminal justice data with a rich set of socio-economic data from the American Community Survey (ACS)(Finlay and Mueller-Smith, 2021). This paper focuses oncriminal, classified by type (e.g., property, drug, or violent) and gravity (e.g., misdemeanor or felony) and incarceration data from prison records.

Although CJARS offers massive improvements over previously available data, it does not cover all jurisdictions of interest over all relevant times. As CJARS continues to expand its data holdings, follow-up analysis can be conducted to expand the sample of both treated and control states. This follow-up work is important, especially given the relatively low number of treated jurisdictions available for study.[8] To generate estimates over comparable samples, I divide the data in to three related samples and present an analysis over each sample. First, the "prison sample" includes 11 states with sufficient data on prison entries and exits to construct criminal histories over the analysis sample (2005-2019): Arizona, Colorado, Florida, Illinois, Michigan, Nebraska, North Carolina, Pennsylvania, Texas, Washington, and Wisconsin (with Arizona, Colorado, and Texas enacting reforms). An alternative "court sample" includes all states with sufficient adjudication records: Arizona, Florida, Maryland, Michigan, New Jersey, North Carolina, North Dakota, Oregon, Wisconsin, and Texas (with Arizona, New Jersey, and Texas enacting reforms). A final "pooled sample" is the union of these two sets composed of 14 states and 4 adopting states. The pooled sample has the advantage of a larger sample but potentially less comparable criminal record coverage.

In order to make these results as comparable as possible to the literature, I make the following sample restrictions. First, I keep only the states with CJARS coverage (listed above). I also restrict to U.S. citizens between the ages of 25 and 64 who are Black, white, and/or Hispanic.[9] In these

[8]Another potential limitation of this approach is that respondents to the ACS may be representative of people with criminal records. Future work in this area would benefit on focusing on administrative records of earnings, especially in the panel setting to address both the representativeness of the sample and allow for within individual identification.

[9]This allows the analysis to avoid questions of continuing education and retirement, as well as for easier comparison

states, respondents with and without a criminal record are used in the analysis (although it is unlikely that the tort reforms will significantly impact employment rates for the population without criminal records).

Merging the ACS and CJARS allows for the study of the full population as well as people with criminal justice involvement. I use a rich set of information collected by the ACS including self-reported race/ethnicity categories, age, years of education, whether a person is currently enrolled in school, the Core-based Statistical Areas (CBSAs) of residency, whether the person is working, the person's yearly earnings, whether or not the are working in the same state as their residence, whether they have moved states in the last year, and date of interview. Linking this information to CJARS allows me to construct information about a respondents criminal record. I code the relevant criminal record as present if the relevant condition is met (a prison sentence, a conviction of a certain type) prior to the ACS interview date. I also code the amount of time that has passed since first criminal-justice exposure as a categorical variable taking different values if the person has no criminal record, had their first event within the past year, had their first event between 1 and 3 years, had their first event between 3 and 5 years, or had their first event longer in the past.

The model unambiguously predicts that employment for workers with criminal records will increase when employer liability for their future actions decreases. To test whether this occurs, I use difference-in-differences and event studies to compare the outcomes for people with criminal records in states after the states enacted negligent hiring reform to similarly situated people in states that did not reform the tort. For this approach to measure the causal impact of negligent hiring reform, I need to assume that people with criminal records in reformed and non-reformed states would have had similar employment trajectories absent the reform. One way to build confidence in this assumption is to show that prior to a liability change, the difference in labor market outcomes between treated and untreated states does not follow a clear trend and is near 0. To evaluate this assumption and generate estimates of the impact of the reform, I estimate the equation below, which parallels other works in the literature (Doleac and Hansen, 2020).

$$\begin{aligned} Outcome_i = \alpha + \beta_1 * \text{Reform}_{m,t} + \beta_2 * \text{Criminal Record} + \beta_3 * \text{Reform} \times \text{Criminal Record} \\ + \theta_D * \mathbf{D} + \lambda_{t\times\text{region}} + \delta_{CBSA} + \delta_{CBSA} \times t + \epsilon_i \quad (1.4.1) \end{aligned}$$

The subscript m indexes Core-based Statistical Areas (CBSAs), i indexes individuals, and t indexes months. From this, I estimate the impact of the reform on the employment outcome of interest for the whole population (β_1) and on the population of interest, people with criminal histories

to previous work such as Doleac and Hansen (2020). I code race/ethnicity as Black non-Hispanic, white non-Hispanic, or Hispanic.

(β_3), while controlling for a criminal record (β_2), a vector of individual characteristics D, including race/ethnicity categories, age fixed effects, fixed effects for years of education, and an indicator for whether the individual is currently enrolled in school, CBSA and regional fixed effects, and CBSA time-trends. The core specifications allow the various controls to vary by criminal record (interacting criminal record with the various controls, e.g. race, age, although state –rather than cbsa–, etc.), but the results are qualitatively quite similar if common controls (no criminal record by control interactions) are imposed (see Appendix Table A.4 for results on the pooled sample without this interaction).

The impact of negligent hiring reform likely varies by state and time (heterogeneous treatment effects), and states implemented the reform in a staggered manner. In the presence of these two features, a standard statistical approach, two-way fixed effects, will not yield estimates of the causal relationship of interest (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Gardner, 2022). To account for this, I follow Gardner (2022) and implement an imputation estimation procedure. This first stage is used to predict counterfactual outcomes in all periods and residualize the observed outcome. To do so, untreated (or not-yet-treated) observations are used to estimate each coefficient (except for the treatment). Then the residualized outcomes are regressed on negligent hiring reform-either indicators for years relative to reform enactment for the event studies or an indicator for before/after reform for the overall difference-in-differences estimate, and the standard errors are adjusted to account for the imputation. However, results are similar if estimated using a TWFE strategy or a stacked difference-in-difference strategy (Deshpande and Li, 2019; Cengiz et al., 2019).

Table 1.1 shows simple before and after summary statistics of reforming and non-reforming states, pre and post reform. Conditional on approval, this table will show the mean employment rates, education, race, age, migration in the last year, and out of state work. The effects of negligent hiring are hinted at in the raw data; increased employment rates for people with criminal records after these reforms.

Table 1.1: Summary statistics (Pooled Sample)

	Mean of people with a felony conviction or prisons stay:		
	Never reformed	Pre NH reform	Post NH reform
Employed	0.56	0.59	0.63
Black	0.24	0.24	0.22
Hispanic	0.06	0.29	0.31
Younger	0.24	0.27	0.22
Mid-age	0.57	0.58	0.55
< High School	0.2	0.24	0.21
< College	0.86	0.88	0.86
Out of state	0.02	0.01	0.01
Migrated last year	0.02	0.02	0.01
Employed - private	0.46	0.48	0.51
Employed - public	0.03	0.04	0.04
Employed - self	0.07	0.07	0.08
Count	208000	68500	48500

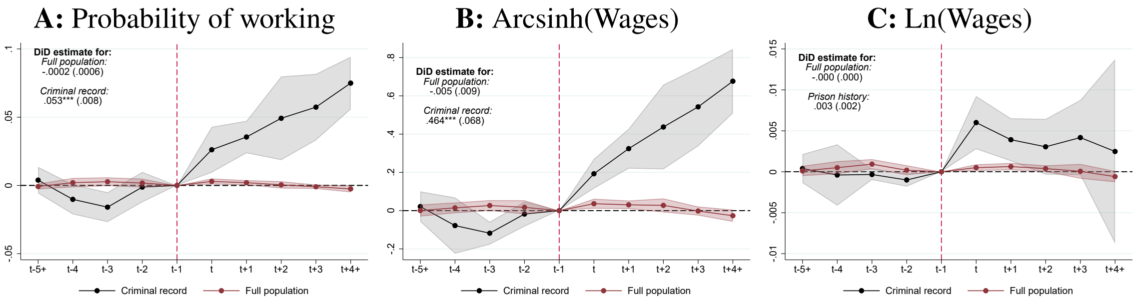
Source: ACS and CJARS (2020).

Notes: Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Figure 1.2 shows three event studies depicting the evolution of employment outcomes before and after negligent hiring liability is reformed in the prison sample. Three different outcomes are shown: in panel (A) the extensive margin (whether a worker worked in the past week) and in panel (B) the impact on both employment and earnings (the inverse hyperbolic sine transform of wage earnings), and (C) the intensive margin which shows the impact on log(wages) and restricts the sample to those who are working. These figures demonstrate that workers with criminal records had similar employment probabilities in states that would eventually reform negligent hiring and those that never reformed the tort. We verify this pattern by noting that the solid black line (which, in Panel A, displays the probability of working in reform states minus the probability of working in non-reform states) is near zero before reform implementation (denoted by t-1 and the vertical, red-dashed line). Before the reform, the 95% confidence interval (the gray shaded area) consistently includes 0 and does not exhibit any evidence of differing pre-trends. However, in states that reformed negligent hiring, people with criminal records are more likely to be employed and earn higher wages after the reform (the solid black line increases from t to the end of the sample). The impact of the reform can be seen immediately upon enactment and seems to grow over time, perhaps as employers gain greater knowledge of the law. While there appears to be some impact on the intensive margin, it is very modest and the results are primarily driven by the extensive margin.[10]

[10]Note there may be a composition question here. If the workers induced into the labor force by the reform are

Figure 1.2: Event Study - Negligent Hiring Reform and Employment (Pooled Sample)



Source: ACS and CJARS (2020).
Notes: Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

These results can also be translated to point estimates, as shown in Table 1.2, which considers the results for the prison sample. Panel A shows several estimation approaches focused on employment status over the past week as the outcome of interest. Panel B uses similar estimation approaches but focuses on a transformation of wage earnings (the inverse hyperbolic sine of wage earnings over the past year). Columns 1 and 2 show two-stage difference-in differences (2SDID) estimated over the full sample and two-way fixed-effects, respectively. Both approaches suggest that reforming negligent hiring liability had little impact on overall employment but significantly improved employment for workers with criminal records by between two and six percentage points. For context, in the sample considered, workers with prison records are about twenty-one percentage points less likely to be employed. This means that negligent hiring reform reduces the gap in employment rates between those with and without prison records by between ten and twenty-five percent. In 2008, the ACS slightly altered how the employment question was asked. Thus, some of the results could be influenced by changes in survey design. To address this technical measurement concern, columns 3 and 4 start the sample in 2008. The results are qualitatively similar when the starting year is varied. Finally, as discussed in additional depth later, a contemporaneous policy, Ban-the-Box, was often tied with negligent hiring reforms. The final columns control for Ban-the-Box adoption. Columns 5 and 6 suggest that controlling for Ban-the-Box legislation does not significantly change the estimated effect of negligent hiring reform. In panel B, the impact on wages is explored. Details regarding the impact of Ban-the-Box are discussed in A.3. All estimation samples show an increase in earnings by thirty-four to forty percent.

entering at lower wages, this would appear as more muted ln(earnings) changes.

Table 1.2: Negligent Hiring Reform on Labor Market Outcomes (Prison Sample)

a: Outcome - Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Negligent Hiring Reform	-0.002	-0.008	-0.002	-0.008	-0.001	-0.001***
	(0.001)	(0.005)	(0.001)	(0.005)	(0.001)	(0.002)
Reform x Criminal History	0.061***	0.041***	0.058***	0.039***	0.059***	0.019***
	(0.008)	(0.009)	(0.009)	(0.007)	(0.005)	(0.01)
Estimation	did2s	twfe	did2s	twfe	did2s	twfe
Sample	prison	prison	prison	prison	prison	prison
Start year	2005	2005	2005	2005	2008	2008
BTB Control	no	no	yes	yes	yes	yes
Obs	10940000	10940000	10940000	10940000	8841000	8841000

b: Outcome - Inverse Hyperbolic Sine (wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	-0.02	-0.04	-0.02	-0.043***
	(0.009)	(0.047)	(0.009)	(0.053)
Reform x Criminal History	0.518***	0.385***	0.521***	0.377***
	(0.151)	(0.072)	(0.164)	(0.059)
Estimation	did2s	twfe	did2s	twfe
Sample	prison	prison	prison	prison
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	10940000	10940000	10940000	10940000

c: Outcome - ln(wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	-0.003***	-0.001**	-0.001***	-0.001*
	(0.000)	(0.001)	(0.000)	(0.001)
Reform x Criminal History	0.007*	0.008***	0.007*	0.008***
	(0.004)	(0.001)	(0.004)	(0.001)
Estimation	did2s	twfe	did2s	twfe
Sample	court	court	court	court
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	8271000	8271000	8271000	8271000

Source: ACS and CJARS (2020).

Notes: State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Next, I consider how robust the results are to sample construction and look at the court sample for people with felony convictions in Table 1.3. While two things are changing between the two samples (felony convictions do not necessarily require a prison sentence and a different selection of states), we see that negligent hiring reform increases employment by a slightly lower four to five percentage points. However, people with criminal records are about seventeen percentage points less likely to be employed than workers without records. Here again, negligent hiring reform accounts for about twenty-five percent of the gap between workers with and without criminal records. These results are again robust to sample start year, estimation approach, and Ban-the-Box controls.

Table 1.3: Negligent Hiring Reform on Labor Market Outcomes (Court Sample)

a: Outcome - Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Negligent Hiring Reform	-0.001**	-0.011***	-0.001**	-0.011***	0	-0.001
	(0.001)	(0.004)	(0.001)	(0.004)	(0.000)	(0.002)
Reform x Criminal History	0.054***	0.045***	0.051***	0.043***	0.049***	0.022***
	(0.007)	(0.012)	(0.008)	(0.011)	(0.004)	(0.006)
Estimation	did2s	twfe	did2s	twfe	did2s	twfe
Sample	court	court	court	court	court	court
Start year	2005	2005	2005	2005	2008	2008
BTB Control	no	no	yes	yes	yes	yes
Obs	9575000	9575000	9575000	9575000	7728000	7728000

b: Outcome - Inverse Hyperbolic Sine (wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	-0.008	-0.075	-0.008	-0.077***
	(0.004)	(0.033)	(0.004)	(0.036)
Reform x Criminal History	0.498***	0.380***	0.476***	0.367***
	(0.06)	(0.082)	(0.064)	(0.07)
Estimation	did2s	twfe	did2s	twfe
Sample	court	court	court	court
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	9575000	9575000	9575000	9575000

c: Outcome - ln(wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0	-0.002**	0	-0.002**
	(0.000)	(0.001)	(0.000)	(0.001)
Reform x Criminal History	0.005***	0.007***	0.005***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Estimation	did2s	twfe	did2s	twfe
Sample	court	court	court	court
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	7235000	7235000	7235000	7235000

Source: ACS and CJARS (2020).

State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Finally, I pool the samples across states with either sufficient prison or court data in Table 1.4. Given the results of the prison sample and the felony conviction sample combining the two samples may yield similar results with more precision. While the results are broadly similar in the pooled sample, they are somewhat more variable ranging between 4 and 7 percentage points for employment and 35 to 50 percent increases in earnings. Here again, as shown by Panel C, the extensive margin is doing much of the work, as the ln(wage) has relatively modest response to negligent hiring reform. In Appendix Table A.3, I present a similar version merging any charge and prison.

Table 1.4: Negligent Hiring Reform on Labor Market Outcomes (Pooled Sample)
a: Outcome - Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Negligent Hiring Reform	0.000	-0.003	0.000	-0.003	0.000	0.002
	(0.001)	(0.004)	(0.001)	(0.004)	(0.000)	(0.002)
Reform x Criminal History	0.053***	0.042***	0.05***	0.089***	0.056***	0.09***
	(0.008)	(0.012)	(0.008)	(0.005)	(0.009)	(0.006)
Estimation	did2s	twfe	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005	2008	2008
BTB Control	no	no	yes	yes	yes	yes
Obs	12880000	12880000	12880000	12880000	10400000	10400000

b: Outcome - Inverse Hyperbolic Sine (wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0.005	-0.01	0.005	-0.01
	(0.009)	(0.038)	(0.009)	(0.042)
Reform x Criminal History	0.464***	0.355***	0.446***	0.342***
	(0.068)	(0.083)	(0.073)	(0.071)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	12880000	12880000	12880000	12880000

c: Panel C: Outcome - ln(wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0.000	-0.001	0.000	-0.001
	(0.000)	(0.001)	(0.000)	(0.001)
Reform x Criminal History	0.003	0.006***	0.003	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	9782000	9782000	9782000	9782000

Source: ACS and CJARS (2020).
State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

I repeat the analysis in Tables 1.2 and 1.4 using a stacked difference-in-difference strategy

(Deshpande and Li, 2019; Cengiz et al., 2019). To implement this I first construct several sets of clean experiments (a data set with one treated state and the set of untreated control states). I then stack each experiment into one large data set (including an indicator for each experiment). I fit the model to the stacked data interacting the coefficients with the sub-experiment indicator. Following Wing (2021), I then cluster the standard errors at the state level to account for duplicated observations. To account for the low number of treated clusters, I also take a randomization inference approach, following recent advances in the literature by Alvarez and Ferman (2023) and described in greater detail in A.1.3. Table 1.5 shows that the results from the stacked regressions are largely similar to the 2SDID and TWFE approaches. The improved inference from Alvarez and Ferman (2023) indicates that while the clustered standard errors were modestly too small, the results (except for one specification), remain significant at conventional levels.

Table 1.5: The Impact of Negligent Hiring Reform on Work (Stacked Regression)
a: Prison sample

	Outcome: Employment			Outcome: Arcsinh(Wage)	
	(1)	(2)	(3)	(4)	(5)
Negligent Hiring Reform	-0.009**	-0.01**	-0.002	-0.063*	-0.071*
	(0.004)	(0.004)	(0.002)	(0.036)	(0.04)
Reform x Criminal History	0.04***	0.039***	0.019*	0.385***	0.377***
	(0.009)	(0.009)	(0.011)	(0.082)	(0.074)
Alvarez/Ferman p	0.003	0.1	0.418	0.021	0.001
Estimation	stacked	stacked	stacked	stacked	stacked
Sample	prison	prison	prison	prison	prison
BTB Control	no	yes	yes	no	yes
Start Year	2005	2005	2008	2005	2005
Obs	10940000	10940000	8841000	10940000	10940000

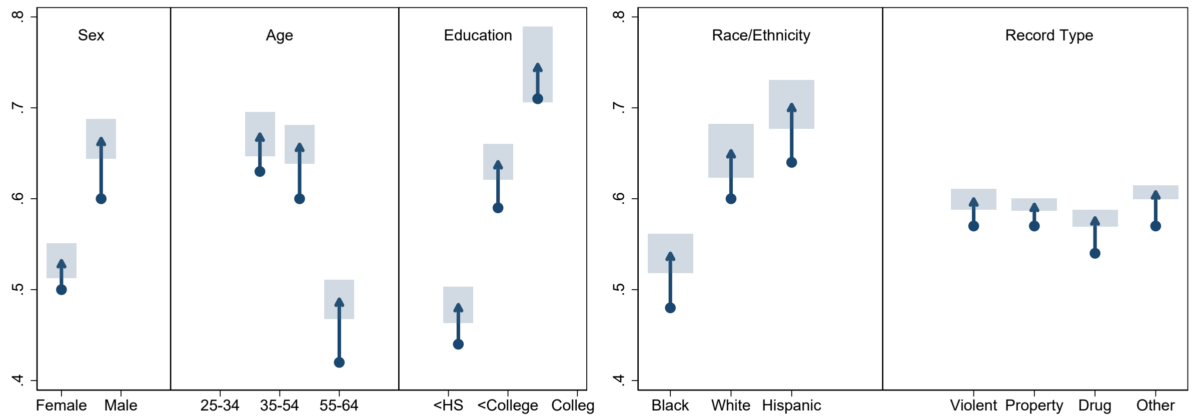
b: Pooled - Prison and Felony Sample

	Outcome: Employment			Outcome: Arcsinh(Wage)	
	(1)	(2)	(3)	(4)	(5)
Negligent Hiring Reform	-0.004	-0.004	0.001	-0.021	-0.024
	(0.004)	(0.004)	(0.003)	(0.037)	(0.038)
Reform x Criminal History	0.045***	0.045***	0.031*	0.39***	0.387***
	(0.014)	(0.014)	(0.006)	(0.068)	(0.065)
Alvarez/Ferman p	0.006	0.006	0.001	0.001	0.001
Estimation	stacked	stacked	stacked	stacked	stacked
Sample	pooled	pooled	pooled	pooled	pooled
BTB Control	no	yes	yes	no	yes
Start Year	2005	2005	2008	2005	2005
Obs	12880000	12880000	10400000	12880000	12880000

Source: ACS and CJARS (2020).
State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. P-values calculated using Alvarez and Ferman (2023) are also displayed. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Negligent hiring reform might impact different populations in different ways. The increase in employment for workers with criminal histories should correspond with the employer's perception of reduced risk. This perception is a function of the probability a potential employee offends on the job (which should remain constant before and after the reform), the harm generated by the offense (unchanged), and the probability the employer is found liable for the offense (changed). The last element (the likelihood of being held responsible) may vary depending on the nature of the potential employee's criminal record. Thus, negligent hiring reform may impact different groups to

Figure 1.3: Heterogeneous Employment Impact of Negligent Hiring Reform



Source: ACS and CJARS (2020).
Notes: Solid dots are group mean employment rates, arrows represent estimated effect size, and the shaded area are 95% confidence intervals. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

different degrees. Additionally, because the liability term is interacted with the expected amount of liability, the reform may have heterogeneous impacts based on employer perception of the perceived underlying risk of offending. I consider the effect of the reform on various subpopulations in Appendix Table A.5. To do so I run several specification where the treatment is allowed to vary by fixed characteristics (interacting the treatment and controls with the subgroups in question. Figure 1.3 shows the results of five different regression. Each dot shows the mean employment rate for each supgroup with a criminal record (so females with a criminal record are employed at a 50 percent rate and men at a 60 percent rate). The arrow and shaded areas are the point estimates and the 95% confidence intervals to document the impact of the reform across different groups (the β_3's).

This evidence suggests that the reform's effect on the probability of employment for people with a violent felony conviction, a property felony conviction, or a drug felony conviction. The reform has the largest effect on people with drug and public order/other felony convictions. The remaining columns show the varying impact of reform by sex, age, education, and race. Overall, these results suggest larger reform effects for workers with records that would be most likely to be disallowed under the reforms (older workers tend to have older records, and drug/public order offenses are less likely to be admissible evidence after reforms) or for workers employers perceive as more at risk to recidivate (male and less educated).

These results suggest that the reform increased employment most substantially for Black men who are somewhat older and less educated. For instance, the reform increases employment for people who are Black and have a felony or prison record by more than 12% (six percentage points) compared to about 8% (five percentage points) for white people with a similar criminal record.

Table 1.6 several other outcomes in the 2SDiD framework (all results are using the pooled sample from 2005-2019). Columns 1-3 include dependent variables that take the value of 1 if that respondent is both actively working and working for a private, public, or self employer respectively. As one might expect given the nature of the tort liability, the largest impacts are for private employers, with more muted impacts for public and self-employers.[11]

Table 1.6: Negligent Hiring Reform on Other Outcomes (Pooled Sample)

	Private employer (1)	Public employer (2)	Self employment (3)	Worked out of state (4)	Migrated into state (5)	Employment Income (raw) (6)
Negligent Hiring Reform	-0.001	0.002	0.000	0.002	0.000	0.01***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.000)	(0.009)
Reform x Criminal History	0.049***	0.001	0.004*	0.001	-0.007***	0.079***
	(0.006)	(0.003)	(0.002)	(0.002)	(0.002)	(0.023)
Estimation	did2s	did2s	did2s	did2s	did2s	Poisson
Sample	pooled	pooled	pooled	pooled	pooled	pooled
Start Year	2005	2005	2005	2005	2005	2005
BTB Control	no	no	yes	yes	yes	yes
Obs	12880000	12880000	12880000	12880000	12880000	12880000

Source: ACS and CJARS (2020).

Notes: State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

1.4.2 The Impact of Negligent Hiring Reform on Recidivism

Whether reformed or not, the tort of negligent hiring imposes additional liability for re-offending but not for first-time offenders. Lowering the employer's negligent hiring liability reduces the cost of hiring individuals with a criminal record but leaves the cost of hiring those without a criminal record the same. If employers are more likely to hire workers with a criminal record after the reforms, and if being employed lowers the probability of recidivating, we expect recidivism to decrease after the reform to a greater extent than overall offenses. Recidivism rates are challenging to measure for various reasons, including a lack of longitudinal data, differences in definitions and time frames across studies, and many other reasons. To measure the impact of negligent hiring

[11]Because employers who hire independent contractors (the self-employed), may still be liable for the contractor's actions under negligent hiring, it is reasonable to expect some modest increase in self-employment. Columns 4 and 5 help us better understand if differential migration and working across state lines are driving the results. Neither appears to be the case, as negligent hiring reform appears to have small impact on these relatively infrequent outcomes. Column 6 uses a quasi-poisson approach (in the TWFE set-up) to assess untransformed wages. This approach shows about an 8% increase in earnings following the reforms.

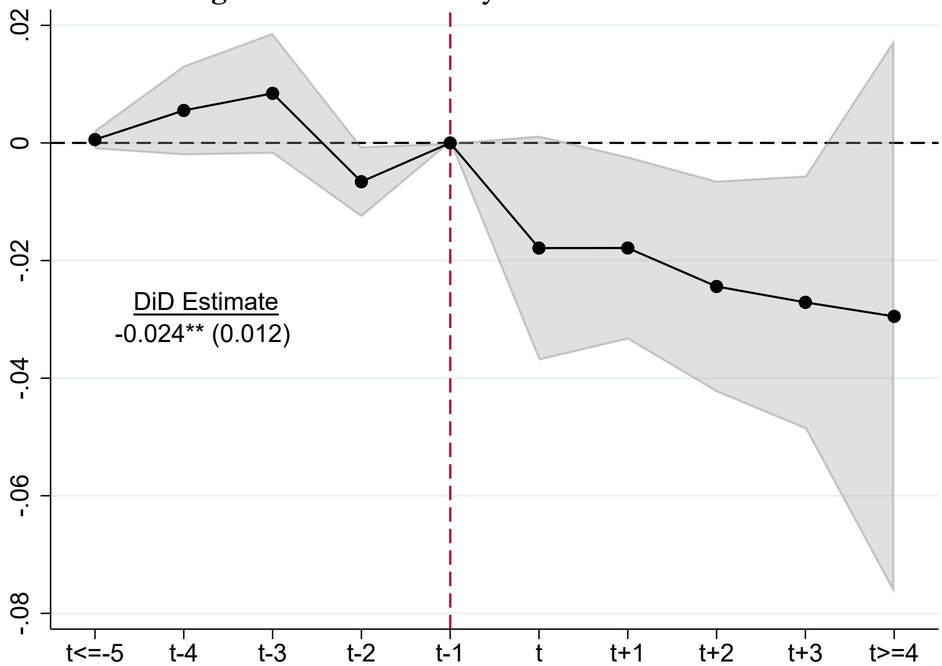
reform on recidivism, I use data from the National Corrections Reporting Program (NCRP).[12] While the data collection efforts underlying the NCRP began in 1983, I use the publicly available data from 1991-2019 containing information regarding prison admissions, prison releases, and year-end prison population counts. Not all states report to the NCRP each year. Therefore, I restrict this analysis to the years after 2005, when the number of reporting states has stabilized and to match the sample considered in the ACS. I construct the recidivism rates based on a unique identifier created by Abt Associates Inc. (the organization that is the collector of the NCRP data on behalf of the BJS) and consider a recidivism event to occur if an individual is re-imprisoned for a new charge after being released.

Limitations of this data include that one cannot observe individuals who re-offend across different states, that one must rely on potentially inconsistent voluntary state reporting, and that the NCRP uses public data where individuals are matched across observations by a third party. In addition, there are some potential challenges with defining recidivism as a court commitment for a new crime, as certain states in the NCRP may conflate new crime prison commitment and technical parole violations. However, results are robust to restricting to states where previous research has suggested the highest quality measurement. Despite these limitations, the NCRP is a commonly used source (Pfaff, 2011; Neal and Rick, 2014; Yang, 2017; Agan and Makowsky, 2018). I count an event as recidivating if the NCRP reports that the individual has been admitted to prison as a "new court commitment" within three years of release and the individual is recorded as having been in prison before in the sample.

The identification strategy here is similar to the previous subsection. The outcome variable is a binary for recidivism, which is defined as three-year prison re-entry from time of release. Each unit of observation is a person released from prison. I also include controls to parallel the employment regression (although here the sample is only those with criminal records). The specification is for this analysis can be similarly represented to the previous sections (although all observations in the data set have a criminal record, removing the triple differences component): $Recidivism_i = \alpha + \beta_1 * \text{Reform}_{s,t} + \theta_D * \mathbf{D} + \delta_s + \delta_s \times t + \epsilon_i$. I include state and year fixed effects (where s indexes states and t years), state specific time trends, and a vector of observable controls $\mathbf{D}$ (last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length). I again use a two-stage difference-in-difference technique but results are robust to various techniques including stacked regressions or TWFE.

[12]Note that West Virginia and Louisiana do not report sufficient data over the time period and are thus dropped. Further discussion of NCRP data issues are available in Prescott et al. (2020). The NCRP is a valuable data set in that it covers a large number of individuals and a large number of states. At the time of writing, the NCRP contains additional states relative to the CJARS dataset, thus making it a better candidate for this section of analysis. Exploring these reform with other data (e.g. court or arrest records), is worth pursuing as these records become more available. The NCRP data used in the following analysis comes from the Inter-university Consortium for Political and Social Research (ICPSR). Data is available from https://www.icpsr.umich.edu/icpsrweb/NACJD/studies/37021/datadocumentation#.

Figure 1.4: Event Study - 3 Year Recidivism

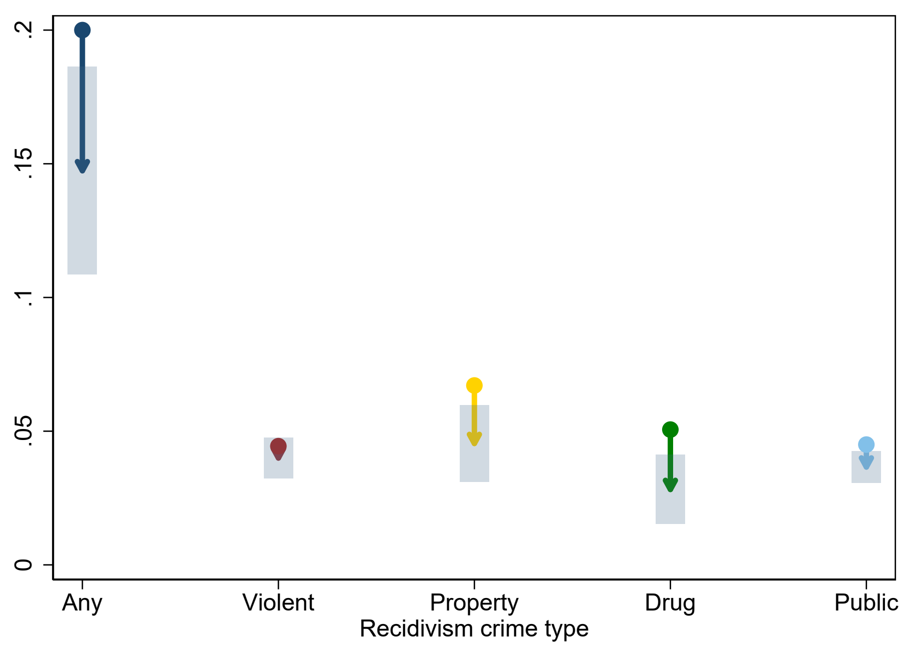


Source: NCRP.
Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Figure 1.4 shows the evolution of prison reentry recidivism rates around negligent hiring reform. Estimates to the left of 0 represent periods leading up to reform implementation. The year before enactment is omitted to have 0 relative difference between reform and non-reform states. As can be seen from the figure, recidivism rates for returning citizens were evolving in parallel across states before the reform, as the point estimates to the left of 0 display no clear trend and are statistically indistinguishable from 0. However, after negligent hiring was enacted, recidivism rates for people released in reforming states began to steady decline relative to non-reform states. This steady decline (from about one percentage point lower in the year of reform to about five percentage points lowers a few years after reform and almost eight percentage points in the longer run) suggests that the impact of the reform may grow over time. This pattern could be because employers learn about the lower liability over time, or it could be due to the interaction of employment and the timing of recidivism events (e.g., work is more likely to prevent recidivism that occurs more than one year after release).

Figure 1.5 and Table A.8 shows the comparison between the probability an individual recidivated in states which passed and states which did not pass negligent hiring reform after controlling for other characteristics of the released population. It controls for as many factors relating to the individual's release as possible in the data (but does not control for other state factors such as the state's unemployment rate at the time of release). Negligent hiring reform is associated with a statistically significant 2.4 percentage point lower recidivism rate (this corresponds to over a 10% reduction to a base rate recidivism rate of about 20%, where recidivism is a new crime re-incarceration within three years of release).[13] These results are robust to a variety of estimation approaches, including stacked event studies or TWFE. This decline is driven primarily by lower recidivism through new property, public order, and drug crimes. While there is some minor evidence that violent crime is also lower, this is a much smaller effect and not statistically significant. Previous research has suggested that stable employment decreases the likelihood of property crimes (as a substitute source of income) but has a less pronounced effect on violent crime (which is less likely to be financially motivated). Thus, these results are consistent with the theory that negligent hiring reform increases employment for individuals with criminal histories and that this employment decreases property and drug crimes (and has less of an impact on violent offending). Notably, there is no evidence that negligent hiring generates a substitution into more harmful violent crimes, as violent crime recidivism appears, if anything, to decline after negligent hiring reform occurs.

Figure 1.5: The Impact of Negligent Hiring Reform on Crime-type of Recidivism



Source: NCRP.

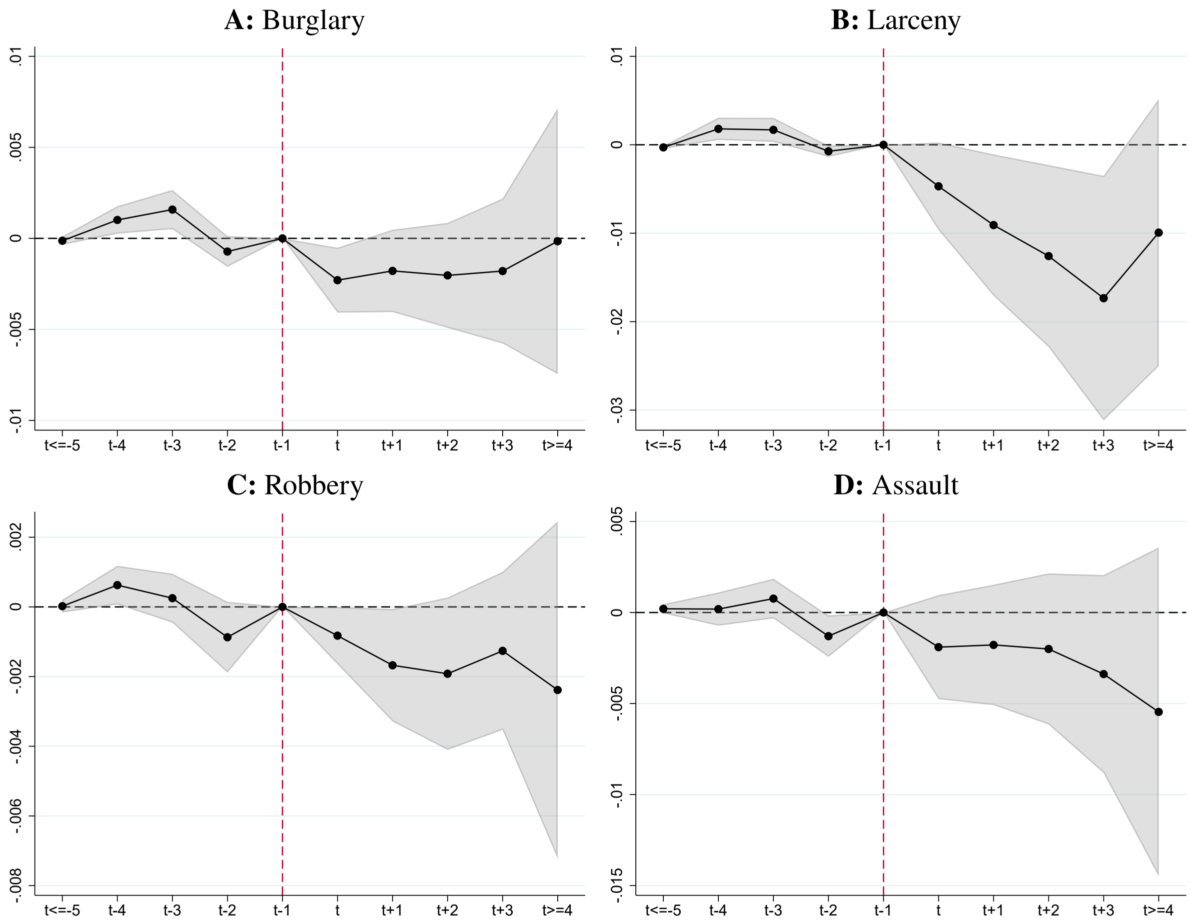
Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Figure 1.5 and A.8 shows large and significant declines in recidivism via a new crime of burglary

[13]Only individuals whose release occurs in a year in which negligent hiring reform is or has been enacted are considered "treated" or impacted by the policy. Notably, this excludes some of the population who is partially treated in that released individuals who return in the year prior to the enactment of the reform (or whose three-year recidivism window overlaps with some portion of the reform years) will benefit from the reform as well. Given this is the case, the estimates are likely biased somewhat towards finding no effect of the reform.

and larceny. Some violent crimes more often associated with income generation, like robbery, exhibit signs of decline after negligent hiring reform. However, other violent crime-specific recidivism, like homicide and rape, does not show clear evidence of decline. These findings are consistent with the theory that lowering negligent hiring increases employment opportunities for people released from prison, which leads to a substitution away from income-generating criminal activity. A lack of decline or even a slight increase in certain other non-income generating violent crimes is also consistent with the underlying economic theory, as employment may generate an equal or greater number of person-to-person interactions.

Figure 1.6: Event study - Negligent Hiring Reform and Recidivism by New Offense Crime Type



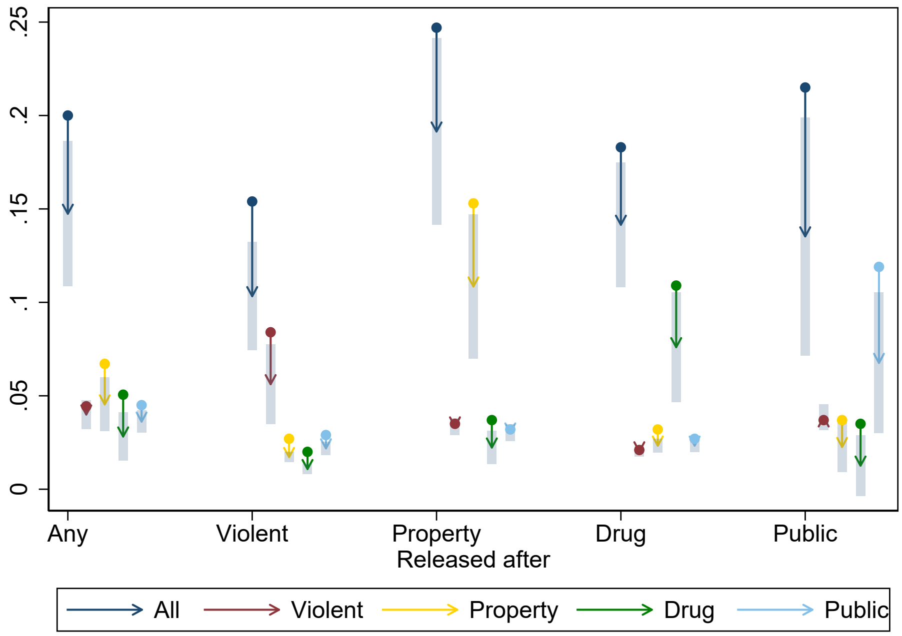
Source: NCRP.

Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Recall that some of the policy concerns driving the laws aimed at clarifying and narrowing negligent hiring liability standards involve the crime-type of the first offense and the nature of re-offense. The reforms also clarify that a tighter connection between previous and subsequent offenses must be present for the employer to be liable. A drug possession incarceration would be unlikely to be relevant to a future homicide conducted by an employee, at least after the reforms are in effect. The proposed mechanism for recidivism reduction is increased employment. Comparing the groups that see reductions in recidivism to those with greater increases in employment, it becomes apparent that groups with larger employment gains after the reform also have larger declines in recidivism rates (e.g., people released after public order and other offenses).

Figure 1.7 (and Appendix Tables A.8 and A.9) is similar in design to Figure 1.5. However, it adds additional granularity by breaking out the analysis by the crime for which the individual was released. The columns partition the released population by the most serious conviction they were imprisoned for, and the rows partition by the rate at which a specific recidivism conviction occurs. For example, in column (1), the coefficient on property crime (-.027) indicates that people returning after a property crime conviction recidivate about 2.7 percentage points (or about 11 percent) less frequently after negligent hiring reform is enacted. Recall that the studied reforms do not remove liability entirely but are focused on limiting liability to new misbehavior that is particularly similar to the past conviction. The lowered recidivism rates appear widespread across release types, with the largest absolute decreases being in the recidivism of people released after public order and property incarceration spells. The reductions in recidivism from these returning citizens are consistent with the fact that these offenses are unlikely to be particularly relevant to a negligent hiring case, and their criminal histories are more likely to be barred as evidence after negligent hiring reforms. The fact that some reforms carve out certain serious violent convictions from liability protections does not seem to dampen the impact of the reforms in aggregate, even in the broader categories containing these offenses. One element of the negligent hiring reform is to tighten the required connection between previous criminal behavior and the triggering event for the negligent hiring cause of action. After these reforms are enacted, employers appear to feel more comfortable with their ability to match releasees to appropriate (non-liability-inducing) jobs.

Figure 1.7: The Impact of Negligent Hiring Reform on Recidivism

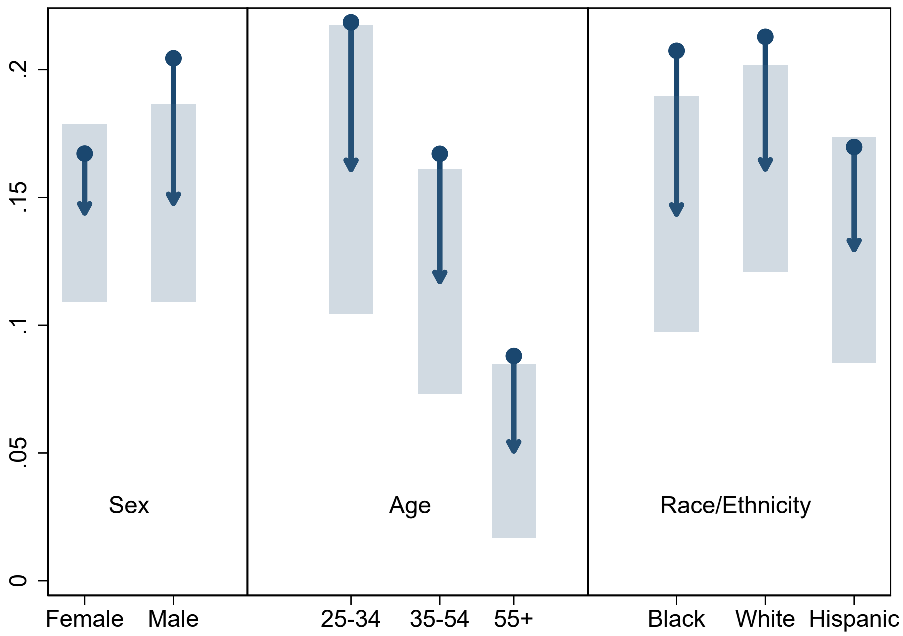


Source: NCRP.

Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

I consider the effect of the reform on various subpopulations for recidivism mirroring the employment heterogeneity presented in Appendix Table A.5 and Figure 1.3. Figure 1.8 and Appendix Table A.10 detail the impact of a DiD specification where the treatment is allowed to vary by fixed characteristics. Each dot shows the mean recidivism for each supgroup and the arrow and shaded areas are the point estimates and the 95% confidence intervals to document the impact of the reform across different groups. The groups that experienced the largest employment gains also seem to have the largest recidivism declines (with men having larger recidivism reductions than women, 2.5 vs 1.7, and Black people having the largest recidivism reductions, 3.5 vs 2.2 for White and 1.4 for Hispanics).

Figure 1.8: The Impact of Negligent Hiring Reform on Recidivism by Subgroup



Source: NCRP.
Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

1.4.3 The Impact of Negligent Hiring Reform on Offense Rates

It is possible that when states decrease employer liability, more or more harmful offenses will occur. It is also possible that "reduced crime due to more employment" will dominate the "new criminal opportunity" effect, and on balance, fewer offenses, or less harmful offenses, will occur. Offense levels will measure reported crime rates broadly. While measures of the number of offenses will encompass recidivism events, they will also include first-time offenders. However, negligent hiring liability and the statutes reforming it are most targeted at repeat offenders. Thus, relative to recidivism, we might expect to see a smaller effect on total offenses. One advantage of looking at offenses is better data coverage across states for generic offending than recidivism.

Studying offenses may better capture general equilibrium effects than the more obviously impacted recidivism rates. Measuring accounts for the possibility that potential offenders are forward-looking enough to consider future employment prospects at the time of the first offense. A related form of this worry would be that over time the common wisdom amongst potential offenders is that the expected punishment for a given crime is lower. If the calculus for the profitability of an initial offense changes (due to changes in future employability driven by changes to negligent hiring), this would be captured in the offense rates but not necessarily in recidivism statistics. A final reason to study offenses is to assess the possibility that by hiring workers with criminal records after negligent hiring reform, employers are displacing other marginal hires who then offend as first-time offenders.

The thought experiment performed here is a simple one: first, did states that passed negligent hiring reform experience fewer criminal offenses in the years following the legislation? To test this relationship, I estimate the relationship between the crime rate and negligent hiring reform using a difference-in-differences identification strategy. Given the longer sample and lack of individual-level data, I estimate this relationship using the doubly-robust methodology of Callaway and Sant'Anna (2021). Specifically, this approach controls for state and year-specific effects, with the outcome of interest being the natural log of crime rate per 100,000 people as reported in the UCR (where property, violent, and finer crime rate measures are considered).[14] While pre-treatment controls are not included in the primary analysis, the results are robust to their inclusion.

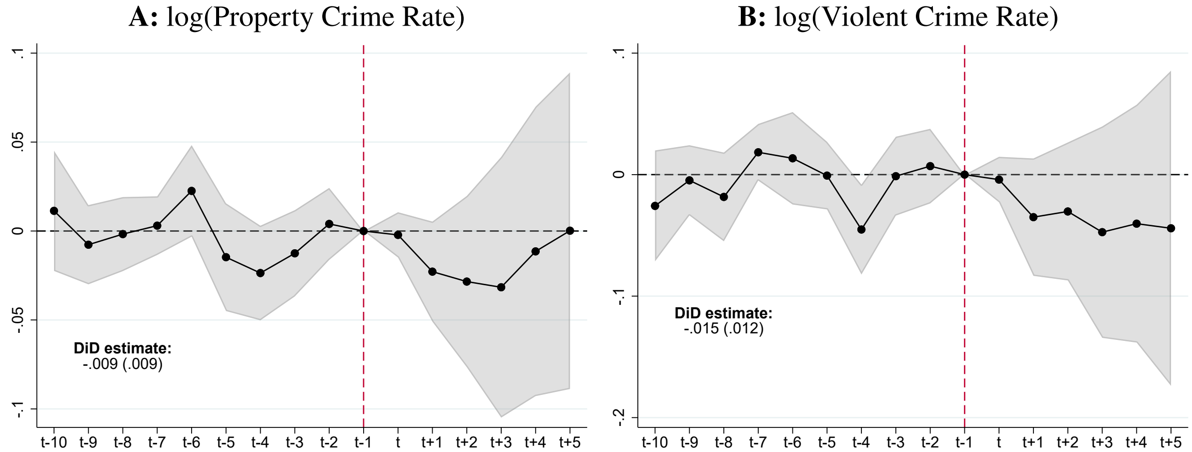
As with the other difference-in-differences exercises, to be a valid estimate of the impact of changes to negligent hiring liability, the states that change their negligent hiring policies must have similar trends in the outcome of interest as unchanged states but for the change in liability. In this case, that means that absent recognition or reformation of the tort, the offense rates in states that altered employer liability would have evolved similarly to offense rates in other states. One way to build confidence in this assumption is to show that prior to a liability change, the difference in offense rates does not follow a clear trend and point esetimates are near 0.

First, I show that before negligent hiring reform, reform and non-reform states had similar trends in offense rates. However, reform states had consistently lower offense rates after reforming negligent hiring liability compared to states that never reformed negligent hiring liability (although this is not statistically insignificant). Figure 1.9 shows the impact of negligent hiring reform on each subsequent period's crime rate. This figure demonstrates that before passing negligent hiring reform, the states were roughly comparable, a fact that is supported by the blue line bouncing around the gray dashed line in all periods before time 0 when the reform takes place (and the standard error bars around the line always include zero as a point estimate showing that the effect is statistically indistinguishable from zero). There is no evidence that states that reformed negligent hiring were on different crime trajectories than non-reform states. However, in the six years after passing the negligent hiring reform, the states that passed the reform have consistently lower levels of offenses than the states that did not pass reforms. Both property crime and violent crime appear to decline. The event study analysis presents some weak evidence that suggests that reducing and clarifying negligent hiring liability lowers criminal activity. This indicates that many states may have more (or at least less clear) liability standards than the offense minimizing point and rules out significant increases in offending due to negligent hiring reform.

Since the UCR data extends back to 1960, it is possible to study changes in offense rates after

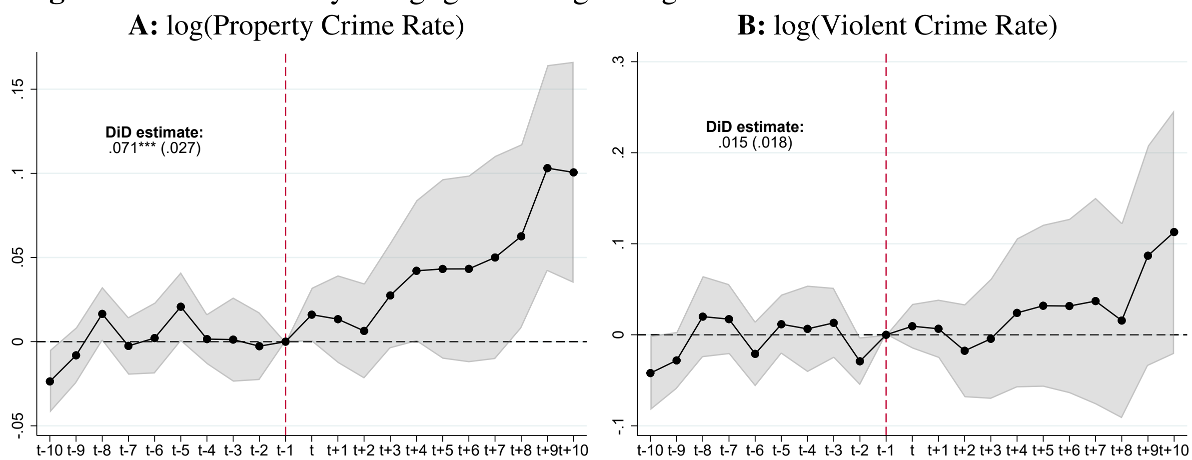
[14] See Prescott and Pyle (2019) for additional discussion of the UCR as a data source.

Figure 1.9: Event study - Negligent Hiring Reform and Offense Rates Event Studies



Source: UCR.

Figure 1.10: Event study - Negligent Hiring Recognition and Offense Rates Event Studies



Source: UCR.

the tort is widely recognized in a state. To construct a measure of state court recognition, I build upon previous scholarship and review case law to code when the highest court in a given jurisdiction first recognized the tort. Figure 1.10 shows that states had similar offending rates before increasing employer liability. After negligent hiring becomes more widely recognized in a jurisdiction, offense rates begin to grow. The number of offenses grows over time, suggesting that additional criminal behavior occurs as people are forced out of the labor market.

Taken as a whole, this evidence suggests that increasing negligent hiring liability does not, in aggregate, protect people from crime. Lowering barriers to employment for people with criminal records appears to also help public safety. Notably, the offense level analysis suggests that lowering negligent hiring reform does not decrease recidivism at the cost of increasing first-time offending. If reducing liability primarily results in substitution between workers who might otherwise resort to criminal activity absent employment, we would not expect to see the relationship observed. These findings suggest that decreasing and clarifying liability improves outcomes (or at least does not worsen) for workers with criminal histories as well as other individuals who might be marginal criminal offenders.

1.5 Discussion and Conclusion

While no one piece of evidence is definitive in the above analysis, the evidence is broadly supportive of the idea that limiting and clarifying the cause of action and appropriate evidence for negligent hiring is likely to improve several important outcomes. Novel administrative data from CJARS and the ACS suggests that the proposed employment mechanism is, in fact, the mechanism at play since individuals with a history of incarceration are more likely to be employed after release in states which have enacted negligent hiring reform. The NCRP recidivism exercises suggest that this achieved whil simultaneously lowering recidivism rates, especially from the groups of released individuals most likely to be impacted by the reform. Finally, the UCR indicates that offenses are, if anything, more likely to decrease after a state passes negligent hiring reform, and property crimes increased after the tort was recognized. These various pieces of evidence suggest that the risks to employers imposed by the common law tort standards for negligent hiring result in more criminal offenses and worse labor market outcomes for potential workers with criminal histories.

The evidence and theory presented in this paper suggest that previous efforts to reform state tort law governing liability for negligent hiring policies have improved public safety and employment outcomes. These reforms contain much of the same language in model legislation by the American Legislative Exchange Council. However, these reforms have been limited to a handful of states. Additional efforts to limit and clarify employer liability for promising returning citizens, such as certifications of employability and expungement, may be expected to have similar positive effects on the targeted populations (both sets of reforms lower p0, the probability of being found responsible for negligently hiring an employee, for a subset of returning citizens).

The theory suggests that alternative approaches, such as capping damages or lowering reputational damage from negligent hiring (lowering N), may also be available. Note that these first two categories of reforms (lowering liability and capping damages) reduce the compensation paid to victims. If the current level of transfers to victims is to be maintained, an additional transfer payment would be needed. One alternative reform supported by the theory of negligent hiring, but for which additional empirical work is necessary, is an expansion of the tax credit available to employers who employ workers with criminal histories (raising R, the expected revenue generated by a hire). If a hiring subsidy fully internalizes the positive externality from employment, imposing liability for offenses gets the monitoring incentives right and preserves the expressive aspects of the tort system toward negligent actions.

The bills that generated the variation used in the empirical work limited employer liability by clarifying and restricting the type of evidence that claimants can introduce in attempting to establish that an employer was negligent in hiring the employee in question. Why does this reform work? In the theoretical analysis, this type of reform was predicted to encourage employers to be more willing to hire (and perhaps pay higher wages to) people with criminal records. This increase is because many of these criminal records will no longer be relevant in a negligent-hiring case should it arise, and thus the employers will face a lower liability risk. In practice, this appears to happen.

There is some theoretical concern that weakening the employer's screening incentive too far will generate more or worse criminal behavior as more dangerous employees are hired into positions that allow them greater opportunities to offend. Before this paper, there was no evidence regarding whether we are currently at the level of liability where changing the screening incentive imposed by negligent hiring liability would increase or decrease the number of offenses and recidivism. However, the evidence presented here suggests that the states that enacted the reform had too much liability (or too low a hiring subsidy). Additionally, lowering employer liability led to the same or less criminal behavior, as both recidivism and offense rates either exhibited no change or declined in the states that narrowed the scope of negligent hiring claims.

Limiting negligent hiring liability to offenses of similar types is not the only way to lower risk. For example, this could be accomplished by barring arrest records, especially those not leading to a conviction, from being used as evidence. Some states have taken an alternative, but perhaps complementary, route by creating a presumption against negligent hiring liability if the employee had received a certification of employability from the state. This policy is a complementary reform but requires prior action by the released individual and thus may be limited in scope and administratively burdensome. In the compensation framework presented in the theoretical analysis earlier in this work, the certification policy moves the employer out of the compensator role. It substitutes the government agency issuing the certifications as an additional screening mechanism. This approach is similar to negligent hiring reform if employers and the government can screen at relatively similar costs, although it does not provide a transfer payment to the victims. In practice, those who receive certificates are more likely to be employed and less likely to recidivate. However, it is unclear how much, if any, of this effect is driven by employers' lowered concern with liability as opposed to pre-existing differences between individuals that receive certificates and those that do not.

Another approach to limiting liability under negligent hiring would be to impose a damage cap in negligent hiring cases (in terms of the model, this is akin to lowering N for a given $H(.)$. Several states have similar legislation in the medical malpractice context. However, a number of these caps have been found unconstitutional, limiting the effectiveness of such a policy. While this lowers employers' expected liability, it does not have the same effect as reducing who qualifies as a negligent hire, in that it caps employers' liability regardless of how clear their negligence was. It thus dulls the incentive for employers to screen employees for previous offenses and to monitor their behavior at work. While it may increase the hiring of released prisoners, it does so in a less targeted manner than the other potential reforms.

The Work Opportunity Tax Credit (WOTC) allows employers to claim a tax credit for people who are on welfare programs or have barriers that discourage workforce participation (this would be akin to a shift of R, which is the revenue received from a hire). Employers who hire people with felony records can claim this credit. To encourage hiring, continued employment, and higher wages, the size of the credit is based on the number of hours worked and qualified wages. The labor market for workers with criminal histories can be expanded by lowering the employer's liability or increasing the revenue the employer receives due to the worker's employment. Increasing the attractiveness of the tax incentives for hiring this population is analogous to increasing the revenue. However, as currently structured, the tax credit appears too small and difficult to obtain to offset an employer's liability (and other concerns).[15]

Tort liability for negligent hiring is designed to encourage employers to screen and supervise their employees and compensate victims of misbehavior for their injuries. By allowing evidence of previous criminal behavior to establish employer liability, negligent hiring doctrine discourages employers from hiring workers with criminal records. In theory, this liability results in firms hiring workers with a lower perceived risk of reoffending and more closely supervising working environments to protect consumers from potential tortious employee conduct. However, it also results in firms hiring fewer workers with criminal histories, which generates additional offenses because these workers cannot find jobs. The act of "negligently hiring" thus has a theoretically ambiguous impact on criminal behavior. By failing to fully consider the lowered probability of offending as a consequence of employment, this liability may currently generate sub-optimal outcomes. These results do not imply that removing negligent hiring liability is necessarily optimal. However, they suggest that, on the margin, there may be some benefits to statutes similar to those enacted in states like Colorado and Texas, which clarify and narrow the role of criminal records in negligent hiring cases.

The experience of the states that have recently clarified and lowered employer liability for

[15]Using application-level data obtained by a Freedom of Information Act request to Virginia's Department of Labor, I find that only 3,272 credits were approved between 2018 and 2020 for workers with a felony conviction out of over 18,000 requested credits (an approval rate of eighteen percent, which is about twenty-five percent lower than the overall approval rate for WOTC requests). Moreover, of these 3,000 or so credits, a small number of employers make up a large share of approved requests (the top 10 employers make up twenty-five percent of the credits), suggesting that knowledge and ability to obtain these credits is low. In addition to lowering the costs of applying for the credit, one can increase program usage and effectiveness by making it more attractive. For example, in a recent RAND survey, expanding the tax credit from twenty-five percent to forty percent and doubling the cap to $5,000 increased the number of employers willing to consider hiring an individual with a felony conviction by over thirty percent.

negligent hiring suggests that moving to a lower liability regime resulted in higher rates of employment and wages for people with criminal histories, as well as lower rates of recidivism. There is no evidence that the offenses that did occur became more harmful. This suggests that employment is an important mechanism driving these results. The empirical evidence is imperfect and better data may yield different results, especially given the small number of states that have enacted reforms and the relatively short follow-up periods available for study. Still, both theory and data suggest that narrowing and clarifying liability for negligent hiring reduces crime and allows additional workers access to the labor market.

CHAPTER II

Estimating the Impact of the Age of Criminal Majority: Decomposing Multiple Treatments in a Regression Discontinuity Framework

(With Michael Mueller-Smith and Caroline Walker)

2.1 Introduction

In the United States, the criminal justice system prosecutes and sentences "juvenile" and "adult" defendants in different ways based on a dual principle of evolving culpability over the age profile and a desire to protect children from the potential long-term harms stemming from justice involvement.[1] Whether a defendant is treated as a juvenile or adult is largely defined according to a discrete function of their age at the time of the offense. Many states today define the cut-off between juvenile and adult, the age of majority, as 18 years old. An offense that occurs one day before the defendant's birthday is likely to be handled in an

[1]We are grateful to Yuehao Bai, Charlie Brown, Carolina Caetano, Jonathan Eggleston, Itzik Fadlon, Keith Finlay, Andreas Hagemann, Sara Heller, Peter Hull, Brian Jacob, Carl Lieberman, Charles Loeffler, Jens Ludwig, Elizabeth Luh, Nikolas Pharris-Ciurej, James Reeves, Mel Stephens, Megan Stevenson, and Brittany Street as well as conference/seminar participants at UC Berkeley, the NBER Interactions Conference, the Virtual Economics of Crime Seminar, Northwestern, the Conference on Empirical Legal Studies, UC San Diego, the UM-UWO-MSU Labo(u)r Day Conference, and the American Law and Economics Association 2023 annual meeting, for their constructive feedback. We thank the National Science Foundation and the Bill & Melinda Gates Foundation for financial support. Any opinions and conclusions expressed herein are those of the authors and do not reflect the views of the U.S. Census Bureau. The U.S. Census Bureau reviewed this data product for unauthorized disclosure of confidential information and approved the disclosure avoidance practices applied to this release (DMS number: 7512453, DRB approval numbers: #CBDRB-FY22-291 & #CBDRB-FY23-088 &CBDRB-FY23-0392, & CBDRB-FY23-0414).

entirely different system (judges, prosecutors, physical location, possible sanctioning options, etc.) than one that occurs one day after. While many of the factors that are associated with culpability and the effectiveness of punishments and supervision evolve gradually over adolescence, the risks defendants face transforms overnight based on a statutorily specified age of criminal majority (Loeffler and Grunwald, 2015a; Lee and McCrary, 2017; Loeffler and Chalfin, 2017). Considering that a large majority of justice-involved individuals have their first contact with the justice system as a teenager (Figure 2.1),[2] a potentially life-altering experience, it is critical that we better understand how these different approaches shape the lives of their caseloads.

In this paper, we leverage a regression discontinuity (RD) design to study a range of impacts stemming from such a sharp change in the criminal justice system generated by the age of criminal majority. Using novel data containing adult and juvenile adjudication records from Wayne County (Detroit), Michigan between 2011 and 2020 developed through work described in Finlay et al. (2022a), we exploit variation generated by whether the alleged criminal conduct occurred just before or just after a defendant's 17th birthday, which was the defined age of criminal majority during our study period. We examine changes to future criminal activities, employment, and earnings and we find that a felony case in the adult system results in about a 20% reduction (0.48 fewer cases) in subsequent felony cases, but that the adult system also generates worse labor market outcomes, lowering annual earnings over the next 5-years by almost 30% (over $600 per year) and increasing poverty rates by about 33%. Examining a range of outcomes is particularly important in this context since differential rates of incarceration across the threshold can generate both good (↓ recidivism) and bad (↓ wages) outcomes as a result of incapacitation,

[2] This fact would be missed when looking at a cross-section of the justice caseload in a given year, the typical format of data collection and reporting by federal agencies. While researchers have long recognized an age-crime curve (see Quetelet (1984) and Sampson and Laub (1995)), the typical composition of the justice caseload involves individuals mainly in their 20's and 30's. It is only through the innovation of multiple decades of longitudinally linked microdata from the justice system in CJARS that this nuanced and detailed perspective on justice-involvement over the life course is possible.

an issue that has been acknowledged but previously unaddressed in prior work which primarily focused on recidivism due to data limitations.

To understand what specific mechanisms generate these overall changes at the discontinuity, we develop a new empirical methodology to estimate the specific treatment effects of a range of disposition and sentencing options that change at the threshold. For instance, both conviction and incarceration rates change depending on whether a defendant is prosecuted through the juvenile or adult criminal justice system. And, given factors like the greater availability of record sealing for juvenile defendants, the long-term implications of a conviction vary depending on which side of the threshold a case falls on. As a result, standard RD strategies cannot differentiate whether changes in outcomes in the overall caseload are attributable to differences in convictions rates, the legal implications of convictions for criminal records, incarceration, or something else entirely.

Previous research has speculated that differential incapacitation time driven by prison sentences might explain the higher recidivism rates for juveniles relative to adults (Lee and McCrary, 2017). We develop a methodology to address this directly. We apply a new regression discontinuity procedure to disentangle the impact not only of the reduced form effect of a case being handled in juvenile versus adult court but also what specific features of the juvenile and adult systems generate the impacts to future recidivism and employment outcomes. Using a machine learning approach and a rich set of covariates, we create predicted juvenile and adult case dispositions and sentencing outcomes to decompose and identify multiple changes in treatment across the discontinuity. The interaction of the predictions with the cutoff rule instrument for actual program take-up, while the uninteracted prediction terms partial out potential sources of omitted variables bias. Intuitively, the procedure allows us to isolate the subset of the analysis sample who would have received intervention X if prosecuted as juveniles and intervention Y if prosecuted as adults, thereby avoiding the problem of multiple interventions changing simultaneously at the threshold. This decomposition allows us to learn not only about the impact of moving between the juvenile and adult criminal justice systems but also about marginal changes in punishments within the juvenile and adult systems (e.g. the marginal impact of incarceration compared to a non-carceral adult punishment). The decomposition exercise provides several important lessons. First, adult prosecution may generate a specific deterrence effect as we observe a causal decrease in recidivism for adult case dismissals relative to juvenile case dismissals. In addition, adult convictions significantly increase the risk of future justice involvement, but empirically this is often offset by being paired with incarceration which prevents crime through incapacitation. This latter point is amplified by our observation that both adult convictions and incarceration lead to decreases in earnings.

Using this framework, we consider 4 policy counterfactuals: (1) raising the age of criminal majority, (2) shifting the proportion of adult defendants receiving case dismissals to match the juvenile system (which is more lenient), (3) eliminating prison sentences for young defendants in the adult criminal justice system, and (4) making adult convictions operate like juvenile convictions (e.g. enhanced access to short-run expungement). We find that all policy options generate net revenue for the government's budget constraint and are attractive to defendants. Because several of these programs generate savings through reducing program costs at the risk of increasing recidivism, the net impact on society after accounting for potential victim costs becomes more ambiguous. Option 3 (eliminating adult incarceration) worsens societal outcomes; option 1 (raising the age of majority) roughly breaks even. However, these conclusions depend on equal weighting of taxpayers, defendants, and future victims in the social welfare calculation, an assumption that policymakers may forego in favor of alternatives that instead seek to address specific distributional inequalities in society. Options 2 and 4 unambiguously benefit all perspectives in our analysis, yielding the most social value through addressing the underlying mechanism that appears to generate the most harm: quasi-permanent adult conviction records.

Our analysis has immediate theoretical and policy relevance. Prompted by a growing body of neuroscience research showing that the brain continues to develop mature decision-making functions past the age of 18 (Center for Law, Brain and Behavior, 2018), eleven states have raised the age of criminal majority to the age of 18 in the last 15 years (including Michigan in 2021).[3] Several states have considered raising the age of majority beyond 18. These reforms highlight the fact that the age of criminal majority is a policy choice. Our analysis helps demonstrate that changes in how adolescent charges are resolved can help improve long-term public safety and productivity if appropriate justice programs are matched to the appropriate set of charged youths.

Additionally, our paper makes a broader methodological contribution which can be used to estimate the relative contribution of distinct underlying mechanisms in regression discontinuity designs with multiple policy levers that jointly change across a discontinuity threshold. This method has potential applications in a number of different fields, including income thresholds that trigger a range of safety net interventions, test score cutoffs that determine enrollment to differently ranked educational institutions with potentially different course and major offerings, or adjusted gross income thresholds that vary multiple tax incentives simultaneously.

2.2 Related literature

A considerable literature documents the important developmental and cognitive differences between children, developing adolescents, and adults, and the out-sized impact of interventions during early, formative years (Gruber, 2001; Heckman and Mosso, 2014). Neuroscientists have shown that youth brains continue to mature well into early adulthood, leaving them highly susceptible to reward- and peer-influence during their teenage years (Center for Law, Brain and Behavior, 2018). This period of physiological development likely plays an important role in the sensitivity of

[3] As part of the law raising the age of criminal majority, juvenile microdata could no longer be accessed by researchers, preventing us from studying the policy change as a source of variation.

adolescents to their environment and also leaves them vulnerable to impulsive decisions (Monahan et al., 2015). There is evidence, for instance, that providing cognitive behavioral therapy to economically disadvantaged youths reduced arrest rates by about one-third, violent arrest rates by up to one-half, and increased school engagement and graduation rates (Heller et al., 2017). While brain maturation plateaus on average by age 25 (Arain et al., 2013), choices that lead to contact with the justice system at earlier ages could have life-long implications, especially as human capital develops dynamically over time, reinforcing earlier choices (Arora, 2019).

Aside from culpability, there is an additional reason to want to protect youth from traditional justice interventions. The National Prison Rape Elimination Commission Report (2018) found that juveniles were at a higher risk of being sexually assaulted while in prison than the average prisoner. Similarly, Justice Policy Institute (2017) provides evidence that juveniles in prison (as opposed to juvenile detention) face a higher risk of being beaten by guards and committing suicide. Such extreme outcomes lay bare the differential cost of justice involvement for youth compared to adults and provide strong motivation for a separate system of punishment and rehabilitation specifically designed for youth.

Economists have studied the age-of-majority threshold as part of two distinct but related literatures. First, researchers have sought to examine offending rates around the discrete jump in punishment severity to test the theory of general deterrence in the population (Hjalmarsson, 2009; Lee and McCrary, 2017; Loeffler and Chalfin, 2017; Lovett and Xue, 2018; Arora, 2019).[4] Findings in this literature have often been quite modest; for instance, Lee and McCrary (2017) finds that the

[4]Levitt (1998) explores related themes, although without exploiting the variation stemming from the age-of-majority discontinuity. Our project helps us understand how Levitt (1998), that shows drops in crime in the overall population at or after the age of majority in states that especially rely on incarceration, might relate to Lee and McCrary (2017), that finds minimal evidence of general deterrence when looking at first-time felony defendants across the age of majority. We find evidence consistent with both of these narratives. First, there isn't a meaningful change in first time offending levels at the age of majority (that is, there is no real evidence of general deterrence in this population). At the same time, we see large declines in future crime associated with the application of incarceration to the adult caseload. The incapacitation here is substantially longer and more meaningful compared to the juvenile system.

odds of committing a crime decrease by only 2% when an individual turns 18 and are subject to adult criminal sanctions, a relatively small decline considering the serious increase in expected punishment. This small degree of behavioral response, however, comports with psychological research showing the developing teenage brain is less focused on long-term consequences of their actions (Blakemore and Choudhury, 2006).

A second line of research examines whether the bundle of interventions that comprise the juvenile justice system discourages future criminal activity compared to the adult criminal justice system. While causal identification has been challenging in this setting, a systematic meta-analysis conducted in 2016 suggests that marginal transfers of individuals under the age of majority into the adult court system ("waiver" in Michigan) have no statically significant impact on future recidivism. However, there is evidence of heterogeneous effect depending on the specific nature of the transfer and the defendant involved (Zane et al., 2016). Additional studies support this summary and find mixed results and generally suggest that being processed as an adult decreases recidivism rates or fails to reject the null of no effect (Bishop et al., 1996; Fagan, 1990, 1996; Fagan et al., 2003; McNulty, 1996; Winner et al., 1997).

Several recent studies have exploited changes in several states' age of majority law in a difference-in-differences framework to understand the impact of moving between juvenile and adult systems (Loeffler and Grunwald, 2015a; Fowler and Kurlychek, 2018; Robinson and Kurlychek, 2019; Loeffler and Braga, 2022).[5] By expanding the treated sample beyond transfers to an entire population, this approach potentially improves both the external and internal validity.[6] This research has largely found that adult prosecutions have either no impact on recidivism (Loeffler and Grunwald, 2015a) or decrease the risk of recidivism (Robinson and Kurlychek, 2019; Loeffler and Braga, 2022).

[5]These states include Connecticut, Illinois, and Massachusetts.

[6]Although, as Arora (2019) points out, potential differences in policing and arrest behavior complicate interpreting these results.

The most promising work in this literature examines defendants just above and below the age-of-majority cutoff and follows their recidivism trends over time (Lee and McCrary, 2017; Loeffler and Grunwald, 2015b). By examining just the subsample of defendants with offenses around the discontinuity, the research design minimizes the risk of omitted variables bias, strengthening the credibility of the research findings. These studies consistently find that those prosecuted through the adult system exhibit modestly lower rearrest rates in the future. A range of theoretical factors may contribute to these findings, however, including differences in specific deterrence, rehabilitation, or incapacitation. Whether the lower rearrest rates are driven by incarceration (costly) versus deterrence (less costly) has not yet been established in the literature, though, which is crucial due to their fundamentally different implications for public policy.

2.3 Criminal justice systems for adult and juvenile defendants

We study the impact of the age of criminal majority in Wayne County, Michigan. During our sample period, the age of majority was 17 (it has since been raised to 18 as of October 2021). This means that an alleged criminal act committed by a person under the age of 17 will be processed in the juvenile system, while criminal behavior alleged to have been committed by someone over 17 will be in the adult criminal courts. Michigan, like many other states, transfers certain severe crimes into the adult system.[7]

The hearing is similar in many ways across the two systems (for instance, both systems can have a jury, judge, defense, and prosecution). Likewise, both the juvenile and adult systems require the state to make its case beyond a reasonable doubt and have similar rules regarding admissible evidence.

There are, however, a number of important differences, beginning with the average cost of prosecution; Wayne county reports an average juvenile case processing cost

[7]Waiver into the adult system is relatively rare. In our sample, under 2% of juvenile cases were transferred. The two most emphasized factors in a waiver hearing are the defendant's previous criminal record and the seriousness of the charged offense. Our analysis sample is restricted to first-charged felonies, putting downward pressure on the first factor.

(in 2016) of $1,927 compared to $1,154 for an adult case. These costs are driven by different staffing needs and economies of scale. Similarly, the menu of available interventions (dispositions and sentences) and the implications of these assignments (availability of record sealing, location, and composition of institutional facilities, etc.) differ dramatically. These differences are detailed below with empirical evidence from our setting discussed in Section 2.6.

2.3.1 Youth criminal justice

In Wayne County, the juvenile system is administered by the Circuit Court Family Division, a specialized division of the legal system with more focus on treatment and rehabilitation. Although many of the same legal standards and procedures apply, Michigan courts have a great deal of discretion in assessing fines, fees, and assigning program participation in juvenile cases. Juvenile cases can be dismissed by the judge, and the judge may also issue a warning to the juvenile and the parents along with this dismissal. Alternatively, a juvenile may be found responsible for their actions, and these judgments may include fines, restitution, community service, imposition of curfews, behavioral/drug assessments and treatment, probation, and supervised residential placements out of the home. The county contracts with five Care Management Organizations (CMOs) that are responsible for adjudicated juveniles within a zip code cluster and provide case management, residential placements, and other services (some of which are subcontracted).[8] Juveniles on probation may be offered mental health services such as cognitive behavioral therapy, regularly screened for substance use, provided academic tutoring, electronically monitored, and/or provided job readiness programming, among other services.

In our sample period, Wayne County reported just over half of those assigned to some sort of probation were assigned to out-of-home supervision.[9] In 2013, the overall length of stay in residential placement was 5.8 months with an average of

[8]The juvenile justice system is funded with a 50/50 cost-sharing plan between the county and state.

[9]Unfortunately, we do not have access to micro data on punishments or services associated with a juvenile case resolution, but annual caseload-wide statistics are publicly available from Wayne County.

7.5 months for secure and 4.3 months for non-secure facilities (Chaney and Reed, 2018). The family court no longer has jurisdiction over a defendant 2 years beyond the maximum age of original jurisdiction (17 in our sample), which places a cap on how long incapacitation can be.

Juvenile defendants and their families face several fees and potential fines. These include mandatory fees such as victim rights assessments, DNA testing, and the cost of care and services such as daily detention fees for youth in out-of-home placements. There are also discretionary fees including fines generated by the statute violation, in-home cost of care services, and fees for court-appointed counsel (Uppal, 2020).

A final critical feature of the juvenile justice system is the greater availability of expungement opportunities.[10] Record sealing limits public access to criminal records and may have important long-term implications given that criminal records have been shown to causally impact outcomes like recidivism, employment, and wages (Pager, 2003b; Mueller-Smith and Schnepel, 2021). Even though Michigan is less generous towards juvenile defendants than many states (Shah and Strout, 2016),[11] several key features make juvenile expungement more common than adult expungement. Most importantly, juveniles are eligible within one year after case disposition, exiting detention, or turning 18. This stands in contrast to a 5-year waiting period for adult expungement, which only starts once a sentence is fully served effectively adding years to the clock. So, while a juvenile defendant might have their record sealed by age 19, a 17-year-old adult defendant might have to wait almost a full decade longer until their late 20's if their sentence came with a 5-year probation sentence. With such a long waiting period, adult defendants may be permanently harmed from diminished labor market experience or additional criminal activity, thereby making them ineligible for an expungment in the first place.[12]

[10]In the state of Michigan, record sealing is achieved via a procedure known as a set aside.

[11]Subsequent legislation has expanded record sealing in Michigan. See Public Acts 361 and 362 of 2020 for juveniles and Public Act 193 of 2020 for adults.

[12]In addition, three additional factors may benefit juvenile defendants with regard to criminal histories. First, juvenile

2.3.2 Adult criminal justice

The criminal division of the Circuit Court handles adult felonies (the district court handles misdemeanors). While services for juveniles are provided by CMOs, the state directly oversees the supervision of the vast majority of adults, regardless of community-based or institutional correctional status.

About 16% of adult charges in our sample (regardless of disposition outcome) were sentenced to some incarceration. The average minimum sentence for new adult entrants to prison in Michigan was 3.6 years in 2012, and the average term of those in prison was 8.9 years (Michigan Department of Corrections, 2012). This figure may be significantly higher for Wayne, as Hornby Zeller Associates (2018) estimate an average prison stay of 23.1 years for Wayne. The adult incarceration system places less emphasis on community supervision and resources targeted at young inmates as the system is designed to accommodate a typically older population. In our sample, 70% of adult defendants were sentenced to some non-carceral punishment. Sentences in this category include probation, restitution or fines, and community service.

In the adult system, a person convicted of no more than one felony offense (or two or fewer misdemeanors) may apply for a record sealing five years after imposition of sentence, completion of probation or parole, or release from prison, whichever is later. The accumulation of additional criminal records during this waiting period will make the defendant permanently ineligible for any expungement for all cases on their criminal history.

expungement allows for more total cases to be sealed compared to adult expungement, which might also impact take-up. Second, a juvenile adjudication for delinquency is in family court and thus is legally not a prior conviction in Michigan, and such individuals do not need to report said criminal histories to employers if solely asked about prior convictions. For ease of exposition however, we refer to delinquency adjudications as juvenile convictions in this paper. Finally, juvenile records are generally differentiated on Law Enforcement Information Network (LEIN) background check reports requested by employers and may be viewed differently as a result.

2.4 Data, econometric specification, and identification assumptions

2.4.1 Data sources

We use novel microdata linked through the Census Bureau data linkage infrastructure and accessed through the Federal Statistical Research Data Centers (FSRDCs) to observe individual socio-economic information as well as individual criminal and employment histories for adult and juvenile defendants in Wayne County, Michigan. Criminal records are measured using the Criminal Justice Administrative Records System (CJARS), which compiles criminal justice records from many jurisdictions and agencies (Finlay and Mueller-Smith, 2021). The CJARS data is supplemented with additional administrative data on juvenile cases provided by the Michigan State Court Administrative Office. CJARS uses a probabilistic matching algorithm (see Gross and Mueller-Smith (2020)) to track individual involvement in the justice system over time and across jurisdictions. In this paper, we use two primary types of records: criminal court charges, which are classified by type (e.g., property, drug, violent) and gravity (e.g., misdemeanor, felony); and correctional data, including incarceration, probation, and parole.

We make use of the detailed information available through the Census Bureau in order to expand the set of outcomes we can study (e.g. employment) as well as to broaden the range of covariates we can leverage in our machine learning model. In the latter case, we use information on the defendant's age from court records, their race and sex from the Census Bureau's Best Race and Ethnicity and Numident files, their household's historic earnings and employment from IRS 1040 and W-2 forms, household composition (based on Finlay et al. (2022c)'s work) and previous charges, convictions, and incarcerations of the adolescent and their family members, and information (e.g. youth poverty rates and median income) about the census tract the youth is living in based on the public ACS 2010 5-year estimates. All data is merged using the Protected Identification Key (PIK)[13] or a geographic identifier,

[13]PIKs are a de-identified person identifier which is assigned to individual records through the Census Bureau's Person Identification Validation System (PVS). The PIK allows linkage across administrative and survey records within

which allows integration of anonymized datasets at the person level.

2.4.2 Sample construction

Following Lee and McCrary (2017), our sample is restricted to first-time felony defendants between the ages of 15 to 18 (i.e. two years on either side of the age of majority threshold).[14] If these defendants had previous charges, they are restricted to misdemeanor charges. The restriction on first-time felony defendants allows us to avoid having repeated observations for the same individual in our regression discontinuity analysis. We focus on the sample of defendants charged between 2011 and 2014 in Wayne County to balance the overall sample size and have a consistent follow-up period.

2.4.3 Empirical specification

To evaluate the reduced form impact of being charged as an adult defendant (relative to a juvenile defendant), we utilize a standard local linear fuzzy regression discontinuity framework:

$$\begin{aligned} Y_i = \alpha + \delta \text{Adult Defendant}_i + \gamma \text{Age at Offense}_i \\ + \beta[\text{Age at Offense}_i > 17] \times \text{Age at Offense}_i + \phi X_i + \epsilon_i \end{aligned} \tag{2.4.1}$$

where Y_i is a youth outcome, Adult Defendant$_i$ indicates the youth was charged through the adult criminal justice system, Age at Offense$_i$ is the continuous running variable measured based on exact date of birth and exact date of offense, and X_i is a vector of observable characteristics. Whether the youth is charged as an adult is

the Census Bureau data infrastructure, specifically matching individuals in the justice system to their tax records and demographic characteristics. Additional detail about PIK rates in the CJARS data can be found in Finlay and Mueller-Smith (2021), but is above 85% provided an individual appears in CJARS at least twice. Additional information about PVS is detailed in Wagner et al. (2014).

[14] In Table B.7 we show the robustness of our results to varying bandwidth windows between 1 and 3 years on either side of the cutoff.

instrumented using the following equation:

$$\begin{aligned}\text{Adult Defendant}_i = \alpha_1 + \delta_1[\text{Age at Offense}_i > 17] + \gamma_1\text{Age at Offense}_i \qquad (2.4.2)\\ + \beta_1[\text{Age at Offense}_i > 17] \times \text{Age at Offense}_i + \phi_1 X_i + \nu_i\end{aligned}$$

where crossing the age of majority threshold (i.e. $[\text{Age at Offense}_i > 17]$) functions as our excluded instrument for Adult Defendant$_i$.[15] Coefficients in this equation have "1" subscripts to denote being in the first stage equation.

The vector of control variables includes a range of defendant characteristics: the time since the defendant's first criminal charge, binned categories for time since last charge (within the last month, between a month and 6 months, more than 6 months and less than a year, greater than a year), whether the defendant has a previous misdemeanor of each crime types (violent, property), the category of charges present in the current case (violent, property, drug, or other), whether the defendant was Black, or male, the number (by crime type) of criminal offenses charged to people living in the defendant's zip code in 2010, household structure, census tract demographic information from the 2010 ACS (age structure, the percent of the tract that is male, Black, White, and Hispanic), tract child poverty rates and income distribution.

2.4.4 Identification assumptions

Our reduced form regression discontinuity evidence requires two critical assumptions. First, the discontinuity has a material impact on case processing and outcomes, and second, whether a defendant ended up on one side of the discontinuity or the other is as good as randomly assigned in order for our estimates to be interpreted causally. Failure to satisfy either of these requirements should raise serious questions about how to interpret our findings.

To address the first concern, which is not mechanically satisfied since defendants

[15] In our empirical implementation, we normalize Age at Offense$_i$ to be centered at zero such that having a positive value indicates crossing the age of majority threshold and negative values indicate being under the threshold. For expositional purposes, we abstract from this detail in the main text.

below the age of majority can be prosecuted as adults if the nature of their offense is sufficiently severe, we study whether there is a sharp change in the probability of a case being processed in the adult system around the age of criminal majority (Figure 2.2 panel A). In our data, a few defendants are waived into the adult system prior to 17, but this is rare. After the age of 17, criminal defendants are automatically processed in the adult system.[16] Ultimately, we observe a jump of 86 percentage points at the cutoff, indicating significant relevance of the age of majority cut-off.

The second concern, often referred to as potential sample imbalance, is whether differences in outcomes are the product of changes in interventions at the threshold or changes in (observed or unobserved) characteristics. There are many theoretical reasons why this key assumption may not hold in our application. First, evidence has shown that in some settings, there is a non-trivial general deterrence effect of the increase in expected punishment at the age of majority threshold. Similarly, it is possible that actors in the justice system (police, prosecutors, etc.) alter their behavior depending on the would-be defendant's age at the offense. These hypothesized behavioral responses to the age of majority threshold would predict that we might expect to see discrete changes in the size of the criminal justice caseload and the composition of the caseload just before and after the discontinuity.

While there are hypothesized reasons why we might see sample imbalance, in our setting the caseload density is smooth through the age of majority (Figure 2.2 panel B). The blue bars represent cases in the juvenile system and the red bars cases in the adult system. In this case, we see a similar number of cases filed just before and after age 17.

Although it is reassuring that caseload is smooth through the discontinuity,[17] for a causal interpretation to be valid, it is important to also asses whether the composition of cases and defendants on either side of discontinuity are similar. We

[16]We do not have consistent data coverage for the date an alleged offense occurs, so we use the defendant's age at the time the charge was filed in the court. While charges are typically filed near to the date of the alleged offense (for the cases that have both offense and filing date the average gap was about a week), this is not always the case and leads to a small number of instances with defendants who have recorded ages over 17 being processed in the juvenile system.

[17]See Table 2.1 for statistical tests.

provide additional evidence whether cases filed just before and just after the age of criminal majority are comparable in Figure 2.3 and B.1, which show the evolution of a range of socioeconomic characteristics over the support of the running variable, with corresponding statistical tests evaluating a discrete jump at the cutoff reported in Table 2.1.[18] We find that defendants just before and just after 17 are of similar race, sex, have similar household composition, and have similar childhood exposure to the criminal justice system.[19] In addition, the evidence indicates that family income and neighborhood wealth are similar across the discontinuity, as are the types of charges bringing the defendant into the criminal justice system. Overall, we find statistical balance on 44 of the 46 characteristics considered, spanning demographic background, neighborhood characteristics, family resources, and criminal histories.[20]

2.5 The impact of adult prosecution for justice-involved youth

Being charged as an adult defendant creates both positive and negative outcomes for youth during the 5-year follow-up period tracked in our study sample. As shown in Figure 2.4 and Table 2.2,[21] prosecution through the adult criminal justice system significantly lowers a range of recidivism outcomes on both the extensive and intensive margins. We find a statistically significant decrease of 7.6 percentage points of facing any criminal proceedings over the next five years (↓ 9%)[22] and 1.02 fewer criminal cases overall (↓ 19%). Total felony level activity drops by a striking

[18] The estimates provided in Table 2.1 follow the same econometric specification described in Section 2.4.3 with the exception of excluding the vector of control covariates X_i from the right-hand side of the equation.

[19] Note that parents of the adolescents in our sample are far more likely to have felony charges and convictions than the average child; in our sample almost 40% of parents have a felony charge, while Finlay et al. (2022c) estimate this number at about 10% in the general population.

[20] Among the 46 observable traits we evaluate, two exhibit modest but statistically significant differences at the discontinuity. These are: whether the defendant is facing a drug charge (β=0.036; p-value = 0.087), and whether anyone in the house had a previous felony conviction while co-residing (β=-0.068; p-value = 0.039). While we believe some imbalance is inevitable given the number of traits we consider, we will control for all observable traits in our regressions to minimize any resulting bias.

[21] Additional results can be found in Figure B.2.

[22] While this decline might seem modest given the high degree of future justice involvement in this population, another way of viewing this is that adult prosecution increases 5-year total desistance by 54% [0.07593/(1-0.8538)].

0.48 cases (↓ 20%) and 0.32 convictions (↓ 16%). Declines in total criminal activity are observed for all major offense types: 0.42 fewer violent charges (↓ 23%), 0.37 fewer property charges (↓ 21%), 0.31 fewer drug charges (↓ 33%), and 0.81 fewer other charges (↓ 23%).[23]

At the same time, the reduction in future charges and convictions comes at the expense of a drop in future labor market activity over the same 5 year follow up period. We observe statistically significant and economically meaningful reductions in annual wage income (↓ $674 or 21%), average number of employers – proxied by the number of distinct W-2 information returns with over $1,500 – per year (↓ 0.08 returns or 19%), and whether the defendant earns sufficient wages to exceed the poverty threshold for a single adult (↓ 2.7 percentage points or 31%). As shown in Table B.7, these regression discontinuity results are robust to varying local weighting, bandwidths, and estimation techniques. Figure B.3 provides further support for our findings, as our results only hold when the actual cutoff is used in the RD, and are imprecise zeros when placebo cutoffs are considered.

This pattern of results is potentially surprising given that crime and employment outcomes are typically negatively correlated with each other absent incapacitation (e.g., Mueller-Smith and Schnepel (2021)), and creates a tension over the trade-offs for policy makers who might decide whether to raise or lower the age of majority. While raising the cutoff threshold (as many states have done in recent decades) might improve the employment trajectories of justice-involved youth, we should also expect an increase in prevailing crime rates. Whether there might be policy alternatives through which society can reap the benefits without the costs requires a careful examination of the underlying mechanisms, which is the focus of our next section.

[23]Note that criminal "cases" can be composed of multiple distinct "charges" and so summing across the effects by offense type should not be expected to add up to the total effect discussed earlier.

2.6 Understanding the mechanisms that drive the impacts of adult prosecution

Multiple levers change at the age of majority cutoff and together generate our previously described estimates on the impact of adult prosecution. These include differences in the likelihood of conviction, the distribution of sentencing outcomes, and the ability to expunge or seal one's criminal record which can be seen in Figure 2.2 panels C through F. While prior research has speculated that changes in incarceration are what drive the estimated treatment effects, data and methodological limitations have prevented more concrete conclusions regarding mechanisms in this literature.

In this section, we develop a novel estimation strategy to disentangle the relative contribution of the multiple underlying treatments. Intuitively, we leverage a rich set of defendant characteristics combined with machine learning prediction methods to identify distinct subsets of our analysis sample who would have experienced disposition and sentencing outcome X if prosecuted as a juvenile but disposition and sentencing outcome Y if prosecuted as an adult. Combined with a non-trivial homogeneity assumption on the distribution of treatment effects conditional on observed covariates, we are able to recover mechanism-specific treatment effect estimates, which deepen our understanding of the impacts observed at the age of majority cutoff and enable us to consider a range of hypothetical policy counterfactuals that aim to maximize the benefits while minimizing the costs of adult prosecution.

2.6.1 Identifying mechanism-specific treatment effects using a regression discontinuity and predicted case outcomes

We build on the multi-valued RD treatment methodology from Caetano et al. (2023) to study identification and estimation in the regression discontinuity setting with a multi-valued treatment variable and unknown counterfactual treatment across the discontinuity. Our identification strategy returns the marginal effects for a range of potential interventions relative to an omitted treatment category.

Our work and econometric methodology also relates to the broader literature analyzing instrumental variable models with multiple potential treatments (Kirkeboen et al., 2016; Feller et al., 2016; Heckman and Pinto, 2018; Hull, 2018; Lee and Salanie, 2018; Caetano and Escanciano, 2021; Mountjoy, 2022). From this literature, we know that it can be difficult to interpret multivariate instrumental variable estimands since they combine comparisons across many treatment margins and compliers. This literature highlights the usefulness of interacting an instrument with covariates and the required constant treatment effects assumption. In our setting, we leverage defendant characteristics in a machine learning model to generate predictions of disposition outcomes. We then interact said probabilities with the exogenous age-of-majority cutoff to instrument for take-up of mutually exclusive disposition outcomes. This requires only a weaker homogeneity assumption within our marginal population of compliers (see Caetano et al. (2023)).

Model.[24] Let $\mathcal{N}$ represent a population, that is divided into two subgroups $\mathcal{N}^j$ (defendants charged as juveniles) and $\mathcal{N}^a$ (defendants charged as adults). Let A_i define the allocation of individual i to one of the two mutually exclusive subgroups, such that $A_i = 1$ if $i \in \mathcal{N}^a$ and $A_i = 0$ if $i \in \mathcal{N}^j$.

Individuals in $\mathcal{N}^j$ receive one of a finite set of K interventions $d_i^j \in D^j$ while individuals in $\mathcal{N}^a$ receive one of a finite set of L interventions $d_i^a \in D^a$.[25] There is no restriction on the degree of overlap between the elements of D^j and D^a; they could be mutually exclusive, have partial overlap, or have complete overlap. Together, D_i captures the full vector of possible treatments, regardless of subgroup. Every individual receives one and only one intervention: $\sum d_i = 1 \; \forall i \in \mathcal{N}$. We approach this as an unordered choice model akin to (Heckman and Pinto, 2018).

[24]The model in this section is intentionally described at a high level for accessibility. For a more complete econometric treatment of this exercise, see Caetano et al. (2023) or Appendix B.4, which applies their framework to our specific approach.

[25]To help make this more concrete, in our setting we consider $K = 2$ {juvenile non-conviction, juvenile conviction plus services} and $L = 3$ {adult non-conviction, adult conviction without incarceration sentence, adult conviction with incarceration sentence}.

Outcome vector Y_i is a function of both individual characteristics (X_i), interventions (d_i^a, d_i^j), and a linear random shock (ϵ_i):

$$Y_i = \alpha + \sum_{k=1}^{K} \delta_k^j \underbrace{\left(d_i^{j,k} \times (1 - A_i)\right)}_{\text{Juvenile treatments}} + \sum_{l=1}^{L} \delta_l^a \underbrace{\left(d_i^{a,l} \times A_i\right)}_{\text{Adult treatments}} + \phi X_i + \epsilon_i \tag{2.6.1}$$

For simplicity, we define A_i to be exogenous and uncorrelated with ϵ_i (achieved in our empirical application through the exogenous discontinuous cutoff rule). Treatment allocation within subgroups, however, may not be exogenous.[26]

With this setup, the typical empirical approach is to estimate equations 2.4.1 and 2.4.2 defined in Section 2.4.3. The resulting $\hat{\delta}$ measures the net difference in outcome Y_i between $\mathcal{N}^a$ and $\mathcal{N}^j$ at the age of majority discontinuity. This parameter can also be stated as a difference in two weighted sums of treatment effects: $\hat{\delta} = \sum_{l=1}^{L} p^l \delta_{a,l} - \sum_{k=1}^{K} p^k \delta_{j,k}$, where p captures the probability of receiving a given subgroup-specific treatment at the discontinuity.

Since A_i is exogenous, the $\mathcal{N}^j$ and $\mathcal{N}^a$ populations are statistically equivalent in expectation, and $E[Y_i]$ for the two groups would be equal in the absence of any d^j or d^a interventions. If it were possible to know the counterfactual treatment assignment for a given i, we could recover unbiased treatment effect estimates (δ^j and δ^a) by estimating the following regression:

$$Y_i = \alpha + \sum_{k=1}^{K} \delta_{j,k} \left(d_i^{j,k} \times (1 - A_i)\right) + \sum_{l=1}^{L} \delta_{a,l} \left(d_i^{a,l} \times A_i\right) + \phi X_i + \gamma_{j,a} + \epsilon_i \tag{2.6.2}$$

where conditioning on the hypothetical treatment assignment regardless of subgroup allocation through a fully saturated set of fixed effects $\gamma_{j,a}$ absorbs the bias.

For example, suppose we could isolate the set of youth for whom we knew with complete certainty that they would receive a case dismissal if prosecuted as a

[26]Note that the specification above imposes a conditional homogeneous treatment effects assumption, which we will rely on later.

juvenile and receive a case dismissal if prosecuted as an adult. If we estimated a separate RD regression on this hypothetical subsample of youth, we would be able to recover a causal estimate of the impact of being charged as an adult as opposed to being charged as a juvenile. This logic can be extended to all possible combinations of juvenile and adult case outcomes, and if we stacked these regressions, we would end up with the fixed effects regression specified above.

Unfortunately, this cannot be estimated since it is unknown and unmeasured what intervention would have been assigned to a given person if, instead of falling on one side of the discontinuity, they ended up on the other. An alternative strategy is to focus on the probability of receiving a given intervention. Using this in conjunction with D_i to instrument for actual take-up can yield unbiased treatment effect estimates.

$$\begin{aligned} Y_i = \alpha + \Delta D_i + \gamma \text{Age at Offense}_i + \beta [\text{Age at Offense}_i > 17] \times \text{Age at Offense}_i \\ + \rho \left(p_i^j \times p_i^a \right) + \phi X_i + \epsilon_i \end{aligned} \tag{2.6.3}$$

$$\begin{aligned} D_i = \alpha_1 + \Delta_1 \left([\text{Age at Offense}_i > 17] \times p_i^j \times p_i^a \right) + \gamma_1 \text{Age at Offense}_i \\ + \beta_1 [\text{Age at Offense}_i > 17] \times \text{Age at Offense}_i + \rho_1 \left(p_i^j \times p_i^a \right) + \phi_1 X_i + \nu_i \end{aligned} \tag{2.6.4}$$

In the above system of equations, the interactions of the probabilities with the exogenous group indicator variable $[\text{Age at Offense}_i > 17]$ act as our excluded instruments. These instruments satisfy the exclusion restriction since both the outcome and first stage equations partial out the uninteracted effect of the predicted probabilities for the entire sample.[27,28]

[27]An interesting special case of this system of equations is when the prediction function has perfect accuracy. In this case, the probability of a given d_i will be either 0 or 1, which will lead this setup to collapse back to the fixed effects model previously described.

[28]For further information, Appendix B.3 provides a series of empirical simulations of the performance of this model under a variety of scenarios, including violations of our modeling assumptions.

2.6.2 Predicting treatment status with machine learning

To surmount the identification problem described in the previous section, we turn to machine learning methods to generate predicted probabilities of juvenile and adult treatment status. We use random forest models to estimate the expected case outcome for each observation.[29] This is a pure prediction exercise in which we allow the model to use a rich set of covariates and allow for complex interactions across criminal histories, current charges, and socioeconomic histories to find patterns in how cases are disposed.

We use the same set of controls that we used in the regression analysis as our random forest predictors, but also add information about year-month of case filing. Controls include the time since the defendant's first criminal charge, binned categories for time since last charge (within the last month, between a month and 6 months, more than 6 months and less than a year, greater than a year), whether the defendant has previous misdemeanor of each crime types (violent, property), the category of charges present in the current case (violent, property, drug, or other), whether the defendant was Black, or male, the number (by crime type) of criminal offenses charged to people living in the defendant's zip code in 2010, household structure and criminal exposure from Finlay et al. (2022c), census tract demographic information from the 2010 ACS (age structure, the percent of the tract that is male, Black, White, and Hispanic), tract child poverty and income distribution.

Using the same set of covariates, we separately estimate two random forest prediction models. First, using the sample of juvenile cases, we estimate the probability that a case ends in (1) no punishment or (2) conviction (delinquency adjudication) and services/punishment in the juvenile system.[30] After estimating this model, we use the model to generate predictions for every case (both juvenile

[29]In random forest models, an ensemble of individual decision trees is used to generate predictions for each tree. Then, a vote is performed across the predicted results and the model selects the final prediction value using a majority vote rule.

[30]While it would be of both theoretical and policy interest to subdivide the second category into the different kinds of sentencing outcomes that can occur in the juvenile caseload, this information was unfortunately unavailable from the data provider.

and adult) in our full sample. While the model has only been trained on juvenile cases, we can use this model to create a prediction for juvenile disposition for every juvenile and adult defendant in our sample since the covariates are common across the full analytic sample. We then perform the analogous exercise for the adult system by estimating a second model based on the adult cases across three sets of outcomes: (1) no conviction, (2) conviction with non-incarceration sentence, or (3) conviction with incarceration sentence.[31] As before, we then use this model, trained on the adult sample, to generate predictions of adult disposition over the full sample (juvenile and adult defendants alike).

This prediction exercise generates five probabilities for every defendant, regardless of whether they were actually prosecuted through the juvenile or adult system: that their case would resolve as (1) no conviction conditional on juvenile prosecution,(2) conviction and services/punishment conditional on juvenile prosecution, (3) no conviction conditional on adult prosecution, (4) conviction and only non-incarceration punishment conditional on adult prosecution, and (5) conviction with incarceration punishment conditional on adult prosecution. In order to evaluate how well our predictions are sorting cases, we produce a confusion matrix of our predicted values compared to realized values, as shown in Table B.1. The "predicted juvenile outcome" for each case is the larger predicted probability between the two potential juvenile outcomes for each juvenile defendant and the "predicted adult outcome" is the largest probability of three potential adult case outcomes for each adult defendant. We see strong performance of the prediction models, with the vast majority of observations appearing along the diagonals (showing that our model is correctly categorizing many of the cases).

Table B.2 demonstrates the importance of non-linearities and interaction terms for our predictions. We show that regressing each prediction on the set of covariates used by the random forest only explains between a quarter and half of our overall

[31]For each model we use 100 estimators and hidden layers with a maximum depth of 15. We set the maximum number of features as the square root of the number of features in the model. We use the Gini impurity criteria to measure the quality of a split. We have a constant learning rate initialized at .0001, an alpha pruning parameter of .0001, and use the Adam solver. We re-weight the prediction sample to equally weight each case outcome.

predictions. In the linear setting, certain features appear to be more important in explaining our predictions. These include race, sex, criminal history, family income, and census tract poverty and crime rates. Other features, like criminal exposure of others in the household, seem less predictive (at least linearly).

As in the traditional RD setting, it is important that our predictions are smooth through the discontinuity. While the predictions are based on the covariates assessed above, it is possible that the random forest introduced some non-linearities that are not smooth. To verify that the random forest did not introduce any non-linearities in the covariates when transforming them into the estimated probabilities, we show that predicted case outcomes are smooth through the discontinuity. As shown in Figure B.4, the random forest did not introduce any evidence of a discontinuity across the discontinuity. That is, similar shares of the populations on either side of the discontinuity are predicted to receive each case outcome, which aligns with the earlier observation that caseload characteristics are balanced across the cutoff threshold.

Table 2.3 shows the first stage of the decomposition exercise. Each column shows the loadings of each treatment onto the set of potential instruments. Each row shows an individual instrument defined as the random-forest-generated probability of an adult case outcome interacted with the probability of a juvenile case outcome times a dummy variable which takes the value of 1 if the case was filed after the defendant was age 17. As one might expect the instruments with adult conviction (adult conviction x juvenile conviction and adult conviction) have large and statistically significant loadings on the endogenous adult conviction case outcome. This means that moving across the age discontinuity from juvenile to adult for defendants predicted to likely have an adult conviction explains much of the variation in who actually receives an adult conviction. Moving to column 2, we see that the coefficients on the instruments with adult conviction are now near 0 or negative. Similarly, the coefficients with adult no conviction are negative. For juvenile no conviction, we see the instruments that include the probability of juvenile no conviction interacted with the defendant being older than 17 (rows 1 and 5 especially) have large negative loadings. What this represents is that moving from the juvenile to the adult system moves these defendants from receiving no conviction in the juvenile system to some adult case resolution.

2.6.3 Findings of mechanisms analysis

We apply this decomposition methodology to better understand which case resolutions (and thus what mechanisms) drive the changes in recidivism and employment seen in the traditional RD exercise in the previous section. Figure 2.5 presents a subset of our results graphically; the full set of findings are presented in Table B.5.[32,33] Our evidence highlights the complex interplay of policy levers and impacts on outcomes that otherwise would be missed using standard regression discontinuity techniques.

Our excluded category are youth who are charged as adult defendants but whose cases are ultimately dismissed. While the choice of excluded category is admittedly subjective, we believe this option provides several interesting and policy-relevant empirical tests: (1) the impact of adult convictions and sentencing outcomes relative to dismissals, and (2) the impact of juvenile charges relative to adult charges.

Relative to an adult case dismissal, adult convictions significantly increase future felony charges and convictions over the next 5 years by 1.6 and 1.8 cases, respectively, and increases the chance of any felony charge by 19 percentage points and any felony conviction by 29 percentage points.[34] For many defendants, however,

[32]In this table, each column represents a different estimated treatment effect relative to an adult defendant with a case dismissal. Each row within the extensive and intensive supercolumns represents a single regression.

[33]As a confirmatory exercise, we also conduct separate RD estimates by predicted adult and juvenile subgroup, which can be found in Table B.3. These results support the conclusions described in our IV results. The first three columns show subgroups that are predicted to have no conviction in the juvenile system and either no conviction in the adult system (column 1), incarceration in the adult system (column 2), or a non-incarceration punishment in the adult system (column 3). Not every combination of juvenile and adult outcome is equally likely; there are only 110 individuals predicted to have their case dismissed in the juvenile system but be incarcerated in the adult system, while there are 2600 who are predicted to be punished in the juvenile system and receive some adult non-carceral punishment. We also provide a simpler subsample analysis in Table B.4, which splits the subsample along various covariates. In Table B.8, we show that our results are robust to the choice of bandwidth used.

[34]This reduction can be seen in declines in total criminal activity for violent, property, and other major offense types: 1.5 fewer violent charges (about 0.7 after restriction to just convictions), 1.6 fewer property charges (1.2), and 1.6 fewer

these increases are neutralized through incarceration, which entirely cancels out the increase in felony recidivism risk stemming from adult convictions.

For labor market outcomes, we observe the opposite dynamic: convictions appear to diminish earnings, which is then reinforced with further declines for those who are also incarcerated. We find especially large impacts for incarceration, with statistically significant and economically meaningful reductions in annual wage income (↓ \$1,968 or 66%), average number of employers – proxied by the number of distinct W-2 information returns – per year (↓ 0.26 returns), and whether the defendant earns sufficient wages to exceed the poverty threshold for a single adult (↓ 6.6 percentage points). Together these results provide further evidence on the potential individual harm generated from adult criminal records (Mueller-Smith and Schnepel, 2021; Agan et al., 2021).

Charging adolescents as juvenile defendants also appears to increase their risk of future felony activity. We observe juvenile prosecution increasing future felony charges and convictions by 1.6 and 1.8 cases respectively and increasing the chance of any felony charge and conviction by 17 and 26 percentage points. Part of what might drive this relationship is that the experience of adult prosecution acts as a form of specific deterrence, which is missing when youth are charged as juvenile defendants. Alternatively stated, juvenile prosecution might impart a false impression of leniency in the adult criminal justice system for youth who are on the cusp of aging into the adult system. Although imprecise, the direction of the coefficients suggest that juvenile prosecution does improve employment trajectories, especially when charges are combined with juvenile convictions (and the corresponding services/interventions that accompany juvenile convictions).

It is remarkable that juvenile convictions, unlike adult convictions, do not appear to significantly worsen outcomes conditional on the impact of being charged. This provides suggestive evidence that convictions do generate differential impacts depending on whether the conviction was made in the juvenile or adult criminal

other charges (1.0). See Table B.5.

justice system. As previously discussed in Section 2.3, there are many legal factors that make these distinct interventions, especially with regard to their long-term implications for holding a criminal record that would show up on a background check and additional rehabilitative services offered to juveniles.

To better understand these dynamics, we plot the evolution of outcomes year-by-year over our five-year follow-up period in Figure 2.6. In this figure, we introduce follow-up impacts on being incarcerated in adult prison (measured from actual institutional confinement records) and observe an immediate and sustained impact of being convicted as an adult defendant and sentenced to incarceration on actual time served in prison. Five years after the initial case filing, adult defendants sentenced to prison spend approximately 140 additional days in prison relative to adult defendants who received case dismissals. There is no meaningful decline in the impact on time confined over time, indicating that our five-year follow-up period is insufficient in duration to fully capture the long-term implications of being charged as an adult defendant as many appear to still be imprisoned at the end of our follow-up period. In contrast, the sizeable gains to crime prevention arising from incarceration previously discussed are mostly concentrated in the first two years after initial case filing.

We find that juvenile prosecution creates an initial protective effect for defendants from ending up in prison, although this wanes within three years following initial case filing. Given that the modest increase in felony convictions appears sustained throughout the follow-up period, something other than just incapacitation likely drives the increase in felony activity observed for juvenile defendants. There is a modest short-run increase in earnings above the poverty line, a result that is echoed with less precision in later years, which suggests some positive outcomes are achieved in spite of the increase in criminal charges for this group.

2.6.4 Robustness exercises

To ensure that our conclusions are not driven by arbitrary functional form or sample construction decisions, we explore a variety of robustness checks in Table B.8. Our findings are qualitatively unchanged when adjusting the bandwidth from 1.5 years to 2.5 years as well as incorporating triangular weights with our main bandwidth of 2 years. The results are similarly robust to fully interacting the random forest predictions with age and jointly with the discontinuity and age. While less well-powered, our results are also robust if we use the covariates entering the random forest in a standard regression to generate the instruments (i.e. removing the non-linear elements).

In addition, as described in Caetano et al. (2023), identification in this setting requires assuming treatment effect homogeneity conditional on the complier population generated from our vector of interacted probabilities. For example, we assume that those tried in juvenile court and not convicted are expected to have the same treatment effect, regardless of whether some may be more likely to be convicted in an adult court than others. This is a strong assumption which warrants careful consideration.

We explore the viability of this assumption through three complementary exercises. First, we rotate through dropping one of each of our six instruments and re-estimate our model, an exercise that is only possible because our main results are overidentified (6 instruments for 4 interventions). Dropping instruments will change the composition of the complier population, allowing us to test the null hypothesis of treatment effect homogeneity across the six different leave-one-out exercises. Columns 2 through 7 of Table B.9 shows our findings. The estimated coefficients are all qualitatively quite similar, and a formal test of their joint equality fails to reject the null hypothesis (column 8), which supports our homogeneity assumption.

The second exercise removes defendant characteristics from the random forest prediction algorithms. Recall that we only require homogeneity conditional on the information contained in the probability scores. The thought behind this exercise is to eliminate traits that might be most likely to generate to treatment effect heterogeneity (e.g., defendant race, defendant sex, defendant criminal history, defendant offense type, etc.). While this will weaken the strength of the prediction models and our first stage estimates, the resulting findings might have weaker homogeneity assumptions given that our identification only relies on differences in defendant disposition outcomes stemming from household and neighborhood characteristics. Column 9 of Table B.9 shows our results, which largely replicate our findings from the full model. In fact, we are unable to reject the null hypothesis that the estimated effects are the same between the full and "slim" models (column 10), consistent again with the required homogeneity assumption.

As a final test, we can consider the p-values from Sargan-Hansen J-test for over-identification in our core results. As discussed in Caetano et al. (2023), a rejection of the null is a sign of potential violations of the homogeneity assumption. In Table B.5 (see notes), we fail to reject the null in the Sargan-Hansen J-test, which provides our last piece of evidence in support of our homogeneity assumption.[35]

2.7 Cost-benefit analysis of policy counterfactuals

We conduct a cost-benefit analysis to evaluate a range of policy counterfactuals that echo recent debates in criminal justice reform efforts.[36] A strength of the decomposition presented in Section 2.6 is that we can use our estimates from that exercise to go beyond simply identifying the net impact of the shift from juvenile to adult prosecution to also considering policy interventions that might isolate specific components of the full array of changes that occur at the age of majority threshold. We evaluate four scenarios: (1) raising the age of criminal majority, (2)

[35]In spite of these empirical tests, some may remain unconvinced. Caetano et al. (2023) derive and bound the bias if the homogeneity assumption is violated. When homogeneity fails, it is possible to misattribute changes in outcomes to incorrect interventions. While the overall RD remains valid, the mechanism decomposition may be cross-contaminated. The degree of bias depends importantly on the variance of the treatment effect heterogeneity in the marginal population, which in our setting appears to be relatively small given the empirical tests described in this subsection.

[36]Given our research design, these exercises are most relevant for first time felony offenders. Whether these findings extend to those with repeated contact the justice system remains a question for future research.

altering the dismissal rate for adult defendants to match that of juvenile defendants, (3) eliminating the use of incarceration for adult defendants, and (4) making adult criminal records have the same expungement options as juvenile criminal records. Across all of these scenarios, the evidence speaks to the net cost for changing policies for youth (i.e. teenagers) charged in the adult system; extrapolating these exercises to the entire adult caseload requires stronger assumptions that we do not believe are satisfied given our research design.

The first policy counterfactual we consider was implemented in Michigan in 2021 and our analysis will inform whether the state can expect this to yield positive social returns. The second exercise allows us to consider whether reforms to how much leniency is granted to young defendants and thus changes to the procedures that might generate this leniency might be socially beneficial. The third exercise addresses calls to reduce the carceral system's reach for adolescents and highlights a major difference between the juvenile and adult systems. The final counterfactual explores the implications of differential access to more immediate record sealing in the juvenile system and reflects recent reforms in Michigan (and elsewhere) to limit the impact of criminal records on other outcomes and provide work readiness training.[37]

We consider the following cost and benefit components: the direct costs of implementing the program, savings from tax revenue from earnings, government savings from changes in recidivism behavior, benefits for the defendants themselves, and savings for future victims. We sum over the first three components to provide an estimate of the net impact per defendant to the government budget constraint. Costs and benefits are measured over the five-year outcome period in our main analyses. While changes to recidivism or earnings are most pronounced early on in our follow-up period limiting the bias resulting from an incomplete follow-up window, changes to time spent in prison remain prominent even five years later

[37]Treating adult criminal records like juvenile records for expungement purposes, however, is a significantly more substantial change than has been considered in recent legislation on clearing criminal records. Whether similar gains could be achieved with a more modest approach (e.g. leaving in place longer waiting periods for expungement) is a question for future research.

(Figure 2.6). Consequently, government savings are likely understated, especially in the first and third exercises.

All four of these scenarios create savings for the government's balance sheet and taxpayers. Immediate savings in program costs are observed, especially for policy options that reduce the reliance on prison. Some of these gains have to be weighed against other government costs associated with increasing future illicit behavior, which tax payers must cover through additional expenses in law enforcement, courts, and correctional supervision.

The largest total budgetary gains ($5,098 per defendant) are observed for the eliminating prison sentences for teenage adult defendants policy option, although this would only impact 18% of the caseload. Raising the age of criminal majority also appears attractive, creating government savings of $4,966 per defendant, especially considering it would have a substantially wider reach in the justice-involved caseload. In both of these policy counterfactuals, the net gains are largely achieved by eliminating costly spending in the justice system on prison.

Similarly, defendants exclusively gain from each of the four policies considered, although some more than others. We include two components in this estimate: (1) the freedom costs of incarceration,[38] and the after-tax wages received by a defendant.[39] Increasing the age of majority creates the largest gains for defendants ($7,117), followed by expanding juvenile record-sealing ($3,899) and eliminating adult incarceration for teenage defendants ($3,674). Interestingly, among these latter two options, the gains in the first are largely driven by improvements in personal income, while in the second scenario defendants largely benefit from avoiding significant time in prison.

While so far all of these policies appear exclusively beneficial for society, the challenge comes when incorporating impacts to future potential victims. Two of the scenarios, raising the age of majority and eliminating adult incarceration, increase total future crime albeit through different hypothesized mechanisms (i.e., ↓ specific

[38]We utilize willingness-to-pay estimates from Abrams and Rohlfs (2011) for the value of a day in prison.

[39]Consistent with our treatment of the government balance sheet, we assume a 10% tax rate for our sample.

deterrence versus $\downarrow$ incapacitation). There are non-trivial increases in both property and violent crime, but given existing estimates on the social costs of crime,[40] the changes in violent crime dominate this exercise. Raising the age of majority creates \$12,053 additional costs per defendant shouldered by future victims, and eliminating adult incarceration for teenagers similarly generates \$11,566 in victim costs per defendant.

How to value these trade-offs, especially when beneficiaries and victims might be drawn from different socio-economic backgrounds, is a normative question best left to policymakers. For completeness, we provide a simple summation totaling across the three considered perspectives (taxpayers, defendants, and future victims) but recognize that other weighting schemes may be preferable in practice due to their distributional implications.

Overall, we find that expanding juvenile record sealing options to teenage defendants in the adult criminal justice system has the largest net social impact. While not the most beneficial option for either tax-payers or defendants, it manages to substantively improve their outcomes while also reducing future crime for potential victims. A similar but less effective "everyone wins" scenario is observed for the increasing dismissal rates for marginal adult defendants option considered.

Raising the age of criminal majority presents the largest gains for defendants but also the largest losses for future potential victims. With equal weighting, this counterfactual comes out as roughly neutral in social welfare. In contrast, eliminating incarceration for teenage adult defendants generates similar losses for future victims but worsens social welfare due to relatively smaller defendant benefits. In this setting, even without incarceration, adult conviction records negatively impact the future trajectories of defendants. However, if policymakers are willing to value taxpayers and defendants at a rate of at least 124% that of future potential victims, this still would be attractive to invest in.

The interplay of costs and benefits highlights the complexity of running a

[40] We rely on (Cohen and Piquero, 2009) for our victim cost estimates.

criminal justice system. For example, conditional on how adult criminal histories operate in this setting, incarceration could be viewed as a productive investment to limit the criminogenic effect of conviction records. However, if society was able alter the impact of the conviction record itself (e.g. removing the scarring mark of a criminal record), incarceration may no longer appear to be as valuable to society.

2.8 Conclusion

Adolescence is a critical period. For first-time felony defendants, their treatment at the hands of the criminal justice system may set an individual on starkly different life-long paths. Both the age at which defendants are treated as adults and what punishments and services they receive from the resolution of their case impact their future earnings and criminal behavior.

This paper makes use of a detailed decomposition of different treatments across the juvenile and adult criminal justice systems relying on previously unavailable rich administrative data. While data limitations in previous work only allowed for speculation regarding the underlying mechanisms, we are able to provide direct evidence on this matter. Incapacitation does play a significant role in reducing aggregate crime rates across the age of majority threshold. In fact, it also compensates for the increased risk of recidivism that comes with a permanent adult criminal record, a fact that has been missed by prior work. But, at the same time, accomplishing this reduction in recidivism is quite expensive due to both the significant program costs and the negative consequences for labor market outcomes in the population.

Our data allows us to provide previously missing measures of the consequences and prevalence of different case outcomes between the juvenile and adult criminal justice systems and the characteristics of those who are charged in either system. In addition, linking juvenile and adult records to a variety of other information allows us to show that adolescents facing different treatment in the criminal justice system are set on starkly different life paths in terms of employment and future time incarcerated. Understanding these hard-to-reach populations would not have been possible without access to the Census data linkage infrastructure.

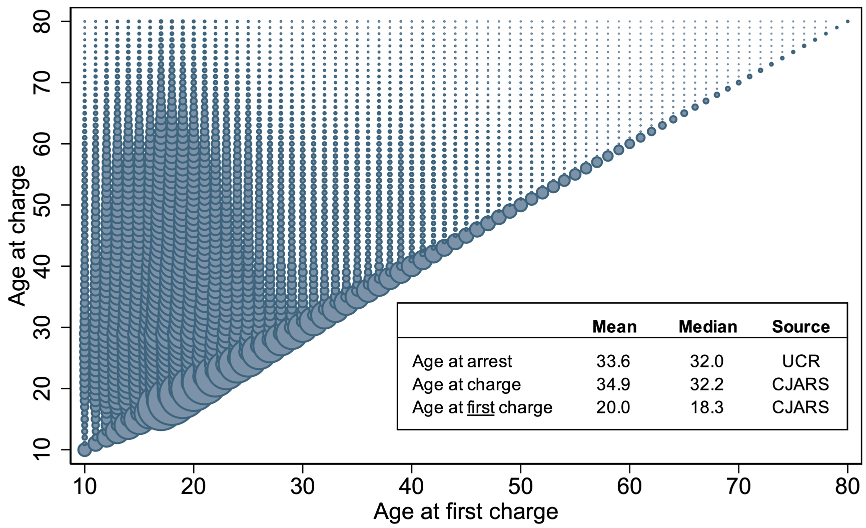
The methodology we develop to answer these questions has broad applicability beyond our specific research question. The combination of machine learning and regression discontinuity could be leveraged in a variety of interesting settings where decomposing multiple simultaneously changing mechanisms at a single discontinuity could provide a deeper understanding of the question or policy being studied.

Our findings have immediate policy relevance. In October 2021, Michigan raised their age of majority from 17 to 18 years old, and our analysis suggests the overall impact will largely be neutral. That said, the policy change should create concentrated benefits for young defendants who will be prosecuted as juveniles under the new policy environment, with distributed victim costs shared across the rest of the population. Our analysis of policy counterfactuals provides guidance on future directions for policy reform in the justice system. In particular, it suggests that limiting adult convictions and increasing opportunities for expedited record sealing for young defendants prosecuted as adults might generate substantial gains shared throughout society.

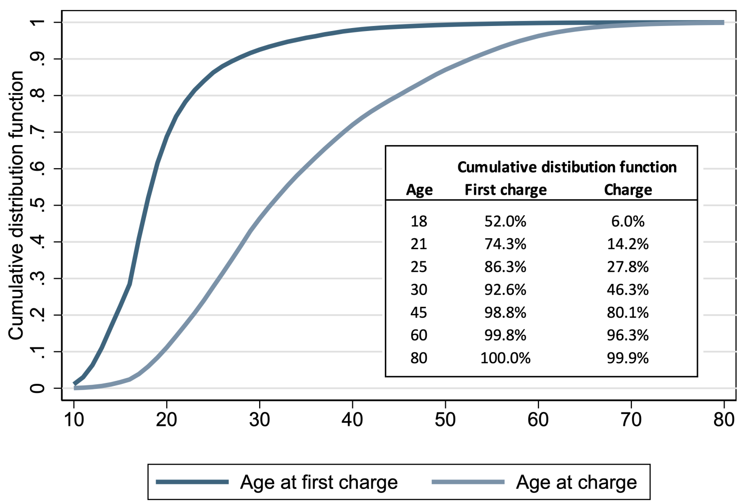
Figures and Tables

Figure 2.1: Age at (first) criminal justice involvement

A: Comparing age at charge and age at first charge



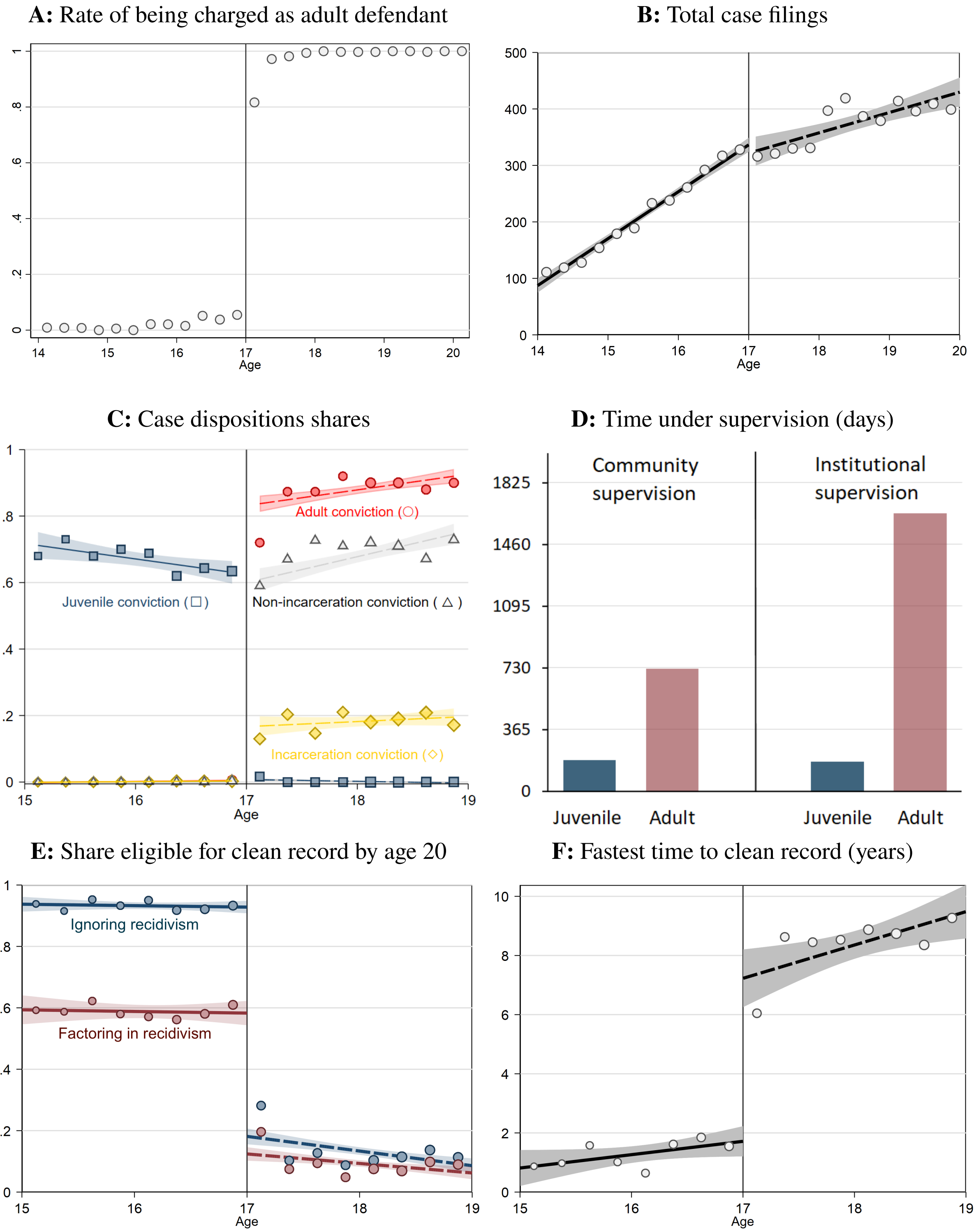
B: Cumulative distribution functions of age at charge and age at first charge



Source: CJARS data and juvenile supplement held at the University of Michigan. Data underlying the UCR calculation are from the 2019 Uniform Crime Report.

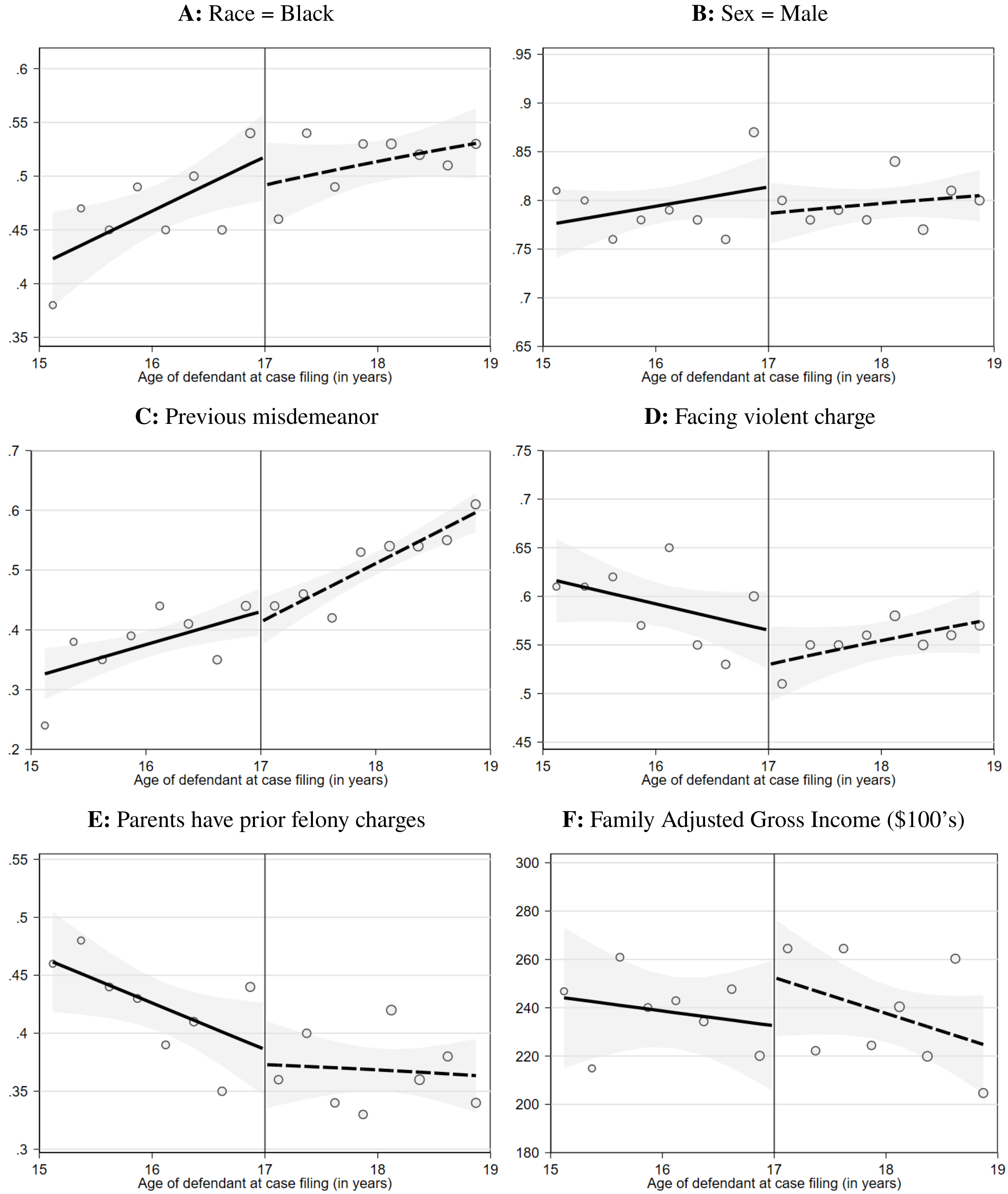
Notes: The sample has been binned with larger dots representing more individuals in the cross-section of the caseload within CJARS. CJARS data is constructed based on the cross-section of individuals charged with juvenile and adult, misdemeanor and felony criminal offenses in Michigan in calendar year 2019 and their corresponding observed criminal histories. Our juvenile justice records in Michigan only extend back to 2010, and the adult criminal justice records often start in the 1990's (start dates vary by county and circuit/district courts). Due to these coverage limitations, we assume a stable age/crime profile and use the observed distribution for non-censored populations to fill in censored data. For those over age 21 in 2019 (potentially impacted by the juvenile justice record censoring), we reallocate the observed density below 20 to fit the observed non-censored distribution from younger cohorts between ages 10 to 20 years old. For those over 41 (potentially impacted by the adult justice record censoring), we iteratively reallocate the full observed distribution to fit non-censored adult age/crime profiles from younger cohorts, with an additional lagged dependent variable model to estimate the decay in the density for previously unobserved first ages in the distribution. The median and mean age of offense are based on a kernel density (bandwidth of 1.5 years) fit across this binned distribution. The UCR reports binned ages for age at arrest.

Figure 2.2: First stage, caseload density, and treatment of juvenile and adult defendants



Source: CJARS data and juvenile supplement held at the University of Michigan.
Notes: The estimates are based off of a sample of all individuals whose first observed felony charge was between 2011 and 2014 in Wayne County Michigan. In panel D, adult results are calculated from the adult observations used elsewhere in the analysis and are calculated conditional on receiving either probation or detention. Juvenile results are based on reported 2013 values from Chaney and Reed (2018), for the full population of juveniles receiving either probation or detention because microdata on more detailed juvenile sentencing is unavailable. In order to best match the reported juvenile statistics, the median is shown for probation and the mean for detention.

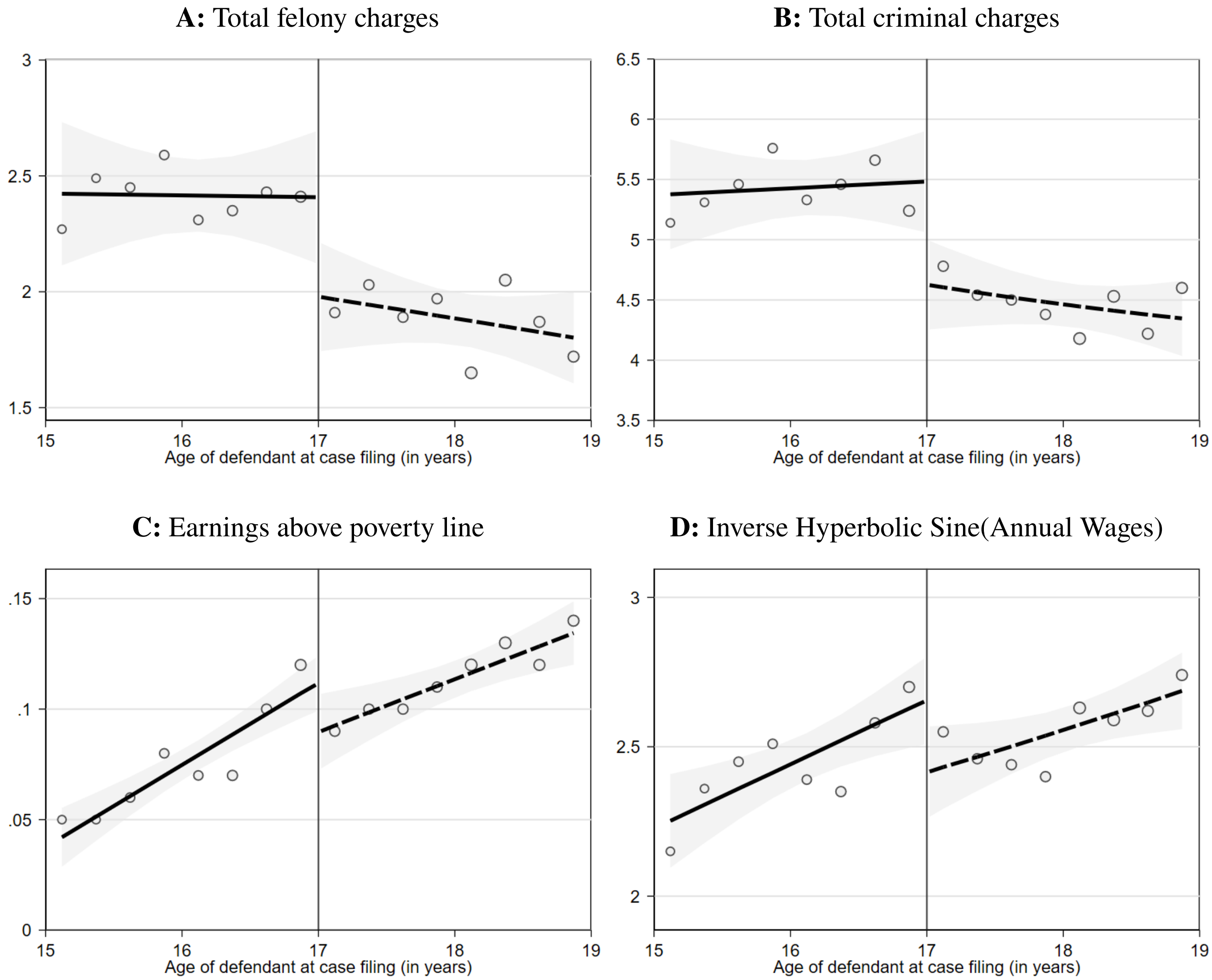
Figure 2.3: Evaluating balance in caseload composition



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

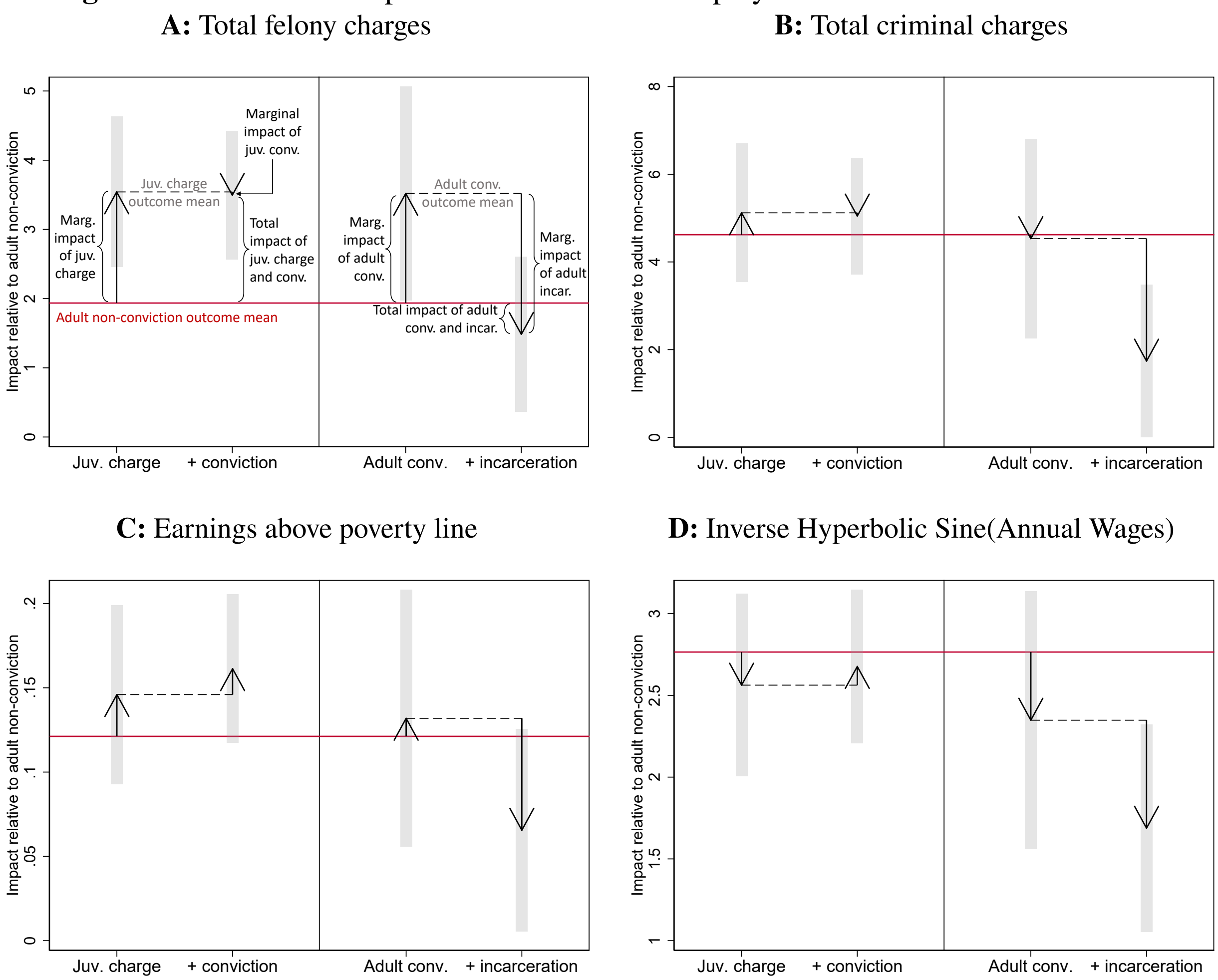
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Previous misdemeanor is an indicator if the defendant had a previous misdemeanor charge. Facing violent charge is an indicator if the defendant is facing a charge for a violent crime within the set of current charges. We construct a measure of parents' exposure to a felony charge in the child's 2010 residence (including biochild-parent; adoptedchild-parent; stepchild-parent; fosterchild-parent; and; unclassified child-parent). We also use information from the 1040 filing to generate the income of the person claiming the defendant as a dependent (coded as 0 if no one claimed the defendant) in 2010.

Figure 2.4: Reduced form: recidivism and employment outcomes across the cutoff



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regression also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

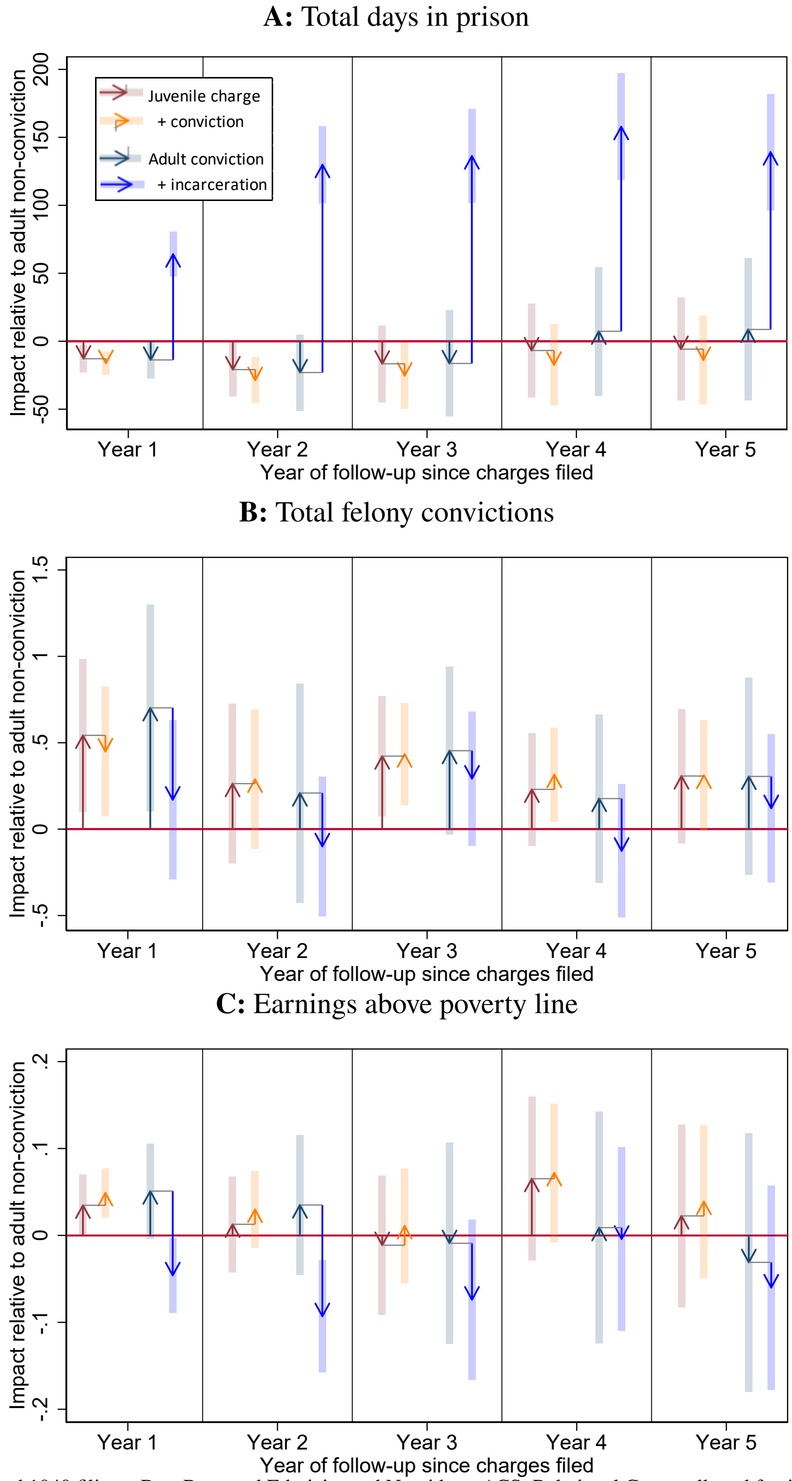
Figure 2.5: Mechanism-specific recidivism and employment treatment effect estimates



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). Sargan-Hansen J for 5-year felony conviction recidivism (intensive) is 3.737, p-value 0.154 and for IHS(Average yearly income over next 5-years), 1.923 p-value 0.382). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022) and #CBDRB-FY23-088 (approved 12/12/2022). Estimates are from an RD decomposition (instrument age 17+ interacted with probabilities) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Figure 2.6: Evolution of mechanism-specific impacts over the follow-up period



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates are from an RD decomposition (instrument age 17+ interacted with probabilities) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table 2.1: Balance of individual, household, neighborhood, and predicted characteristics

Dependent variable	RD Pt Est (Std Error)	Dependent variable	RD Pt Est (Std Error)
Caseload Density Test			
Total caseload size	-0.007	Census tract percent youth poverty	-2.515
	(0.053)		(1.751)
Youth Characteristics		Census tract median household earnings	1779
Black	-0.029		(1408)
	(0.034)	Census tract mean household earnings	2293
Male	-0.031		(1523)
	(0.027)	Census tract percent white	-2.295
Any misdemeanor history	-0.02		(2.953)
	(0.033)	Census tract percent black	2.117
Number of violent misdemeanors history	-0.025		(3.149)
	(0.03)	Census tract percent hispanic	-0.288
Number of property misdemeanors history	0.00		(0.736)
	(0.038)	Census tract percent male	0.292
Days since first charge	-0.077		(0.337)
	(0.05)	Census tract percent age 15 to 19	-0.325
Facing violent felony charge	-0.04		(0.259)
	(0.033)	Census tract percent age 20 to 24	0.019
Facing property felony charge	0.017		(0.213)
	(0.033)	2009-10 violent charges in zip	-0.007
Facing drug felony charge	0.036*		(0.052)
	(0.021)	2009-10 property charges in zip	-0.003
Household Characteristics			(0.035)
Total parents in 2010 house	0.004	2009-10 drug charges in zip	-0.002
	(0.05)		(0.024)
Total other adults in 2010 house	-0.073	2009-10 public order charges in zip	-0.004
	(0.057)		(0.006)
Family 2010 Adjusted Gross Income ($100's)	23.27	**Predicted Indices**	
	(21.91)	Prob adult no conviction	-0.008
Household 2010 Adjusted Gross Income ($100's)	14.27		(0.01)
	(35.21)	Probability adult conviction	0.008
Previous charge in house	-0.031		(0.01)
	(0.026)	Probability adult incarceration	-0.01
Parent previous charge in house	-0.042		(0.013)
	(0.029)	Estimated probability of no juvenile punishment	0.018
Previous felony charge in house	-0.032		(0.014)
	(0.032)	Estimated probability of juvenile punishment	-0.018
Parent previous felony charge in house	-0.014		(0.014)
	(0.033)	P(Adult conv.) x P(Juv no punish)	0.016
Previous felony conviction in house	-0.068**		(0.012)
	(0.033)	P(Adult conv.) x P(Juv punish)	-0.009
Parent previous felony conviction in house	-0.009		(0.013)
	(0.024)	P(Adult incarc.) x P(Juv no punish)	0.003
Previous incarceration in house	-0.019		(0.006)
	(0.024)	P(Adult incarc.) x P(Juv punish)	-0.013
Parent previous incarceration in house	-0.004		(0.009)
	(0.014)	P(Adult no conv.) x P(Juv no punish)	0.002
Neighborhood Characteristics			(0.005)
Census tract percent poverty	-0.846	P(Adult no conv.) x P(Juv punish)	-0.01
	(1.267)		(0.008)

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Coefficients are estimated using a linear IV approach with robust standard errors restricted to sample cases with defendants between the ages of 15 and 19. Whether a case occurs prior to or after age 17 is used as an instrument for whether the case is in the adult system. The IV estimate of the case being in the adult system is displayed. Case load density is tested using a McCrary test with default parameters.

Table 2.2: Impact of adult prosecution on 5 year recidivism and employment outcomes

	Extensive			Intensive		
	RD Pt Est (SE)	IV Pt Est (SE)	Juv. Mean	RD Pt Est (SE)	IV Pt Est (SE)	Juv. Mean
First stage:						
Charged as adult defendant	0.860***		.017			
	(0.002)					
Recidivism:						
Any charge	-0.0655***	-0.07593***	0.8538	-0.8847***	-1.019***	5.368
	(0.0219)	(0.02546)		(.2855)	(0.3288)	
Any conviction	-0.0426*	-0.04937*	0.7498	-0.5205**	-0.5759**	3.998
	(0.0253)	(0.02945)		(.2539)	(0.2809)	
Felony charge	-0.0333	-0.0386	0.5058	-.4359**	-.4840**	2.393
	(0.0278)	(0.03223)		(0.1968)	(0.2185)	
Felony conviction	-0.0255	-0.0296	0.4646	-0.2963*	-0.3188*	1.954
	(0.0278)	(0.03223)		(0.1757)	(0.1891)	
Recidivism by type of offense:						
Violent charges	-0.0087	-0.01016	0.4395	-0.3644**	-0.4237**	1.877
	(0.0278)	(0.03227)		(0.1668)	(0.1939)	
Property charges	-0.0360	-0.0419	0.4224	-0.3161*	-0.3675*	1.746
	(0.0279)	(0.0324)		(0.1657)	(0.1927)	
Drug charges	-0.0488*	-0.0568*	0.2897	-0.2674**	-0.3109**	0.9426
	(0.0253)	(0.02939)		(0.1161)	(0.135)	
Other charges	-0.0888	-0.1032	0.7525	-0.7003***	-0.8143***	3.604
	(0.0258)	(0.02997)		(0.2236)	(0.26)	
Employment:						
Average # of W-2s per year	-0.0545**	-0.07597**	0.3919			
	(0.0238)	(0.03322)				
Earnings above poverty line	-0.0251**	-0.02666**	0.08072			
	(0.0117)	(0.01248)				
IHS(Annual Wages)				-0.2349**	-0.2856**	2.475
				(0.1026)	(0.1248)	
Annual W-2 Wages ($100's)				-5.711*	-6.738*	32.27
				(3.289)	(3.881)	

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). Earnings in 2020 dollars. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table 2.3: First stage estimates of RD decomposition

	First stage on outcome:			
	(1)	(2)	(3)	(4)
Interaction of 17+ dummy and:	Adult conviction	Adult incarceration	Juvenile no conviction	Juvenile conviction
P(Adult conv.) x P(Juv no conv.)	0.8321***	0.04727	-1.069***	0.2397***
	(0.0496)	(0.03167)	(0.04932)	(0.03104)
P(Adult conv.) x P(Juv conv.)	1.034***	-0.1516***	0.0732***	-1.007***
	(0.02949)	(0.02199)	(0.02081)	(0.0269)
P(Adult incarc.) x P(Juv no conv.)	0.4357***	0.6775***	-0.01151	0.2224**
	(0.1596)	(0.151)	(0.1438)	(0.1053)
P(Adult incarc.) x P(Juv conv.)	0.2083***	1.278***	0.1961***	-0.1352*
	(0.07961)	(0.07928)	(0.05066)	(0.07025)
P(Adult no conv.) x P(Juv no conv.)	0.1181	-0.4428***	-1.499***	0.4818***
	(0.1529)	(0.1187)	(0.1399)	(0.1191)
P(Adult no conv.) x P(Juv conv.)	-0.226***	-0.5915***	0.4853***	-1.32***
	(0.07161)	(0.06029)	(0.05105)	(0.05988)
F-Stat	720	110	320	680
Observations	4700	4700	4700	4700

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Coefficients are the first stages of decomposition exercise with robust standard errors restricted to sample cases with defendants between the ages of 15 and 19. Whether a case occurs prior to or after age 17 interacted with each variable indicated in the first column is used as an instrument for whether the case is in the adult system. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table 2.4: Cost-benefit of policy counterfactuals per impacted defendant (2020 dollars)

	Policy Counterfactuals			
	Raise the age of criminal majority	Increase adult dismissal rate to match juvenile rate	Eliminate adult incarceration for young adults	Make juvenile record sealing available to young adults
Share of young adult caseload affected:	100%	19%	18%	88%
A) Net impact to government budget:	$4,966	$2,523	$5,098	$4,245
Average program savings for government	$9,102	$1,427	$14,879	$0
Tax revenue from earnings	$326	$9	$188	$345
Government savings from changes to future crime:				
– Law enforcement	-$9,178	$1,679	-$5,742	-$1,107
– Court resources	-$1,396	$153	-$839	-$207
– Correctional supervision	$6,112	-$744	-$3,388	$5,214
B) Savings for potential victims:	-$12,053	$1,240	-$11,566	$4,689
Property crime reduction (negative for gain)	-$547	$15	-$275	-$141
Violent crime reduction (negative for gain)	-$11,506	$1,224	-$11,291	$4,830
C) Benefits for defendants:	$7,117	$116	$3,674	$3,899
Non-wage benefits of non-incarceration freedom	$4,185	$37	$1,981	$798
Post-tax personal income	$2,932	$79	$1,694	$3,101
TOTAL (A + B + C)	**$31**	**$3,878**	**-$2,794**	**$12,834**

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relations and exposure from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022).

CHAPTER III

Who Benefits from Corporate Tax Cuts? Evidence from Banks and Credit Unions around the TCJA

(With Edward Fox)

3.1 Introduction

The central provision of the 2017 Tax Cuts and Jobs Act (TCJA) cut the corporate tax rate from 35% to 21%.[1] This rate cut is estimated to reduce federal revenue by more than $1 trillion over the next decade (Congressional Budget Office, 2018).[2] Understanding who ultimately benefits from this provision is thus a critical policy question. There is, however, little scholarly consensus on who bears corporate taxes and in turn who will get the benefit of the rate cut.

As Auerbach (2018) recently observed, part of the reason for the divergence in estimates of who bears the corporate tax arises from the difficulty of identifying "credible natural experiments for corporate tax reforms or to control for the many developments occurring within countries at the same time as corporate tax changes." Studies at the state or local level often provide cleaner identifying variation (e.g., Fuest et al. (2018); Suárez Serrato and Zidar (2016)) than studies of national-level

[1]We are grateful to Jim Hines, Zachary Liscow, and participants at the American Law and Economic Association 2022 conference, the National Tax Association 2022 meeting, the University of Texas Law and Economics workshop, the 2023 NYU-UCLA-Berkeley Tax Conference, and many seminar participants at the University of Michigan.

[2]Likewise, the Act cut taxes on owners of pass-through businesses, dropping the top rate (on qualifying income) from 39.6% to 29.6%, by adding §199A and also cutting the top marginal rate from 39.6% to 37%. The Act also reorganized the U.S. international taxation of businesses. These latter changes are unlikely to directly affect the small and medium size banks we study here.

corporate tax changes, which use firms in other countries as a control. But capital flows much more easily across subnational borders than national ones. This higher capital elasticity will tend to reduce capital's share of subnational corporate taxes, making it a potentially unreliable indicator for the incidence of national corporate and other capital taxes. As a result, it is important to study large national corporate tax changes like the TCJA directly, but it is also difficult to find good control firms, particularly because the Act reshaped both corporate and pass-through taxation of U.S. firms.

We contribute a partial solution to this problem by studying a novel natural experiment: looking at the effect the TCJA on banks using credit unions as controls. Credit unions were not taxed both before and after the Act. Banks and credit unions compete with each other to attract deposits, provide banking services, and make residential and consumer loans (DeYoung et al., 2019; DiSalvo and Johnston, 2017). Credit unions are thus often quite similar to small and medium size banks, aside from their tax status, making them a natural control group.[3]

We gather quarterly data on the universe of U.S. credit unions and banks from 2013-2019. We then run difference in differences ("DD") regressions around the TCJA. In the DD regressions, we find an economically and statistically significant uptick in the rates paid on deposits by banks after the TCJA. We estimate that the TCJA tax cut increased the price for-profit banks pay on deposits by about 0.8 basis points. When we measure the intensity of treatment by the decrease in each bank's taxes compare to the pre-period, we find that a 100 dollar decrease in taxes generates a 74 dollar increase in returns to capital of which 22 dollars are deposit expenditures. By contrast, we see little evidence that the TCJA increased wages for employees, investments in fixed assets by banks, or lowered loan rates for borrowers from banks.

[3]As the Independent Community Bankers Association puts it "The differences [between community banks and credit unions], are much less pronounced than their operational commonalities: credit unions and community banks provide similar financial products, compete for the same customers, and report to parallel federal regulatory agencies." This should be taken with a grain of salt given the incentives of the organization making the statement, but academic work suggests in a lot of ways it is accurate.

The apparent pass-through of the tax cut only to depositors is a bit difficult to interpret. Depositors have a dual role at banks: they are both lenders of capital to the bank and at the same time often customers of the bank's payment services. Given that we see relatively little pass-through of the tax cut to other bank customers (i.e., borrowers), we tentatively suggest that the pass-through to borrowers is likely in their capacity as capital providers rather than an implicit cut in the price of payment services. In turn, this suggests that capital-both equity and debt providers-bear the corporate tax here (Harberger, 1962; Fox, 2020).

We thus contribute to the always active literature on corporate tax incidence and more specifically the emerging literature on the effect of the TCJA's changes to the income taxation of businesses (e.g. Hanlon et al. (2019); Dowd et al. (2020); Gale and Haldeman (2021)). We believe our identification strategy for isolating the causal impact of the Act on wages, investment, capital lenders, and customers is among the cleanest thus far put forward to analyze the incidence of the corporate rate cut. In addition, firms owned by suppliers (e.g., Land O'Lakes), customers (e.g., REI), or lacking owners altogether (e.g., Kaiser Permanente) compete with traditional enterprises owned by shareholders in a variety of industries (Hansmann (2000)). Nevertheless, our paper is the first we are aware to use the different ways these firms are taxed (or sometimes not taxed) to help identify the incidence of changes to taxing shareholder-owned businesses. In other contexts, this strategy may be useful going forward.

A few caveats are worth discussing at the outset as well. Most specifically, small and medium size banks focus more on lending to businesses than credit unions, which traditionally have primarily made home and consumer loans. We believe this likely explains the noisiness of our estimates regarding interest rates on banks' overall loan portfolio and make us less confident we have isolated the effect of the Act on this margin. More generally, our method will not capture what might be loosely termed "quasi-general equilibrium" effects of the TCJA. That is, if the TCJA increased economic activity outside the banking sector, this might equally lift the activity of banks and credit unions, and therefore, in our difference in difference framework, we will not detect the effect. With that said, the smaller the (quasi-partial-equilibrium) effects we measure, the less likely it is that there would be important general equilibrium effects. In addition, if the nature of competition between credit unions and banks results in credit unions following bank pricing for loans and deposits, we may understate the effect of the Act as banks cut interest rates on loans in reaction to their lower cost of capital, and credit unions follow suit.

Finally, there need not be a single answer to the incidence of the TCJA rate cuts across firms or industries. We are looking at firms (small and medium size financial intermediaries) and markets (making home and small business loans and taking deposits) that are largely domestic. Depending on the model, this domestic nature may shift incidence of the tax toward capital compared to industries where production is more easily mobile. Our findings therefore may not be fully generalizable to other industries. To better understand several of these potential limitations, we implement a two-industry (banking and the rest of the economy), two-sector (corporate and non-corporate) model and demonstrate how generalizable our empirical findings might be under various assumptions.

The structure of the paper is as follows: Part 3.2 describes the institutional background, Part 3.3 describes the empirical approach and identification strategy, Part 3.4 discusses the data, and Part 3.5 presents the results, Part 3.6 calibrates a model, and Part 3.7 briefly concludes.

3.2 Institutional Background

3.2.1 Credit Unions and Banks

Credit unions are financial cooperatives which are owned by their depositors, known as "members." Membership in a given credit union is limited to those who share common bond(s). For many credit unions that common bond is employment at the same firm or a particular industry, or residing in the same city or neighborhood. Like banks, credit unions take deposits and make loans. Credit unions work somewhat differently than banks along a couple dimensions; for example, firm governance.[4] In addition, credit unions are run to benefit their member/owners largely through their transactions with the firm-i.e. the credit union paying members higher deposit rates or offering them lower rates on loans.[5] By contrast, in a simple model, banks are thought to maximize profits and return them pro-rata to shareholders, and do not adjust prices away from profit maximization in their dealings with shareholders.

While credit unions are often small institutions, in aggregate they make up a sizeable fraction of the U.S. banking sector. In the 4th quarter of 2019, there were 5,349 credit unions, with $1.43 trillion of assets (2010 dollars), which represents 7.8% of all assets belonging to firms in the banking sector.[6] Likewise, as shown below in our description of the data, many credit unions are similar in size to small and medium size banks. Credit unions offer their members financial services that largely parallel those of banks: checking and savings accounts, certificate of deposits (CDs), home mortgage loans and auto-loans and other consumer credit. Likewise, banks and credit unions are regulated by parallel agencies to ensure safety and soundness: for credit unions, the National Credit Union Administration (NCUA) and for banks, the FDIC, OCC and Federal Reserve. In addition, deposits are federally insured at both credit unions (via the NCUA) and at banks (via the FDIC).

Despite credit unions using a somewhat different governance mechanism and approach, previous work has provided substantial evidence that small and mid-sized commercial banks are in direct competition with credit unions in a variety of products

[4]As at banks, members govern the credit union by electing a board, which in turn chooses and supervises the full-time management. However, unlike in banks, voting at credit unions is usually on the basis of one member one-vote, rather than proportional to the capital contributed by the owner. This potentially exacerbates the collective action issues already problematic at shareholder owned firms with dispersed owners. As a result, credit union governance is often even more management driven than at banks (Goth et al., 2012).

[5]As a result, credit unions are sometimes referred to as non-profits, but this is not strictly true in the sense that credit unions can return profits to members in the form of dividends, known as "patronage dividends." About one in ten credit unions pay a patronage dividend in a given year (DeYoung et al., 2019). In our analysis, we use variables which include such patronage dividends as payments to depositors.

[6]Defined here as all banks, thrifts, and credit unions. Unless otherwise noted, we use "banks" to mean banks and thrifts.

including consumer credit, savings products, and payment services (DeYoung et al., 2019; DiSalvo and Johnston, 2017; Tokle and Tokle, 2000; Feinberg, 2001; Hannan, 2003). One important difference, however, is that credit unions traditionally have made a much smaller percentage of their loans to businesses. [7]

This leads us to expect that, but for the tax changes discussed below, that the prices paid on deposits, and wages paid to employees will evolve in parallel at both sets of institutions. This is perhaps less true of the average interest rate on the overall loan portfolio of banks and credit unions, because of the different importance of business loans.

3.2.2 Taxation of Banks and Credit Unions and the 2017 TCJA

Federal and state chartered credit unions are exempt from all taxes, apart from property taxes (12 U.S.C. §1768). This exemption dates from 1937 (Tatom, 2005). The traditional justification for not taxing credit unions is that they are operated largely on a not-for-profit basis and help provide access to financial services to otherwise under-banked, low- and moderate-income individuals. Treasury considers this treatment of credit union a tax expenditure which costs the federal government $2 billion per year, about the same size as the estimated cost to the fisc of carried interest. The non-taxation of credit unions was unaffected by the TCJA.[8] It is unclear whether this tax treatment serves its purpose as DeYoung et al. (2019) and DiSalvo and Johnston (2017) cast some doubt on whether in fact credit union customers are more likely to be lower or moderate-income, finding if anything they are higher income in many product categories than customers of small and medium size banks.

By contrast, banks are taxed under the corporate tax (known in tax argot as C-corporations) or pass-throughs as the case may be, with about 2/3rds being

[7]In addition, credit unions have been less likely to securitize their residential loans than small and medium size banks (DiSalvo and Johnston, 2017).

[8]There were small changes in the TCJA that affected credit unions peripherally. Under the Act, tax-exempt organizations including credit unions are now required to pay a 21% excise tax on the five highest paid employees' compensation that individually exceed $1 million annually.

C-corps. Prior to the TCJA of 2017, large banks appear to have faced higher "effective" tax rates than large firms in many industries (Fox and Vanderpool, 2017) .[9] That is likely to be true of small and medium size banks as well given that their business is overwhelmingly domestic.

The TCJA was enacted in December of 2017 and cut the statutory corporate tax rate from 35% to 21% starting in 2018.[10] In our sample, just comparing the mean of the total income tax rate[11] before and after the TCJA, the rate of (income tax)/(pre-tax profit) falls from 29% in 2014-2017 to about 20% in 2018-2019 at the median firm. So the rate falls by about 1/3 compared to the pre-period. The TCJA also radically transformed the U.S. international tax system. This, however, did not have much direct effect on our small and medium size banks given their domestic orientation. The Act also placed new limits on the ability of businesses to deduct net interest payments under §163(j), but this again is unlikely to affect banks directly as they are almost always net recipients of interest payments. However, this limitation could impact the willingness of businesses to take on new debt financing from banks or credit unions. Because of banks' greater propensity to lend to businesses the 163(j) limitation may have a greater effect on demand for their loans. With that said, many borrowers from small and medium size banks would be exempt from the 163(j) limit because their gross receipts fell below the threshold established in §163(j)(3).[12] In addition, the Act placed new limits on the deductibility of home mortgage interest, which again may affect demand for home loans from both banks and credit unions.[13]

[9] As Auerbach (2018) points out these simple calculations of effective rates from profits and income taxes reported on financial statements usually ended up higher or lower based on the level of retained earnings "indefinitely" reinvested abroad by foreign subsidiaries.

[10] The TCJA's rate cut was pro-rated for firms with fiscal years running across January 1, 2018. So if a firm's fiscal year runs from November 1, 2017 to October 31, 2018, its tax rate would be roughly $\frac{2}{12} * 35\% + \frac{10}{12} * 21\% = 23.33\%$. One can think of the average rate on income earned in calendar year 2018 income as 21% if the firm earns its profits relatively evenly throughout the year. On the margin, however, the relevant rate for additional income generated by decisions influenced by the TCJA would be the blended rate, here of 23.33% .

[11] Our data does not break out federal income tax compared to state and local or foreign income taxes, though the latter seems likely to be very small.

[12] See IRS, FAQs Regarding the Aggregation Rules Under Section 448(c)(2) that Apply to the Section 163(j) Small Business Exemption (last accessed December 2022).

[13] Again this should have an equal effect on banks and credit unions given the similarity of their home loan applicants

3.2.3 Regulatory Changes to Credit Unions During Pre-Period and After Post-Period

There were a few regulatory changes to credit unions during the pre-period of our study and after the post-period. All allowed credit unions to function more like banks. In particular, in 2015 and 2016, the NCUA promulgated final rules loosening the meaning of the common bond requirement both for credit unions whose bond was based on employment or other association, and for ones based on geography.[14] The primary effect of these rules would seem to make it easier to found new credit unions (e.g., (NCUA, 2016b)), but given our balanced panel requirements these new credit unions will not show up in our sample.[15] Also in 2016, the NCUA enacted a final rule loosening existing restrictions on credit union's ability to make business loans beginning January 1, 2017 (NCUA, 2016a). Finally, after the post-period, in January 2020, the NCUA promulgated a new rule easing restrictions on credit unions acquiring their capital from non-members.

3.3 Empirical Approach and Identification Strategy

Economic theory and empirical investigations have variously suggested that along with corporate equity holders, other economic participants may also bear corporate tax incidence: other providers of capital (e.g., Harberger (1962)), labor (e.g., Fuest et al. (2018)), and customers (Baker et al. (2020)). Our estimation strategy is to compare how the prices paid to depositors, wages paid to employees, interest charged to borrowers, and investment in fixed structures varies across banks and credit unions before and after the TCJA.

The basic identifying assumption is that—absent tax changes—in each of these

(see DiSalvo and Johnston (2017). The Act limited deduction of mortgage interest to the first $750,000 of a loan rather than $1 million §163(h)(3)(F)(i)(II). In addition by doubling the standard deduction, it substantially reduced the portion of borrowers who will find it advantageous to deduct home mortgage interest.

[14] The 2016 rule was challenged by a banker's association as violating the Administrative Procedure Act. In 2018, in Am. Bankers Ass'n v. NCUA, 306 F.Supp.3d 44, the district court vacated a large portion of the rule. In 2019, the ruling was appealed to the D.C. Circuit, which rejected most of the district court's decision, letting most of the original rule stand. 934 F.3d 649 (2019).

[15] To a lesser extent the rules might allow existing credit unions, particularly those whose common bond is geographic, to sign up new customers.

outcomes, the behavior of banks would have evolved in parallel to that of credit unions. This assumption can be evaluated by examining the trends of the two groups before the TCJA. But as with every DD design, we cannot in fact test the accuracy of the assumption during the treatment period. The threats to identification are those that usually come along with any DD: economic or policy changes in the post-period, unrelated to the TCJA, that drive different outcomes in banks and credit unions.

3.4 Data

An advantage of using financial institutions in our analysis is that although we study mostly non-publicly traded firms, their data is publicly available, audited, and regularly reported to supervisory institutions. Commercial banks report information quarterly to the Federal Financial Institution Examination Council (FFIEC) and this information is published in the Reports on Condition and Income (Call Reports). An analogous process occurs for credit unions. Credit unions report detailed financial information in their Call Reports which is then published by the National Credit Union Association (NCUA). Quarterly data is reported under standard accounting principles in calendar year-to-date format. We combine these sources to construct a quarterly panel.

From the accounting information, we construct categories that theory predicts may (or may not) differentially respond to changes in the corporate tax. In the banking sector, these categories include each firm's pre-tax profit and return on assets. We additionally construct netput prices, quantity, and total inlay or outlay for loans, labor, and deposits. Finally, we construct measures of the value of premises, book equity, noninterest income received, and risk-weighted assets. For ease of comparison, we adjust all quantities to constant 2010 dollars.

3.4.1 Sample

For our identification strategy to be valid, we need to compare similar credit unions and commercial banks. Not all commercial banks in our sample can be compared against credit unions. We therefore restrict our sample to firms with significant overlap in terms of asset size and drop firms that have missing data or have an extreme value for return on assets (top one or bottom percent).[16] As noted above, credit unions tend to be smaller firms than small and medium size banks. In order to compare credit unions to similarly situated private banks, we keep firms with average assets greater than the 10th percentile of commercial banks ($50 million). We impose a similar restriction on the largest firms by restricting our sample to firms with average assets less than the 95th percentile of credit unions ($815 million). We also impose a balanced panel and restrict our sample to banks that report in every quarter. To ensure we are picking up changes from the TCJA and not changes driven by differences between particular credit unions or banks, we follow the existing literature (e.g. DeYoung et al. (2019)) by removing banks (or credit unions) that do not have any comparable institutions in the sample; we implement this by keeping only banks the FDIC has coded as "community banks."

After these restrictions, about 20% of the for-profit banks are organized as S corporations in our sample, which we drop because we are unable to observe the intensity of the TCJA tax cut received by pass-throughs. To focus on the impact of the TCJA we restrict our sample to run from 2014 through the end of 2019 in order to avoid the period following the Great Recession and the tumultuous financial situation caused by the COVID-19 pandemic. Table 3.1 shows summary statistics:[17]

[16]Winsorizing at those thresholds produces nearly identical results.

[17]The similarity of the price paid on deposits between credit unions and banks is perhaps surprising given that in most savings products, the average interest rate advertised by credit unions is higher than at banks (NCUA 2021). The source of this apparent anomaly is that a greater share of bank deposits are CDs which pay higher interest which leads to the overall deposit rate being similar despite credit unions paying a higher rate in any given product type.

Table 3.1: Summary Statistics

	Banks		Credit Unions	
	mean	sd	mean	sd
Age (Years)	88.8	41.1	66.7	13.7
Taxes	228222	430327	0	0
ln(Assets)	19.2	0.8	19.0	0.8
ln(Investments)	17.88	0.84	17.32	1.05
ln(Deposits)	19.02	0.80	18.86	0.82
ln(Premises)	14.91	1.21	15.00	1.36
Deposit Share Check and Save	0.3084	0.1575	0.5774	0.1540
Deposit Share CDs	0.3299	0.1478	0.1703	0.0923
Deposit Share Money Market	0.1623	0.1244	0.1663	0.1181
Price of Deposits	0.0014	0.0008	0.0012	0.0008
Price of Labor	16380	4772	14933	3877
Price of Loans	0.0128	0.0023	0.0124	0.0023
Firm x Quarter Obs.	48312		37392	

Notes: The netput interest rates for deposits and loans and wages per employee are quarterly so the annual rate is approximately 4x that shown in the table.

3.5 Results

The TCJA provided a substantial tax cut to for-profit banks in our sample. Before the fourth quarter of 2017, the median for-profit bank in our sample had an effective total income tax rate of about 29%. After the TCJA, they paid out 20% of their pre-tax net income as taxes. The median quarterly net income of the for-profit banks in our selected sample from 2018-2019 was $574,556 (the mean was $909,654).

We estimate the following event study for each variable, y, of interest using:

$$y_{it} = \sum_r [\beta_r \mathbb{1}\{(t - 2018Q1 = r) * \text{Taxed Bank}\} + \eta_i + \gamma_t + \phi * t * \mathbb{1}\{\text{Taxed Bank}\} + \epsilon_{it} \tag{3.5.1}$$

In this standard event study specification, the indicator function β_r traces out the event study, illustrating differences in the evolution of various outcome variables over the sample between treated and control firms. In our preferred specification, we use variation within a firm (controlling for firm fixed effects: η_i) after netting out state-quarter variation (state by quarter fixed effects: γ_t) and different trend growth ($\phi * t * \mathbb{1}\{\text{Taxed Bank}\}$). Our estimated beta coefficients trace out the treatment effect over time relative to 2017Q4, the period before the TCJA which is set to 0.[18] As there is only one "event" (tax-cut) in our analysis, we also present Conley and Taber (2011) style p-values for our analysis and the related bootstrapped distributions in the appendix. Our appendix also contains several robustness checks, including showing the results with and without detrending, with and without entropy balance weights, and using alternative control groups including thrifts and S-corps. The results are qualitatively similar across these robustness checks.

3.5.1 Event studies

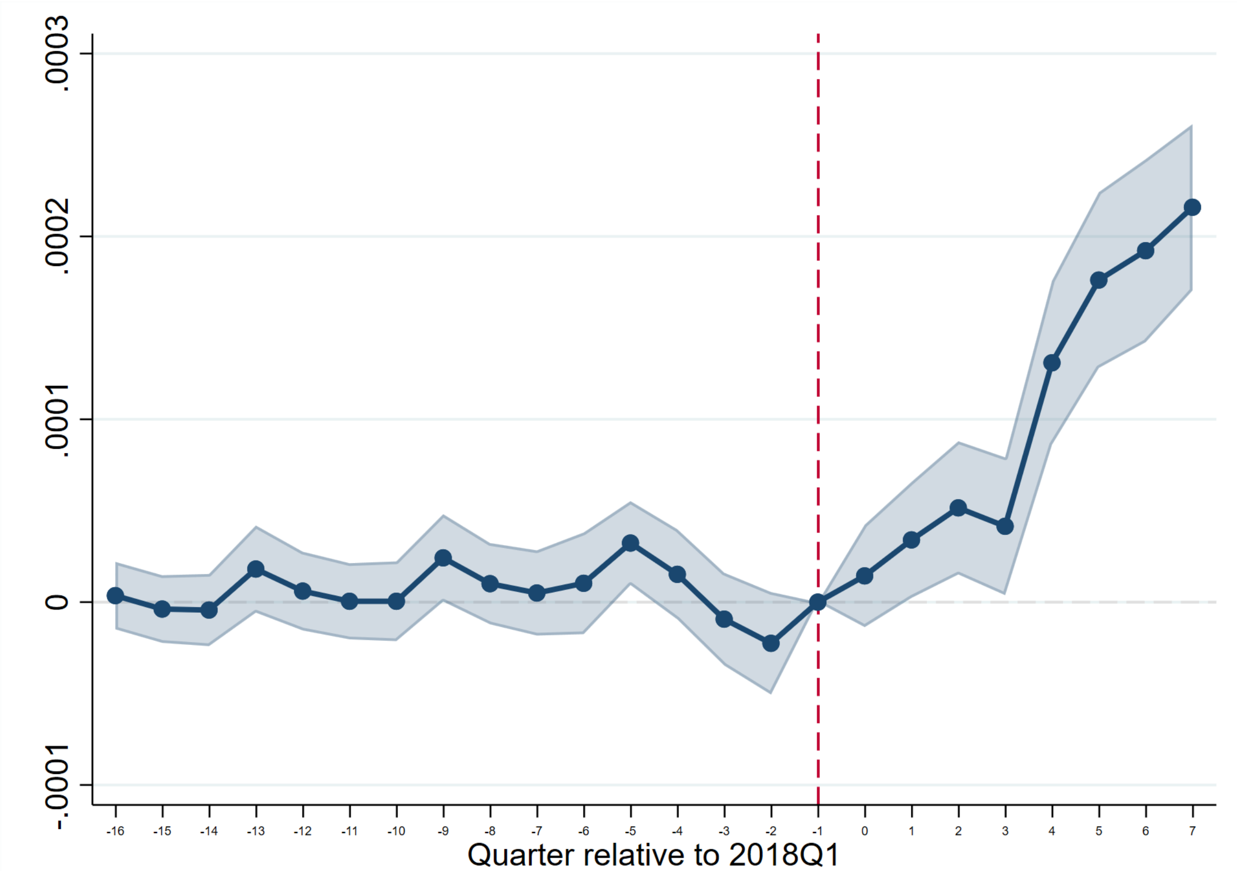
3.5.1.1 Depositors

Depositors. One group of particular interest in our setting is depositors. Absent distortions or other differing factors at credit unions and banks, depositors at for-profit banks and depositors at credit unions should receive similar returns on their deposits. However, relative to credit unions, previous research has found that depositors at for-profit banks seem to receive lower rates. As noted above, in this context, depositors are acting both as lenders of capital and purchasers of financial/payment services (checking accounts, ATM network, etc.). Our event study suggests that prior to the TCJA, the price paid on deposits was evolving in similar ways regardless of whether a firm was a credit union or a for-profit bank and the parallel trends assumption is satisfied (with or without the time trend included in the primary specification). After the TCJA, the price banks were paying on deposits increased relative to the price credit union were paying. This effect increase from about .5 basis point per quarter in 2018 to 2 basis points in 2019. Given the low prevailing rates paid on deposits in the sample period, this represents a large relative increase. These results are robust to controlling for the share of deposits in various categories (e.g. CD's, regular savings, checking interest) and interactions of these

[18]We smooth each event outcome variable to adjust for consistent quarterly patterns that differ slightly between for profit banks and credit unions. We do so by partialling out quarter dummies.

deposit shares with changes in the federal funds rate. The robustness of the result suggests that it is not caused by changes in the composition of bank deposits or changes in the federal funds rates around the TCJA. We have also repeated our analysis around a past increase in federal funds rates from 2004 to 2006 and observe a null result, increasing our confidence that the pattern in Figure 3.4 is not caused by changes in the federal funds rate during our study period.

Figure 3.1: Quarterly Price of Deposits Event Study



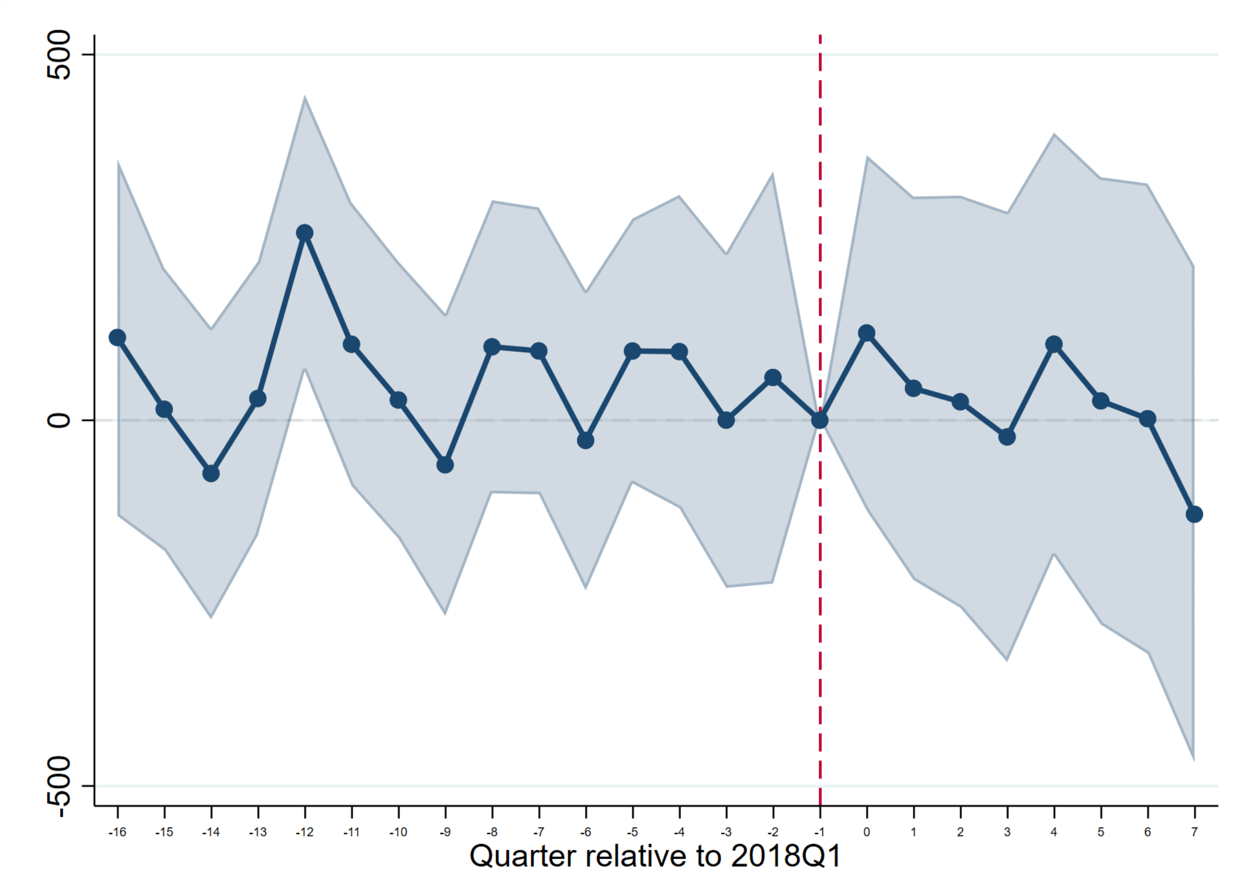
Notes: This figure displays the event study coefficient (β) on for-profit banks. The black dashed lines display the 95% confidence interval with standard errors clustered at the firm level. The red dashed line is through 2017Q4 (t=-1) the period prior to full TCJA exposure and is the omitted event study coefficient.

3.5.1.2 Employees

Next, we perform a similar exercise for labor. If workers bear some of the corporate tax, we might expect wages per worker (or total wages paid to all workers) to increase after the TCJA in for-profit banks relative to their credit union peers.

We find little evidence that labor received a significant portion of the tax cut in our setting. The event study indicates that the change average quarterly spending per full time employee was similar in for-profit banks and credit unions after the TCJA than in 2017Q4. The 95% confidence interval typically rules out raises greater than $400 per quarter. Equivalent event studies on the total wage bill as opposed to wages per employee look very similar.[19]

Figure 3.2: Quarterly Price of Labor Event Study



Notes: This figure displays the event study coefficient (β) on for-profit banks. The black dashed lines display the 95% confidence interval with standard errors clustered at the firm level. The red dashed line is through 2017Q4 (t=-1) the period prior to full TCJA exposure and is the omitted event study coefficient.

[19]As noted in the introduction, the difference in difference model will fail to pick up an increase in wage per worker at banks caused by the Act if credit unions follow suit in order to avoid losing employees. Still, we would expect to see a change in the total wage bill at banks compared to credit unions if workers were benefiting substantially from the Act.

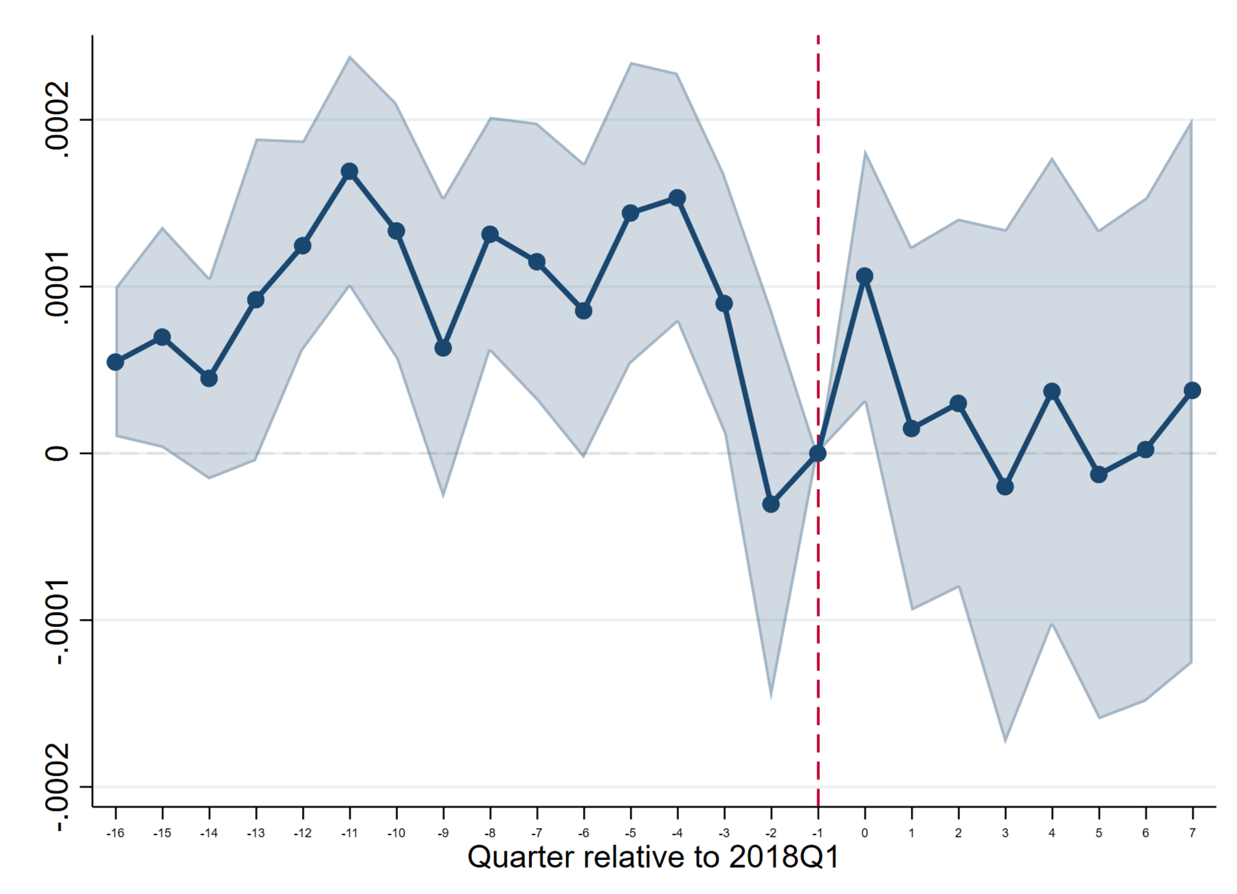
3.5.1.3 Loans

One other netput that might respond to the corporate tax rates is the price of loans offered to customers, for the reasons outlined in Baker et al. (2020) if firms are not perfectly competitive. The estimates from loans are quite noisy, but do run counter to what theory predicts. The coefficients are generally positive starting in 2018, which implies that, if anything, the tax cut resulted in higher loan prices. Theory suggests an opposite signed treatment. This evidence suggests that little to none of the subsidy is passed through to customers in the form of more efficient investment or cheaper access to capital through loans. But we have less confidence in this estimate both because of the differences in business loans for credit unions and the absence of parallel trends without controlling for a linear time trend. Equivalent event studies for interest income from loans (i.e. price x quantity) are flatter, suggesting that if anything banks relatively decreased their quantity of lending in the wake of the Act.

3.5.1.4 Physical investment

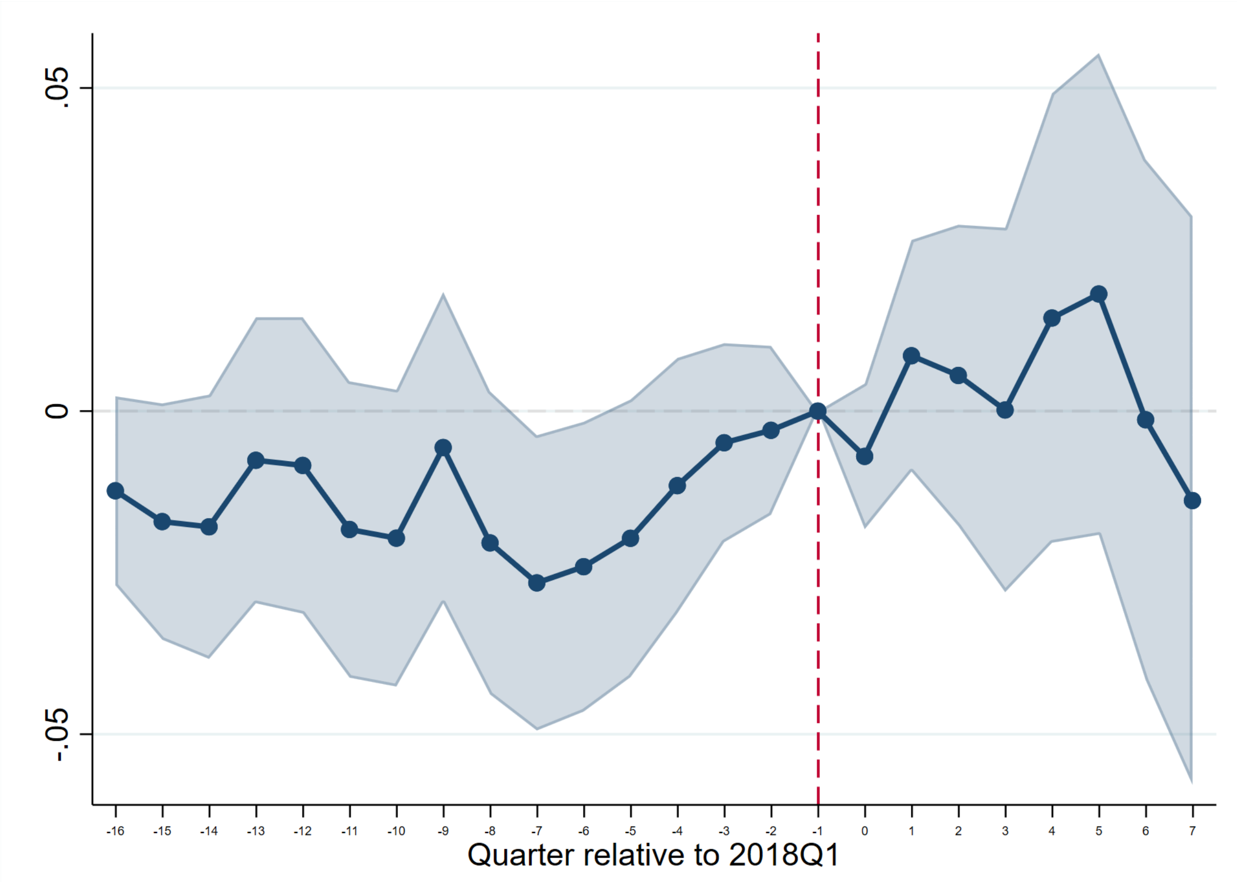
The tax cut in theory lowers the cost of capital for firms, enabling them to expand investments in equipment and structures (and other long-lived assets of which making long-term loans might be one). To examine this potential margin of response, we look at the change in the log of the value of premises. However, there is little evidence that the tax cut increased for-profit banks' relative premises. Most of the point estimates are modestly positive. Nevertheless, there does not appear to be a trend break around the TCJA.

Figure 3.3: Quarterly Price of Loans Event Study



Notes: This figure displays the event study coefficient (β) on for-profit banks. The black dashed lines display the 95% confidence interval with standard errors clustered at the firm level. The red dashed line is through 2017Q4 (t=-1) the period prior to full TCJA exposure and is the omitted event study coefficient.

Figure 3.4: Log Premises Event Study



Notes: This figure displays the event study coefficient (β) on for-profit banks. The black dashed lines display the 95% confidence interval with standard errors clustered at the firm level. The red dashed line is through 2017Q4 (t=-1) the period prior to full TCJA exposure and is the omitted event study coefficient.

3.5.2 Aggregated Treatment Effects

It is also helpful to translate these results into an aggregated treatment effect without the full saturation of year coefficients. We run the analogous difference in differences regression to estimate the total TCJA effect using:

$$y_{it} = \beta * post * \mathbb{1}\{\text{Taxed Bank}\} + \eta_i + \gamma_t + \phi * t * \mathbb{1}\{\text{Taxed Bank}\} + \epsilon_{it} \quad (3.5.2)$$

In this specification, *post* takes a value of 1 starting in 2018 quarter 1. Our coefficient of interest is β, which will estimate the total impact of the TCJA on for-profit taxed banks from 2018 through 2019. The results for prices are shown in Table 3.2.

Our estimates are similar to DeYoung et al. (2019) who estimate that the non-taxation of credit union as a subsidy is primarily passed on to depositors. We estimate that the TCJA tax cut increased the price for-profit banks pay on deposits by about .8 basis points per quarter or 3.2 basis points per year. Lowering the tax burden on banks appears to benefit depositors. For the reasons discussed above, we are cautious about interpreting the coefficient on loan rates other than as ruling out important pass through of the tax cut to customers on this margin if the identifying assumption holds.

Table 3.2: TCJA Impact on Prices

	Quarterly price of: Deposits (bps)	Labor	Loans (bps)
TCJA x for-profit	0.00826***	-118.5*	-0.00452
	(0.00117)	(66.22)	(0.00422)
Conley and Taber (2011) p-value	.007	.093	.058
For-profit ave.	0.113	16108.3	1.267

Notes: Robust standard errors in parentheses, clustered at the firm level. All regressions include firm and quarter by state fixed effects. Deposit price regression controls for the interaction of pre-treatment CD and money-market shares of deposits and the federal funds rate. The loan regression controls for the interaction of pre-treatment business, real estate, commercial real estate and consumer loans as a share of total loans with the federal funds rate.

We likewise use Equation (2) to generate a similar set of estimates for overall expenses, rather than just prices, to better translate where each dollar of the tax cut ends up. These estimates will take into account both quantity and price changes. In Table 3.3:, we run Equation (2) using total spending or income for a given category in a quarter in place of the price. "Capital" is defined at payments to depositors and other lenders of capital, dividends, and changes in book value of equity.

Table 3.3: TCJA Impact on netputs

	Quarterly flow of:					
	Taxes	Capital	Deposit	Labor	Loan	Premise
TCJA	-180084.1***	131367.3*	40221.5***	7517.4	-2956.2	21880.1
	(8646.2)	(70294.2)	(7588.4)	(8125.4)	(28235.2)	(15745.5)
CT p-value	0.00	0.01	0.00	0.45	0.89	0.72

Notes: Robust standard errors in parentheses, clustered at the firm level. All regressions include firm and quarter by state fixed effects.

Finally, we use an instrumented difference-in-differences design (see Duflo (2001)), where we instrument the change in tax bill with the indicator for TCJA treatment. A different way to think about this is we measure the intensity of treatment by the difference of taxes within a bank pre and post-TCJA. The results of this analysis are shown in Table 3.4:

Table 3.4: TCJA - Scaled (IV) DiD

	Quarterly flow of:				
	Capital	Deposit	Labor	Loan	Premise
Taxes	-0.738*	-0.223***	-0.0417	0.0151	-0.121
	(0.395)	(0.0410)	(0.0452)	(0.144)	(0.0875)

Notes: Robust standard errors in parentheses, clustered at the firm level. All regressions include firm and quarter by state fixed effects.

This analysis suggests that a 100 dollar decrease in taxes is associated with a 74 dollar increase in returns to capital inclusive of about about a 22 increase in deposit expenditures. The impact of taxes on labor expenditure and loan income cannot be statistically be distinguished from 0. The estimate for both labor and loans rule out the possibility that significant portions of the tax cut are passed through to labor in the form of wages or customers in the form of lower loan rates, assuming the identifying assumptions hold.

Notably, we see pass through of the tax cut to depositors but not other customers (i.e., borrowers from banks). As a result, we tentatively conclude that the pass-through of the tax cut to depositors is likely in their capacity as capital providers to the bank, rather than as customers of payment services. That in turn suggests that capital providers (either equity or lenders) got the benefit of the corporate tax cut embodied in the 2017 Act at least in banks, and—depending on one's views of the caveats noted in the introduction—quite possibly in other areas as well.

3.6 Model

There are two main questions a model can help with interpreting our empirical findings: 1) when might our difference-in-differences analysis fail to estimate the object(s) of interest and how generalizable are estimates from one sector (here banking) to the broader economy. This section proceeds to implement and describe an established model to help answer these questions.

Our setting can be well represented in the Differentiated Production Model developed in (Gravelle and Kotlikoff, 1993). This model has two industries, 1 and 2, which in our setting can be thought of as the financial-banking industry and the rest of the economy. In each industry there is a corporate and a noncorporate good. Our evidence is concentrated on one industry, with corporate production (for-profit banks) and non-corporate production (credit unions). This model is useful in understanding how our results might generalize.

The four goods are denoted C_1, N_1, C_2, and N_2 where C_1, and N_1, (C_2, and N_2) are the corporate and noncorporate goods in the two industries (indicated by the subscripts). While C_1 and N_1, are not identical goods, they are closer substitutes than C_1 and C_2. In our setting that simply assumes that consumers are more like to switch between credit union and community banks, than say, buying safes or putting the money into durable goods.

The model builds off of several primitives, most notably a utility function of the form:

$$U = [a(d_1 C_1^{(1-1/\eta)} + (1-d_1)N_1^{(1-1/\eta)})^{(1-1/\phi)/(1-1/\eta)} + (1-a)(d_2 C_2^{(1-1/\eta)} + (1-d_2)N_2^{(1-1/\eta)})^{(1-1/\phi)/(1-1/\eta)}]^{-1/(1-1/\phi)} \quad (3.6.1)$$

Here, a, d_1, and d_2 are share parameters (which the model will solve for), η refers to the within-industry elasticity of substitution and ϕ the between industry elasticity (which we will choose). The model also relies on a a standard economy wide budget constraint ($P_{C_1}C_1 + P_{N_1}N_1 + P_{C_2}C_2 + P_{N_2}N_2 = I$, where I is national income, and P's indicate prices) and CES production functions for each good ($Q_j = H_i[(1-b_i)L_j^{-\rho_i} + b_i K_j^{-\rho_i}]^{-1/\rho_i}$ for $i = 1, 2$; $j = C_i, N_i$).

With these equations in place and the calibration of several parameters we can generate several useful comparative statics to help contextualize our difference-in-difference estimates. To generate useful comparative statics from this model we need the following values: $t_1 = .08$, $t_2 = .19$, $t = .35$, where t is the corporate tax rate and t_i is the average corporate tax rate in each industry; M_i, the capital income share in each industry (we use .6 and .2 respectively following Gravelle and Kotlikoff (1993)); a national income number (we use 296); and choices about the relevant elasticities (ρ_i, η, ϕ). For additional details on how this model is solved see Gravelle and Kotlikoff (1993). Solving this model allows us to understand how our DiD estimates (movements between capital and labor in industry 1) might translate to burdens in broader (domestic) economy.

Under our baseline calibration with pre-TCJA corporate tax rates, the initial allocation of capital and labor (as a share of their total) is as follows: $K_{N_1} = 31.3\%$, $K_{C_1} = 6.2\%$, $K_{N_2} = 35.3\%$, $K_{C_2} = 27.2\%$; $L_{N_1} = 9.5\%$, $L_{C_1} = 0.9\%$, $L_{N_2} = 64.6\%$, $L_{C_2} = 24.9\%$ (the price of N_1 is .78, and N_2 is .92 where the price of each corporate good is set to 1).

Adjusting the corporate tax rate to $t = .2$ and scaling t_1, t_2 similarly, generates the following allocations: $K_{N_1} = 29.2\%, K_{C_1} = 6.9\%, K_{N_2} = 32.8\%, K_{C_2} = 31\%$; $L_{N_1} = 9.1\%, L_{C_1} = 1.1\%, L_{N_2} = 61.0\%, L_{C_2} = 28.8\%$ (the price of N_1 .88, and N_2 is .96).

As one would expect, lowering the corporate tax rate moves resources into the corporate sector of each industry. The gap between capital in the corporate and non-corporate sector in industry 1 declines by 2.8 percentage points (25.1-22.3) and in the second industry by 6.3 percentage points (8.1-1.8). This suggests that the quantity responses we see between the taxed and non-taxed banking sectors may not fully generalize to the broader economy, but can be informative if we are willing to take a stand on several relevant parameters.

3.7 Conclusion

We analyze the effect of the TCJA on small and medium size banks using credit unions as controls. Using a difference in difference framework, we find consistent evidence that banks raised the (relative) amount they paid to capital holders (inclusive of depositors) after the Act.

This increase represents roughly three-quarters of the total tax savings enjoyed by banks. By contrast, we see little, if any, evidence of pass through of the tax cut to employees or customers who borrow from these banks or increase in investment in physical assets. This provides additional evidence about the impact of corporate tax cuts in the banking sector, and with some additional assumptions, evidence on who bears the corporate tax.

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1 Appendix

A.1.1 Some representative cases

Negligent hiring case law presents an uncertain standard for employers. It is helpful to review the contours of some representative decisions to understand why. The selected cases will help to highlight the central questions posed by the tort: what is the scope of the duty; what are the bounds of foreseeable (proximate) harms; when should a jury decide these questions? These cases also highlight the fact-intensive nature of the disputes and the unpredictability of case outcomes. In Hersh v. Kentfield Builders, Inc., the plaintiff-customer Melvin Hersh visited a model home for an appointment with the defendant-business's president Norman Steel. While waiting for the meeting, the plaintiff was seriously injured in an unprovoked attack from employee Benton Hutchinson who had been tasked to do clean-up work and odd jobs at the model homes. The relevant facts for determining negligence in this case were that ten years before he attacked Hersh, Hutchinson had been convicted of manslaughter, and he also had a conviction for carrying a concealed weapon. The parties disagreed about whether the judge or the jury should decide whether the prior conviction was sufficient to warn the employer of

Hutchinson's violent propensities. The trial court initially left the question up to the jury, which found in favor of the plaintiff. The Court of Appeals set aside the verdict after determining that "there was no evidence in the record to support the conclusion of negligence." The Supreme Court of Michigan, reversing the Court of Appeals, ultimately decided that "whether the employer knew or should have known of Hutchinson's vicious propensities should not be determined by any court as a matter of law, but by the jury." Some scholars have noted that when these decisions are left up to the jury, "the reported decisions offer a limited amount of guidance for employers who seek to avoid liability for negligent hiring." A minority of courts have been more hesitant to leave this question to the jury and are more willing to settle the question of foreseeability as a matter of law.

Some courts are willing to find liability even though the previous criminal behavior was not directly related to the harmful act in question. For instance, the court considered an employee's prior convictions for rape, armed robbery, and residential burglaries sufficiently related to murder to make the employer liable for negligent hiring. Another case found negligent hiring liability for employing a supervisor who had "served prison time for bank robbery, had been arrested for shoplifting, drug possession, a solicitation to buy drugs, disorderly conduct, and solicitation of a sex act, had admitted to using at least seven different aliases, was involved in some physical altercations at work, and participated in a doctor-supervised methadone program" who later "harassed and threatened a female employee." In Hines v. Aandahl Constr. Co., the court upheld a negligent hiring claim when a contracted painter assaulted homeowners, based on the painter's drug history and a recent theft of another client's computer. However, there was no previous violent behavior. Moreover, the employee's previous misbehavior does not need to be recent. In Estate of Arrington v. Fields., the jury found the defendant liable for exemplary damages for hiring an employee (as security) who shot the plaintiff. However, the employee's prior actions that gave rise to the negligent hiring claim were nonviolent (burglary with intent to commit theft, grand larceny, burglary, theft, and a bogus check charge), and his last conviction was more than 13 years before the date of hire. Other courts and juries have been more reluctant to find liability. For instance, in Kirlin v. Halverson, the court looked at the employee's previous criminal record, which included a charge of aggravated and simple assault and resisting arrest in a domestic dispute (although the employee was only convicted of resisting arrest) and a number of other previous charges and citations. The employee, hired to perform HVAC repair and services, assaulted the plaintiff, an employee of a competitor, at the job site. Reviewing this history and the offending employee's relatively infrequent interaction with others on the job, the court rejected the negligent hiring claim. It concluded that to decide otherwise would run counter to public policy. In another case, a company employed a man as a truck driver, and this employee later picked up a stranded motorist and raped and murdered her. On appeal, the court dismissed the negligent hiring claim, deciding that prior convictions for arson and aggravated assault were not enough to establish a violation of the duty to hire reasonably competent employees. The court did note, however, that previous courts faced with different criminal histories had found the employer negligent. The court stated that while hiring a driver with prior convictions for arson and assault is not negligent in this case, hiring someone with a history of a violent sex crime might be (especially if those crimes were connected to driving or hitchhiking). In another case, an Ohio court dismissed a negligent hiring claim for hiring an employee with a previous conviction for indecent exposure at a city park who later sexually assaulted the plaintiff (a coworker). The court discounted two previous unsubstantiated investigations for sexual abuse and dismissed the claim, deciding that there was insufficient evidence.

A.1.2 Legal background on negligent hiring

The competing interests balanced by the tort of negligent hiring often come to a head in the discussion of proximate cause (although a minority of courts will alternatively frame the dispute around the scope of the duty of care). To answer whether the harm caused by negligently hiring the employee was the type of harm that was foreseeable at the time of hire, some courts have attempted to provide structure by looking towards "the number and nature of prior acts of wrongdoing by the employee, and the nexus or similarity between the prior acts and the ultimate harm caused." Other courts have applied a less structured totality of the circumstances analysis.

The theories put forward by various courts are echoed in previous scholarship that has explored several potential justifications for maintaining negligent hiring as a cause of action. For example, one paper offers two public policy reasons for the tort: compensating victims harmed by employees and spreading the loss from the victim to all customers. Note that this argument relies on the fact that the offender-employee is likely to be judgment-proof and unable to compensate the victim directly. Other scholarship points out that the tort is intended to encourage employers to hire safe and competent employees for a given role.

While courts frame the causes of action for negligent hiring in slightly different ways, some common themes emerge. The Restatement (Second) of Torts describes the principle that an employer may be liable for harm caused by employees "who, to his knowledge, are in the habit of misconducting themselves in a manner dangerous to others." In general, the following elements need to be shown to sustain the cause of action: "(1) the employer owed the [plaintiff] a duty of reasonable care; (2) the employer breached the duty; and (3) the breach proximately caused the [plaintiff's] harm." Proving these elements often turns on a showing of the following facts: (1) employment; (2) that hiring an employee with this employee's characteristics was likely to harm third parties; (3) the employer's actual or constructive knowledge of such characteristics; (4) the employee's act or failure to act is the actual and proximate cause of the plaintiff's injuries.

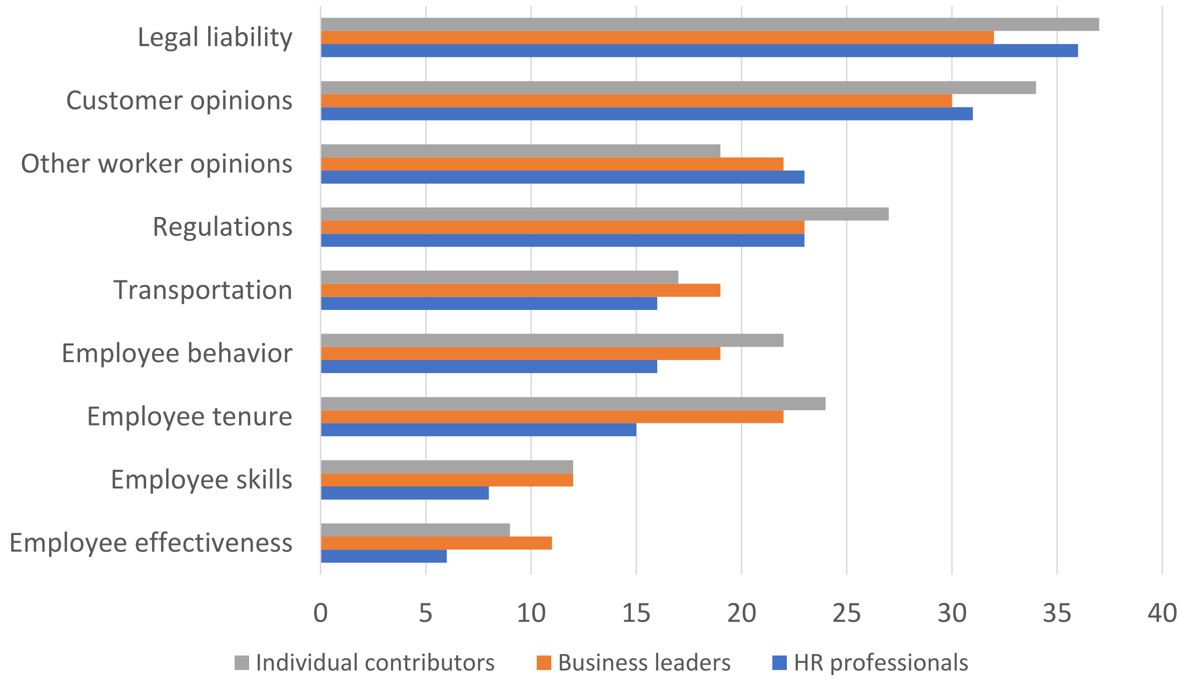
Many courts are aware of the difficult trade-offs inherent in the tort of negligent hiring. They note that without a tight proximate cause requirement, the "employer would essentially be an insurer of the safety of every person who happens to come into contact with his employee simply because of his status as employee." However, the tort may be needed to redress the wrong of the "negligence of the employer in the hiring or retention of employees whose qualities unreasonably expose the public to a risk of harm" and "addresses the risk created by exposing members of the public to a potentially dangerous individual."

A.1.3 Inference with differences-in-differences with staggered treatment and few treated clusters

Inference with differences-in-differences with staggered treatment and few treated clusters can be challenging. Donald and Lang (2007) and Conley and Taber (2011) show that DiD estimators are not consistent or generally asymptotically normal when studying a few treated units. This can lead to either over or under-rejection of the null. While there have been substantial advances to aid in inference in these settings (Conley and Taber, 2011; Ferman and Pinto, 2019; MacKinnon and Webb, 2020; Hagemann, 2020), several of these methods are not straightforward to apply in the context of the recent advances in DiD estimators seeking to account for heterogeneous treatment effects and staggered adoption (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Gardner, 2022).

Inference in the negligent hiring context has several of these challenging features. There are a small number of reforming states (no more than 10), the states are of unequal size (generating heterogeneity), and adoption of reform is staggered. However, recent work by Alvarez and Ferman (2023) builds on Ferman and Pinto (2019) to allow for inference in this setting. This setting matches well with Ferman and Pinto (2019), in which outcomes come from aggregating data from individuals in units over time and the authors show that under many spatial and temporal correlation can be parametrically modeled. The intuition of the approach matches (Ferman and Pinto, 2019), with the addition of needing to estimate each "building block" parameter of that makes up the aggregated DiD estimate.

Figure A.1: Society for Human Resource Management (2021) survey of why organizations are concerned about hiring workers with criminal records



In order to implement this approach, I follow Alvarez and Ferman (2023) and estimate and aggregate the building block parameters in a stacked regression approach approach (separately estimating each treated state compared to all untreated states) and then aggregating akin to example 1 of Alvarez and Ferman (2023) across these comparisons. This returns the average post-treatment effect on the treated units using all pre-treatment periods in constructing the estimator.

As with Conley and Taber (2011) this approach uses the residuals from the control units to estimate the distribution of the errors of the treated units and allows for the type of heteroskedasticity of error terms considered in Ferman and Pinto (2019).

A.1.4 Additional tables and figures

Table A.1: Negligent hiring adoption cases

State	Case	Year
Alabama	Nash v. Segars, 682 So. 2d 1364	1996
Alaska	Svacke v. Shelley, 359 P.2d 127	1961
Arizona	McGuire v. Arizona Protection Agency, 125 Ariz. 380	1980
Arkansas	American Auto. Auction, Inc. v. Titsworth, 292 Ark. 452	1987
California	Evan F. v. Hughson United Methodist Church, 8 Cal. App. 4th 828	1992
Colorado	Connes v. Molalla Transport System, Inc., 831 P.2d 1316 (Colo. 1992)	1992
Connecticut	Stiebitz v. Mahoney, 144 Conn. 443	1957
D.C.	487 A.2d 610 (D.C. 1985)	1985
Delaware	Draper v. Olivere Paving & Constr. Co., 54 Del. 433	1962
Florida	Mallory v. O'Neil, 69 So. 2d 313	1954
Georgia	C. K. Sec. Systems, Inc. v. Hartford Acci. & Indem. Co., 137 Ga. App. 159	1975
Hawaii	Janssen v. American Hawaii Cruises, 69 Haw. 31	1987
Idaho	Doe v. Garcia, et al., 131 Idaho 578 (1998)	1998
Illinois	Becken v. Manpower, Inc., 532 F.2d 56	1976
Indiana	n/a	1901
Iowa	Godar v. Edwards, 588 N.W.2d 701	1999
Kansas	Balin v. Lysle Rishel Post No. 68, 177 Kan. 520, 280 P.2d 623 (1955),	1955
Kentucky	Oakley v. Flor-Shin, Inc., 964 S.W.2d 438	1998
Louisiana	Smith v. Orkin Exterminating Co., 540 So. 2d 363	1989
Maine	Fortin v. The Roman Catholic Bishop of Portland, 2005 ME 57, 871 A2.d 1208	2005
Maryland	Evans v. Morsell, 284 Md. 160, 165 (Md. 1978).	1978
Massachusetts	Carson v. Canning, 180 Mass. 461	1901
Michigan	Bradley v. Stevens, 329 Mich. 556	1951
Minnesota	Ponticas v. K.M.S. Invs., 331 N.W.2d 907	1983
Mississippi	Eagle Motor Lines v. Mitchell, 78 So. 2d 482, 486-87 (Miss. 1955)	1955

Table A.1: Negligent hiring adoption cases (continued)

State	Case	Year
Missouri	Strauss v. Hotel Continental Co., 610 S.W.2d 109, 112 (Mo. App. 1980)	1980
Montana	Vollmer v. Bramlette, 594 F. Supp. 243, 248 (D. Mont. 1984))	1984
Nebraska	Greening v. School Dist., 393 N.W.2d 51	1986
Nevada	Rockwell v. Sun Harbor Budget Suites, 925 P.2d 1175	1996
New Hampshire	Cutter v. Town of Farmington, 498 A.2d 316, 320 (N.H. 1985)	1973
New Jersey	Di Cosala v. Kay, 91 N.J. 159	1982
New Mexico	F & T Co. v. Woods, 92 N.M. 697	1979
New York	Vanderhule v. Berinstein, 285 A.D. 290	1954
North Carolina	Pleasants v. Barnes, 19 S.E.2d 627	1942
North Dakota	Schlenk v. Northwestern Bell Tel. Co., 329 N.W.2d 605	1983
Ohio	Ruta v. Breckenridge-Remy Co., 1980 Ohio App. LEXIS 12410	1980
Oklahoma	Mistletoe Express Service, Inc. v. Culp, 353 P.2d 9 (Okla. 1960)	1960
Oregon	Hansen v. Cohen, 276 P.2d 391 (Or. 1954)	1954
Pennsylvania	Dempsey v. Walso Bureau, Inc., 431 Pa. 562	1968
Rhode Island	Welsh Mfg. v. Pinkerton's, 474 A.2d 436	1984
South Carolina	Cf. Degenhart v. Knights of Columbus, 309 S.C. 114, 116-17,	1992
South Dakota	Rehm v. Lenz, 1996 SD 51, 1121, 547 N.W.2d 560.	1996
Tennessee	Mooney v. Stainless, Inc., 338 F.2d 127 (6th Cir. 1964)	1964
Texas	Estate of Arrington v. Fields, 578 S.W.2d 173 (Tex. Civ. App. 1979)	1979
Utah	Retherford v. AT&T Comm. of Mountain States, Inc., 844 P.2d 949 (Utah 1992).	1992
Vermont	Huminski v. Lavoie, 787 A.2d 489, 520-521 (Vt. 2001)	2001
Virginia	Big Stone Gap Iron Co. v. Ketron, 102 Va. 23, 45 S.E. 740, 102 Am. St. Rep. 839	1903
Washington	Scott v. Blanchet High Sch., 50 Wn. App. 37	1987
West Virginia	Thomson v. McGinnis, 195 W. Va. 465	1995
Wisconsin	Miller v. Wal-Mart Stores, Inc., 580 N.W.2d 233	1998

Table A.1: Negligent hiring adoption cases (continued)

State	Case	Year
Wyoming	Cranston v. Weston County Weed & Pest Bd., 826 P.2d 251	1992

Table A.2: Employment Gaps Literature Summary

Author	Data	Technique	Employment	Hr. Wage	Earnings	Sample
	Individual Longitudinal Surveys					
Freeman (1991)	NLSY	Simple Regression	21-24%			Full NLSY
Grogger (1992)	NLSY	IV (previous work)	15-24%			Full NLSY
Western (2002)	NLSY	Panel FE		7-19%		Full NLSY, at Risk NLSY
Allgood et al. (1999)	NLSY	Simple Regression			12%	Youth Prison
Western and Beckett (1999)	NLSY	Panel RE	12%			Youth Prison
Western (2006)	NLSY	Panel FE	9.7%-15.1%	12.4%-24.7%	32.2-36.9%	Risky NLSY
Raphael (2007)	NLSY	Panel FE	13-23%	17-23%		Risky NLSY
James et al. (2010)	NLSY	Panel FE	19%	11%	40%	Full NLSY
Geller et al. (2006)	FFCWS	PS weighting	2-7%	10-30%		Fathers only
Richey (2015)	NLSY	IV (w/ monotonicity)	0-19%	0-39%	0-46%	White Men
Richey (2015)	NLSY	IV (w/ monotonicity)	0-29.5%	0-44%	0-43%	Black Men
Finlay (2008)	NLSY	DD around internet access	7%	8.7%	18.7%	Full NLSY
	Administrative Data					
Waldfogel (1994)	Fed Courts	Panel FE	5-9%		12-28%	Ex-inmates to non-imprisoned convicts
Grogger (1995)	California	Panel FE	3-8%		11-30%	UI Data
Nagin and Waldfogel (1998b)	AoC	Panel FE	5.4%		7.7%	Fraud offenders
Lalonde and Cho (2008)	Illinois	Panel FE	INCREASE 4pp			Female inmates
Kling (2006)	California & Florida	Panel FE	0		INCREASE 0-33%	UI Data
Pettit and Lyons (2007)	Washington	Panel FE	INCREASE 0-30%	0-4%		UI Data
Harding et al. (2018)	Michigan	IV (Judge)	INCREASE 4-14pp			UI Data
Mueller-Smith (2015)	Texas	IV (Judge)	4.5-9pp		42%-89%	UI Data (1 yr. duration)
Bhuller et al. (2019)	Norway	IV (Judge)	43.6%		48%	Previously Employed
Dobbie et al. (2018)	Pennsylvania & Florida	IV (Judge)	24.7%		16.1%	Tax data
Mueller-Smith and Schnepel (2021)	Texas	RD	50%		183%	UI Data (10 yr impact on earnings)

Table A.3: Negligent Hiring Reform on Labor Market Outcomes (Pooled Sample, Any Charge Record)

Panel A: Outcome - Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Negligent Hiring Reform	0	-0.003	0	-0.003	0	0.002
	(0.001)	(0.004)	(0.001)	(0.004)	(0)	(0.002)
Reform x Criminal History	0.053***	0.042***	0.05***	0.089***	0.056***	0.09***
	(0.008)	(0.012)	(0.008)	(0.005)	(0.009)	(0.006)
Estimation	did2s	twfe	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005	2008	2008
BTB Control	no	no	yes	yes	yes	yes
Obs	12880000	12880000	12880000	12880000	10400000	10400000

Panel B: Outcome - Inverse Hyperbolic Sine (wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0.001	-0.023	0.001	-0.023***
	(0.01)	(0.041)	(0.01)	(0.044)
Reform x Criminal History	-0.051***	0.254***	-0.052***	0.242***
	(0.019)	(0.041)	(0.017)	(0.035)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	12880000	12880000	12880000	12880000

Panel C: Outcome - ln(wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0	-0.001	0	-0.001***
	(0)	(0.001)	(0)	(0.001)
Reform x Criminal History	0.003***	0.006***	0.003***	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	9782000	9782000	9782000	9782000

Source: ACS and CJARS (2020).

State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Table A.4: Negligent Hiring Reform on Labor Market Outcomes - no record x covariate controls (Pooled Sample)

Panel A: Outcome - Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Negligent Hiring Reform	0.000	-0.003	0.000	-0.003	0.000	0.002
	(0.001)	(0.004)	(0.001)	(0.004)	(0.000)	(0.002)
Reform x Criminal History	0.053***	0.042***	0.05***	0.089***	0.056***	0.09***
	(0.008)	(0.012)	(0.008)	(0.005)	(0.009)	(0.006)
Estimation	did2s	twfe	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005	2008	2008
BTB Control	no	no	yes	yes	yes	yes
Obs	12880000	12880000	12880000	12880000	10400000	10400000

Panel B: Outcome - Inverse Hyperbolic Sine (wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0.005	-0.01	0.005	-0.01
	(0.009)	(0.038)	(0.009)	(0.042)
Reform x Criminal History	0.464***	0.355***	0.446***	0.342***
	(0.068)	(0.083)	(0.073)	(0.071)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	12880000	12880000	12880000	12880000

Panel C: Outcome - ln(wage earnings)

	(1)	(2)	(3)	(4)
Negligent Hiring Reform	0.000	-0.001	0.000	-0.001
	(0.000)	(0.001)	(0.000)	(0.001)
Reform x Criminal History	0.003	0.006***	0.003	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)
Estimation	did2s	twfe	did2s	twfe
Sample	pooled	pooled	pooled	pooled
Start year	2005	2005	2005	2005
BTB Control	no	no	yes	yes
Obs	9782000	9782000	9782000	9782000

Source: ACS and CJARS (2020).

State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Table A.5: Heterogeneous Employment Impact of Negligent Hiring Reform

Reform impact on felony or prison history by:									
Felony type		Sex		Age		Education		Race/Ethnicity	
Violent	0.03***	Female	0.032***	Age 25-34	0.041***	< High school	0.043***	Black	0.06***
	(0.006)		(0.01)		(0.012)		(0.01)		(0.011)
	[0.57]		[0.5]		[0.63]		[0.44]		[0.48]
Property	0.024***	Male	0.066***	Age 35-54	0.06***	< College	0.051***	White	0.053***
	(0.003)		(0.011)		(0.011)		(0.01)		(0.015)
	[0.57]		[0.6]		[0.6]		[0.59]		[0.6]
Drug	0.039***			Age 55-64	0.069***	College	0.038*	Hispanic	0.064***
	(0.01)				(0.02)		(0.01)		(0.01)
	[0.54]				[0.42]		[0.71]		[0.64]
Other	0.04***								
	(0.01)								
	[0.57]								
Estimation	did2s		did2s		did2s		did2s		did2s
Sample	court		pooled		pooled		pooled		pooled
BTB Control	yes		yes		yes		yes		yes
Obs	7728000		10400000		10400000		10400000		10400000

State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926) and (CBDRB-FY23-P2295-R10669).

Table A.6: The Impact of Negligent Hiring Reform on Recidivism (2SDID)

	(1)	(2)	(3)	(4)	(5)
	Any	Violent	Property	Drug	Public
Neg. Hiring Reform	-0.0243**	-0.005*	-0.009	-0.005	-0.009**
	(0.012)	(0.003)	(0.007)	(0.003)	(0.004)
Mean	0.195	0.042	0.067	0.053	0.042
Obs	6743916	6743916	6743916	6743916	6743916

Source: NCRP.

Notes: Each column is an estimate is from a separate Gardner (2022) regression. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

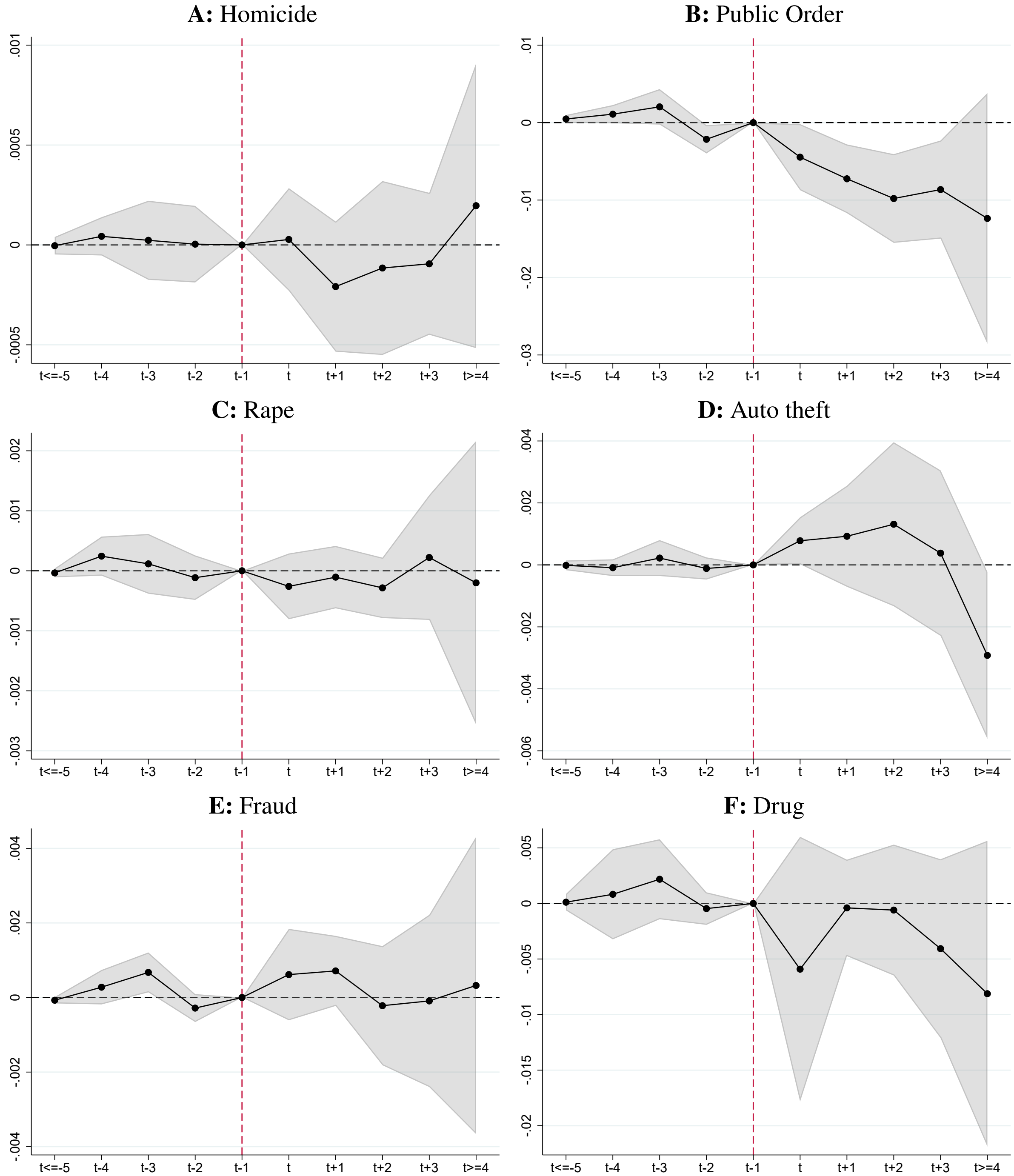
Table A.7: The Impact of Negligent Hiring Reform on Recidivism (TWFE)

	(1)	(2)	(3)	(4)	(5)
	Any	Violent	Property	Drug	Public
Neg. Hiring Reform	-0.020**	-0.003	-0.009***	-0.004	-0.005**
	(0.008)	(0.003)	(0.003)	(0.004)	(0.002)
Mean	0.195	0.042	0.067	0.053	0.042
Obs	6743916	6743916	6743916	6743916	6743916

Source: NCRP.

Notes: Each column is an estimate is from a separate TWFE regression. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Figure A.2: Negligent Hiring Reform and Recidivism by New Offense Crime Type



Source: NCRP.

Notes: Shaded area is 95% confidence interval using Gardner (2022). Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Table A.8: Heterogeneous Employment Impact of Negligent Hiring Reform

	(1)	(2)	(3)	(4)	(5)
	Any	Violent	Property	Drug	Public
Violent	-0.021*	-0.017	-0.002	-0.000	-0.005
	(0.013)	(0.012)	(0.013)	(0.007)	(0.005)
	[0.153]	[0.08]	[0.028]	[0.022]	[0.028]
Property	-0.027***	-0.000	-0.032**	0.004	-0.002
	(0.009)	(0.008)	(0.013)	(0.005)	(0.006)
	[0.236]	[0.033]	[0.148]	[0.036]	[0.029]
Drugs	-0.025*	-0.001	-0.002	-0.021*	-0.006
	(0.015)	(0.008)	(0.013)	(0.011)	(0.006)
	[0.179]	[0.021]	[0.032]	[0.107]	[0.025]
Public Order	-0.026	0.000	0.001	-0.000	-0.032***
	(0.018)	(0.007)	(0.013)	(0.005)	(0.011)
	[0.21]	[0.035]	[0.038]	[0.036]	[0.113]

Source: NCRP.

Notes: Each column is an estimate is from a separate Gardner (2022) regression. Means are in brackets. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Table A.9: Heterogeneous Employment Impact of Negligent Hiring Reform

	(1) Any	(2) Violent	(3) Property	(4) Drug	(5) Public
Homicide	0.008	-0.008	0.011	0.002	0.002
	(0.032)	(0.007)	(0.015)	(0.010)	(0.007)
Neg Mansl	0.011	-0.010*	0.010	0.007	0.004
	(0.027)	(0.006)	(0.015)	(0.009)	(0.006)
Rape, SA	-0.045	-0.042***	0.006	0.002	-0.015*
	(0.029)	(0.015)	(0.014)	(0.009)	(0.009)
Robbery	-0.023*	-0.020*	-0.004	0.000	-0.001
	(0.013)	(0.010)	(0.011)	(0.007)	(0.004)
Assault	-0.019**	-0.005	-0.009	-0.003	-0.007
	(0.008)	(0.016)	(0.012)	(0.005)	(0.005)
Other Violent	0.005	-0.014	0.007	0.004	0.005
	(0.016)	(0.008)	(0.012)	(0.007)	(0.004)
Burglary	-0.037***	0.001	-0.044***	0.004	-0.003
	(0.009)	(0.008)	(0.012)	(0.005)	(0.005)
Larceny	-0.028**	-0.005	-0.019	0.001	-0.007
	(0.013)	(0.009)	(0.018)	(0.006)	(0.006)
Auto Theft	0.005	-0.010	0.007	0.004	0.002
	(0.014)	(0.006)	(0.019)	(0.004)	(0.006)
Fraud	-0.014	0.001	-0.028***	0.007	0.002
	(0.016)	(0.009)	(0.007)	(0.005)	(0.009)
Other Property	-0.016	0.012	-0.041***	0.003	0.003
	(0.017)	(0.009)	(0.012)	(0.007)	(0.007)
Drugs	-0.025*	-0.001	-0.002	-0.021*	-0.006
	(0.015)	(0.008)	(0.013)	(0.011)	(0.006)
Public Order	-0.026	0.000	0.001	-0.000	-0.032***
	(0.018)	(0.007)	(0.013)	(0.005)	(0.011)
Other	-0.007	0.005	0.005	0.010	0.010
	(0.021)	(0.009)	(0.008)	(0.007)	(0.007)

Source: NCRP.

Notes: Each column is an estimate is from a separate Gardner (2022) regression. Means are in brackets. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Table A.10: Heterogeneous Employment Impact of Negligent Hiring Reform

Reform impact new crime recidivism by:					
Sex		Age		Race/Ethnicity	
Female	-0.017	Age 25-34	-0.026**	Black	-0.0348**
	(0.014)		(0.011)		(0.015)
	[0.16]		[0.21]		[0.21]
Male	-0.025**	Age 35-54	-0.026	White	-0.022*
	(0.012)		(0.017)		(0.012)
	[0.20]		[0.17]		[0.20]
		Age 55-64	-0.008	Hispanic	-0.014
			(0.027)		(0.017)
			[0.08]		[0.15]

Source: NCRP.

Notes: Each column is an estimate is from a separate Gardner (2022) regression. Means are in brackets. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

Table A.11: The Impact of Negligent Hiring Reform on Recidivism (Stacked Regression)

	(1)	(2)	(3)	(4)	(5)
	Any	Violent	Property	Drug	Public
Neg. Hiring Reform	-0.020**	-0.003	-0.009***	-0.005	-0.003*
	(0.009)	(0.003)	(0.003)	(0.005)	(0.003)
Alvarez/Ferman p	0.08	0.12	0.01	0.15	0.05

Source: NCRP.

Notes: Each column is an estimate is from a separate stacked difference-in-difference regression. State clustered robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p< 0.01. P-values calculated using Alvarez and Ferman (2023) are also displayed. Recidivism is defined as three-year prison re-entry. Controls: state and year fixed effects, last offense type, number of previous offenses and its square, race, gender, admission/release year, time served squared, release type, and sentence length. Data from 2005-2019.

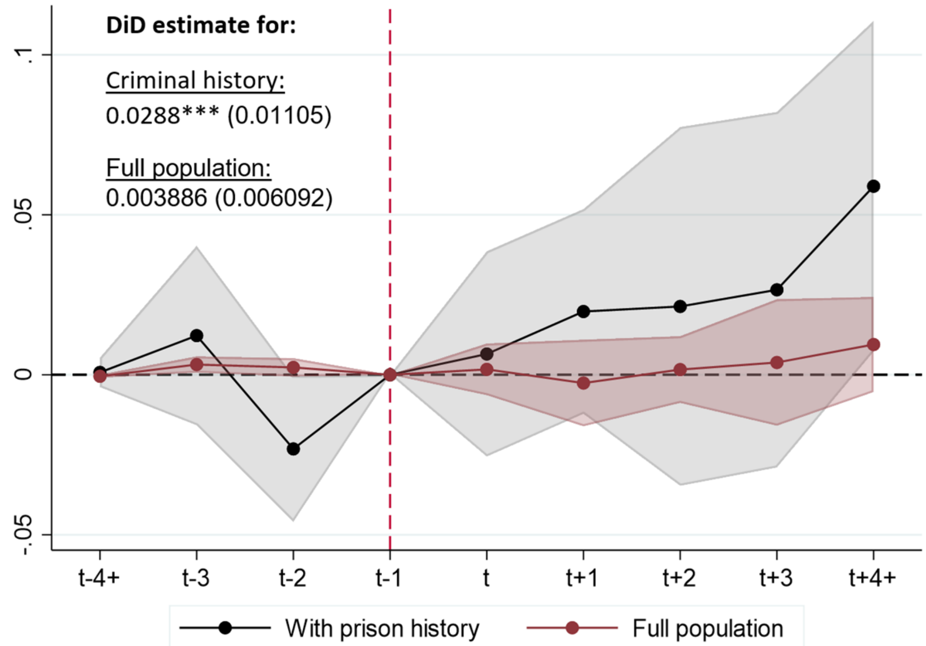
A.1.5 The Interaction of Negligent Hiring Reform and Ban-the-Box

Liability for negligent hiring and efforts to reform the tort interact with a web of other criminal justice reforms. One movement particularly connected to negligent hiring reform is Ban-the-Box (BtB). Ban-the-Box laws disallow employers to ask about criminal records until late in the hiring process and have become more widespread in recent years. The adoption of BtB is often tied with negligent hiring reform. For instance, the same bill that New Jersey adopted BtB rules required a gross negligence standard to be reached for negligent hiring claims. Indiana's reform limiting what criminal history can be presented in negligent hiring claims also preempted the local jurisdiction's ability to implement Ban-the-Box laws. If either BtB or negligent hiring reforms impact employment outcomes, failure to account for both reforms in an analysis could introduce bias. For example, if both reforms increase employment for workers with criminal histories, failure to control for both policies would cause an overestimate of the increases to the studied policy.

However, it is worth noting that, unlike negligent hiring reform, BtB doesn't change the underlying economics of actually employing workers with criminal histories-it only alters the information available to employers (and changes the screening costs). While not entirely free from controversy, academic research has found that employers remain reluctant to hire workers with criminal histories. Restricting access to direct information on criminal records results in employers using age, race, and sex as a proxy for the probability of past criminal behavior. Previous research has suggested that Ban-the-Box causes young black men to receive fewer callbacks after applying for a job and are less likely to be employed Agan and Starr (2017); Doleac (2016). The research thus far has found some weak evidence for increased employment in the public sector but minimal labor market improvements overall for individuals with criminal histories Rose (2021); Raphael (2021).

To assess whether BtB might be a potential confounder for my analysis of negligent hiring reform, I estimate Equation 1, substituting BtB reform in place of negligent hiring reform and restricting to the prison sample. While subsequent analysis should place additional focus on other groups, the analysis presented below is confined to the group previous research has indicated the most likely to benefit from BtB: young, less educated white males (white men aged 25-34 with no college degree who are not currently in prison). It is important to remember that previous research has suggested that other groups, namely people of color, may face worse opportunities due to BtB reforms. Figure A.3 suggests that BtB policies increase employment for young white men who have previously been in prison (albeit to a

Figure A.3: Event Study - Ban-the-Box and Employment of Young Less Educated White Males



Source: ACS and CJARS (2020).
Notes: Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2295. (CBDRB-FY22-P2295-R9926)

lesser degree than negligent hiring reform). This finding is important and relevant both independently from negligent hiring reform and as a justification for including BtB policies as a control in the negligent hiring reform analysis. In addition, this finding suggests a second look at BtB, given greater data availability and the implementation of other policies that have shaped hiring incentives for workers with criminal (e.g., EEOC enforcement, negligent hiring reform, etc.), is warranted.

A.1.6 PSID Employment Analysis Replication

One data source for such questions is nationally representative surveys that ask individuals about their past behavior and/or track people over time and ask follow-up questions. I use the Panel Study of Income Dynamics (PSID) for this study. The PSID consists of data covering 1969-2017 and includes almost 80,000 individuals. I supplement the main PSID survey with an additional module, the Transition to Adulthood Supplement. Despite the more substantial number of individuals surveyed in the PSID, the number of individuals within the sample with criminal justice exposure is much smaller (862 people), leading to challenges in drawing statistical inferences from this population. This is a challenge in this section as well.

Additionally, constructing a measure of criminal justice exposure in the PSID is problematic. This paper proposes several potential methods for doing so, but none is a perfect measure of the precise object of interest to this study, the presence of an observable (by employers) criminal record for a given individual. This paper uses the following construction for generating criminal histories: if the reason an individual is a non-respondent in a given year is that they are in jail or prison, they are marked as having a criminal record starting in that year. This measure is both under and over-inclusive, as not all individuals who spend time in jail will have an observable criminal record, and some individuals may have had a criminal record without spending time in jail. As above, the analysis relies on two sources of potential variation in exposure to the policy change. First, there may be variation in policy exposure for individuals with a criminal history when a state changes its negligent hiring liability. Second, individuals may gain a new criminal history by offending in a state with an already existing negligent hiring reform.[1] If lowering negligent hiring liability increases employment prospects, we would expect the enactment of the reform to improve labor market outcomes for individuals with a criminal history but not for those without a record. Additionally, we expect individuals who offend within a state with a negligent hiring reform to experience smaller employment penalties than those who offend in states without negligent hiring reforms.

For inference to be valid, it is important to ensure enough individuals with convictions are in the sample and that these individuals are roughly comparable between the states that have and have not enacted negligent hiring reform. Below I present the summary statistics. The first panel splits the sample across states that

[1]The data does like fully capture all individuals with a criminal history. Measuring criminal exposure with noise will bias results towards zero, so the impact of the studied reforms may be larger in magnitude than what is documented here.

have and have never enacted negligent hiring reform. One hundred twenty-nine individuals have been incarcerated in states with negligent hiring reform; 732 individuals were released in states that have not enacted reforms to curb liability for negligent hiring. States with negligent hiring reform look similar regarding state-wide earnings and employment measures to states without. However, states with negligent hiring reform are more likely to have enacted other policies related to an employer's ability to use criminal history in hiring decisions. Specifically, negligent hiring reform states are more likely to have open internet access (defined as having a relatively low-cost publicly available criminal history database) to the public for previous convictions and are more likely to have enacted "Ban-the-Box" rules. The second panel splits the sample across people who have ever been flagged as incarcerated. As expected, individuals who have been incarcerated are more than twice as likely to be unemployed (defined as looking for work), less likely to be working currently, and, contingent on being employed, earn less money.

Table A.12: PSID summary statistics

	Neg Hiring Reform				Incarcerated			
	No		Yes		No		Yes	
	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean
Ever Incarcerated	73429	0.006	5469	0.011	78386	0.000	512	1.000
Neg hiring reform	73429	0.000	5469	1.000	78386	0.069	512	0.119
Unemployed	56778	0.081	3983	0.101	60257	0.081	504	0.206
Employed	40714	0.840	3585	0.837	43868	0.842	431	0.537
Log earnings	64714	9.980	5303	10.148	69537	9.997	480	9.355
Log wage	64100	2.635	5321	2.766	68942	2.647	479	2.313
Internet	73429	0.129	5469	0.317	78386	0.142	512	0.207
BtB	73429	0.015	5469	0.169	78386	0.025	512	0.047
Public BtB	73429	0.053	5469	0.378	78386	0.075	512	0.127

In the table below, I identify the impact of negligent hiring reform on employment outcomes by estimating the following regression:

$$Y_{it} = X_{it}\beta_1 + \beta_2 \text{Inc}_{it} + \beta_3 \text{NHR}_{st} + \beta_4 \text{NHR}_{st} * \text{INC}_{it} + \beta_5 \text{ANH}_{st} + \beta_6 \text{ANH}_{st} * \text{INC}_{it} + \beta_7 Z_{st} + \lambda_t + \lambda_i + \epsilon_{it}$$

In this equation, Y is a relevant labor market outcome (employment, earnings) for individual i, in year t. X is a vector of time-varying individual controls such as years of work experience (and its square), age (and its square), Inc. is whether an individual has been incarcerated (and thus has a criminal record) by year t, ANH is an indicator for having recognized negligent hiring, NHR is an indicator for negligent hiring reform, Z is a vector of time-varying state labor market characteristics (average wage and unemployment rate in a state), and the lambda terms are individual and year fixed effects. The coefficient associated with negligent hiring reform interacted with the previous incarceration allows us to observe the differential impact of passing such legislation on the employment prospects for those with and without criminal histories. Including a control for individual fixed effects allows inference to be drawn from changes within a given individual-in other words, this strategy is robust to unobserved differences between individuals (something that is likely a concern when comparing individuals interacting with the criminal justice system). The earnings results are similar after dropping the individual fixed effect and including additional demographic controls.

Table A.13: Impact of negligent hiring in the PSID

	(1) Unemployed	(2) Unemployed	(3) Working	(4) Working	(5) Ln(Earn)	(6) Ln(Earn)	(7) Arcsinh(Earn)	(8) Arcsinh(Earn)
Neg. Hiring Reform	0.0000653	0.00115	-0.00160	-0.0132	-0.0625**	-0.0379	-0.168	-0.132
	(0.00313)	(0.00725)	(0.0122)	(0.0162)	(0.0233)	(0.0357)	(0.0908)	(0.129)
Neg. Hire Adopt	0.00382	0.00981	0.000690	-0.00542	-0.00969	0.0111	0.0248	-0.111
	(0.00239)	(0.00491)	(0.00656)	(0.00803)	(0.0167)	(0.0276)	(0.0616)	(0.0998)
Previously Inc.	0.00288	0.0746	-0.00329	-0.120**	0.0165	-0.557***	-0.769	-1.598**
	(0.0502)	(0.0376)	(0.0560)	(0.0346)	(0.193)	(0.150)	(0.696)	(0.556)
Neg. Hire Ref. x Inc.	-0.0276	0.0629	0.0127	-0.0355	0.460*	0.482**	1.848**	0.619
	(0.0279)	(0.0558)	(0.0401)	(0.0420)	(0.180)	(0.176)	(0.565)	(0.822)
Neg. Hire Adopt x Inc.	0.0205	0.0856*	-0.0129	-0.0604	-0.232	-0.202	-0.689	-0.825
	(0.0493)	(0.0375)	(0.0523)	(0.0312)	(0.157)	(0.170)	(0.588)	(0.583)
Person FE	Yes	No	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

State clustered robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

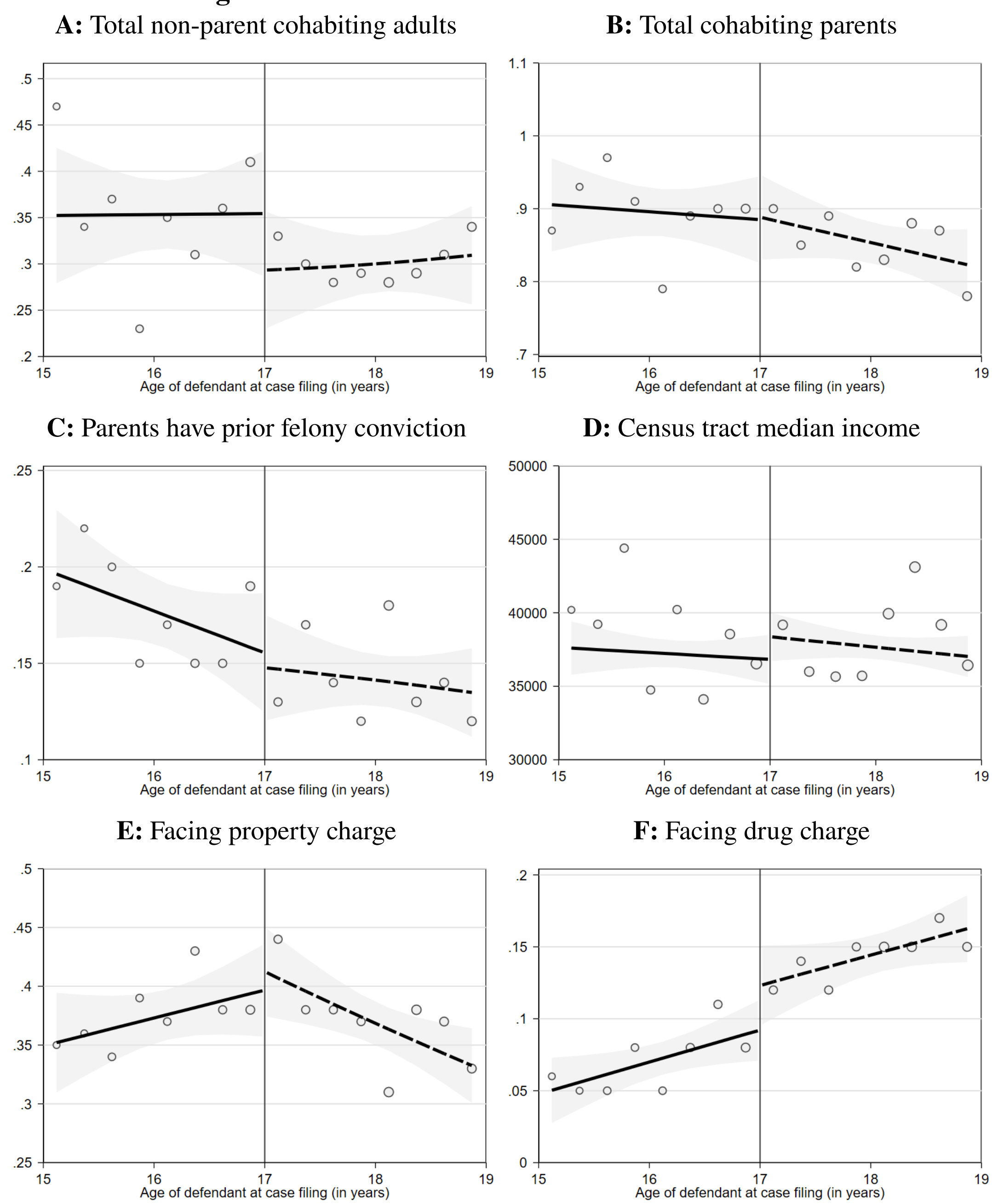
Also controls for education, age, state economic conditions, and work experience

APPENDIX B

Appendix to Chapter 2

B.1 Supplementary Results

Figure B.**1:** Additional evidence on caseload balance

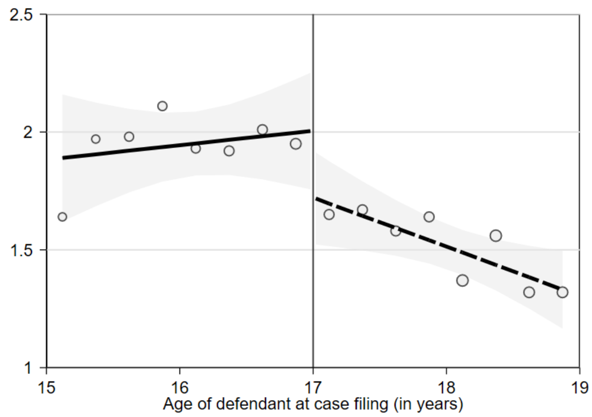


Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from (Finlay et al., 2022c).

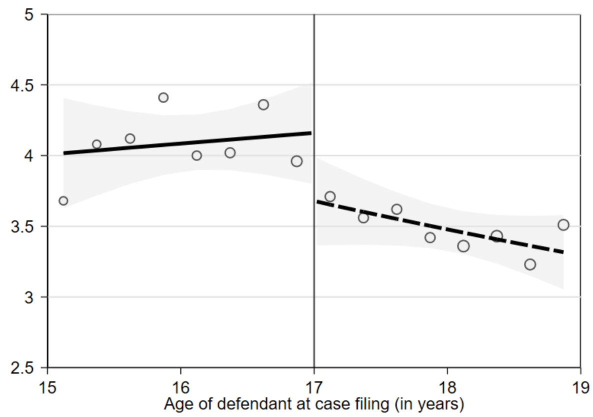
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022).

Figure B.2: Additional crime and employment outcomes across the cutoff

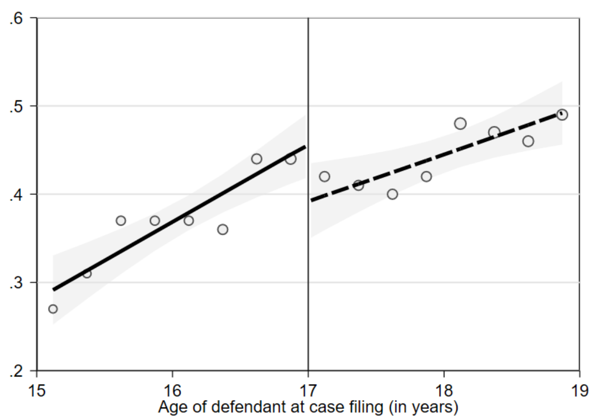
A: Total felony convictions



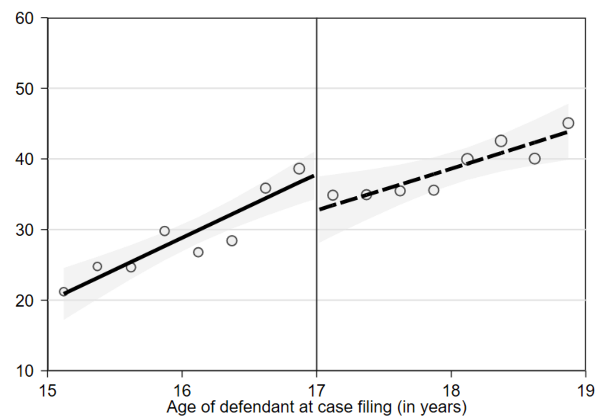
B: Total criminal convictions



C: Annual W-2's Filed

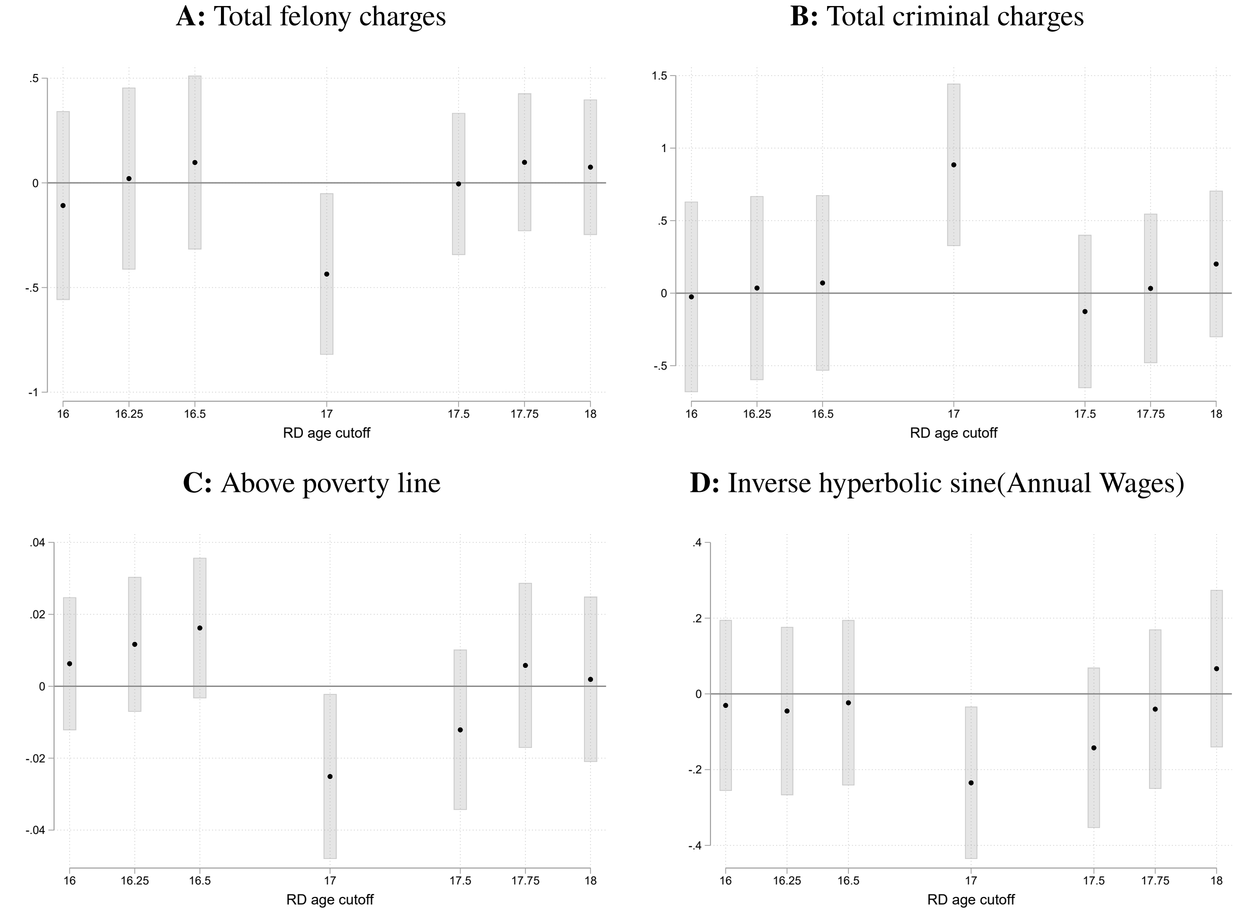


D: Annual W-2 Wages



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from (Finlay et al., 2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors restricted. Regression also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Figure B.**3:** Reduced form: recidivism and employment outcomes across placebo and true cut-offs

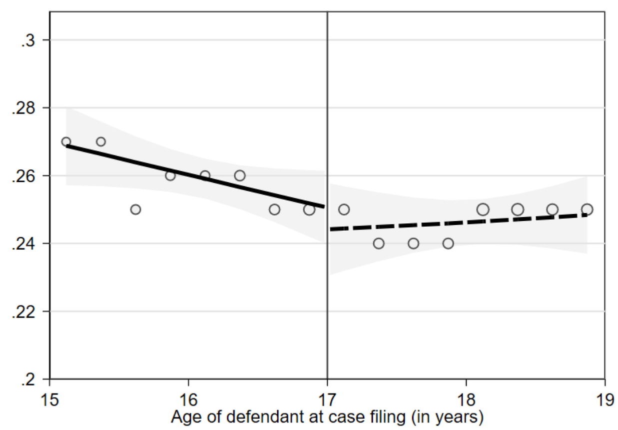


Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

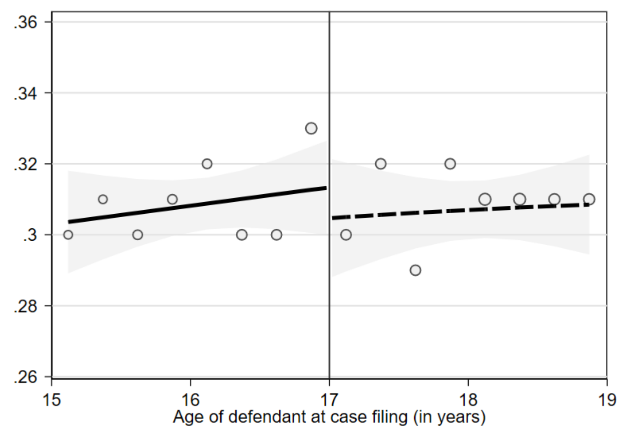
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of observation within 2-years of the cut-off. All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022), #CBDRB-FY23-0392 (approved 7/10/23) and #CBDRB-FY23-0414 (approved 7/17/23). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regression also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Figure B.4: Reduced form: predicted dispositions across the discontinuity

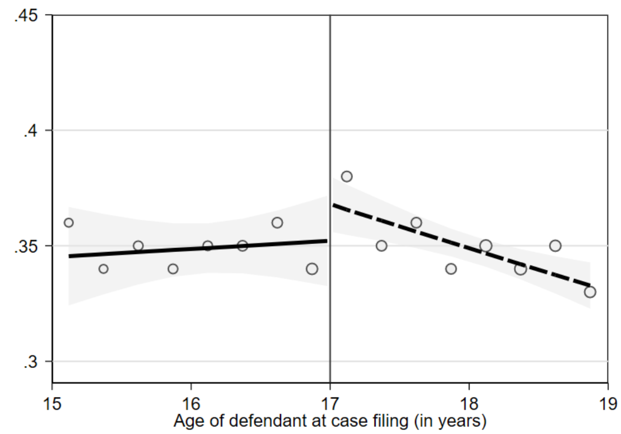
A: Probability of adult dismissal



B: Probability of adult incarceration



C: Probability of juvenile dismissal



Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from (Finlay et al., 2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022).

Table B.1: Confusion matrices and distribution of out-of-sample predictions

Number of realized (rows)	Number predicted (columns)		
Panel A: Confusion matrix among adult defendants			
	No Conviction	Conviction + incarceration	Conviction + other punish.
Adult no conviction	**250**	20	D
Adult conv. + incar.	D	**500**	30
Adult conv. + other punish.	D	150	**1,800**
Panel B: Confusion matrix among juvenile defendants			
	No conviction	Conviction + services	
Juvenile no conviction	**600**	60	
Juvenile conv. + services	30	**1,400**	
Panel C: Out-of-sample predictions among adult defendants			
	No conviction	Conviction + services	
Adult no conviction	40	250	
Adult conv. + incar.	50	450	
Adult conv. + other punish.	350	1,600	
Panel D: Out-of-sample predictions among juvenile defendants			
	No Conviction	Conviction + incarceration	Conviction + other punish.
Juvenile no conviction	D	80	550
Juvenile conv. + services	40	200	1,200

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases).All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). D indicates a cell is small and suppressed to preserve confidentiality.

Table B.2: Regression of random forest generated probabilities on explanatory variables

	(1) P(Adult conviction)	(2) P(Adult incarceration)	(3) P(Juvenile conviction)
Black	-0.792*	1.81***	-0.205
	(0.426)	(0.466)	(0.602)
Male	2.907***	12.33***	3.333***
	(0.643)	(0.546)	(0.821)
Any misdemeanor history	5.547***	6.418	-0.394
	(2.15)	(5.022)	(5.693)
Days since first charge	0.267	-0.015	0.723
	(0.404)	(0.508)	(0.512)
Charged in past month	-3.91*	-4.925	6.121
	(2.274)	(5.081)	(5.781)
Charged between one month and last half year	-3.654*	-3.885	8.556
	(2.189)	(5.049)	(5.707)
Charged between half year and last year	-2.889	-4.938	8.46
	(2.214)	(5.065)	(5.726)
Employed in 2010	0.05	0.345	-2.164*
	(1.203)	(1.23)	(1.153)
Number of violent misdemeanors history	1.412***	-0.016	-0.76
	(0.389)	(0.48)	(0.615)
Number of property misdemeanors history	0.551**	0.688	-0.645*
	(0.269)	(0.472)	(0.378)
Facing violent felony charge	-1.515***	15.34***	6.703***
	(0.554)	(0.7)	(0.713)
Facing property felony charge	3.039***	2.566***	3.827***
	(0.535)	(0.699)	(0.698)
Facing drug felony charge	13.26***	-11.52***	-1.552
	(0.776)	(0.872)	(0.972)
Previous charge in house	1.684*	0.604	-0.266
	(1.02)	(1.079)	(1.377)
Parent previous charge in house	-1.09	1.078	1.013
	(0.755)	(0.836)	(1.012)
Previous felony charge in house	-0.894	-0.479	1.436
	(0.648)	(0.741)	(0.91)
Parent previous felony charge in house	0.642	0.589	0.427
	(0.543)	(0.597)	(0.735)
Previous felony conviction in house	-0.256	0.586	-0.171
	(0.537)	(0.575)	(0.722)
Parent previous felony conviction in house	0.928	-0.702	0.306
	(0.636)	(0.722)	(0.94)
Previous incarceration in house	0.978	-1.176*	0.05
	(0.596)	(0.631)	(0.86)
Parent previous incarceration in house	0.769	-0.429	0.468
	(0.931)	(1.053)	(1.371)
Family 2010 AGI	0.002**	-0.007***	-0.004***
	(0.001)	(0.001)	(0.001)
Household 2010 AGI	0.001	0.00	-0.002**
	(0.00)	(0.00)	(0.001)
Parents in 2010 house	-0.166	-1.324***	-1.565***
	(0.309)	(0.334)	(0.438)
Other adults in 2010 house	0.987***	-0.406	0.299
	(0.196)	(0.259)	(0.31)
2009-10 violent charges in zip	-3.657***	1.424	-0.717
	(1.064)	(1.154)	(1.587)
2009-10 property charges in zip	-0.205	-1.025	-0.186
	(1.592)	(1.691)	(2.312)
2009-10 drug charges in zip	3.248*	-0.062	0.29
	(1.752)	(1.958)	(2.415)
2009-10 public order charges in zip	-26.86	-89.62***	-71.95
	(29.68)	(30.41)	(46.02)
2009-10 public order charges in zip	8.456	7.349	38.79***
	(5.23)	(5.519)	(7.008)
Unable to identify zip	-2.342***	10.63***	8.373***
	(0.728)	(0.736)	(0.978)
Unable to identify tract	-14.33***	16.04***	-11.2***
	(0.389)	(0.447)	(0.567)
Tract percent poverty	0.236***	-0.012	-0.086
	(0.058)	(0.067)	(0.089)
Tract percent youth poverty	-0.091***	0.006	-0.011
	(0.031)	(0.037)	(0.05)
R-squared	0.422	0.552	0.273
Obs	4700	4700	4700

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022).

Table B.3: Simple RD estimated on samples defined by predicted case dispositions

	5-year employment from RD over subsample:					
	Predicted juvenile no conviction and adult:			Predicted juvenile conviction and adult:		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
	No conviction	Incarceration	Non-Incar. punishment	No conviction	Incarceration	Non-Incar. punishment
Recidivism:						
Felony Charge	3.924**	-1.277	-0.691	-2.231	-1.216*	-0.278
	(1.552)	(1.946)	(0.495)	(1.589)	(0.671)	(0.284)
Felony Conviction	2.56*	-1.62	-0.07	-2.103	-1.155*	-0.223
	(1.496)	(1.735)	(0.435)	(1.489)	(0.618)	(0.244)
Charge	-0.167	-5.103	-1.191	-2.264	-2.252**	-0.577
	(2.646)	(3.536)	(0.801)	(1.786)	(0.894)	(0.429)
Conviction	-2.88	-4.808	-0.439	-2.101	-1.679**	-0.282
	2.067	2.763	0.696	1.667	0.788	(0.367)
Employment:						
IHS (Annual Wages)	1.167	-1.025	0.07	0.342	-0.236	-0.274
	(1.016)	(1.34)	(0.301)	(0.697)	(0.336)	(0.162)
5-year income (1000s)	-19.95	9.118	-0.737	-17.41	-4.43	-4.357
	(17.75)	(32.15)	(11.58)	(21.17)	(5.846)	(4.387)
W2 worked w/in 5yr	-0.187	-0.045	0.005	-0.121	-0.125	-0.054
	(0.229)	(0.286)	(0.093)	(0.198)	(0.069)	(0.044)
Earnings above poverty line	-0.124	-0.013	-0.007	-0.06	-0.008	-0.026
	(0.08)	(0.115)	(0.037)	(0.076)	(0.02)	(0.016)
Left obs	<30	60	500	50	250	1100
Right obs	40	50	350	200	550	1500

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table B.4: Simple RD estimated on samples defined by covariates

		Race		Sex		Facing felony type:			Family AGI vs median	
	Full sample	Black	Not black	Male	Female	Violent	Property	Drug	Above	Below
Recidivism (intensive):										
Felony conviction	-0.361*	-0.543**	-0.224	-0.469**	-0.042	-0.838***	0.18	0.383	-0.199	-0.568*
	(0.188)	(0.272)	(0.258)	(0.214)	(0.381)	(0.251)	(0.294)	(0.529)	(0.252)	(0.281)
Charge	-1.022***	-1.493***	-0.618	-1.272***	-0.376	-1.618***	-0.415	-1.089	-0.903**	-1.198**
	(0.326)	(0.461)	(0.459)	(0.364)	(0.715)	(0.423)	(0.496)	(1.113)	(0.433)	(0.492)
Employment:										
IHS (Annual Wages)	-0.275**	-0.123	-0.33**	-0.206	-0.395**	-0.055	-0.598***	-1.012**	-0.144	-0.329*
	(0.124)	(0.178)	(0.168)	(0.144)	(0.181)	(0.164)	(0.192)	(0.417)	(0.17)	(0.175)
Earnings above poverty line	-0.027**	-0.016	-0.03	-0.023	-0.039*	-0.009	-0.053***	-0.065	-0.01	-0.04**
	(0.012)	(0.015)	(0.019)	(0.014)	(0.021)	(0.015)	(0.02)	(0.047)	(0.018)	(0.016)
Incarceration:										
Prison days	125***	161.9***	81.78**	124.4***	111.7**	149.1***	141.9***	64.68	106.5***	148***
	(27.92)	(42.1)	(36.38)	(32.13)	(55.45)	(40.33)	(40.14)	(64.47)	(35.82)	(43.49)
Jail days	26.67***	20.98**	32.01***	25.4***	32.02***	12.67	40.92***	31.04	24.94***	27.46***
	(6.8)	(10.08)	(9.172)	(7.943)	(11.83)	(9.112)	(10.39)	(20.41)	(8.871)	(10.35)
Disposition:										
Juvenile conviction	-0.682***	-0.726***	-0.638***	-0.691***	-0.656***	-0.749***	-0.704***	-0.597***	-0.639***	-0.73***
	(0.023)	(0.03)	(0.034)	(0.025)	(0.055)	(0.027)	(0.037)	(0.081)	(0.031)	(0.033)
Adult no conviction	0.081***	0.075***	0.087***	0.062***	0.159***	0.085***	0.099***	0.026	0.092***	0.069***
	(0.014)	(0.02)	(0.019)	(0.015)	(0.035)	(0.019)	(0.02)	(0.036)	(0.018)	(0.021)
Adult incareration	0.171***	0.201***	0.142***	0.179***	0.135***	0.185***	0.191***	0.112***	0.126***	0.225***
	(0.017)	(0.026)	(0.024)	(0.02)	(0.035)	(0.025)	(0.027)	(0.035)	(0.022)	(0.028)
Adult conv. non-incareration	0.748***	0.725***	0.771***	0.759***	0.706***	0.73***	0.71***	0.862***	0.782***	0.706***
	(0.02)	(0.029)	(0.028)	(0.023)	(0.044)	(0.029)	(0.032)	(0.048)	(0.026)	(0.031)
Left obs:	2000	1000	1100	1600	400	1200	700	100	1100	900
Right obs:	2700	1400	1300	2200	600	1500	1000	400	1400	1300

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table B.5: Mechanism-specific treatment effect estimates (IV decomposition)

	Extensive					Intensive				
	Juvenile Charge	Juvenile Conv.	Adult Conv.	Adult Incar.	Adult No. Conv. Ave.	Juvenile Charge	Juvenile Conv.	Adult Conv.	Adult Incar.	Adult No. Conv. Ave.
Recidivism:										
Charge	-0.016	0.013	-0.049	-0.292***	0.761	0.499	-0.073	-0.09	-2.793***	4.621
	(0.062)	(0.052)	(0.086)	(0.068)		(0.807)	(0.678)	(1.161)	(0.888)	
Conviction	0.027	-0.001	0.016	-0.228***	0.674	0.896	-0.067	0.69	-1.994***	3.371
	(0.071)	(0.060)	(0.101)	(0.08)		(0.686)	(0.574)	(0.989)	(0.766)	
Felony charge	0.173**	-0.008	0.193*	-0.257***	0.455	1.606***	-0.048	1.584**	-2.037***	1.936
	(0.077)	(0.065)	(0.11)	(0.088)		(0.555)	(0.4739)	(0.788)	(0.572)	
Felony conviction	0.257***	-0.003	0.29***	-0.226***	0.379	1.767***	0.032	1.847***	-1.491***	1.356
	(0.078)	(0.065)	(0.11)	(0.087)		(0.489)	(0.414)	(0.693)	(0.505)	
Recidivism by type of offense:										
Violent charges	0.120	-0.002	0.164	-0.236***	0.402	0.961*	-0.065	0.859	-1.546***	1.648
	(0.077)	(0.065)	(0.11)	(0.087)		(0.485)	(0.411)	(0.692)	(0.515)	
Property charges	0.037	-0.012	0.045	-0.286***	0.318	0.423	-0.188	0.345	-1.632***	1.28
	(0.078)	(0.066)	(0.111)	(0.086)		(0.463)	(0.387)	(0.675)	(0.516)	
Drug charges	0.105	0.004	0.073	-0.125	0.277	0.093	0.014	-0.2	-0.242	0.883
	(0.070)	(0.059)	(0.101)	(0.079)		(0.331)	(0.285)	(0.475)	(0.38)	
Other charges	-0.017	0.032	-0.067	-0.371***	0.64	0.023	0.018	-0.593	-1.612**	3.03
	(0.072)	(0.060)	(0.101)	(0.08)		(0.638)	(0.538)	(0.913)	(0.71)	
Employment:										
Average # of W-2s per year	0.076	0.035	0.04688	-0.2567***	0.4924					
	(0.075)	(0.063)	(0.1057)	(0.0813)						
Earnings above poverty line	0.025	0.015	0.0107	-0.06637**	0.1212					
	(0.027)	(0.022)	(0.03889)	(0.03065)						
Annual W-2 wages						1.96	4.54	-0.87	-19.68**	42.42
						(7.42)	(6.16)	(10.89)	(8.61)	
IHS(Annual wages)						-0.202	0.114	-0.4169	-0.6602**	2.765
						(0.284)	(0.239)	(0.4025)	(0.3237)	

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). Sargan-Hansen J for 5-year felony conviction recidivism (intensive) is 3.737, p-value 0.154 and for IHS(Average yearly income over next 5-years), 1.923 p-value 0.382). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022) and #CBDRB-FY23-088 (approved 12/12/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table B.6: Impact of case outcomes on crime and employment outcomes by follow-up year (IV decomposition estimates)

	Juvenile No Conv.	Juvenile Conv.	Adult Conv.	Adult Incar.	Adult No Conv. Ave.		Juvenile No Conv.	Juvenile Conv.	Adult Conv.	Adult Incar.	Adult No Conv. Ave.
						Recidivism (intensive):					
Felony Conviction						Charge					
Year 1	0.744**	0.449	0.702**	-0.532**	0.341	Year 1	0.447	0.145	0.186	-0.635**	1.008
	(0.316)	(0.296)	(0.305)	(0.235)			(0.429)	(0.398)	(0.421)	(0.313)	
Year 2	0.211	0.288	0.209	-0.309	0.371	Year 2	0.461	0.394	0.141	-0.566*	1.072
	(0.317)	(0.313)	(0.324)	(0.206)			(0.429)	(0.406)	(0.433)	(0.306)	
Year 3	0.398	0.434*	0.454*	-0.162	0.261	Year 3	-0.059	-0.214	-0.171	-0.358	0.864
	(0.251)	(0.233)	(0.248)	(0.198)			(0.417)	(0.378)	(0.422)	(0.33)	
Year 4	0.046	0.316	0.177	-0.303	0.189	Year 4	-0.155	-0.031	-0.254	-0.989***	0.875
	(0.239)	(0.217)	(0.248)	(0.197)			(0.397)	(0.375)	(0.414)	(0.333)	
Year 5	0.3	0.311	0.305	-0.184	0.193	Year 5	-0.037	0.132	0.008	-0.246	0.803
	(0.293)	(0.257)	(0.291)	(0.219)			(0.446)	(0.402)	(0.453)	(0.356)	
						Employment:					
Average # of W-2s per year						Earnings above poverty line					
Year 1	0.012	0.192**	0.248***	-0.511***	0.337	Year 1	0.004	0.049**	0.051*	-0.097***	0.027
	(0.095)	(0.083)	(0.093)	(0.073)			(0.028)	(0.023)	(0.028)	(0.022)	
Year 2	-0.067	0.108	-0.021	-0.072	0.644	Year 2	-0.024	0.03	0.035	-0.128***	0.095
	(0.193)	(0.177)	(0.189)	(0.142)			(0.043)	(0.036)	(0.041)	(0.033)	
Year 3	-0.086	0.065	-0.085	-0.224*	0.485	Year 3	-0.059	0.011	-0.009	-0.065	0.125
	(0.156)	(0.137)	(0.151)	(0.118)			(0.06)	(0.053)	(0.059)	(0.047)	
Year 4	0.178	0.15	0.078	-0.055	0.538	Year 4	0.051	0.072	0.009	-0.013	0.163
	(0.172)	(0.157)	(0.168)	(0.129)			(0.068)	(0.063)	(0.068)	(0.054)	
Year 5	-0.067	0.108	-0.021	-0.072	0.644	Year 5	-0.013	0.039	-0.031	-0.029	0.197
	(0.193)	(0.177)	(0.189)	(0.142)			(0.077)	(0.07)	(0.076)	(0.06)	
						Incarceration:					
Prison days						Jail days					
Year 1	-5.664	-16.17**	-13.75**	77.81***	12.03	Year 1	0.241***	-4.333**	19.39	-12.24	6.196
	(7.163)	(6.706)	(7.004)	(8.399)			(6.738)	(5.676)	(6.429)	(5.379)	
Year 2	-4.101	-28.64**	-23	152.9***	29.17	Year 2	1.299	-3.233	9.138	-6.183	7.603
	(14.13)	(13.25)	(14.29)	(14.47)			(8.704)	(7.174)	(8.237)	(7.73)	
Year 3	2.257	-25.34	-16.34	152.6***	38.69	Year 3	0.202	1.474	5.363	-13.89	3.114
	(19.58)	(19)	(19.92)	(17.62)			(7.88)	(7.293)	(8.121)	(7.61)	
Year 4	15.91	-17.4	7.338	150.5***	46.48	Year 4	22.68**	19.51**	21.51**	-8.367	7.722
	(23.85)	(23.26)	(24.18)	(20.03)			(9.467)	(8.464)	(9.192)	(6.747)	
Year 5	11.32	-13.77	8.697	130.5***	49.88	Year 5	-16.48	-3.897	-6.758	-10.03	4.29
	(26.33)	(25.48)	(26.71)	(21.82)			(8.57)	(7.687)	(9.133)	(8.854)	

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relations and exposure from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Coefficients are estimated using a linear IV approach with robust standard errors restricted to sample cases with defendants between the ages of 15 and 19. Regressions also control for the full set of variables.

Table B.7: Regression Discontinuity Bandwidths and Controls (IV estimates)

	Bandwidth in years around age 17:					2 yr. bw &		Triangular
	1	1.5	2 (main)	2.5	3	No controls	RD robust	weights
Recidivism (intensive):								
Felony conviction	-0.359	-0.346	-0.361*	-0.251	-0.347**	-0.406**	-0.42	-0.343
	(0.288)	(0.222)	(0.188)	(0.168)	(0.152)	(0.193)	(0.3)	(0.222)
Charge	-0.68	-0.811**	-1.022***	-0.783***	-0.939***	-1.05***	-0.564	-0.777**
	(0.49)	(0.384)	(0.326)	(0.289)	(0.261)	(0.335)	(0.516)	(0.382)
Employment:								
IHS (Annual Wages)	-0.238	-0.208	-0.275**	-0.306***	-0.267***	-0.271*	-0.196	-0.240
	(0.185)	(0.144)	(0.124)	(0.11)	(0.099)	(0.141)	(0.214)	(0.162)
Earnings above poverty line	-0.043**	-0.034**	-0.027**	-0.023**	-0.014	-0.024*	-0.042**	-0.030**
	(0.019)	(0.015)	(0.012)	(0.011)	(0.01)	(0.013)	(0.021)	(0.015)
Incarceration:								
Prison days	76.58*	105.2***	125***	136.7***	135***	108.1***	58.57	93.05***
	(41.77)	(33.08)	(27.92)	(24.74)	(22.32)	(28.55)	(41.31)	(31.86)
Jail days	15.01	25.27***	26.67***	25.39***	22.2***	25.97***	17.27*	22.89***
	(10.38)	(7.994)	(6.8)	(6.068)	(5.468)	(6.841)	(10.08)	(7.826)
Left obs:	1200	1600	2000	2300	2500	2000	2400	2000
Right obs:	1200	2000	2700	3500	4200	2700	3900	2700

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022) and #CBDRB-FY23-088 (approved 12/12/2022). Coefficients are estimated using a linear IV approach with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; the time since last charged category; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offense character of the zip code of the defendant's residence (measured between 2008-2010); an indicator for whether the defendants residence is matched into a Wayne County tract or zip code.

Table B.8: Regression Discontinuity Decomposition Bandwidths

	Bandwidth (years) around 17:			Triangular	OLS based	(DZ, Z) ×
	1.5	2 (main)	2.5	weights	instruments	predictions
<u>Felony conviction recidivism (intensive)</u>						
Juvenile No Conv.	1.968**	1.709**	1.355**	1.879**	1.259	2.637
	(0.84)	(0.773)	(0.612)	(0.871)	(1.117)	(1.757)
Juvenile Conv.	1.888**	1.558**	1.318**	1.991**	2.73**	2.701
	(0.778)	(0.729)	(0.579)	(0.82)	(1.065)	(1.656)
Adult Conv.	2.036**	1.584**	1.495**	2.134**	2.424**	3.044
	(0.85)	(0.788)	(0.62)	(0.881)	(1.175)	(1.871)
Adult Incar.	-1.485**	-2.037***	-1.419***	-1.754***	-1.503*	-2.443**
	(0.599)	(0.572)	(0.463)	(0.624)	(0.818)	(1.242)
<u>IHS (Annual Wages)</u>						
Juvenile No Conv.	-0.701	-0.4476	-0.427	-0.826*	-1.397	-1.946*
	(0.486)	(0.4054)	(0.363)	(0.5)	(0.802)	(1.016)
Juvenile Conv.	-0.333	-0.08815	-0.108	-0.481	-1.215	-1.635*
	(0.444)	(0.3715)	(0.335)	(0.46)	(0.781)	(0.95)
Adult Conv.	-0.629	-0.4169	-0.433	-0.865*	-1.357	-2.335**
	(0.489)	(0.4025)	(0.363)	(0.505)	(0.859)	(1.098)
Adult Incar.	-0.681*	-0.6602**	-0.69**	-0.321	-1.714**	0.593
	(0.381)	(0.3237)	(0.296)	(0.397)	(0.595)	(0.819)
Left obs:	1600	2000	2300	2000	2000	2000
Right obs:	2000	2700	3500	2700	2700	2700

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022), #CBDRB-FY23-088 (approved 12/12/2022), and #CBDRB-FY23-0392 (approved 7/10/23). Estimates are from an RD decomposition (instrument age 17+ interacted with probabilities except where otherwise indicated) with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

Table B.9: Regression Discontinuity Treatment Homogeneity

	Main Results	*Leave-one-instrument-out Results*						Joint test of coeff. equal. (2) - (7)	*"Slim" pred. model*	Joint test of coeff. equal. (1) & (9)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Felony conviction recidivism (intensive)										
Juvenile No Conv.	1.709**	1.803***	1.997***	1.595**	1.69**	1.277*	2.308***	0.951	1.851**	0.629
	(0.773)	(0.696)	(0.749)	(0.699)	(0.69)	(0.733)	(0.753)		(0.864)	
Juvenile Conv.	1.558**	1.771***	1.949***	1.688***	1.798***	1.513**	2.575***	0.935	1.867**	0.674
	(0.729)	(0.645)	(0.645)	(0.644)	(0.639)	(0.662)	(0.749)		(0.798)	
Adult Conv.	1.584**	1.805***	2.23***	1.743**	1.783**	1.535**	2.541***	0.941	1.971**	0.593
	(0.788)	(0.699)	(0.756)	(0.697)	(0.693)	(0.721)	(0.767)		(0.883)	
Adult Incar.	-2.037***	-1.474***	-1.832***	-1.556***	-1.132*	-1.516***	-1.317**	0.977	-1.668**	0.471
	(0.572)	(0.507)	(0.584)	(0.508)	(0.616)	(0.504)	(0.521)		(0.682)	
IHS (Annual Wages)										
Juvenile No Conv.	-0.448	-0.348	-0.155	-0.382	-0.441	-0.478	-0.406	0.997	-0.466	0.709
	(0.405)	(0.408)	(0.451)	(0.411)	(0.405)	(0.435)	(0.442)		(0.586)	
Juvenile Conv.	-0.088	-0.115	0.058	-0.018	-0.087	-0.108	-0.0354	1.000	-0.207	0.546
	(0.372)	(0.372)	(0.384)	(0.377)	(0.371)	(0.385)	(0.445)		(0.532)	
Adult Conv.	-0.417	-0.457	-0.041	-0.351	-0.371	-0.439	-0.370	0.991	-0.363	0.735
	(0.403)	(0.404)	(0.484)	(0.407)	(0.404)	(0.418)	(0.457)		(0.597)	
Adult Incar.	-0.660**	-0.644**	-0.994**	-0.619*	-0.919**	-0.662**	-0.648**	0.968	-1.513***	0.154
	(0.323)	(0.324)	(0.409)	(0.326)	(0.401)	(0.324)	(0.328)		(0.463)	
Instrument dropped:	–	P(Ad. conv.) ×P(Ju. no conv.)	P(Ad. conv.) ×P(Ju. conv.)	P(Ad. incar.) ×P(Ju. no conv.)	P(Ad. incar.) ×P(Ju. conv.)	P(Ad. no conv.) ×P(Ju. no conv.)	P(Ad. no conv.) ×P(Ju. conv.)		–	
Covariates used in prediction models:										
Defendant traits	x	x	x	x	x	x	x			
Household characteristics	x	x	x	x	x	x	x		x	
Neighborhood information	x	x	x	x	x	x	x		x	

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022) and #CBDRB-FY23-088 (approved 12/12/2022). A joint test of all coefficient equality in the leave-one-out robustness yields a p-value of 1. A joint test of the coefficients from the slim random forest model and the full random forest model is .961 for recidivism and .553 for IHS(Annual Wages). Coefficients are estimated using a linear IV approach with robust standard errors. Regressions also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; the time since last charged category; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offense character of the zip code of the defendant's residence (measured between 2008-2010); an indicator for whether the defendants residence is matched into a Wayne County tract or zip code.

B.2 Details of Cost Benefit Calculations

The first row of Table 2.4 details the share of the relevant population impacted by the reform. This represents the share of the adult caseload who would be impacted by the reform and is used as a scaling factor throughout the remaining rows. For instance, 100% of the adult population considered would be impacted by raising the age of majority, while only those incarcerated (19%) would be impacted in order to achieve parity in conviction rates. 18% would be impacted by eliminating prison, and 88% (everyone with a conviction) would be impacted by expanding juvenile record sealing.

The next row shows the average program savings. All values are displayed in 2020 dollars. These are calculated by multiplying the relevant coefficients from our causal estimates with the corresponding costs associated with the interventions (detailed below). The calculation varies by scenario. In order to calculate the first scenario, raising the age of majority, we multiply the reduced form coefficients on the number of days of initial prison/probation/jail/parole times by their daily marginal cost from various sources. The unit cost is high for prison ($107) and jail ($119) and much lower for probation ($3) and parole ($8) (Henrichson and Galgano (2013); Henrichson et al. (2015); Bureau (2019)). We add to this the full cost of juvenile probationary ($6,118) and additional court processing costs $805 Hornby Zeller Associates (2018). For scenario 2, program costs are decreased by the marginal change (the coefficient on adult conviction in the decomposition) in initial jail and probation days. For scenario 3, program costs are changed by the marginal decrease (the coefficient on adult incarceration in the decomposition) in prison and parole time and scaled up by the commensurate marginal change in initial probation and jail costs. We define program costs as zero in scenario 4, where the intervention being considered is simply adjusting the legal rules authorizing who is eligible to apply for an expungement of one's criminal record and when.

The next row shows the amount of tax revenue that the reform generates

(10% of the earnings generated). For scenario 1 this is simply the reduced form estimate on earnings times 10%. For scenario 2, this is the share impacted times negative one times the coefficient on adult conviction. For scenario 3, the share is multiplied by negative one times the coefficient on incarceration. For scenario 4, the share impacted is multiplied by the difference between the coefficient on juvenile conviction and adult conviction.

We then calculate the costs due to changes in recidivism. We estimate the change in the number of offenses for each reform using our decomposed RD estimates to translate the increase or decrease in specific crime type recidivism (we include larceny, trespass, robbery, assault, sexual assault, drug dealing, and drug possession). Additional criminal behavior generates costs via enforcement (via investigation costs). For most charges, the price of investigation is around $1,000-$2,000, but costs are lower for drug possession ($483) and much higher for violent crimes (e.g., $12,211 for assault) (Caulkins (2010); Hunt et al. (2019)). To calculate court costs from each additional offense, we use the estimated change in the number of charges and multiply this by the price per prosecution for that charge type. The price per prosecution is less variable than the price of investigation: the price for most charges is within a few hundred dollars of $1,000, reflecting the modest amount of time spent per case in the U.S. justice system (Schlueter et al. (2014); Hunt et al. (2017)). We calculate the estimated change in time for each correctional facility generated by recidivism using our RD decomposition with days over the next 5 years spent in each type of supervision.

We also include the costs of crime to victims as estimated using Cohen and Piquero (2009). Violent offenses include robbery, sexual assault, and other violent crimes. Property crimes include burglary, larceny, trespass, and other property crimes.Assaults include both aggravated and simple assault and are priced according to the proportion of the offense reduction for each type. Similarly, sexual assaults are composed of both rape as well as misdemeanor sexual assault and are priced according to each in proportion. Because we are unable to directly place a value on other uncategorized offenses, we use the value for simple assaults for other violent crimes and larceny for other property crimes.

Finally we include the value to defendants in two ways. First we assign the average willingness to pay for a day of freedom from Abrams and Rohlfs (2011) ($17 per day). We also include the after tax wages a defendant generated by the policy change.

Table B1: Estimates underlying cost-benefit calculation

	RD estimate	Juv. no conv.	Juv. conviction	Adult conv.	Adult incar.
Larceny	-0.1222	0.596	0.05916	0.4037	-1.641
	(0.164)	(0.5805)	(0.5259)	(0.5887)	(0.4378)
Trespass	-0.1424	0.09244	0.08649	-0.02892	-0.179
	(0.06456)	(0.2344)	(0.2049)	(0.2317)	(0.1826)
Robbery	-0.1356	0.2701	0.3411	0.2428	-0.2794
	(0.06489)	(0.2254)	(0.2131)	(0.2309)	(0.1684)
Assault	-0.2603	0.7159	0.2588	0.3109	-0.891
	(0.1475)	(0.537)	(0.4905)	(0.5399)	(0.405)
Sexual assault	0.0137	-0.0734	-0.1257	-0.07823	-0.1588
	(0.02665)	(0.1032)	(0.0795)	(0.09064)	(0.08097)
Drug dealing	-0.004107	-0.0352	-0.08835	-0.0445	-0.2298
	(0.0601)	(0.1773)	(0.1564)	(0.1699)	(0.1545)
Drug use	-0.2999	0.0975	0.1343	-0.1693	-0.1542
	(0.1313)	(0.4449)	(0.4274)	(0.4618)	(0.3729)
Homicide	-0.0264	-0.03109	-0.02634	-0.04192	-0.1063
	(0.05162)	(0.1591)	(0.1734)	(0.2016)	(0.1156)
Other violent	-0.417	0.6407	0.7552	0.5093	-0.9953
	(0.1683)	(0.6003)	(0.5658)	(0.61)	(0.4432)
Other property	-0.1984	0.1742	-0.02277	-0.1334	-0.2277
	(0.114)	(0.3546)	(0.334)	(0.3785)	(0.3039)
Other	-0.7977	-0.2092	-0.07107	-0.7365	-1.54
	(0.252)	(0.8832)	(0.8141)	(0.8919)	(0.6893)
Prison days	135.6	19.72	-101.3	-37.06	664.3
	(27.74)	(77.74)	(74.76)	(78.85)	(71.14)
Jail days	27.58	7.943	9.524	48.64	-50.71
	(6.799)	(20.35)	(17.75)	(20.37)	(17.94)
Parole days	12.98	8.114	-0.6321	6.445	54.32
	(6.919)	(17.76)	(17.73)	(17.94)	(18.87)
Probation days	441.3	-51.58	-42.97	505.9	-436.7
	(32.8)	(101.5)	(92.13)	(99.32)	(81.89)
Earnings	-3258	-4132	3453	-463	-10450
	(1877)	(5743)	(5125)	(5786)	(4573)

Source: CJARS; IRS W2 and 1040 filings; Best Race and Ethnicity and Numident; ACS; Relational Crosswalk and family exposure measures from Finlay et al. (2022c).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of 4700 observations between the ages of 15 and 19 (2000 juvenile cases and 2700 adult cases). All results were approved for release by the U.S. Census Bureau, Data Management System number: P-7512453 and approval number #CBDRB-FY22-291 (approved 6/24/2022). Estimates use a linear IV (instrument age 17+) with robust standard errors. Regression also control for the following variables: a linear age trend on either side of the discontinuity; whether the defendant was black; sex; time since first misdemeanor charge; time since last charged; whether the defendant worked in 2010; the number and crime type of previous crimes; the crime categories faced in the current charge; how many parents and other adults the defendant shares a mafid with; the number and type of offenses of the zip code of the defendant's residence (measured between 2008-2010); whether the defendant's residence is matched into a Wayne County tract or zip code.

B.3 Simulation exercise to test methodology

This appendix presents a number of simulation exercises to assess the performance of our methodological approach under a range of scenarios. We employ the following data generating process from Section 2.6:

$$Y_i = \sum_{m=1}^{10} \beta_m x_i^m + \sum_{k=1}^{K} \delta_{j,k}\, d_i^{j,k} + \sum_{l=1}^{L} \delta_{a,l}\, d_i^{a,l} + \epsilon_i$$

where $K = 2$ and $L = 2$ for simplicity. Each covariate x_i^m is drawn independently as $N(0, 1)$. Treatment assignment $\{d_i^{j,1}, d_i^{j,2}, d_i^{a,1}, d_i^{a,2}\}$ are defined as follows:

$$d_i^{j,1} = 1\,[\tau_i < 0] \times 1 \left[\sum_{m=1}^{10} \beta_m^{j,1} x_i^m + \nu_i^{j,1} > 0 \right]$$

$$d_i^{j,2} = 1\,[\tau_i < 0] \times 1 \left[\sum_{m=1}^{10} \beta_m^{j,2} x_i^m + \nu_i^{j,2} \geq 0 \right]$$

$$d_i^{a,1} = 1\,[\tau_i \geq 0] \times 1 \left[\sum_{m=1}^{10} \beta_m^{a,1} x_i^m + \nu_i^{a,1} > 0 \right]$$

$$d_i^{a,2} = 1\,[\tau_i \geq 0] \times 1 \left[\sum_{m=1}^{10} \beta_m^{a,2} x_i^m + \nu_i^{a,2} \geq 0 \right]$$

The parameterization of these questions (described below) should leave roughly 25% of the sample in each of the four possible treatment assignments. The goal of the empirical exercise is to estimate the δ coefficients from the outcome equation, or treatment effects.

We consider six scenarios that cover a range of potential empirical settings. In case 1, all β's and δ's are set equal to zero; essentially outcomes and treatment assignments are generated at random. It is a baseline exercise to ensure that our methodology does not over-reject the null hypothesis due to quirks of sample overfitting. In case 2, we maintain that δ's are set equal to zero (no true treatment effect), but allow the β's to be non-zero. This again helps verify that model overfitting does not arbitrarily lead to rejections of the null hypothesis.

For the remaining cases (3 through 6), we assign defined treatment effects as follows: $\delta_{j,1} = 1$, $\delta_{j,2} = 2$, $\delta_{a,1} = 0$, and $\delta_{a,2} = -1$. In case 3, we set $\beta^{j,1}$, $\beta^{j,2}$, $\beta^{a,1}$, and $\beta^{a,2}$ all equal to zero, which will make the first stage purely a function of the forcing variable τ_i and the random shock ν_i. In effect, this should break the relevance assumption of our methodology since there are no subgroup characteristics that can help identify treatment counterfactuals. This scenario tests the performance of our model when one of our key assumptions does not hold, and should be expected to produce biased estimates.

In case 4, all of the β coefficients are non-zero, and we permit the empirical estimation to utilize all 10 covariates. Case 5 replicates case 4, except that the tenth covariate is withheld from the econometrician thereby creating omitted variables bias in naive ordinary least squares (OLS). Our methodology should return unbiased estimates of the true treatment effects. In the final scenario, we introduce treatment effect heterogeneity, allowing $\delta_{j,1}$ to vary with one of the ten covariates. This setup violates our homogeneity assumption, and should generate an unsigned bias in our methodological approach.

To conduct the simulations, we draw a sample of 10,000 observations according to the data generating process outlined above. Each iteration resamples the covariate values, the forcing variable, and the random shocks (ϵ and ν's). For any non-zero coefficient that is not explicitly assigned a value (e.g. β's), we draw values at random from $N(0, 1)$ with each iteration.

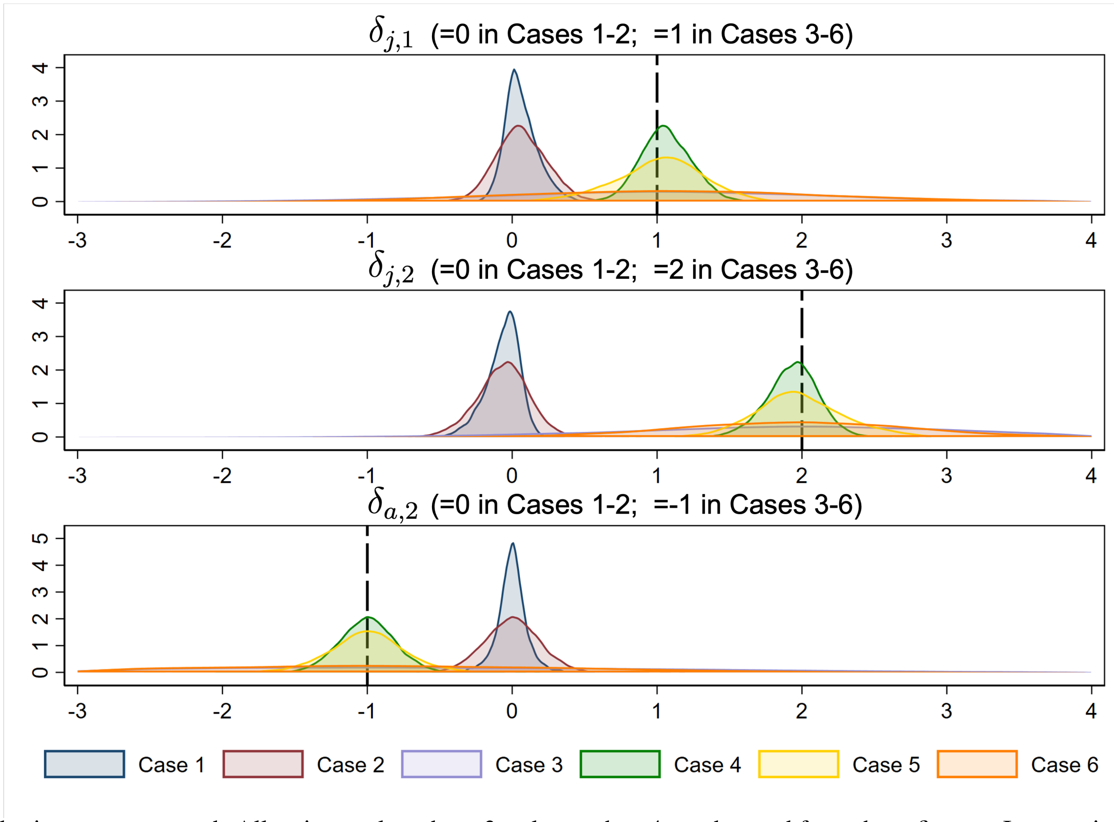
For speed and ease of simulation, we use the characteristics to predict treatment assignment in a simple OLS regression, although in practice a neural net or other predictive approach may be generally preferred. Using the sample to the left of the discontinuity, we estimate the probability of receiving each treatment available to observations on the left of the discontinuity. Similarly we estimate the likelihood of receiving treatments for observations to the right of the discontinuity. Using these models, each trained on 50% of the observations, we use the models to generate predictions for every case (both to the left and to the right of the discontinuity) in our full sample. We then interact these predictions to generate our set of four instruments (prediction of left treatment 1 times prediction of right treatment 1 ; prediction of left treatment 1 times prediction of right treatment 2 ; prediction of left treatment 2 times prediction of right treatment 1 ; prediction of left treatment 2 times prediction of right treatment 2). To estimate our treatment effects we regress the outcome on the running variable, our covariate predictions, and the treatments instrumented by the interaction of our predictions and the discontinuity indicator. Intervention $d_i^{a,1}$ is our excluded category, and since $\delta_{a,1} = 0$ (for cases 3 through 6) the resulting treatment effect estimates will comparable to the parameters in the defined data generating process without further normalization.

Figure C1 shows the results of these exercises. Cases 1 and 2 yield estimates for $\delta_{j,1}$, $\delta_{j,2}$, and $\delta_{a,2}$ that are tightly centered around zero, which is as expected. Our integration of machine learning into RD methods does not arbitrarily create non-zero treatment effect estimates that reject the null hypothesis when no such effect exists.

Cases 3 through 6 are each centered around the true treatment effects. Case 3 (weak IV) and case 6 (heterogeneous treatment effects) both show quite a wide dispersion around the true treatment effect, demonstrating two ways in which this method can break down and yield unreliable estimates when at least one of our fundamental assumptions are violated.

Cases 4 and 5 reassuring behave quite well. Whether the outcome and treatment assignment equations are or are not impacted by omitted variables bias in the empirical estimation, the estimation strategy reliably returns point estimates close to the true effect.

Figure C1: Simulation exercises to assess methodology for potential bias



Distributions are truncated. All estimates less than -3 or larger than 4 are dropped from these figures. In no estimate are more than 2.3% of estimates dropped by this truncation.

B.4 Mueller-Smith, Pyle, and Walker (2022) in the Caetano et al. (2023) framework

This section describes the econometric technique explicitly in the Caetano et al. (2023) framework. Formally, we are applying the method from this paper with a uniform kernel and bandwidth $h = 2$, and we are using instruments derived from our random forest approach.

In the Caetano et al. (2023) notation we describe our treatment vector of four potential treatments and one omitted category: $T_i = (T_{1i}, T_{2i}, T_{3i}, T_{4i})'$, where

$$
\begin{aligned}
T_{1i} =& 1(j, \not{c}) = 1(\text{tried in juvenile court, not convicted}) \\
T_{2i} =& 1(j, c) = 1(\text{tried in juvenile court, convicted}) \\
T_{3i} =& 1(a, c, \not{p}) = 1(\text{tried in adult court, convicted, no prison}) \\
T_{4i} =& 1(a, c, p) = 1(\text{tried in adult court, convicted, prison}) \\
T_{0i} =& 1(a, \not{c}) = 1(\text{tried in adult court, not convicted}). \quad \leftarrow \text{excluded category}
\end{aligned}
$$

We call Y_i the outcome (various measures of recidivism and employment); Z_i is the centered running variable (age) with $z_0 = 17$ as the RD cutoff, and we use controls W_i (our interacted probabilities of each potential treatment,

$$
\begin{aligned}
W_i = (&P_i(j, \not{c}) \times P_i(a, \not{c}), P_i(j, \not{c}) \times P_i(a, c, \not{p}), P_i(j, \not{c}) \times P_i(a, c, p), \\
&P_i(j, c) \times P_i(a, \not{c}), P_i(j, c) \times P_i(a, c, \not{p}), P_i(j, c) \times P_i(a, c, p))'),
\end{aligned}
$$

and X_i (the other covariates entering linearly).

We estimate δ by 2SLS:

$$
\begin{aligned}
Y_i =& \delta' T_i + \phi' W_i + \beta' X_i + \epsilon_i \\
T_i =& \rho' W_i D_i + \psi' W_i + \theta' X_i + \nu_i
\end{aligned}
$$

In this framework we recover the treatment effects when the following assumptions are met. First, continuous selection at z_0: $\mathbb{E}[\beta_i X_i + \epsilon_i | W_i, Z_i = z]$ and $\mathbb{E}[\phi_i | W_i, Z_i = z]$ are continuous in z at z_0. $\delta_i, T_i(z) \perp\!\!\!\perp Z_i | W_i, Z_i \in (z_0 - \varepsilon, z_0 + \varepsilon)$ for some small $\varepsilon > 0$. Second, monotonicity: $T_{li}(z)$ is monotonic in z near z_0. We also need relevance, which in this setting means $E[\Delta_T(W_i)\Delta_T(W_i)']$ is invertible ($\delta = E[\Delta_T(W_i)\Delta_T(W_i)']^{-1} E[\Delta_T(W_i)\Delta_Y(W_i)]$). Finally, homogeneity in W_i of the expected treatment effects conditional on W_i and compliers for that treatment level: $\delta_l(W_i) := E[\delta_{li} | W_i, \lim_{e \downarrow 0}(T_{li}(z_0 + e) - T_{li}(z_0 - e)) \neq 0] = \delta_l$.

Caetano et al. (2023) shows how this identification strategy works under homogeneity:

$$\lim_{r\downarrow 0}(E[Y_i|W_i, Z_i = z_0 + r] - E[Y_i|W_i, Z_i = z_0 - r]) =$$
$$\delta' \lim_{r\downarrow 0}(E[T_i|W_i, Z_i = z_0 + r] - E[T_i|W_i, Z_i = z_0 - r]) +$$
$$\lim_{r\downarrow 0}(E[\beta' X_i + \epsilon_i|W_i, Z_i = z_0 + r] - E[\beta' X_i + \epsilon_i|W_i, Z_i = z_0 - r])$$

Applying the classical RDD assumption of continuity in z at z_0 of $E[\beta' X_i + \epsilon_i|W_i, Z_i = z]$ yields $\Delta_Y(W_i) = \delta'\Delta_T(W_i)$ where $\Delta_Y(W_i) := \lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z]$ and $\Delta_T(W_i) := \lim_{z\downarrow z_0} E[T_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[T_i|W_i, Z_i = z]$. The relevance assumption in this setting requires at least 4 linearly independent values of W_i, so that $E[\Delta_T(W_i)\Delta_T(W_i)']$ is invertible.

Caetano et al. (2023) also provides insight into what is recovered when the homogeneity assumption is not met.

$$\begin{aligned}\lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] &= \lim_{z\downarrow z_0} E[\delta_i' T_i|W_i, Z_i = z]\\ &+ \lim_{z\downarrow z_0} E[\phi_i|W_i, Z_i = z]' W_i\\ &+ \lim_{z\downarrow z_0} E[\beta_i' X_i + \epsilon_i|W_i, Z_i = z]\end{aligned}$$

$$\begin{aligned}\lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z] &= \lim_{z\uparrow z_0} E[\delta_i' T_i|W_i, Z_i = z]\\ &+ \lim_{z\uparrow z_0} E[\phi_i|W_i, Z_i = z]' W_i\\ &+ \lim_{z\uparrow z_0} E[\beta_i' X_i + \epsilon_i|W_i, Z_i = z]\end{aligned}$$

Assuming $\mathbb{E}[\phi_i|W_i, Z_i = z]$ is continuous in z at z_0 (as in the classical RD setting) yields

$$
\begin{aligned}
\lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z] = \\
&\lim_{z\downarrow z_0} E[\delta_i' T_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[\delta_i' T_i|W_i, Z_i = z] \\
&+ \lim_{z\downarrow z_0} E[\beta_i' X_i + \epsilon_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[\beta_i' X_i + \epsilon_i|W_i, Z_i = z]
\end{aligned}
$$

Assuming the classic RD assumption of continuity in z at z_0 of $E[\beta' X_i + \epsilon_i|W_i, Z_i = z]$ removes the last term:

$$
\begin{aligned}
\lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z] = \\
&\lim_{z\downarrow z_0} E[\delta_i' T_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[\delta_i' T_i|W_i, Z_i = z]
\end{aligned}
$$

The additional classical RDD assumptions $\delta_i, T_i(z) \perp\!\!\!\perp Z_i$ near z_0 and $T_{li}(z)$ monotonic on z near z_0. This yields:

$$
\begin{aligned}
\lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z] = \\
&\delta(W_i) \lim_{z\downarrow z_0} E[\delta_i' T_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[\delta_i' T_i|W_i, Z_i = z]
\end{aligned}
$$

where $\delta_l(W_i) := \lim_{r\downarrow 0} E[\delta_i|W_i, T_{li}(z_0 + r) - T_{li}(z_0 - r) \neq 0]$.Thus we have

$$
\begin{aligned}
\Delta_Y(W_i) :=& \lim_{z\downarrow z_0} E[Y_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[Y_i|W_i, Z_i = z] \\
=& \delta(W_i)\delta' \Delta_T(W_i) \\
\Delta_T(W_i) :=& \lim_{z\downarrow z_0} E[T_i|W_i, Z_i = z] - \lim_{z\uparrow z_0} E[T_i|W_i, Z_i = z]
\end{aligned}
$$

When the homogeneity assumption *is* satisfied, $\delta(W_i) = \delta$.

$$\delta_l(W_i) = E[\delta_{li}|W_i, \lim_{e\downarrow 0}(T_{li}(z_0 + e) - T_{li}(z_0 - e)) \neq 0]$$
$$= \begin{cases} E[\delta_{1i}|W_i, Z_i = z_0, 1(j, \cancel{c}) = 1], \text{ for } l = 1 \\ E[\delta_{2i}|W_i, Z_i = z_0, 1(j, c) = 1], \text{ for } l = 2 \\ E[\delta_{3i}|W_i, Z_i = z_0, 1(a, c, \cancel{p}) = 1], \text{ for } l = 3 \\ E[\delta_{4i}|W_i, Z_i = z_0, 1(a, c, p) = 1], \text{ for } l = 4 \end{cases}$$

Thus when the homogeneity assumption is violated, we recover

$$\bar{\delta}_1 = E[\omega_1(W_i)\delta_1(W_i)] + \sum_{l=2}^{4} E[\omega_l(W_i)\delta_l(W_i)],$$

where $E[\omega_1(W_i)] = 1$ and $E[\omega_l(W_i)] = 0$, for $l = 2, 3, 4$ (and analogously for $\delta_{l=2,3,4}$). That is, we identify the treatment of interest which is contaminated by an average of the other LATEs with weights that average to zero.

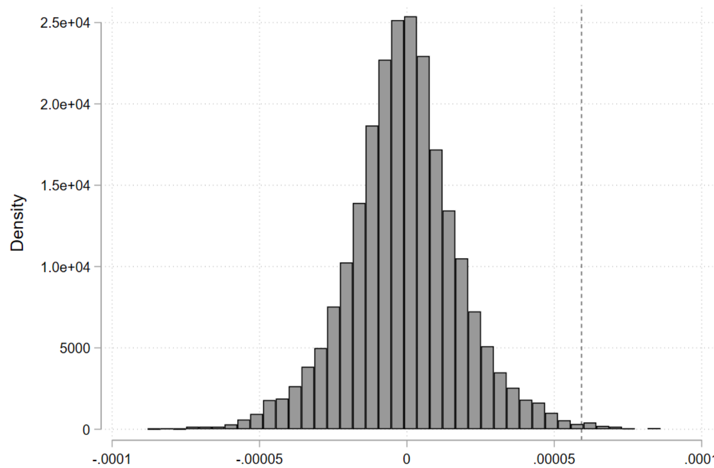
APPENDIX C

Appendix to Chapter 3

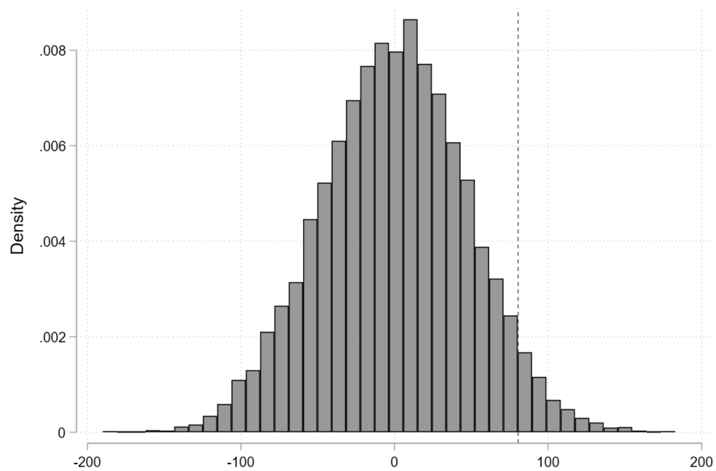
C.0.1 Randomization inference

One way of viewing the TCJA is as a study design with only one event, which can create standard errors that are too small even when clustered at the state level. One way to correct for this potential issue is to conduct placebo tests in the spirit of Conley and Taber (2011). In each of the placebo tests below, we randomly choose a treatment year between 2007 and 2017. We then randomly assign 57% of the sample "treatment" status after that date (57% of observations in our base sample are treated). As in the primary specification, we study 3 years before and 2 years after the placebo treatment. We rerun the placebo estimates 10,000 times in order to create a distribution of coefficients under a null where the treatment effect is by construction 0, and thus tells us the likelihood of observing under coefficients as large as under the null. Histograms of these distributions are displayed below, and p-values are listed in the table in the main text.

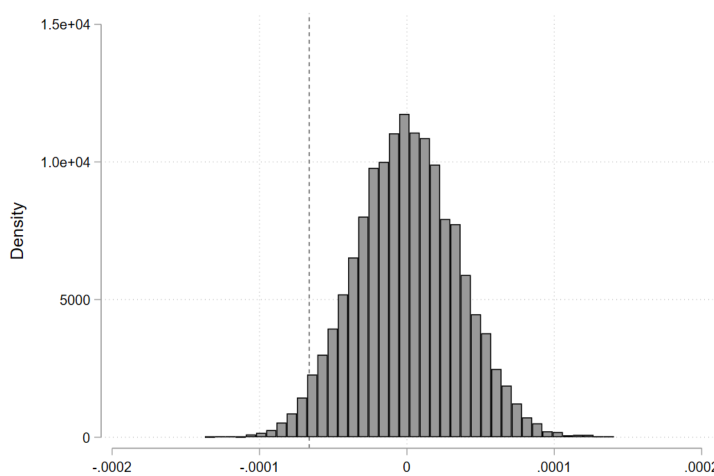
Figure C1: (Conley and Taber, 2011) inference



A: Deposit price

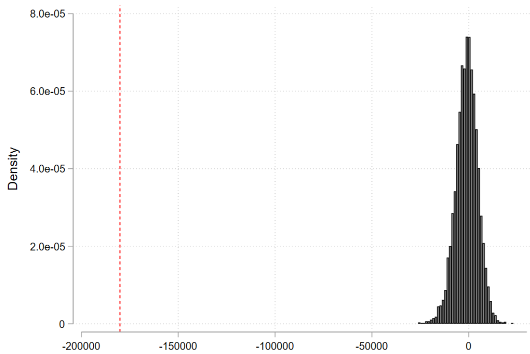


B: Labor price

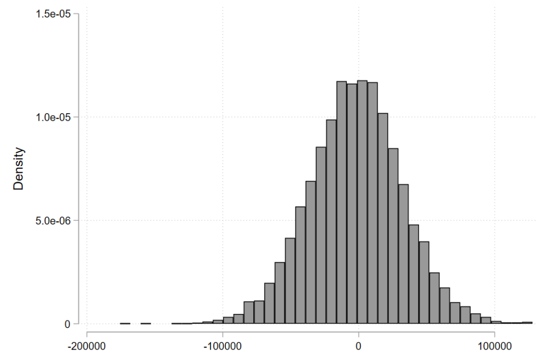


C: Loan price

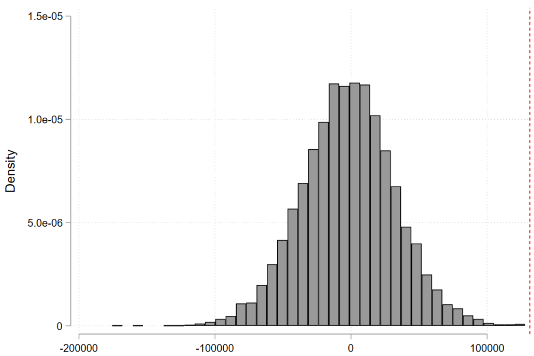
Figure C2: Quarterly flows randomization inference:



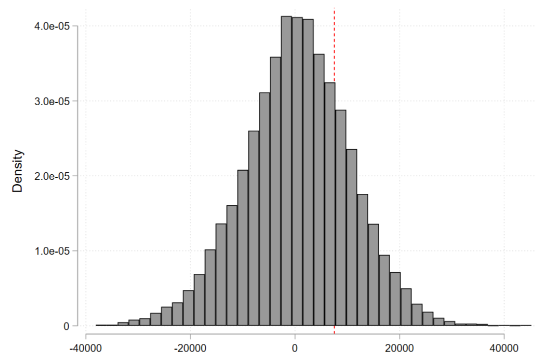
A: Taxes



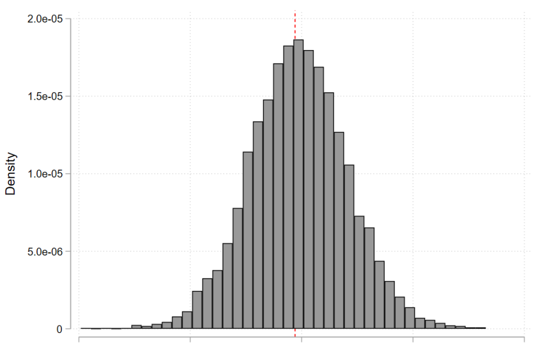
B: Capital



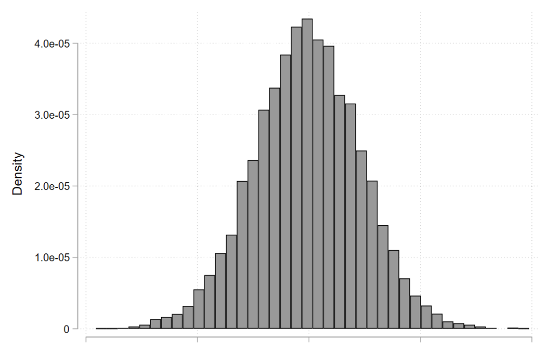
C: Deposits



D: Labor



E: Loans



F: Premises

C.0.2 Event study robustness to specification choices

There are several choices that we made in the empirical work. We explored several different specifications and control groups. While there are some differences across these choices, the results are qualitatively robust to whatever choices we make. In panels A and B we change the event study specification but keep the comparison between credit unions and traditional private banks. In panel A, we show the same sample as our core specification, but do not include any trend controls; we also show a version without trends, but with weights. The weighting scheme we use is entropy balancing. (Hainmueller, 2012) This approach reweights the samples (from the unrestricted universe of credit unions and traditional for-profite banks) in order to upweight the most comparable credit unions to private banks. In particular, we reweight on age, the log of assets, labor expense, investments, deposits, and share of CD deposits in the pre-treatment period (and apply these weights to the full sample). Panel B shows the core regression from the text of the paper (trends), as well as a version with both trend controls and weighting. Summary statistics for the unweighted, unrestricted sample as well as the reweighted sample are displayed below.

Panels C-E alter the relevant comparison group. In panel D the figure shows the coeffecient on private banks, with the counterfactual group is S-Corps. Here the TCJA lowered the tax burden on both are traditional private banks as well as the S-Corps, but (likely) to a lesser extent for S-Corps. Due to the relatively unclear first-stage, it is unsuprising that this comparison shows

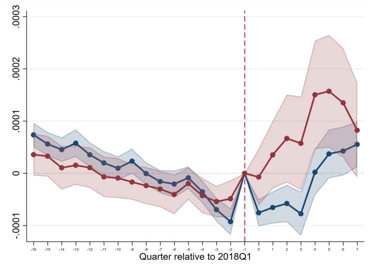
Table C1: Unweighted, unrestricted summary statistics

	Banks		Credit Unions	
	mean	sd	mean	sd
Age	90	40	64	14
Taxes	302432	843171	0	0
ln(Assets)	19.1	1.1	17.6	1.7
ln(Investments)	17.8	1.1	16.1	1.8
ln(Deposits)	18.9	1.1	17.4	1.7
ln(Premises)	14.7	1.6	12.9	3.0
Deposit Share Check and Save	0.30	0.16	0.18	0.67
Deposit Share CDs	0.33	0.15	0.15	0.12
Deposit Share Money Market	0.16	0.13	0.11	0.13
Price of Deposits	0.0013	0.0008	0.0011	0.0010
Price of Labor	16421	4995	13287	4463
Price of Loans	0.0129	0.0024	0.0138	0.0040
Firm x Quarter Obs.	59158		103128	

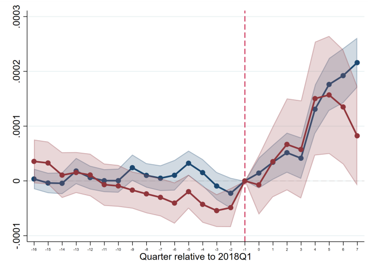
Table C2: Weighted summary statistics

	Banks		Credit Unions	
	mean	sd	mean	sd
Age	49	36	64	14
Taxes	399625	1538584	0	0
ln(Assets)	18.5	1.5	17.6	1.7
ln(Investments)	17.3	1.4	16.1	1.8
ln(Deposits)	18.3	1.5	17.4	1.7
ln(Premises)	14.0	2.0	12.9	3.0
Deposit Share Check and Save	0.40	0.18	0.67	0.20
Deposit Share CDs	0.20	0.12	0.15	0.12
Deposit Share Money Market	0.21	0.18	0.11	0.13
Price of Deposits	0.0009	0.0007	0.0011	0.0010
Price of Labor	16777	65667	13287	4463
Price of Loans	0.0137	0.0037	0.0138	0.0040

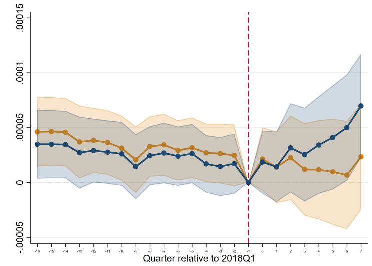
Figure C3: Deposit price



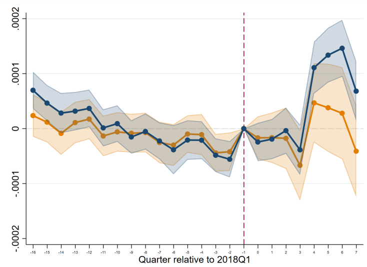
A: No weighting, no trends (blue) and weighting (red)



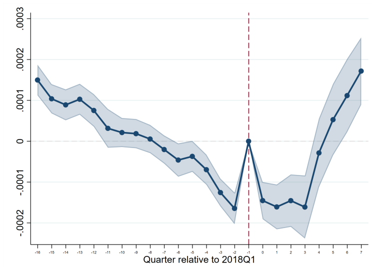
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

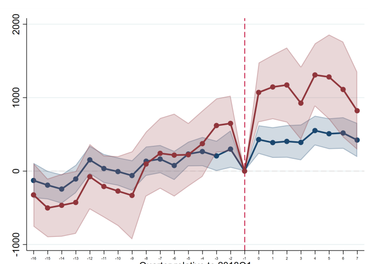


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

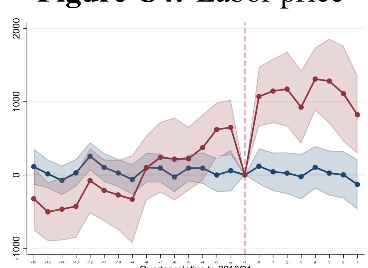


E: CU v non-stock comparison, trends (blue)

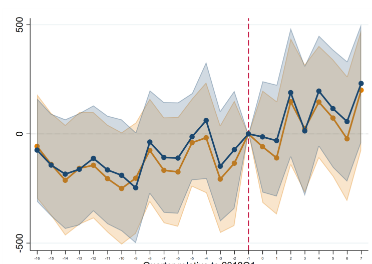
Figure C4: Labor price



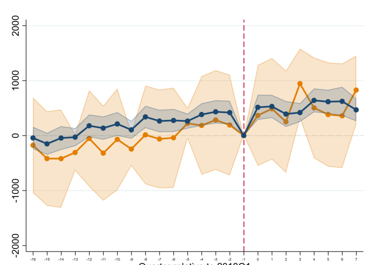
A: No weighting, no trends (blue) and weighting (red)



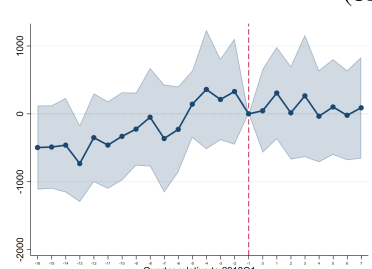
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

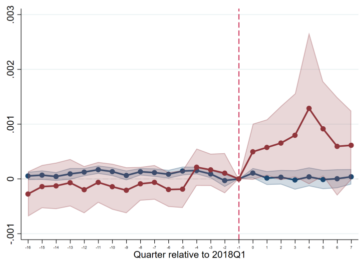


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

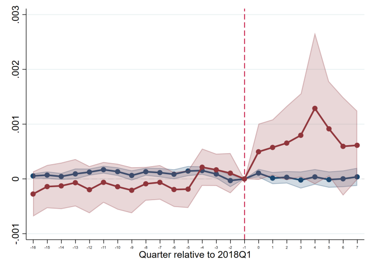


E: CU v non-stock comparison, trends (blue)

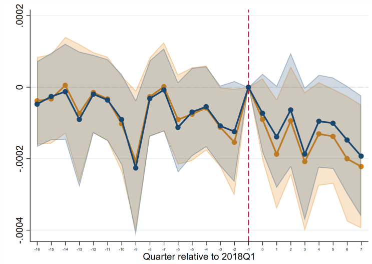
Figure C5: Loan price



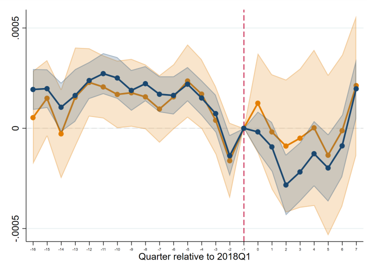
A: No weighting, no trends (blue) and weighting (red)



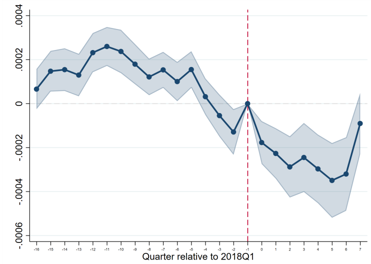
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

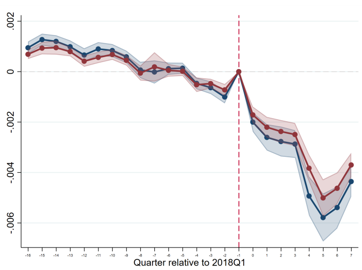


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

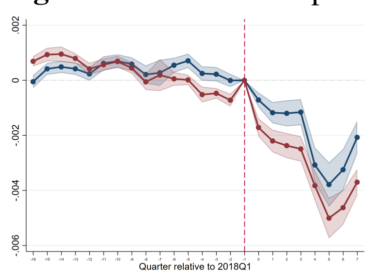


E: CU v non-stock comparison, trends (blue)

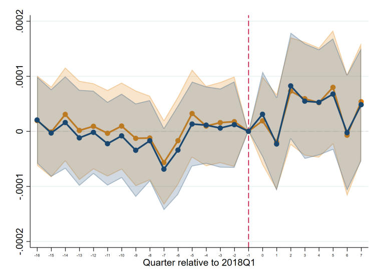
Figure C6: Investment price



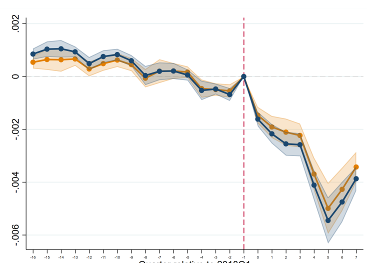
A: No weighting, no trends (blue) and weighting (red)



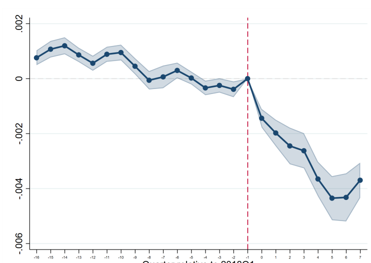
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

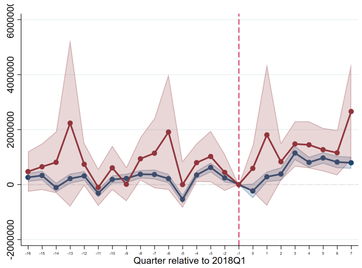


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

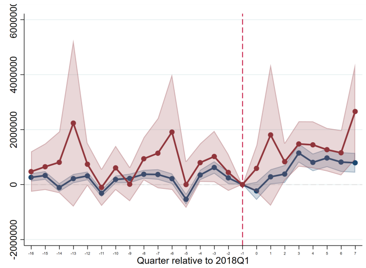


E: CU v non-stock comparison, trends (blue)

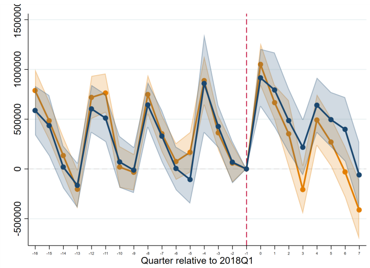
Figure C7: Capital payments



A: No weighting, no trends (blue) and weighting (red)

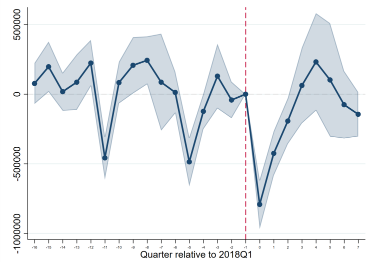


B: trends (blue) and trends + weights (red)



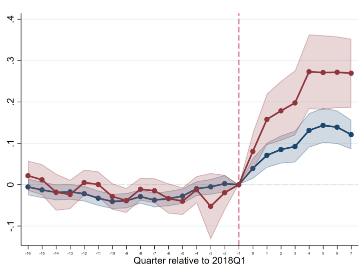
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

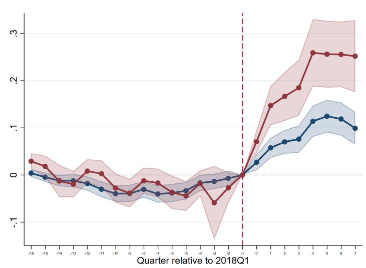


E: CU v non-stock comparison, trends (blue)

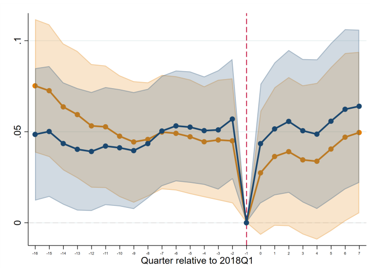
Figure C8: Log deposit expense



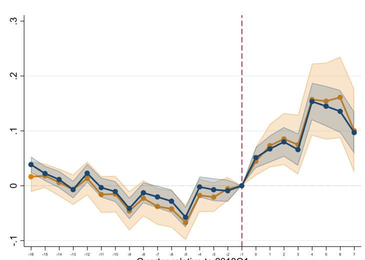
A: No weighting, no trends (blue) and weighting (red)



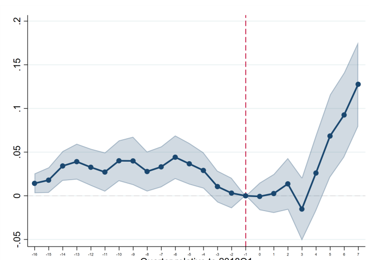
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

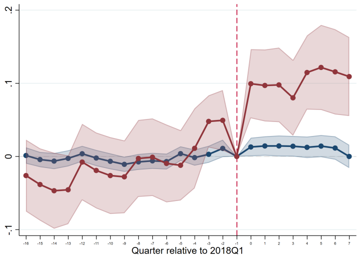


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

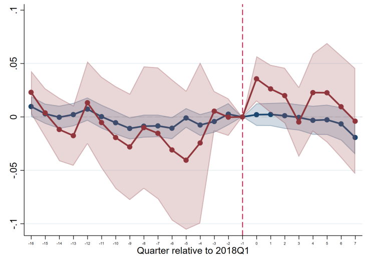


E: CU v non-stock comparison, trends (blue)

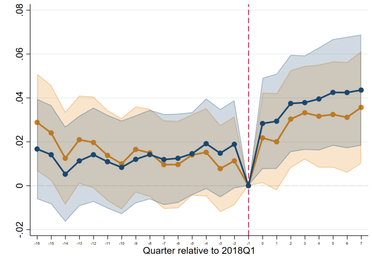
Figure C9: Log of labor expense



A: No weighting, no trends (blue) and weighting (red)

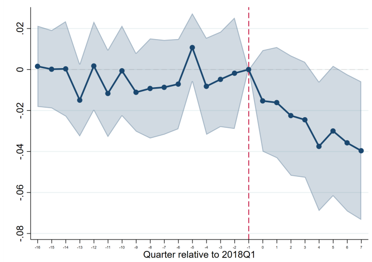


B: trends (blue) and trends + weights (red)



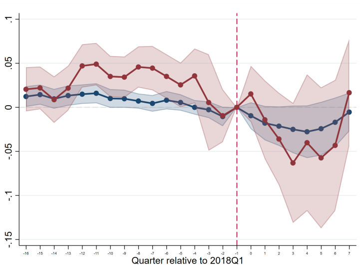
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

D: CU v Scorp comparison, trends (blue) and trends + weights (orange)



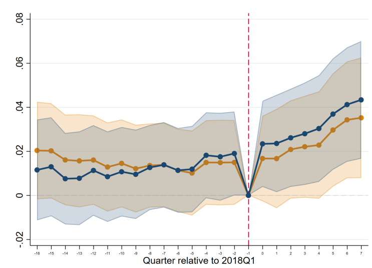
E: CU v non-stock comparison, trends (blue)

Figure C10: Log of loan expense

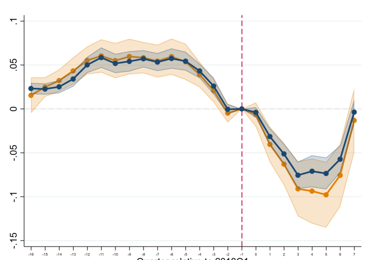


A: No weighting, no trends (blue) and weighting (red)

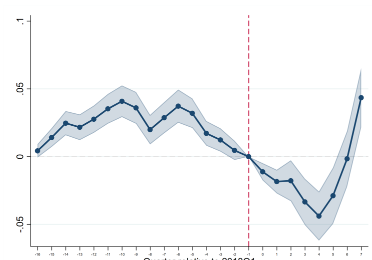
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

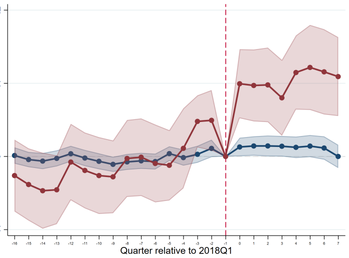


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

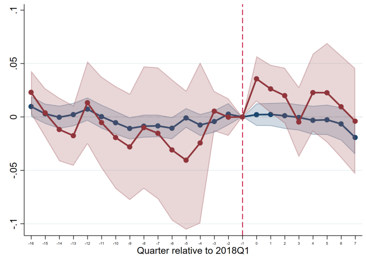


E: CU v non-stock comparison, trends (blue)

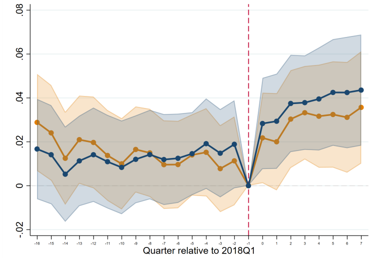
Figure C11: Log of investment expense



A: No weighting, no trends (blue) and weighting (red)

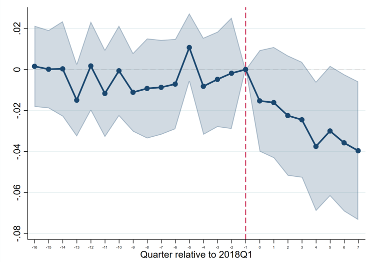


B: trends (blue) and trends + weights (red)



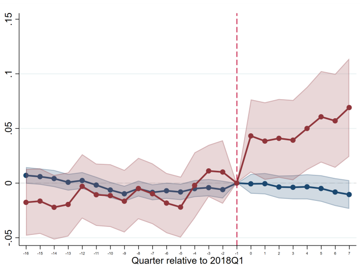
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

D: CU v Scorp comparison, trends (blue) and trends + weights (orange)



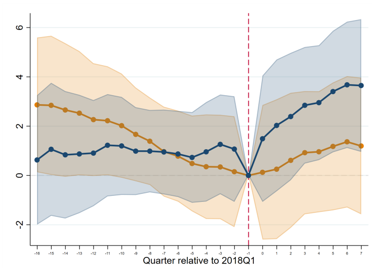
E: CU v non-stock comparison, trends (blue)

Figure C12: Log of labor quantity

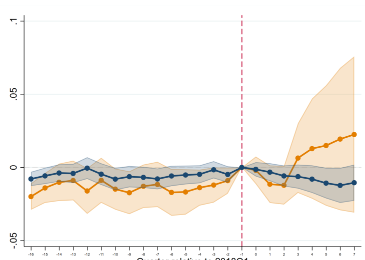


A: No weighting, no trends (blue) and weighting (red)

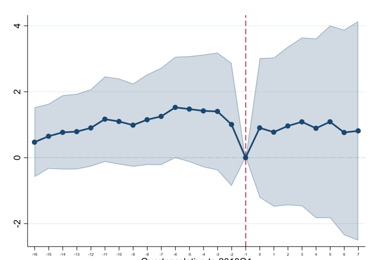
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

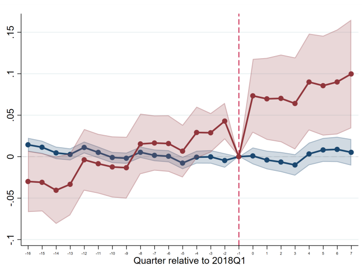


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

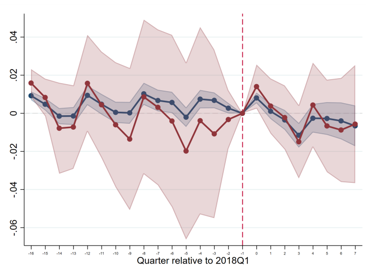


E: CU v non-stock comparison, trends (blue)

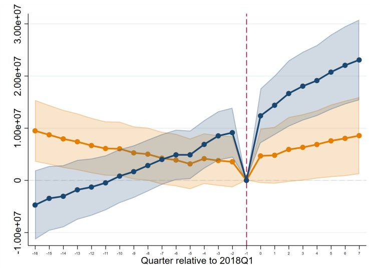
Figure C13: Log of loan quantity



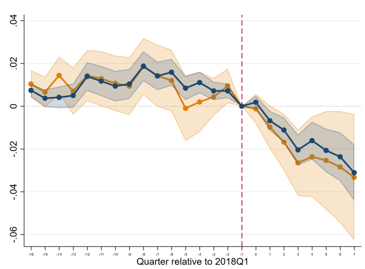
A: No weighting, no trends (blue) and weighting (red)



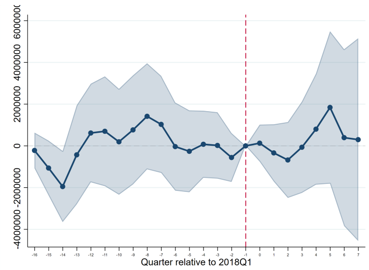
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

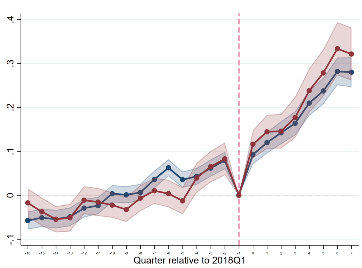


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

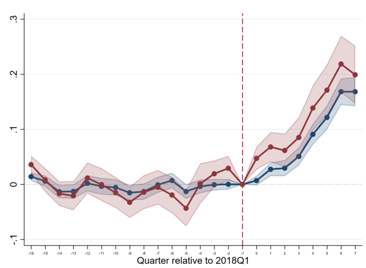


E: CU v non-stock comparison, trends (blue)

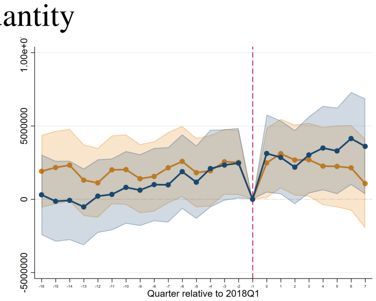
Figure C14: Log of investment quantity



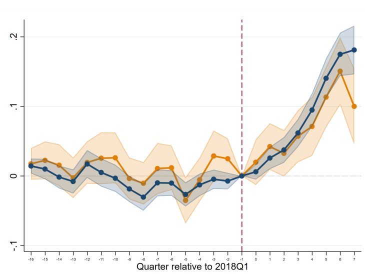
A: No weighting, no trends (blue) and weighting (red)



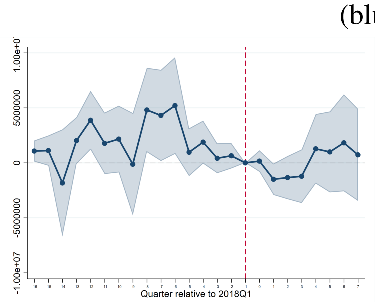
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

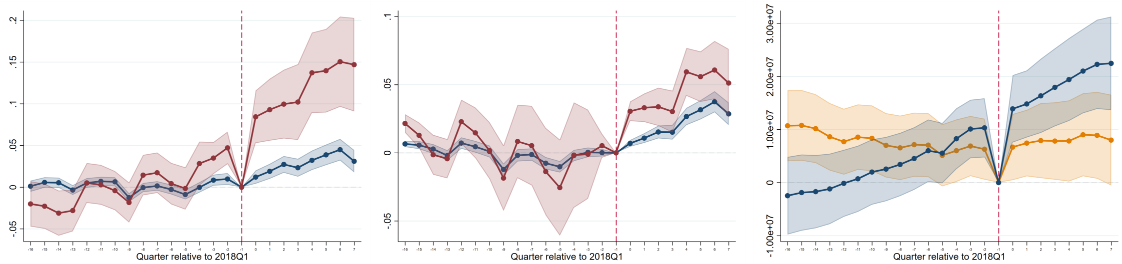


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)



E: CU v non-stock comparison, trends (blue)

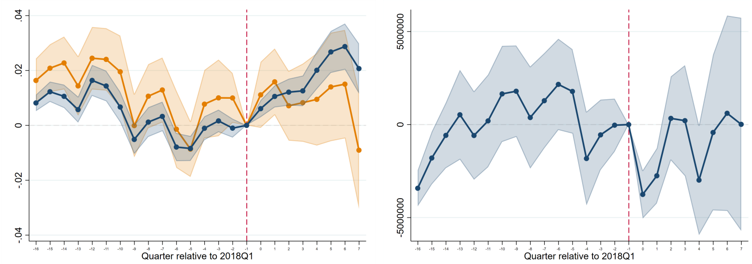
Figure C15: Log of deposits quantity



A: No weighting, no trends (blue) and weighting (red)

B: trends (blue) and trends + weights (red)

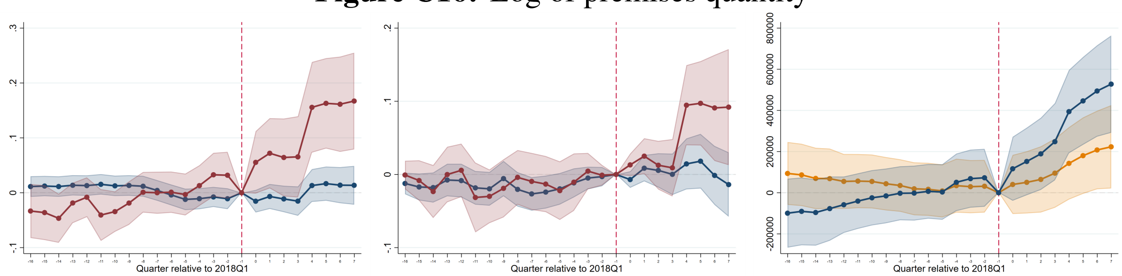
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)



D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

E: CU v non-stock comparison, trends (blue)

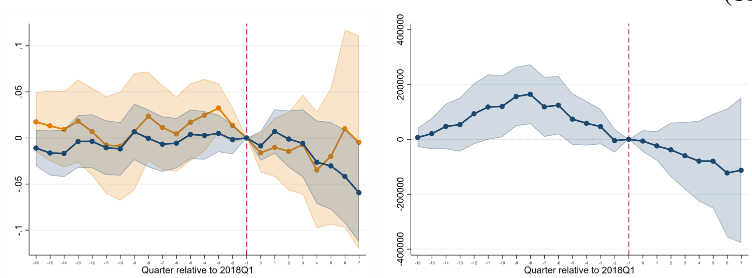
Figure C16: Log of premises quantity



A: No weighting, no trends (blue) and weighting (red)

B: trends (blue) and trends + weights (red)

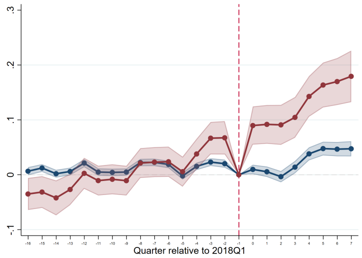
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)



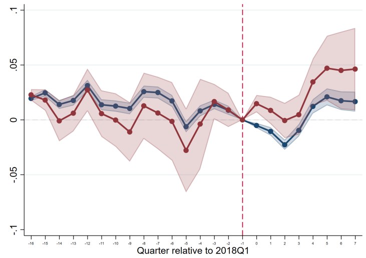
D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

E: CU v non-stock comparison, trends (blue)

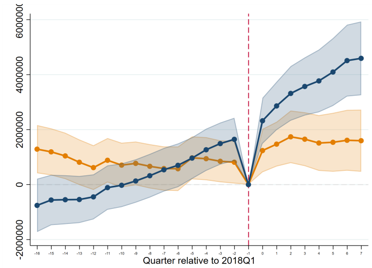
Figure C17: Log of equity quantity



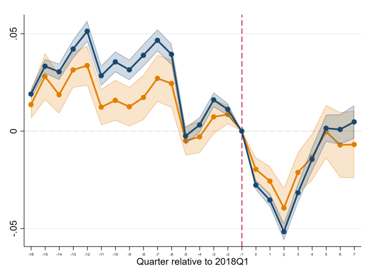
A: No weighting, no trends (blue) and weighting (red)



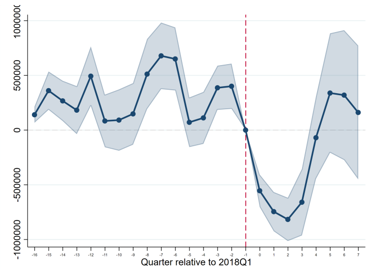
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

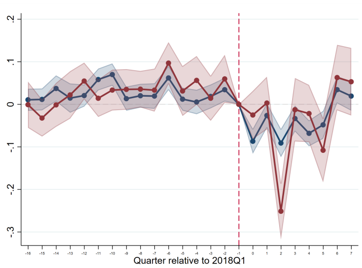


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

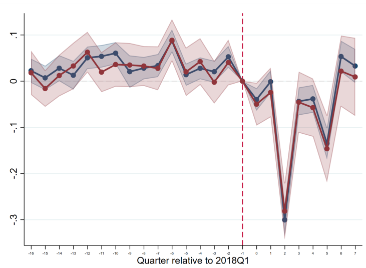


E: CU v non-stock comparison, trends (blue)

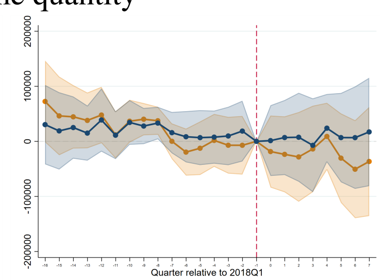
Figure C18: Log of non-interest income quantity



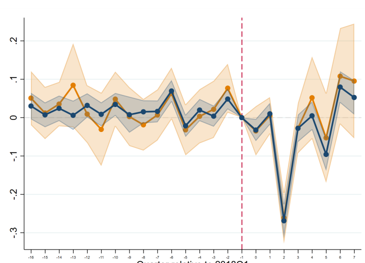
A: No weighting, no trends (blue) and weighting (red)



B: trends (blue) and trends + weights (red)



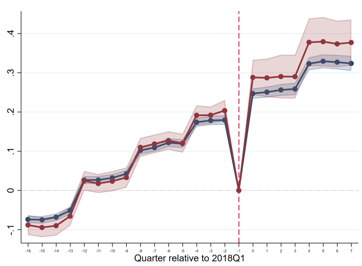
C: Bank v Scorp comparison, weighting (orange) and nothing (blue)



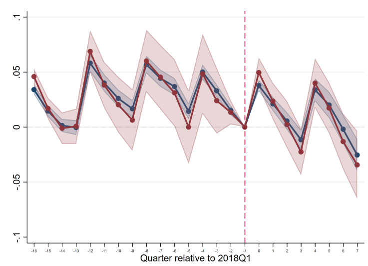
D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

E: CU v non-stock comparison, trends (blue)

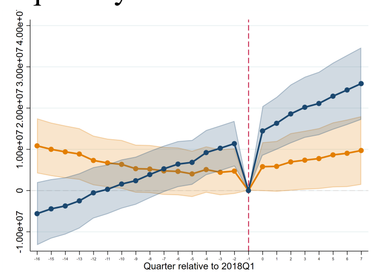
Figure C19: Log of risk-weighted assets quantity



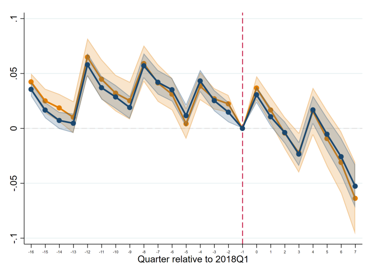
A: No weighting, no trends (blue) and weighting (red)



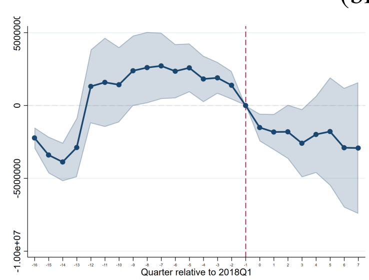
B: trends (blue) and trends + weights (red)



C: Bank v Scorp comparison, weighting (orange) and nothing (blue)

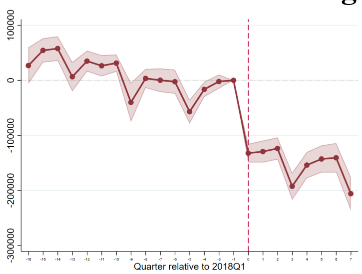


D: CU v Scorp comparison, trends (blue) and trends + weights (orange)

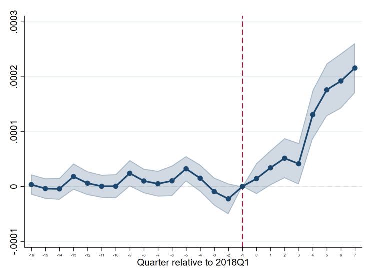


E: CU v non-stock comparison, trends (blue)

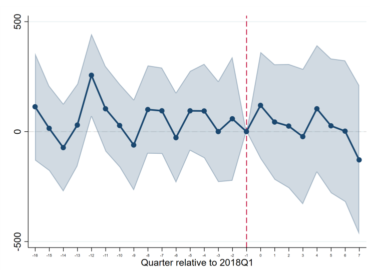
Figure C20: Additional Event Studies



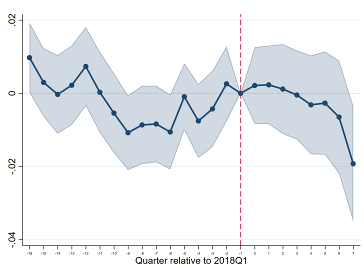
A: Taxes



B: Deposit Rates

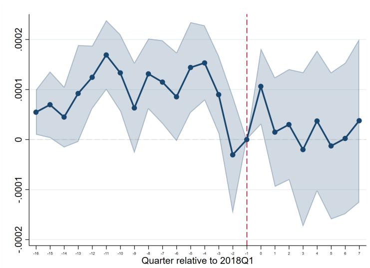


C: Wages

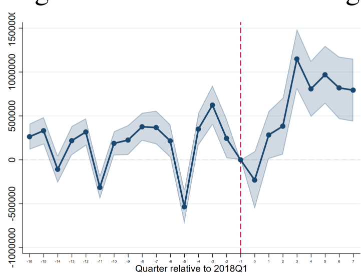


D: Log wages

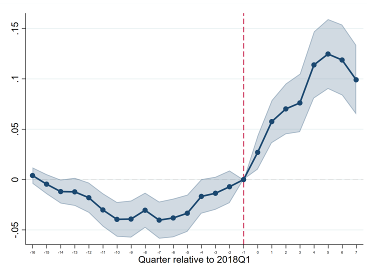
E: Log premises



F: Loan prices



G: Payments to capital



H: Log deposits expense

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