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EXPUNGEMENT OF CRIMINAL CONVICTIONS: AN EMPIRICAL STUDY

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ABSTRACT

Laws permitting the expungement of criminal convictions are a key component of modern criminal justice reform efforts and have been the subject of a recent upsurge of legislative activity. This debate has been almost entirely devoid of evidence about the laws' effects, in part because the necessary data (such as sealed records themselves) have been unavailable. We were able to obtain access to deidentified data that overcomes that problem, and we use it to carry out a comprehensive statewide study of expungement recipients and comparable non-recipients. We offer three key sets of empirical findings. First, among those legally eligible for expungement, just 6.5% obtain it within five years of eligibility. Drawing on patterns in our data as well as interviews with expungement lawyers, we point to reasons for this serious "uptake gap." Second, those who do obtain expungement have extremely low subsequent crime rates, comparing favorably to the general population—a finding that defuses a common public-safety objection to expungement laws. Third, those who obtain expungement experience a sharp upturn in their wage and employment trajectories; on average, within two years, wages go up by 25% versus the pre-expungement trajectory, an effect mostly driven by unemployed people finding jobs and very minimally employed people finding steadier or higher-paying work.

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INTRODUCTION

Today, approximately 19 million Americans have felony conviction records,[1] and an unknown—but presumably much larger—number have misdemeanor conviction records.[2] In recent years, policymakers, civil rights advocates, and scholars have paid increasing attention to the substantial barriers to employment,[3] housing,[4] and social integration[5] that these records can pose, not to mention the hundreds of collateral legal consequences that typically flow from criminal convictions, such as restrictions on public benefits eligibility and occupational licensing.[6] Taken together, these hurdles have been described as amounting to a "new civil death,"[7] and on a collective scale, this phenomenon magnifies racial disparities in employment and other outcomes as well, due to disparities in the distribution of criminal records. As a result, a core part of this century's emergent criminal-justice reform movement has been a search for effective policy levers to mitigate the reentry barriers faced by people with criminal records. This effort is picking up steam, with two-thirds of U.S. states adopting one or more such policies in 2018 alone.[8]

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[1] Sarah K.S. Shannon et al., *The Growth, Scope, and Spatial Distribution of People with Felony Records in the United States, 1948–2010*, 54 DEMOGRAPHY 1795, 1806 (2017). When arrests are added, seventy-five million Americans—a third of adults—have criminal records. FBI, NOVEMBER 2018 NEXT GENERATION IDENTIFICATION (NGI) SYSTEM FACT SHEET 1 (2018), https://perma.cc/5KQJ-UBL5.

[2] No studies currently document the total number of Americans with misdemeanor convictions. However, statistics collected between 2008 and 2016 indicate that misdemeanors routinely make up over 70% of a state's criminal caseload. *See* Megan Stevenson & Sandra Mayson, *Contributions: The Scale of Misdemeanor Justice*, 98 B.U. L. REV. 731, 746 n.81 (2018).

[3] *E.g.*, NAN ASTONE, MICHAEL KATZ & JUILA GELATT, URBAN INST., INNOVATIONS IN NYC HEALTH & HUMAN SERVICES POLICY: YOUNG MEN'S INITIATIVE 2 (2014), https://www.urban.org/sites/default/files/publication/32651/413057-Innovations-in-NYC-Health-and-Human-Services-Policy-Young-s-Men-s-Initiative.PDF; *Fact Sheet: President Obama Announces New Actions to Promotion Rehabilitation and Reintegration for the Formerly-Incarcerated*, THE OBAMA WHITE HOUSE ARCHIVES (Nov. 2, 2015), https://obamawhitehouse.archives.gov/the-press-office/2015/11/02/fact-sheet-president-obama-announces-new-actions-promote-rehabilitation.

[4] *E.g.*, ANNE MORRISON PIEHL, THE HAMILTON PROJECT, PUTTING TIME LIMITS ON THE PUNITIVENESS OF THE CRIMINAL JUSTICE SYSTEM 9 (2016), https://www.hamiltonproject.org/assets/files/reducing_punitiveness_piehl_policymemo.pdf; MARIE CLAIRE TRAN-LEUNG, SARGENT SHRIVER NAT'L CTR. ON POVERTY LAW, WHEN DISCRETION MEETS DENIAL: A NATIONAL PERSPECTIVE ON CRIMINAL RECORDS BARRIERS TO FEDERALLY SUBSIDIZED HOUSING 1 (2015), https://povertylaw.org/files/docs/WDMD-final.pdf.

[5] *E.g.*, Letter from Eric Holder, U.S. Attorney Gen., to State Attorneys Gen. (April 18, 2011), https://csgjusticecenter.org/wp-content/uploads/2014/02/Reentry_Council_AG_Letter.pdf; Amy L. Solomon, *In Search of a Job: Criminal Records as Barriers to Employment*, NIJ J., June 2012, at 42, 44, https://www.ncjrs.gov/pdffiles1/nij/238488.pdf.

[6] Gabriel J. Chin, *The New Civil Death: Rethinking Punishment in the Era of Mass Conviction*, 160 U. PA. L. REV. 1789, 1807–1814 (2012); Alexandra Natapoff, *Misdemeanor Decriminalization*, 68 VAND. L. REV. 1055, 1089–1094 (2015); *National Inventory of Collateral Consequences of Conviction*, CSG JUSTICE CENTER, https://niccc.csgjusticecenter.org (last visited Mar. 9, 2019) [hereinafter *CSG Inventory*].

[7] Chin, *supra* note 6, at 1789; *see also* JAMES B. JACOBS, THE ETERNAL CRIMINAL RECORD 4 (2015) (referring to this phenomenon as "the eternal criminal record").

[8] A major new report by the Collateral Consequences Resource Center documents an "extraordinary number of laws passed [by thirty-three states and territories] in 2018 aimed at reducing barriers to

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Perhaps the policy levers with the greatest theoretical potential are laws that allow conviction records to be wholly expunged or, at least, sealed from public view. (We will refer to such laws collectively as "expungement laws," although this shorthand elides some differences.[9]) Expungement offers the possibility of sweeping aside a wide range of legal and socioeconomic consequences at once; these laws typically authorize individuals to apply for jobs, housing, schools, and benefits as though their convictions did not exist.

Today, a substantial majority of U.S. states provide some form of expungement procedure for otherwise-valid adult convictions.[10] Many states have recently adopted, or are presently considering, new expungement laws or expansions to existing ones.[11] In 2018, Pennsylvania became the first state to adopt a sweeping program of *automatic* expungement of adult criminal convictions—specifically, minor, nonviolent misdemeanors, after 10 crime-free years. Other states may soon follow suit. In California, a pending automatic-expungement bill would be even more ambitious, encompassing minor felonies as well as misdemeanors and eliminating the waiting period.[12] More typically, expungement laws require individuals to go through a judicial process to apply for relief, usually giving judges the discretion to deny the petition. Many states have stringent eligibility requirements as to crime type or severity or the number of convictions on the individual's record, and many have waiting periods.[13]

Despite the considerable legislative ferment and the excitement that surrounds "clean slate" initiatives in the civil rights and criminal justice reform worlds, what has been missing from the debate is hard evidence about the effects and true potential of conviction expungement laws. Empirical studies in this area have been difficult to carry

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successful reintegration for individuals with a criminal record," calling it the "most productive legislative year since a wave of 'fair chance' reforms began in 2013." CCRC Staff, *Press Release: New Report on 2018 Fair Chance and Expungement Reforms (Updated)*, COLLATERAL CONSEQUENCES RES. CTR. (Jan. 10, 2019), https://ccresourcecenter.org/2019/01/10/press-release-new-report-on-2018-fair-chance-and-expungement-reforms/#more-18004; *see* MARGARET LOVE & DAVID SCHLUSSEL, COLLATERAL CONSEQUENCES RES. CTR., REDUCING BARRIERS TO REINTEGRATION: FAIR CHANCE AND EXPUNGEMENT REFORMS IN 2018, at 1 (2019), https://ccresourcecenter.org/wp-content/uploads/2019/01/Fair-chance-and-expungement-reforms-in-2018-CCRC-Jan-2019.pdf.

[9] The details vary by jurisdiction, and many advocates use the terms interchangeably. Typically, a true "expungement" legally eliminates it from the state's perspective. "Sealing" maintains the record for some limited state purposes (e.g., law enforcement investigations of future crimes) but insulates it from public view.

[10] Two useful websites contain frequently updated collections of these laws; at our last check, we found slightly different information at the two sites, but both include at least thirty-six states with some form of expungement law for valid, nonpardoned, and nonvacated adult criminal convictions. *See 50-State Comparison: Judicial Expungement, Sealing, and Set-Aside*, COLLATERAL CONSEQUENCES RES. CTR. (Feb. 2019), https://ccresourcecenter.org/state-restoration-profiles/50-state-comparisonjudicial-expungement-sealing-and-set-aside [hereinafter *CCRC State Survey*] (providing details of these policies); *Clean Slate Clearinghouse*, CSG JUSTICE CTR., https://cleanslateclearinghouse.org/compare-states/ (same).

[11] For example, in 2018 alone, twenty states passed twenty-eight bills extending eligibility for expungement or sealing to new classes of offense or offender. LOVE & SCHLUSSEL, *supra* note 8, at 5.

[12] Timothy Williams, *Rap Sheets Haunt Former Inmates. California May Change That*. N.Y. Times (Mar. 11, 2019), https://www.nytimes.com/2019/03/11/us/california-criminal-records-bill.html (describing California's newly introduced automatic expungement bill).

[13] *See supra* note 10; *see also infra* Part I.B (describing this legal landscape in more detail).

out. Expunged criminal records are, obviously, not typically available to study—and other relevant outcome data, such as wage information or employment status, are also protected by privacy laws. While there are many persuasive theoretical reasons to believe that expungement laws will have large and important effects across many domains, the dearth of empirical evidence and understanding is a significant hindrance to reform and experimentation, and leaves policymakers almost entirely in the dark.

In this Article, we present the results of an unprecedented statewide study that overcomes existing limitations on research on expungement and seeks to fill various key policy-relevant gaps in our empirical knowledge. Pursuant to a data-sharing agreement with the State of Michigan, we were able to obtain complete, deidentified criminal records from the Michigan State Police on all individuals who had obtained criminal record "set-asides" (Michigan's term for record-sealing) as of March 2014, as well as full criminal history records for much larger comparison groups of individuals with convictions that were not set aside. Further, the state matched these criminal histories with detailed wage and employment data for the same individuals from the state's unemployment insurance program. Michigan is an ideal setting in which to study expungement laws: it is a large, diverse state with criminal justice challenges typical of the U.S. today. Moreover, its expungement law is broadly representative of expungement laws in the country, but it has existed for longer than most, allowing us to study results over time. Features of its eligibility requirements also facilitate our research design, as discussed below.

We use our unique data to investigate a number of interrelated empirical questions, which can be grouped into three main areas of inquiry. First, we examine the critical question of "uptake rate": the rate at which those who are legally eligible for set-asides actually receive them. We find that Michigan's set-aside uptake rate is discouragingly low; our best estimate is that only 6.5% of eligible individuals receive them within five years of the date at which they first qualify. Although our data do not identify unsuccessful applicants, it is clear from follow-up inquiries with the Michigan State Police that the low uptake rate can be primarily attributed to individuals' failure to apply, rather than to denials of applications by judges. To better understand the uptake process, we examine the characteristics of set-aside recipients and their offenses, and assess whether some characteristics are predictive of uptake. We then use these data, plus interviews with Michigan set-aside lawyers and advocates for people with criminal records, to inform a discussion of why people might not apply for set-asides despite their potential benefits.

Second, we investigate set-aside recipients' subsequent criminal offending. We find very low rates of recidivism: just 6% of all set-aside recipients are rearrested within five years of receiving their set-aside (and only 2% are rearrested for violent offenses), while reconviction rates are even lower. Indeed, set-aside recipients' recidivism rates compare favorably with the Michigan population as a whole. We do not claim that these low rates are necessarily *because* of set-asides, although there are several channels by which set-aside receipt could potentially contribute to lower recidivism risk. Another likely explanation is that people who have limited criminal records to begin with and have gone at least five years since their last conviction are simply very low-risk. This finding is consistent with a broader empirical literature on patterns of desistance from crime. It also

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defuses the most common policy argument *against* expungement laws: that the public (including employers and landlords) has a safety interest in knowing about the prior records of those with whom they interact.

Third, we examine the employment consequences of criminal record expungement. We find that after accounting for an individual's prior employment and wage history, as well as broader changes in the state's economy, set-aside recipients experience considerable gains shortly after receipt. Within one year, on average, an individual's odds of being employed (earning any wages at all) increase by a factor of 1.13; her odds of earning at least $100/week (a slightly more demanding employment measure) increase by a factor of 1.23; and her reported quarterly wages increase a factor of 1.23 (increasing to 1.25 by the next year). These results suggest that those with expunged records gain access to more and better-paying jobs. To be sure, one has to be cautious about drawing causal inferences here; it is very possible that some of the gains come about because people choose to seek expungement at a time that they are especially motivated to seek improvements in their economic situation. Nonetheless, as we discuss in Part IV, there are good reasons to believe that at least a large fraction of the improvement that we observe results from the cleaner record itself.

We provide background on expungement laws and on our research setting in Part I, and then we present the three major components of our analysis—the take-up rate, recidivism rates, and socioeconomic outcomes—in Parts II, III, and IV, respectively. In the Conclusion, we address some limitations and future research possibilities, and discuss policy implications. Our findings tell a good news/bad news story: when expungement is not automatic (and takes time, effort, and even money), only a very small share of the people eligible for relief actually apply for and receive an expungement—but those who do see clear improvements in economic outcomes and pose little public safety risk. Taken together, these findings have a clear policy upshot: they support the expansion of expungement availability, an easing of the procedural hurdles associated with seeking expungement, and in particular the emerging movement to make expungement occur automatically, rather than depending on a demanding application process.

I.   EXPUNGEMENT POLICIES AND RELATED RESEARCH

Several large bodies of scholarly research, as well as active policy debates and commentary, inform and motivate our study. We introduce the key issues and scholarship here. In Section A, we describe the many hurdles people with public criminal records face; paring back these hurdles is the core policy motivation for expungement laws. In Section B, we add some further detail to the Introduction's description of the current legal and policy landscape surrounding expungement. In Section C, we discuss the very limited empirical research that exists on expungement and identify the key unanswered empirical questions that we seek to address. In Section D, we turn to our specific empirical setting, describing Michigan's expungement law and the data that we use to investigate its effects.

A. *The Economic and Legal Aftermath of a Criminal Conviction*

The consequences of criminal convictions do not end when offenders complete their formal sentences. For many individuals, punishments such as probation, fines, and even

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incarceration may be swamped in importance by what comes next: exclusion from employment, obstacles to social integration, and a vast array of collateral legal consequences that often last a lifetime.[14] A growing body of academic research documents the scope, ubiquity, and size of these hurdles.[15]

First, people with criminal records face serious employment barriers—indeed, these barriers may exceed those facing any other disadvantaged group.[16] While many aspects of offenders' backgrounds, as well as the interruptions to work and education experienced by those who are incarcerated, may put them at greater risk of unemployment than the general population,[17] the criminal record itself also seems to directly harm employment prospects.[18] Many employers report that they take steps to avoid hiring former offenders.[19] Their motivations for doing so vary. The Department of Labor has reported that many employers are driven by "bias and stigma."[20] Some believe that former offenders cannot be trusted.[21] Others fear a negligent-hiring lawsuit if an employee hired with a criminal record commits a crime while on the job.[22]

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[14] Sarah B. Berson, *Beyond the Sentence – Understanding Collateral Consequences*, NIJ J., Sept. 2013, at 25, https://www.ncjrs.gov/pdffiles1/nij/241927.pdf; *CSG Inventory*, *supra* note 6.

[15] *See, e.g.*, Anastasia Christman & Michelle Natividad Rodriguez, *Research Supports Fair Chance Policies*, NAT'L EMP. L. PROJECT (Aug. 2016), https://www.nelp.org/publication/research-supports-fair-chance-policies; THE PEW CHARITABLE TRS., COLLATERAL COSTS: INCARCERATION'S EFFECT ON ECONOMIC MOBILITY (2010), https://www.pewtrusts.org/~/media/%20legacy/uploadedfiles/pcs_assets /2010/collateralcosts1pdf.pdf.

[16] Harry J. Holzer et al., *Perceived Criminality, Background Checks, and the Racial Hiring Practices of Employers*, 49 J.L. & ECON. 451 (2006); Devah Pager, *The Mark of a Criminal Record*, 108 AM. J. SOC. 937 (2003).

[17] DEBBIE MUKAMAL, U.S. DEP'T OF LABOR, FROM HARD TIME TO FULL TIME: STRATEGIES TO HELP MOVE EX-OFFENDERS FROM WELFARE TO WORK (2001), https://hirenetwork.org/sites/default/files/ From%20Hard%20Time%20to%20Full%20Time.pdf; AMY L. SOLOMON ET AL., URBAN INST., FROM PRISON TO WORK: THE EMPLOYMENT DIMENSIONS OF PRISONER REENTRY 8–11 (2004) https://www.urban.org/sites/default/files/publication/58126/411097-From-Prison-to-Work.PDF; JEREMY TRAVIS ET AL., URBAN INST., FROM PRISON TO HOME: THE DIMENSIONS AND CONSEQUENCES OF PRISONER REENTRY (2001), http://research.urban.org/UploadedPDF/from_prison_to_home.pdf.

[18] CHERRIE BUCKNOR & ALAN BARBER, CTR. FOR ECON. & POLICY RESEARCH, THE PRICE WE PAY: ECONOMIC COSTS OF BARRIERS TO EMPLOYMENT FOR FORMER PRISONERS AND PEOPLE CONVICTED OF FELONIES (2016), http://cepr.net/images/stories/reports/employment-prisoners-felonies-2016-06.pdf?v=5; SCOTT H. DECKER ET AL., CRIMINAL STIGMA, RACE, GENDER, AND EMPLOYMENT: AN EXPANDED ASSESSMENT OF THE CONSEQUENCES OF IMPRISONMENT FOR EMPLOYMENT 56 (2014), https://www. ncjrs.gov/pdffiles1/nij/grants/244756.pdf.

[19] MICHELLE NATIVIDAD RODRIGUEZ & MAURICE EMSELLEM, THE NAT'L EMP'T LAW PROJECT, 65 MILLION "NEED NOT APPLY": THE CASE FOR REFORMING CRIMINAL BACKGROUND CHECKS FOR EMPLOYMENT 1 (2011), https://www.nelp.org/wp-content/uploads/2015/03/65_Million_Need_Not_ Apply.pdf; Holtzer et al., *supra* note 16, at 9–11; Sarah Esther Lageson et al., *Legal Ambiguity in Managerial Assessments of Criminal Records*, 40 L. & SOC. INQUIRY 175, 191 (2015).

[20] MUKAMAL, *supra* note 17.

[21] Harry J. Holzer, *Collateral Costs: Effects of Incarceration on Employment and Earnings Among Young Workers*, *in* DO PRISONS MAKE US SAFER? THE BENEFITS AND COSTS OF THE PRISON BOOM 239, 248 (Steven Raphael & Michael Stoll eds., 2009).

[22] MUKAMAL, *supra* note 17; SOC'Y FOR HUMAN RES. MGMT. & CHARLES KOCH INST., WORKERS WITH CRIMINAL RECORDS 2 (2018), https://www.shrm.org/hr-today/trends-and-forecasting/research-and-

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Experimental results confirm employers' reluctance to hire former offenders. Pager had matched pairs of testers, differing only in criminal history, apply for a range of employment positions.[23] She found that applicants without records received more than twice as many job offers. More recently, Agan and Starr sent around 15,000 fictitious job applications paired by race to entry-level jobs mainly in the restaurant and retail industry.[24] They found that when employers asked about criminal history, those without records received 63% more callbacks, even though the records in question were relatively minor. Many national employers, influenced by the national "Ban the Box" movement and resulting law changes, have removed questions about criminal history from initial job applications—but these employers typically still conduct background checks before finalizing a hire.[25] In 2012, 87% of randomly sampled employers performed criminal background checks on at least some employees; 69% performed background checks on *all* employees.[26] The recent expansion in this practice may be attributed to easier and less costly Internet-based searches.[27] Almost all states place court records on the Internet,[28] and private companies, such as Westlaw and LexisNexis, also market criminal history databases.[29]

In addition to these employment consequences, criminal convictions bring with them a wide range of other "collateral" legal consequences—that is, consequences that are not part of the sentence, but are a function of a wide array of civil laws. Licensing restrictions categorically exclude previously convicted individuals from hundreds of professions.[30] These individuals are often prohibited from receiving various social services, including welfare and health benefits, public housing, and food stamps.[31] Whole families can be

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surveys/Documents/SHRM-CKI%20Workers%20with%20Criminal%20Records%20Issue%20Brief%202018-05-17.pdf; Holzer, *supra* note 21, at 243.

[23] Pager, *supra* note 16.

[24] Amanda B. Agan & Sonja Starr, *Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment*, 133 Q.J. ECON. 191 (2018); Amanda Agan & Sonja Starr, *The Effect of Criminal Records on Access to Employment*, AM. ECON. REV.: PAPERS AND PROC., May 2017, at 560..

[25] Agan & Starr (2018), *supra* note 24.

[26] SOC'Y FOR HUMAN RES. MGMT., SHRM SURVEY FINDINGS: BACKGROUND CHECKING—THE USE OF CRIMINAL BACKGROUND CHECKS IN HIRING DECISIONS (2012), https://www.shrm.org/hr-today/trends-and-forecasting/research-and-surveys/Documents/2012BackgroundCheck_Criminal_FINAL.pptx.

[27] JACOBS, *supra* note 7, at 6; Jeffrey Selbin et al., *Unmarked? Criminal Record Clearing and Employment Outcomes*, 108 J. CRIM. L. & CRIMINOLOGY 1, 17–20 (2018).

[28] *Privacy/Public Access to Court Records: State Links*, NAT. CTR. FOR ST. CTS., https://www.ncsc.org/topics/access-and-fairness/privacy-public-access-to-court-records/state-links (last visited Mar. 9, 2019).

[29] Ben Geiger, *The Case for Treating Ex-Offenders as a Suspect Class*, 94 CALIF. L. REV. 1191, 1198–99 (2006).

[30] CHIDI UMEZ & REBECCA PIRIUS, NAT'L CONFERENCE OF STATE LEGISLATURES, BARRIERS TO WORK: IMPROVING EMPLOYMENT IN LICENSED OCCUPATIONS FOR INDIVIDUALS WITH CRIMINAL RECORDS (2018), http://www.ncsl.org/Portals/1/Documents/Labor/Licensing/criminalRecords_v06_web.pdf.

[31] Jeremy Travis, *Invisible Punishment: An Instrument of Social Exclusion*, *in* THE COLLATERAL CONSEQUENCES OF MASS IMPRISONMENT 18–25 (Marc Mauer & Meda Chesney-Lind eds., 2002); AM. BAR ASSOC., *Collateral Standards and the Discretionary Disqualification of Convicted Persons*, *in* ABA STANDARDS FOR CRIMINAL JUSTICE 7–9 (3d ed. 2004); Geiger, *supra* note 29, at 1204–1206; LEGAL ACTION CTR., AFTER PRISON: ROADBLOCKS TO REENTRY (2004), https://www.lac.org/roadblocks-to-reentry/upload/lacreport/LAC_PrintReport.pdf.

evicted from public housing on the basis of one member's convictions.[32] Exclusion from housing renders ex-offenders homeless at high rates—for instance, in 1997, the California Department of Corrections estimated that 10% of its parolees were homeless.[33] The specifics of these exclusions vary from state to state, although some are encouraged or required by federal law.[34] They also vary based on crime type.[35]

Because so many Americans have conviction records, these consequences have a large aggregate impact. This includes spillover effects on family members never convicted of any crime; the Center for American Progress estimates that almost half of U.S. children have a parent with some form of criminal record (including arrests).[36] In addition, because criminal records are not equally distributed across the population,[37] the effects of these collateral consequences are disproportionately concentrated in certain subpopulations, especially young black men.[38] This concentration of criminal records may therefore be a significant contributor to racial disparities in employment and other socioeconomic outcomes.

B. Sealing of Criminal Records: The Legal and Policy Landscape

At least thirty-six states have adopted statutes that permit adult criminal convictions to be sealed, set aside, or expunged.[39] These laws generally require state agencies to ignore the affected individual's criminal record for most non-law-enforcement purposes, thereby lifting any statutory barriers to public employment, licensing, and benefits. The laws also give an individual the legal right to respond "no" when an employer or landlord asks if the applicant has a criminal record. In most states (including Michigan), sealed convictions remain available for law enforcement purposes, and for sentencing in the event of a subsequent crime.

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[32] Jenny Roberts, *Why Misdemeanors Matter: Defining Effective Advocacy in the Lower Courts*, 45 U.C. DAVIS L. REV. 277, 299 (2011).

[33] CAL. DEP'T OF CORRECTIONS, PREVENTING PAROLEE FAILURE PROGRAM: AN EVALUATION (1997), https://www.ncjrs.gov/pdffiles1/Digitization/180542NCJRS.pdf.

[34] MUKAMAL, *supra* note 17; GWEN RUBINSTEIN & DEBBIE MUKAMAL, *Welfare and Housing: Denial of Benefits to Drug Offenders*, *in* INVISIBLE PUNISHMENT: THE COLLATERAL CONSEQUENCES OF MASS IMPRISONMENT 41–42 (Marc Mauer & Meda Chesney-Lind eds. 2002); Geiger, *supra* note 29, at 1204.

[35] LEGAL ACTION CTR., *supra* note 31.

[36] Rebecca Vallas et al., *Removing Barriers to Opportunity for Parents With Criminal Records and Their Children*, CTR. FOR AM. PROGRESS (Dec. 10, 2015, 12:01 AM), https://www.americanprogress.org/issues/poverty/reports/2015/12/10/126902/removing-barriers-to-opportunity-for-parents-with-criminal-records-and-their-children.

[37] See U.S. DEP'T OF JUSTICE, BUREAU OF JUSTICE STATISTICS, PRISONERS IN 2016, at 10 (2018), https://www.bjs.gov/content/pub/pdf/p16.pdf (showing imprisonment rates at 1609, 857, and 274 per 100,000 for black, Hispanic, and white adults, respectively); Robert Brame et al., *Demographic Patterns of Cumulative Arrest Prevalence by Ages 18 and 23*, 60 CRIME & DELINQ. 471, 476 (2014) (finding about half of black men have been arrested by age twenty-three compared to thirty-eight percent of white men).

[38] Andi Mullin, *Banning the Box in Minnesota—and Across the United States*, COMMUNITY CATALYST (Dec. 2, 2013), https://www.communitycatalyst.org/blog/banning-the-box-in-minnesota-and-across-the-united-states; THE PEW CHARITABLE TRS., *supra* note 15, at 27.

[39] *See supra* note 10 and accompanying text; *see also* Simone Ipsa-Landa & Charles E. Loeffler, *Indefinite Punishment and the Criminal Record: Stigma Reports Among Expungement-Seekers in Illinois*, 54 CRIMINOLOGY 387, 392 (2016).

The specific eligibility requirements for record expungement vary widely. For example, some states' laws exclude certain classes of crimes, such as violent felonies.[40] Waiting periods also vary. In some states, at least for some categories of crime, individuals can apply for expungement immediately after completing their sentence (although evidence of rehabilitation must generally be shown, which can be harder for immediate applicants).[41] Other states have minimum waiting periods ranging from one to twenty years.[42] In some states (including Michigan), it is illegal for an employer to discriminate on the basis of a conviction that has been set aside, if the employer is aware of it.[43] The Collateral Consequences Resource Center and the Clean State Clearinghouse provide comprehensive state-by-state information on expungement laws.[44]

Beyond adult conviction records, most states have other expungement policies covering at least some other types of criminal records, such as juvenile records, non-conviction records, or convictions that have been overturned or are subject to successful collateral attack.[45] Many also have deferred adjudication programs available for certain defendants (for example, first-time drug offenders or youthful offenders), in which no conviction is ever entered if the defendant completes certain requirements.[46] These laws raise related empirical and policy questions, and debates about them might well be able to draw on our findings. But we focus here on state laws that expunge otherwise-valid adult convictions that have become final; these types of bills have been expanding in recent years and are subjects of active political debate in many states. Although supported by the American Bar Association,[47] and by advocacy groups such as the National Employment Law Project, the Center for American Progress, and Community Legal Services,[48] they have also met with considerable political opposition, principally from employers, who want access to information they consider relevant to hiring decisions.[49]

The most recent wave of efforts to expand and improve expungement laws—often referred to by advocates as the "Clean Slate" movement—has focused to a large degree

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[40] *E.g.*, N.C. GEN. STAT. § 15A-145.5; OKLA. STAT. §§ 18(A)(12).

[41] *See e.g.*, CAL. PENAL CODE §§ 1203.4, 1203.4a; ARIZ. REV. STAT. § 13-907.

[42] *See CCRC State Survey*, *supra* note 10.

[43] In Michigan, to "use" or divulge information about a set-aside conviction is a misdemeanor. MICH. COMP. LAWS § 780.683.

[44] *See supra* note 10.

[45] *See CCRC State Survey*, *supra* note 10.

[46] *See id.* (describing deferred adjudication programs).

[47] *E.g.*, ABA Standards 1981, Standard 23-8.2; Lucian Dervan, *Report to the House of Delegates*, 2019 A.B.A. SEC. CRIM. JUST. 1, https://www.americanbar.org/content/dam/aba/administrative/house_of_ delegates/resolutions/ 2019-midyear/2019-midyear-109b.pdf.

[48] *See Release: Removing Barriers to Economic Opportunity for Americans with Criminal Records Is Focus of New Multistate Initiative by CAP, NELP, and CLS*, CTR. FOR AM. PROGRESS (Sept. 12, 2017), https://www.americanprogress.org/press/release/2017/09/12/437592/release-removing-barriers-economic-opportunity-americans-criminal-records-focus-new-multistate-initiative-cap-nelp-cls.

[49] *See, e.g.*, Matt Byrne & Penelope Overton, *Lawmakers Consider Sealing, Erasing Records of Past Cannabis Convictions*, PROVIDENCE J., PORTLAND PRESS HERALD (Jan. 10, 2019), https://www. pressherald.com/2019/01/10/legislators-consider-sealing-expunging-records-of-old-cannabis-convictions; Nancy Reardon*, Activists Want Law to Seal Criminal Records Sooner,* PATRIOT LEDGER (July 28, 2009), https://www.patriotledger.com/x836550449/Activists-want-law-to-seal-criminal-records-sooner.

on the potential for *automatic* expungement of certain criminal records after a certain period of time.[50] So far, the watershed success in this area has been the adoption of Pennsylvania's Clean Slate Act, which extended court-ordered criminal record sealing to encompass a broader set of offenses and created an automatic computerized process for sealing certain eligible records, including minor nonviolent misdemeanors after 10 years without a subsequent conviction.[51] The Clean Slate Bill received nearly unanimous legislative endorsement and was supported by an overwhelming majority of Pennsylvania residents.[52] Although Pennsylvania's law currently is the only one of its kind, similar reform efforts are now underway in several other states.[53] Importantly, in February 2019, California's Assembly began to consider its own automatic expungement bill,[54] receiving significant media attention and with commentators predicting relatively easy passage, notwithstanding its broader substantive scope (applying to all misdemeanors and to some felonies resulting in probation).[55] The law also imposes no lengthy waiting period: probationers may apply immediately upon completing probation, and those with other sentences such as fines may apply a year after their convictions, provided that the sentence is complete.[56] In addition, a federal bill introduced in August 2018 would allow automatic expungement one year after completing the sentence for marijuana offenses and certain other minor drug offenses.[57]

Many states and local jurisdictions have also adopted other laws designed to reduce barriers to employment of people with records. The most important category (along with expungement laws) are Ban-the-Box laws and policies, which typically bar employers from asking about records on initial job application forms and in initial interviews. Thirty-three states and over 150 cities and counties have passed Ban-the-Box laws governing public employers, and a further 11 states and 17 cities now extend them to private employers.[58] In terms of the number of people affected, Ban-the-Box laws are far more sweeping than typical expungement laws are, because they apply to all criminal records, with no eligibility requirements. However, expungement, for those that do obtain it, offers potentially far more significant relief from the consequences of criminal

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[50] *See* Clean Slate Campaign, https://cleanslatecampaign.org..

[51] *See Governor Wolf: "My Clean Slate" Program Introduced to Help Navigate New Law* (Jan. 2, 2019), https://www.governor.pa.gov/governor-wolf-my-clean-slate-program-introduced-to-help-navigate-new-law; *Frequently Asked Questions About Clean Slate*, CMTY. LEGAL SERVS. OF PHILA. (June 26, 2018), https://clsphila.org/learn-about-issues/frequently-asked-questions-about-clean-slate.

[52] *See* J.D. Prose, *Pennsylvania Becomes First State With 'Clean Slate' Law for Nonviolent Criminal Records*, TIMES (June 28, 2018, 5:00 PM), https://www.timesonline.com/news/20180628/pennsylvania-becomes-first-state-with-clean-slate-law-for-nonviolent-criminal-records.

[53] *See Release*, supra note 48.

[54] Assembly Bill No. 1076, 2019–20 Gen. Assemb., Reg. Sess. (Conn.), https://leginfo.legislature.ca.gov/faces/billTextClient.xhtml?bill_id=201920200AB1076 (Feb. 21, 2019).

[55] *See* Williams, *supra* note 12 (describing California's legislative activity).

[56] *See* Assembly Bill No. 1076, *supra* note 54.

[57] H.R. 6669, 115th Cong. (2018).

[58] Beth Avery & Phil Hernandez, Ban The Box—Fair Chance Guide, NAT'L EMP'T LAW PROJECT 1, 1 (Sept. 2018), https://s27147.pcdn.co/wp-content/uploads/Ban-the-Box-Fair-Chance-State-and-Local-Guide-September.pdf; *A State Agenda for America's Workers*, ECON. POL'Y INST. (Dec. 3, 2018), https://www.epi.org/publication/state-agenda-for-americas-workers.

convictions, cutting across different domains of life and, in effect, legally erasing the conviction. In contrast, Ban-the-Box laws typically only affect employer practices, and only affect the timing of employers' receipt of information; they can still refuse to hire an applicant after completing the background check.[59] Still, Clean Slate advocates typically see the two types of reforms as complementary, and many advocacy organizations have pushed both.[60]

Some states have also adopted laws that substantively restrict the use employers can make of criminal record information—for example, requiring that they only rely on information that is job-relevant.[61] These laws essentially replicate guidance long since given by the federal Equal Employment Opportunity Commission, which holds that overly sweeping bans on employees with records amount to disparate-impact racial discrimination.[62] Unfortunately, courts rarely enforce these restrictions,[63] and many employers throughout the country have continued to implement blanket exclusions, notwithstanding EEOC's guidance.[64] These practices, and the difficulty of eliminating them through other legal mechanisms, are among the motivations for expungement laws.

C. Research Questions and Existing Empirical Research on Expungement

There has been very little empirical research on any of the many questions surrounding expungement laws, despite the clear importance of these inquiries to policymakers and the lives of millions of Americans. In truth, most of these questions really *cannot* be answered effectively absent comprehensive access to individual-level data on people whose records have been expunged, and because those records are generally unavailable, research has been stymied. Fortunately, by taking advantage of our unusual and robust data access, we examine several key questions that have long remained unanswered.

First, there is essentially no research on the question of "uptake" in the context of expungement of convictions. The basic question relates to the uptake rate: when people are legally eligible for expungement, how often do they actually apply for and receive it? Beyond this question, we also seek to provide a richer picture of *who* receives relief— what kinds of convictions they have, what types of sentences they served, their demographics—and how their profiles compare to the broader pool of eligible persons, i.e., what factors predict actual receipt of relief. Answering these questions can help us to

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[59] *See* Agan & Starr (2018), *supra* 24.

[60] *See, e.g.*, Angela Hanks, *Ban the Box and Beyond*, CTR. FOR AM. PROGRESS (July 27, 2017, 9:03 AM), https://www.americanprogress.org/issues/economy/reports/2017/07/27/436756/ban-box-beyond/.

[61] *See* Beth Avery & Phil Hernandez, *supra* note 58.

[62] *See EEOC Enforcement Guidance*, U.S. EQUAL EMP'T OPPORTUNITY COMM'N (Apr. 25, 2012), https://www.eeoc.gov/laws/guidance/arrest_conviction.cfm.

[63] *See* Ben Geiger, *The Case for Treating Ex-Offenders as a Suspect Class*, 94 CAL. L. REV. 1191, 1199–1203 (2006).

[64] Miriam J. Aukerman, *The Somewhat Suspect Class: Towards a Constitutional Framework for Evaluating Occupational Restrictions Affecting People with Criminal Records*, 7 J.L. & SOC'Y 1, 26 (2005); *see also* U.S. DEP'T OF LABOR, *From Hard Time to Full Time: Strategies to Help Move Ex-Offenders from Welfare to Work* (2001), https://www.hirenetwork.org/pdfs/From_Hard_Time_to_Full_Time.pdf.

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better understand the problem of "uptake gaps," and can also inform the discussion of expungement's effects. Given that the subsample that actually receives relief is a highly selected one, in order to speculate as to the potential results of extending expungement laws to a broader and possibly dissimilar population, we need to understand that selection process.

No study has addressed these questions. However, in certain other post-conviction-relief contexts, there have been a few attempts to explore issues related to uptake. The most comprehensive such effort is a recent working paper by Colleen Chien estimating the uptake rates for several programs: President Obama's clemency initiative allowing certain federal inmates to apply for sentence commutation; California's Proposition 47, which allows some felony convictions to be reduced to misdemeanors; and California's Proposition 64, which legalized marijuana and provided various forms of relief for those previously convicted on marijuana charges.[65] It finds uptake rates of about 3%, 9%, and 3%, respectively (within a relatively short time period, especially for the newer Proposition 64).[66] Chien also analyzes a sample of background-check data concerning "gig" jobseekers and finds evidence that many reports contain *nonconviction* records (e.g., arrests not leading to convictions), which are clearable under state law.[67] Research by Christopher Uggen and co-authors has also found large uptake gaps in voting-rights-restoration procedures for people disenfranchised due to felony convictions.**[68]** Although none of these estimates focus on the expungement of convictions, they all suggest a general problem: when criminal-justice relief mechanisms require individuals to go through application procedures, many people who might benefit from them will not do so.

Second, we investigate subsequent recidivism outcomes for expungement recipients, in order to address the concern, often raised by opponents of record-sealing, that the public has a safety interest in being able to identify criminal offenders.[69] We are unaware of any other empirical research on this question, which we are able to investigate because we have recipients' full criminal records.[70] Evidence on the recidivism question is critical

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[65] Colleen V. Chien, *The Second Chance Gap*, Working Paper (2018), https://papers.ssrn.com/sol3/papers.cfm? abstract_id=3265335.

[66] *Id.* at 16–18. The study does not use individual-level data to identify eligible cases and track their outcomes; rather, it compares publicly reported figures on the estimated numbers of people eligible for relief and on the number of applications.

[67] *Id.* at 24. The rates at which such nonconviction records appeared varied substantially by state. Note that because cleared records do not appear in background check data, Chien's approach does not identify the number or share of eligible people who *do* clear nonconviction records; rather, it simply shows that there are many who have not done so. To estimate uptake rates for expungement among those eligible requires access to cleared records, which researchers typically have not had.

[68] CHRISTOPHER UGGEN ET AL., THE SENTENCING PROJECT, 6 MILLION LOST VOTERS: STATE-LEVEL ESTIMATES OF FELONY DISENFRANCHISEMENT 13 (2016), https://www.sentencingproject.org/wp-content/uploads/2016/10/6-Million-LostVoters.pdf.

[69] T. Markus Funk, *The Dangers of Hiding Criminal Pasts*, 66 TENN. L. REV. 287, 298–99 (1999); Margaret Colgate Love, *Starting Over With a Clean Slate: In Praise of a Forgotten Section of the Model Penal Code*, 30 FORD. URB. L.J. 1705, 1726 (2003); Michael Mayfield, *Revisiting Expungement: Concealing Information in the Information Age*, 1997 UTAH L. REV. 1057, 1065–70.

[70] Theoretical work has been done on this question that suggests expungement likely would decrease recidivism. Murat C. Mungan, *Reducing Crime Through Expungement*, J. ECON. BEHAV. ORG. 137 (Mar.

to address objections from those who fear endorsing expungement policies because of the possibility that they could increase recidivism risk.

Third, we examine the employment consequences of expungement. Although potential employment benefits are core to the policy case for expungement laws, they have proven especially difficult to study because both expunged criminal records and wage and employment records are typically confidential. The above-cited research showing that criminal records impair employment opportunities provides a strong intuitive reason to believe that expunging those records should have the opposite effect. However, this intuition could prove to be incorrect, and the magnitude of any effect is unknown. In particular, many scholars have expressed concern that expungement may fail to hide criminal history information from inquisitive employers—for example, if news stories, mug shots, or other information about past offenses can be easily found with a Google search or other digital means ("Everything posted online is there forever…"[71]), or if private criminal records databases do not effectively delete expunged records.[72] This objection has been speculative, but we are able to evaluate it empirically. In addition, the waiting periods built into expungement laws may undermine any potential benefits because these restrictions mean that expungement cannot help during the critical period immediately after conviction or reentry.[73] People with records often also tend to face many other employment disadvantages besides the record itself, potentially limiting the benefits of expungement.[74]

To date, the most on-point research is a recent study by Jeffrey Selbin, Justin McCrary, and Joshua Epstein, which tracks labor market outcomes for 235 clients of a Berkeley Law School clinic who pursued either expungement or another form of relief (which allowed the reduction of felonies to misdemeanors).[75] The authors found

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2017). *See also* Ericka B. Adams et al., *Erasing the Mark of a Criminal Past: Ex-Offenders' Expectations and Experiences with Record Clearance*, 19 PUNISHMENT & SOC'Y 23 (2017) (finding, based on interviews with 40 people, that expungement encourages attitudinal shifts that could potentially be associated with reduced recidivism).

[71] Victoria Cumbow, *Everything Posted Online Is There Forever, Even After It's Been Deleted*, AL.COM (Mar. 30, 2011), http://blog.al.com/breaking/2011/03/everything_posted_online_is_th.html; *see also* Sarah Esther Lageson, *There's No Such Thing as Expunging a Criminal Record Anymore*, SLATE, Jan. 7, 2019; Tribune Wire Reports, *Experts: Deleted Online Information Never Actually Goes Away*, CHI. TRIBUNE (Aug. 21, 2015), https://www.chicagotribune.com/bluesky/technology/chi-deleted-online-information-never-goes-away-20150821-story.html.

[72] Jenny Roberts, *Expunging America's Rap Sheet in the Digital Age*, 2015 WISC. L. REV. 321, 341–346; Sharon M. Dietrich, *Preventing Background Screeners from Reporting Expunged Criminal Cases*, SHRIVER CTR. (Apr. 2015); Eldar Haber, *Digital Expungement*, 77 MD. L. REV. 337 (2018); Clay Calvert & Jerry Bruno, W*hen Cleansing Criminal History Clashes with the First Amendment and Online Journalism: Are Expungement Statutes Irrelevant in the Digital Age?*, 19 COMMLAW CONSPECTUS 123 (2010); Love, *supra* note 69, at 1719, 1725–26; Pierre H. Bergeron & Kimberly A. Eberwine, *One Step in the Right Direction: Ohio's Framework on Sealing Criminal Records*, 36 UNIV. Toledo L. REV. 595, 597 (2005); Marc A. Franklin & Diane Johnsen, *Expunging Criminal Records: Concealment and Dishonesty in an Open Society*, 9 HOFSTRA L. REV. 733, 745–48 (1981); Michael Mayfield, *Revisiting Expungement: Concealing Information in the Information Age*, 1997 UTAH LAW REVIEW 1057, 1068–69 (1997).

[73] Franklin & Johnsen, *supra* note 72, at 739; JACOBS, *supra* note 7, at 131; Selbin et al., *supra* note 27, at 52.

[74] See *supra* note 18.

[75] Selbin et al., *supra* note 27, at 17–20.

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suggestive evidence of employment gains. As they acknowledged, however, their small sample size did not "allow for precise estimation,"[76] and the study does not present statistical estimates with standard errors (which allow readers to evaluate the degree of imprecision). In addition, their sample is not necessarily representative of those who obtain expungement statewide, and their data did not allow them to answer the additional questions discussed above regarding uptake and crime outcomes. So while their study provides a welcome start, its objectives were relatively modest. In contrast, we use a large, statewide sample that includes *all* set-aside recipients as well as a large comparison group of eligible offenders, and are able to use these data to address a variety of interrelated questions with strong statistical power.

D. Our Empirical Setting and Data

In Michigan, expungements are referred to as "set-asides," so we will use that term when presenting our Michigan-specific analysis and results. Michigan's set-aside law, MCL 780.621, has been in effect since the 1960s, although expungements were very rare before a law change in 1983. It has undergone some recent changes, mostly expansions. Because of the time range of our data, the most relevant version of the law is the one that was in effect (with no material changes) from 1983 until mid-2011. Our empirical analyses focus on persons eligible under that law's requirements, and to set-asides granted before the law changed. The fact that Michigan has had a set-aside law for decades distinguishes it from the majority of other states, and makes it a particularly useful focus of empirical study; this long experience can inform other states that have recently adopted or are considering adopting similar bills. Cumulatively, tens of thousands of people have received set-asides under Michigan's law, and many received them long enough ago to allow a substantial subsequent period to observe outcomes.

Michigan's set-aside law imposes a five-year waiting period, which is in the middle of the range of the waiting periods that other states' expungement statutes apply. In the pre-2011 version, the five-year clock began running at sentencing, unless the defendant served a term of incarceration after sentencing, in which case it ran from release. Any reconviction during this five-year period disqualified the defendant from a set-aside. Essentially, then, the law required five "clean" years, excluding time behind bars. The statute covered (and still covers) almost all types of crimes, including most violent felonies. The principal exceptions are traffic offenses, sex offenses that are subject to registration requirements, and the most serious class of felonies, those carrying potential life-imprisonment terms. The wide range of crimes otherwise covered is another advantage of Michigan's law for research purposes; it allows us to compare outcomes and uptake rates across many important offense types and grades.

However, Michigan does impose stringent limitations on set-aside eligibility based on the *length* of the offender's record—that is, the number of convictions (nonconviction records like arrests are not relevant to eligibility). While this has now been loosened slightly, in the pre-2011 version, set-asides were strictly limited to people with exactly one conviction on a single charge. Prior convictions, subsequent convictions, and simultaneous convictions—even multiple charges stemming out of the same incident—

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[76] *Id.* at 49.

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are all disqualifying. On a policy level, this has incongruous results; for example, a person with a single very serious felony may be eligible for a set-aside, while a person with two simultaneous misdemeanor counts arising from one incident is not. However, the simple, bright-line nature of these eligibility requirements offers advantages from a research perspective; for example, it makes it easier to code set-aside eligibility and other variables related to the nature of the criminal record.

For those meeting the eligibility requirements, set-asides are not automatic. Rather, they require the applicant to go through an elaborate application process, which we describe in detail in Part II.C. The court is not required to grant the set-aside—it "may" grant the request if warranted by the applicant's subsequent behavior and "consistent with the public welfare." Still, grant rates are fairly high. Although our data do not identify unsuccessful applications, the Michigan State Police (MSP), who process applications and also implement the set-asides once they are granted, provided figures for 2016 and 2017 that indicate that approximately 75% of applications are successful.[77]

To carry out our study, we entered a data-sharing agreement with multiple Michigan state agencies, who worked with us to develop a merged and deidentified dataset. MSP provided comprehensive Michigan criminal histories (RAP sheets) on almost every set-aside criminal conviction in Michigan history through March 2014,[78] amounting to nearly 30,000 cases, plus a larger group of other records meeting requirements that we discuss below. These were linked to wage and employment information on the same individuals from the state Unemployment Insurance Agency and Workforce Development Agency. Matching was based on Social Security numbers, so those without such numbers were excluded from the analysis of wage data. The matching was carried out by the Department of Technology, Management, and Budget, and all records were deidentified before the department turned them over to us.[79]

In addition to the records for individuals who actually received a set-aside, we obtained records for a large group of people who were, at the time of their convictions, potentially legally eligible for set-asides in the future, if they subsequently succeeded in meeting the five-clean-years requirement. Specifically, this group consisted of individuals with a first criminal offense (occurring between 1999 and 2008) that was on a list of legally eligible offense codes that we developed on the basis of the set-aside statute and provided to the MSP for a data query. Because MSP asked us to keep our request reasonable in size, we excluded from this list certain common petty misdemeanors—such as dog leash law and hunting and fishing violations—which are almost never set aside, even though they are legally eligible.[80] We discuss the implication of these exclusions below.

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[77] Email from Ted Kilvington, Court Reporting Coordinator, Mich. State Police, to Simmon Kim, Research Associate, University of Michigan Empirical Legal Studies Center (Feb. 22, 2019) (on file with authors).

[78] A small share of the most recent records were inadvertently dropped, as discussed below.

[79] In the initial stages of our work on this project, in order to shape our records requests from MSP, we also received and analyzed court records from Michigan's Judicial Data Warehouse. However, because of incompleteness of these records, we did not include them in the ultimate data analysis.

[80] We identified these as among Michigan's most common first offenses based on a preliminary analysis of courts data from the Judicial Data Warehouse, but these offenses are essentially not found at all

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Our data have some other limitations. For our criminal record and economic outcome data, we lack information on out-of-state convictions (affecting our coding of eligibility and recidivism) and out-of-state income (affecting our wage and employment analysis). Our wage and employment information comes from the state unemployment insurance system, which covers wage and salary workers quite comprehensively but misses the self-employed or those employed "under the table." We discuss these issues as they pertain to our analyses below.

Our most serious data challenge involves missing records. The state's process for matching and deidentifying our criminal history and employment outcomes data inadvertently dropped certain observations. Data patterns make clear that their procedure identified duplicate records produced by overlapping data pulls by MSP, but then mistakenly deleted *both* copies. Fortunately, we were able to restore virtually all of the dropped records that concerned set-aside recipients because we had already received a complete version of that part of the dataset slightly earlier. We were unable to restore some records for individuals who had not received set-asides, however.[81] Through information from MSP, we were able to identify the number of lost records (about 9% of the non-recipients in the original data pull). As we discuss below, the nature of these drops only minimally affects our uptake analysis and should not affect our employment analysis at all. However, the problem did lead us to focus our recidivism outcome analysis on set-aside recipients alone; the dropped non-set-aside-receiving cases were individuals with at least two convictions, so omitting them would have distorted our analysis.[82]

Another data challenge concerns the date fields in the criminal records data. When MSP effectuates set-asides, they change the disposition field in the record to denote a set-aside, but their data entry system does not have a separate field for the set-aside date. Instead, MSP's practice in most years was to change a field called "judgment date" to the set-aside date. This practice caused the information originally stored in the judgment date field to be lost—specifically, the conviction date, which is what we use in most cases to calculate the start date for the five-year set-aside eligibility clock.[83] Because some of our

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in the complete dataset on all set-asides, perhaps suggesting that people do not bother to set aside records so minor that may impose no serious burdens. Some cases were also inadvertently excluded because MSP formatted offense codes in inconsistent ways that did not match our list; these exclusions seem substantively random. We also excluded minor-in-possession cases, which in Michigan were (until the offense was decriminalized in 2016) often subject to a separate diversion process for youthful offenders.

[81] By the time we diagnosed the problem, our data-sharing agreement period and grant period had expired, and the state authorities informed us that they could not reconstruct the original dataset for us or repeat the process.

[82] We requested data on people with certain kinds of first convictions or second convictions within a ten-year period; people with first *and* second convictions meeting these criteria were included as duplicates and then doubly dropped. The other set of duplicates dropped were set-aside recipients who also met the eligibility criteria for the comparison group, but this was the set that we were able to restore.

[83] Set-aside eligibility in nonincarceration cases runs from sentencing, which is usually a few weeks later than the case disposition, Email from Jonathan Sacks, Appellate Defender, State Appellate Defs.' Office, to Sonja Starr (February 22, 2019) (on file with authors); Email from Kim Thomas, Professor, Univ. of Mich. Law Sch., to Sonja Starr (Feb. 20, 2019) (on file with authors), and has no recorded date in the data. MSP informed us, however, that when they make their official determinations of set-aside eligibility,

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analyses (especially of uptake rates) require us to know *both* the set-aside eligibility date and the set-aside grant date, we had to impute the lost conviction date based on the incident, arrest, and charge dates. We believe this adds fairly minimal measurement error. The imputation should be correct on average, so our resulting uptake estimates should not be biased, even if we might get the date of eligibility wrong by a month or two for some individual observations.

In a small subset of set-aside cases, MSP changed the disposition code but did *not* change the judgment date field to the set-aside date, leaving no record of when the set-aside occurred. These cases are identifiable because the judgment date is too close to other key dates in the case (for example, the arrest) to be a plausible set-aside date given the required five-year waiting period. We removed these cases from the recidivism and employment analyses, which require set-aside dates; the lag time from other events in the case to set-aside receipt is for too unpredictable to be imputed without introducing significant measurement error. In our analyses of set-aside uptake rates within certain periods of time, we cannot ignore these cases or we would understate uptake, so instead we make assumptions about their temporal distribution to estimate bounds on uptake rates. Fortunately, the practical importance of this problem is small; there is good reason to believe that most of these unknown-date set-asides occurred after June 2011, when we see a sudden drop in the number of known-date set-asides to virtually zero, reflecting a change in MSP's data-recording practice.[84] This date happens to coincide with the substantive changes in the set-aside law that (as noted above) took place in 2011, so the focus of our work is on set-asides taking place before that date anyway.

II.   THE UPTAKE GAP: WHO SEEKS AND RECEIVES SET-ASIDES?

Challenges surrounding uptake may be the most underappreciated problem concerning expungement policies—and indeed, similar problems arise in many access-to-justice contexts. Uptake challenges potentially arise whenever the state imposes significant burdens—such as fees or onerous administrative requirements—on the exercise of a right or opportunity, or whenever it is difficult for potential beneficiaries to learn about those rights or opportunities. And a right or opportunity that is too difficult for most people to exercise is effectively empty. Accordingly, in this Part, we address questions surrounding set-aside uptake (here defined as successful receipt of a set-aside), which have never previously been examined empirically.

In Section A, we estimate the overall five-year set-aside uptake rate. In Section B, we turn to the question of *who* successfully obtains set-asides, presenting descriptive statistics on recipients and regression analyses to assess which individual and case characteristics predict set-aside receipt. We use these analyses in Section C, along with

they rely on the conviction date as a proxy for sentencing, so we do the same here. Email from Nick Romanov, IT Specialist, Mich. State Police, to J.J. Prescott (March 19, 2014) (on file with authors).

[84] MSP confirmed that set-aside dates are not recorded in their current data and that the number of set-asides granted has only increased since 2011. *See* Email from Ted Kilvington, *supra* note 77 (stating that 2,594 set-asides were processed in 2017). Assuming set-aside frequency at least did not drop, we believe at least half the unknown-date set-asides in the earliest version of our complete set-aside dataset (which ended in December 2012) must have occurred between June 2011 and December 2012, which leaves at most 3,000 occurring before then (about 13% of the number of known-date set-asides).

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qualitative insights provided by our interviews with set-aside experts, to inform a discussion of uptake hurdles and their implications.

A. Estimating Uptake Rates

The first step in estimating set-aside uptake rates is identifying which cases are legally eligible for set-asides. In our case, the relevant eligible pool is defined first by the parameters of the records query that we asked MSP to implement, as described in Part I.D, and second by some further refinements we carry out thereafter. The sample parameters are as follows:

- *Only first-time offenders convicted on a single criminal count.*[85]
- *Crime of conviction matches an offense code on a list of offenses eligible for set-asides.*[86]
- *Sentencing for the eligible count took place between January 1999 and May 2001.*[87]
- *The individual was not subsequently convicted for any crime within five years of sentencing.*[88]
- *The individual was not sentenced to incarceration on eligible offense.*[89]
- *No out-of-state driver's license in the MSP arrest record.*[90]

The five-year uptake rate is the percentage of this eligible group that has received a set-aside within five years of the eligibility date (that is, within ten years of sentencing). We make two further assumptions in order to calculate this uptake rate, which relate to

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[85] This was a pre-2011 eligibility requirement. As discussed below, we are only able to screen for prior offenses that took place in Michigan.

[86] We are confident that our offense list only contains offenses that are in fact legally eligible for set-asides. However, as described in Part I.D and discussed below, our list is not entirely inclusive of all eligible offenses.

[87] Thus, set-aside eligibility would kick in between January 2004 and May 2006. This sample is as recent as possible while still making it possible to track outcomes for five years after eligibility; the main estimates we give are a five-year uptake rate. We treat May 2011 as the end of our tracking period because two important changes occurred in June 2011: the eligibility law changed (making more people eligible but also rendering some formerly eligible persons ineligible), and MSP ceased to record the set-aside date. For cases receiving set-asides, because of MSP's data recording practices, the sentencing date is imputed.

[88] This is also a legal requirement, and again we implement this filter based on Michigan data alone. Our eligible pool consists of people who became eligible after five years, and we estimate set-aside receipt rates within the next five years. Some members of this sample could have lost eligibility at some point during the second five-year period due to a subsequent conviction, although as we will see in the recidivism analysis in Part III, this is rare.

[89] We focus on non-incarceration cases (which, as we will see below, constitute the large majority of all set-aside cases) because we have a more accurate measure of the start date of the set-aside eligibility clock, which runs from sentencing. In incarceration cases, the eligibility start date is based on release from incarceration, and because our data tell us only the sentence and not the actual release date, we cannot account for possible early release or credit for time served pretrial.

[90] In our data, driver's license numbers have been removed, but for most observations we know whether the individual had a driver's license and, if so, the state that issued that license. We exclude individuals with out-of-state driver's licenses to reduce the likelihood of miscoding eligibility on account of unobservable out-of-state convictions, but we retain those observations who affirmatively did not have a driver's license or who had a license but it is unclear which state issued it.

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the data problems identified above in Part I.D. First, we assume the distribution of unknown-date set-asides in our sample, in terms of time elapsed since the case's disposition, is roughly similar to the distribution of known-date set-asides. We show below that our conclusions are robust to alternative assumptions. Second, we assume that the missing non-set-aside records would *not* have met the criteria for inclusion in this sample, and thus we do not adjust our estimate to account for them. We believe this latter assumption is largely correct; dropped cases all had at least two convictions within our sample's time parameters, few of which would meet the 5-year nonrecidivism criterion. To the extent this assumption is mistaken, we understate the number of eligible cases and thus overestimate the uptake rate (but not likely by much).

Table 1. Uptake Rates: Receipt of Set-Aside Within 5 Years of Eligibility		
Version	**5-Year Uptake**	**N**
1. Main	6.5%	9,103
2. Accounting for Unknown Date Set-Asides: Lower Bound	5.7%	9,103
3. Accounting for Unknown Date Set-Asides: Upper Bound	6.7%	9,103
4. Add Incarcerated (< 1 Year)	5.4%	14,223

In Table 1, we present our main uptake estimate, which is discouragingly low. Of eligible offenders, only 6.5% received set-asides within five years of becoming eligible. The remaining 93.5% combines people who did not apply for set-asides and who had their applications denied. However, as noted above, we learned from MSP that in 2016 and 2017 combined, 74% of set-aside applications MSP received were ultimately granted by courts. If we assume that all of these applicants were legally eligible and that the same ratio applied during the relevant time period for our estimate (2004-2011), 6.5% of those eligible for set asides received them within five years, another 2.3% had applications denied, and 91.2% did not apply during the first five years of eligibility. This extrapolation may actually overstate the share of *eligible* people who apply for set-asides, because some denials are based on ineligibility.[91]

In Section C, we discuss reasons for this poor uptake. But first, how confident can we be in our estimates? In particular, given that our sample is both overinclusive and underinclusive in some ways, is our main uptake estimate for this sample a good proxy for set-aside uptake for the full eligible population during these years? To inform this question, the remaining rows of Table 1 present estimates based on alternative assumptions and sample definitions. Given that our finding is one of very low uptake, we use the term "conservative" below to refer to refer to assumptions that will likely lead to *overestimation* of uptake.

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[91] Interview with Tracey Brame, Associate Dean, Cooley Law School (March 22, 2019).

First, in Rows 2 and 3, we alter our timing assumption for the unknown-date set-asides. Ninety such cases appear in this sample. Our initial assumption that their timing distribution mirrors that of the known-date set-asides is conservative; there is strong reason to believe that these set-asides disproportionately occurred after May 2011 (and therefore after the five-year period), when MSP stopped changing the disposition dates. We estimate bounds on the possible scope of error from this assumption. If we assume that *none* of the unknown-date set-asides occurred within five years of eligibility, or, instead, we apply most conservative possible assumption (that they *all* occurred within five years), the resulting uptake estimates range only from 5.7% to 6.7%. Because our original approach was conservative, we assume that the correct uptake rate (if all other assumptions are valid, that is) lies somewhere between 5.7% and 6.5%.

In Row 4, we add to the sample cases involving incarceration of up to one year, testing the effect of our exclusion of incarceration cases. We assume that the defendant served the full incarceration term, commencing on the date of sentencing. This assumption is also conservative because it ignores credit for time served and possible early release; it thus errs on the late side in estimating the release date, and may include slightly more than five years of eligible time in the "five-year uptake" estimate. After adding these cases to the sample, the uptake rate drops to 5.4%, indicating that people who have been sentenced to incarceration receive set-asides at lower rates than other eligible persons. Thus, our choice to focus on non-incarceration cases has the effect of increasing our main uptake estimate. We do not include a five-year estimate for those serving longer incarceration terms because we do not have a sufficient follow-up period for most of these cases. However, given the evidence that those who were incarcerated have a lower uptake rate, it seems likely that including those with longer incarceration terms would only lower the rate further.

There are a few uncertainties about our uptake estimates that we cannot address directly with our data. In particular, we have no records of out-of-state or federal convictions, and so our "eligible" pool probably includes some people who are not in fact eligible due to such convictions. Our exclusion of people with out-of-state licenses mitigates this problem, but does not eliminate it, as people can move or commit crimes across borders. Including ineligible cases in the eligible pool will produce an underestimate of the uptake rate. However, back-of-the-envelope calculations suggest that this problem is minor. The Census Bureau estimates that about 1.8% of lower-income Midwesterners (the best available proxy for our sample) move across state lines each year.[92] When we apply this rate of attrition to our sample, and extrapolate from the within-Michigan reconviction patterns in our data, it implies that we might fail to observe disqualifying reconvictions within 5 years for about 1% of the purportedly eligible population.[93] We have a similar problem with potentially disqualifying *prior* adult

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[92] *See Geographical Mobility: 2017 to 2018* tbl.1-12, U.S. CENSUS BUREAU (Nov. 2018), https://www2.census.gov/programs-surveys/demo/tables/geographic-mobility/2018/cps-2018/tab01_12.xls.

[93] We assume that, per the state issuing their driver's license, our sample members were Michigan residents at the time of their convictions. The mobility estimate implies that 7.3% of our sample would have moved from Michigan by five years; for that subgroup, we would miss on average the last 2.5 years of their criminal history. In our sample, those who do not get reconvicted by 2.5 years have a reconviction rate of 13% in the next 2.5 years; 13% times 7.3% is about 1%.

convictions for people who might have previously lived elsewhere; this problem might be twice as large or so given the age distribution of our sample. It is also possible for Michigan residents to commit crimes in other states, although the great majority of crimes are committed very close to home; we estimate that we might have missed prior or subsequent cross-border convictions for perhaps another 7% or 8% of the sample.[94] All in all, missing out-of-state convictions likely caused us to overstate the size of the eligible group by no more than 10% to 12%, and therefore to understate the uptake rate by less than one percentage point. Meanwhile, the federal-conviction problem is likely small enough to be ignored, because federal convictions are extremely rare relative to state convictions. [95]

In addition, our eligible pool of people with records is defined by the parameters of the data query that MSP conducted, which, as discussed in Part I.D, is itself limited by the list of statutory codes of eligible crimes that we provided. This source of error likely cuts in the opposite direction, causing us to *overestimate* uptake, because the largest category of legally eligible offenses that we excluded from the list are petty offenses that we know are very common first offenses in Michigan but are almost never set aside. If we were to include them, the estimated uptake rate would be lower. Although we inadvertently excluded some additional observations due to irregular formatting of statutory codes, these exclusions were very likely effectively random—that is, there is no reason to expect that the observations with irregular formatting had either higher or lower uptake rates compared to those included in the sample.

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[94] A large body of research finds that the average distance from an offender's home to crime locations is around one or two miles. *See* Michael Townsley & Aiden Sidebottom, *All Offenders Are Equal, but Some Are More Equal than Others: Variation in Journeys to Crime Between Offenders*, 48 CRIMINOLOGY 897, 899–900 (2010) (reviewing literature). One piece of evidence that most crimes are committed in state comes from our data: among arrestees with driver's licenses, 96% are from Michigan. If we make the conservative assumption that all of the balance are out-of-state residents (not just people with old licenses) and we assume conversely that about 96% of the recidivism committed by Michigan residents is in-state (not a necessary inference, but a plausible approximation), it would imply that we have missed reconvictions before five years in about 2.7% of our sample. We could have missed prior convictions over a somewhat longer period (11 years on average in our sample), some of which would presumably have involved the same individuals.

[95] The federal district courts in Michigan sentenced 1223 people for felonies or non-petty misdemeanors in 2017. U.S. SENTENCING COMM'N, STATISTICAL INFORMATION PACKET FISCAL YEAR 2017: STATE OF MICHIGAN 11 tbl.8 (2017), https://www.ussc.gov/sites/default/files/pdf/research-and-publications/federal-sentencing-statistics/state-district-circuit/2017/mi17.pdf. By comparison, in 2017, caseload figures suggest that the Michigan state courts entered roughly 340,000 criminal convictions of any sort. MICH. COURTS, 2017 COURT CASELOAD REPORT, https://courts.michigan.gov/education/stats/Case load/reports/statewide.pdf (last visited Mar. 9, 2019). This figure is based on adding felony and misdemeanor (including traffic) guilty pleas and trial verdicts in circuit and district courts, under the rough estimate that 80% of trial verdicts are convictions. The Michigan Department of Corrections reported approximately 47,000 persons convicted of felonies entering corrections in 2016. Kahryn Riley, *Coping with the Growing Number of Felons in Michigan*, MACKINAC CTR. FOR PUB. POL'Y (Aug. 27, 2018), https://www.mackinac.org/coping-with-the-growing-number-of-felons-in-michigan. Although these numbers are not precisely comparable, the difference in scale is obvious, and suggests that only a very small percentage of the single-Michigan-conviction offenders in our sample are likely to have a federal record, making a negligible difference to our uptake estimate.

Overall, we are confident that our main five-year uptake estimate of 6.5% is quite accurate for our sample, and is in the ballpark of the correct figure for the true population of eligible persons in Michigan during the years in question (2004–11); our best guess is that the true uptake rate is probably lower. While the lack of data from out-of-state or federal courts may have biased our estimate downward, these biases cannot be very large. Meanwhile, the assumptions we make about the missing cases and the unknown set-aside dates are designed to bias our uptake estimate upward, and our constraints on the statutory code as well as the exclusion of cases with incarceration terms almost surely did so as well. In any event, the substantive story is clear: very few of those who are legally eligible for set-asides receive them within five years of becoming eligible. Indeed, even if the true five-year rate were 10%—substantially higher than any version of our estimates—it would still be strikingly low.

What about after five years? We cannot directly estimate a longer-term uptake rate because of the time range of our data on the eligible-group sample. However, we do have data on the full universe of set-aside recipients in Michigan up to 2011. Among non-incarceration cases in that sample, at the time of set-aside receipt, 44% have an elapsed time since eligibility of more than 5 years.[96] Assuming the same pattern holds for the cases in our eligible sample, extrapolating from our main uptake estimate would predict a *lifetime* uptake rate of 11.8% for those not sentenced to incarceration. Even this rate means that 88% will never get relief---and those who get them after many years miss out on many years of potential benefits.

B. Who Receives Set-Asides?

Only a small minority of people with records in Michigan are eligible for set-asides, and only a small minority of those individuals in fact receive them. What distinguishes eligible persons who receive a set-aside from those who do not? To develop insight into possible reasons for the low uptake rate and to develop hypotheses about policies that might increase it, we assess the characteristics of the individuals who *do* receive set-asides and their cases, and we investigate which of those characteristics are usefully predictive of an eligible person receiving a set-aside.

In Table 2, we provide descriptive statistics of the relevant populations. The statistics in Column 1 refer to the complete population of all Michigan set-aside recipients with known dates prior to June 2011 (when the law changed). Columns 2 and 3 report summary statistics for the main sample used for our uptake-rate analysis in Table 1. Column 2 covers individuals within that sample who *did* receive set-asides within 5 years of becoming eligible, and Column 3 covers the entire uptake sample (most of whom did not receive set-asides). Columns 2 and 3 can be usefully compared to one another to shed light on how set-aside recipients differ from the broader eligible group. Some of the figures for set-aside recipients look a little bit different in Columns 1 and 2; this may arise from the various constraints on the Column 2 sample.

While set-aside recipients have diverse personal and case characteristics, some patterns stand out. Set-aside convictions are, relative to the eligible pool, much more likely to be felonies; they are more likely to be property offenses, less likely to be drug

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[96] *See infra* Figure 1 and accompanying text.

crimes, and equally likely to be violent offenses. But although set-asides are not limited to minor crimes, most set-aside recipients were not sentenced to incarceration for the set-aside offense. Among all set-aside recipients historically, only 29% have been incarcerated at all, and only 2% have been incarcerated for more than one year. (Because our main uptake sample is defined to exclude those incarcerated, we do not provide figures in Columns 2 and 3.). Relative to all of those eligible, set-aside recipients are more likely to be female and more likely to be black.
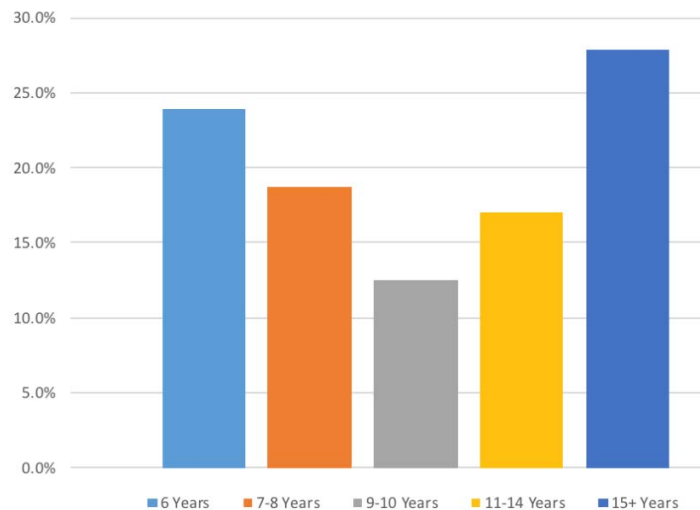
Table 2

	(1)	(2)	(3)
	All Set-Asides	Uptake Sample Set-Asides	Full Uptake Sample
Individual Characteristics			
Black	30.4%	39.8%	30.9%
White	65.6%	54.4%	67.0%
Other Race	4.0%	5.7%	2.1%
Mean age at sentencing	26.50	29.64	30.43
Male	60.9%	54.2%	64.9%
Employed When Became Eligible	66.0%	67.0%	64.6%
Employed Quarter Before Set-Aside	60.6%	53.8%	
Quarterly Wages When Became Eligible	$4,968	$5,013	$6,387
Quarterly Wages Quarter Before Set-Aside	$5,160	$3,463	
Case Characteristics			
Felony	44.2%	48.9%	30.1%
Crime Type			
Violent	14.9%	28.0%	26.2%
Drug	18.4%	18.2%	15.2%
Property/Economic	53.2%	39.8%	31.6%
Other	18.4%	18.2%	15.2%
Incarcerated	28.6%		
Incarcerated >1yr	1.9%		
Years from conviction to set-aside (Median)	9.6		
N (note: lower for some variables)	22,004	522	9,103

We report employment measures for both the quarter that the recipient became eligible for the set-aside and the quarter before actually receiving it, which may offer suggestive evidence about whether employment changes influence the timing of set-aside

applications.[97] Here, we see some notable differences between the uptake sample and the full sample of set-aside recipients: in the former, employment rates and wages are far lower just before the set-aside than they were at the time of eligibility, whereas in the full sample employment is modestly lower and average wages are actually higher. This difference may reflect the fact that the uptake sample is narrowly defined temporally, and the 5-year observation period for the uptake outcome (ending between 2009 and 2011 for all members of the sample) included a major economic crash; for this reason, these numbers may not actually suggest any broader relationship between employment setbacks and set-aside timing. We explore that potential relationship further below.

Figure 1. Years Elapsed Between Sentencing and Set-Aside

As Table 2, Column 1 shows, at the time of the set-aside, the median time elapsed since sentencing is 9.6 years.[98] In Figure 1, we show more detail on the distribution of time elapsed, focusing on those not sentenced to incarceration, all of whom would have become eligible after five years had elapsed. Figure 1 makes clear that there is wide variation in the time lag between eligibility and set-aside receipt. On the one hand, the single year with by far the highest set-aside rate is the first year after the five-year waiting period expires (Year 6 since sentencing, 25% of the total). This suggests a pent-up demand effect: some set-aside recipients likely anticipate becoming eligible ahead of time and apply more or less as soon as they can. The monthly rate, not shown in the graph, peaks at 4 months after becoming eligible; this too is consistent with the pent-up demand theory, since set-asides typically take a few months to process.[99] The annual rate

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[97] The wage data are discussed in more detail in Part IV.

[98] We do not show this figure in Column 2 because that sample was already constrained to those receiving set-asides within five years of becoming eligible.

[99] Interview with Michael Kiehne, Attorney, Michigan Legal Help, via phone (Feb. 14, 2018) (estimating that 4–6 months is typical for one large Michigan county); Interview with Chioke Mose-Telesford, Deputy Director of Workforce Development for the City of Detroit, via phone (Feb. 27, 2019) (explaining that Detroit's legal assistance program has recently reduced wait time to two months through concerted efforts to accelerate the process but that previously 160–190 days would have been typical). MSP advised us typically about 6 to 10 weeks elapse *after* MSP runs the criminal background check and returns a report deeming the individual eligible before the set-aside is granted by the judge.

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declines continuously thereafter (the last two bars in the graph are taller only because they represent periods of 5 or more years). Still, 44% of set-asides take place after more than five years of eligibility (Year 11 or beyond), and 28% take place after more than 10 years of eligibility (Year 15 or beyond).

In Figure 2, we present uptake rates by county for the six largest counties in Michigan (in descending order from the left), and for Michigan's other 77 counties combined. The figure reveals considerable local variation. Michigan's largest counties are primarily urban and suburban; five of the six (all but Kent) are in Southeastern Michigan, within an hour of Detroit. Nevertheless, Oakland County's rate is more than triple that of Genesee County, which it borders. All of the larger counties have higher uptake rates than the rest of the state combined. However, by 2014, courts in every county in Michigan had granted at least one set-aside, and at least ten had been granted in 77 out of 83 counties (with the remaining six all being among Michigan's least populous).

Figure 2. Uptake Rates by County

Many of the individual and case characteristics listed in Table 2 are correlated with one another, and with the county as well, which makes inference from this table fraught. It could be, for instance, that the higher uptake rate for black individuals is entirely explainable by county-to-county variation; uptake rates are in general lower in more rural, predominantly white Michigan counties. For this reason, in Table 3, we turn to regression analysis to test which individual and case characteristics remain predictive of set-aside uptake when other characteristics are held constant. We use logistic regression, a common approach for analyzing binary outcomes like set-aside receipt.[100] We present our results as odds ratios, which represents a *multiplier* of the odds of the set-aside occurring. An odds ratio greater than 1 means that the variable is associated with increased odds of set-aside receipt when other variables are held constant.[101]

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[100] Ordinary-least-squares regression produces substantively indistinguishable results when compared to the marginal effects from the logistic regressions.

[101] Odds have a technical definition, and while they are not the same as probability, higher odds do mean a higher probability, and when probabilities are quite low (as they are here), the odds ratio is a good

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	(1)	(2)	(3)	(4)	(5)
	5-Year Uptake			**Set-Aside Received in a Given Quarter**	
Male	0.663**	0.654**	0.671**	0.693**	0.692**
	(0.064)	(0.067)	(0.058)	(0.051)	(0.052)
Black	0.989	0.825	0.846[+]	0.936	0.948
	(0.109)	(0.098)	(0.086)	(0.079)	(0.083)
Age at conviction	0.988**	0.986**	0.986**	0.988**	0.990**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Felony	2.552**	2.129**	2.211**	2.262**	2.446**
	(0.284)	(0.262)	(0.227)	(0.194)	(0.219)
Violent Offense	0.464**	0.545**	0.525**	0.554**	0.571**
	(0.068)	(0.085)	(0.070)	(0.064)	(0.069)
Drug Offense	1.243	1.487**	1.306*	1.400**	1.468**
	(0.170)	(0.221)	(0.171)	(0.150)	(0.164)
Public Order/Other Offense	1.272*	1.297[+]	1.331*	1.387**	1.449**
	(0.153)	(0.175)	(0.150)	(0.128)	(0.140)
Incarcerated			0.553**	0.676**	0.672**
			(0.057)	(0.057)	(0.058)
Wage (Previous Quarter)				1.004	
				(0.007)	
Employed (Previous Quarter)				1.768**	
				(0.158)	
Past-year 20% wage loss					2.268**
					(0.196)
Conviction Year FE	✓	✓	✓	✓	✓
County FE		✓	✓	✓	✓
Years since conviction				✓	✓
No. of Observations	9,080	8,406	13,465	515,964	474,670

Notes: Results are odds ratios. Robust standard errors reported in parentheses. Columns 4 and 5 are panel regressions; observations are person-quarters and standard errors are clustered on the person. [+],*, ** represent significance at the 10%, 5%, and 1% level, respectively. Base category for the offense type is property/economic.

rough approximation of the probability ratio. So, for example, an odds ratio of 1.5 for variable X would suggest that other things equal, individuals with characteristic X have roughly 1.5 times the probability of receiving a set-aside compared to individuals without characteristic X.

In Columns 1 through 3 of Table 3, we study the determinants of whether an individual received a set-aside within 5 years of becoming eligible—the same outcome reported in Table 1.[102] We estimate the following equation:

$$\text{logit}(FiveYearUptake_i) = \alpha + \beta_1 Male_i + \beta_2 Black_i + \beta_3 ConvAge_i + \beta_4 Felony_i \\ + \gamma_c CrimeType_{ci} + \delta_y ConvYear_{yi} + \epsilon_i.$$

The outcome variable $FiveYearUptake$ is an indicator for whether the individual received a set-aside within five years of eligibility. $Male$ and $Black$ are indicators for the individual's reported gender and race; all non-black races are combined into one base category, which is 94% white. $ConvAge$ is the defendant's age in years at the time of conviction. $Felony$ is an indicator for whether the crime of conviction is a felony, and $CrimeType$ is a set of indicators for the type of crime: drugs, violent, other, with property/economic crimes omitted. $ConvYear$ is a set of indicators for the year of conviction. Column 1 presents our baseline results. In Column 2, we also control for county effects. In Column 3, we add individuals incarcerated for their set-aside eligible crime for up to one year to the sample, and add to the Column 2 specification an indicator for whether the individual was incarcerated.

The Column 1 regression results show that, other things equal, uptake rates are much higher among women and among people who were relatively young at the time of conviction. Table 2's suggestion of a higher uptake rate for black individuals completely disappears in the regressions, suggesting that it was due to other correlated characteristics. As for crime characteristics, the regressions all confirm that people with felony convictions have, other things equal, more than twice the odds of receiving set-asides. By contrast, people who have been incarcerated (even for short periods) and people convicted of violent crimes have only about half the odds of a set-aside as otherwise-similar individuals; this pattern was not apparent in the summary statistics in Table 2. A possible theory for these results is that felony convictions make individuals much more likely to apply (because felonies trigger a wide range of collateral consequences), but judges are less likely to grant set-asides in more serious or violent cases. Taken together, the pattern indicates that those most likely to get set-asides are people (especially women) with relatively minor felony convictions—i.e., felonies not resulting in jail time.

The regressions shown in Columns 4 and 5 also analyze set-aside receipt, but replace the five-year uptake rate with a different outcome variable: whether a set-aside is received in a particular quarter of the year. These analyses help us to understand the influences on set-aside probability that vary over time—and in particular, to ask whether individuals' immediate employment history (such as a recent job loss) drives the choice to apply for a set-aside. The dataset in these analyses has a panel structure, meaning it contains separate observations for each quarter for each individual in the dataset. The

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[102] For Columns 1 and 2 of Table 3, the sample is the same one used for the main uptake estimate in Table 1 (with constraints described in Section A), and for Column 3, we add cases with incarceration up to one year (the same sample used in Table 1, Col. 4).

sample includes most of the individuals included in the Column 3 sample, so long as we have wage data linked for them. The regression takes the following structure:

$$\text{logit}(SetAsideReceipt_{it}) = \alpha + \beta_1 Male_i + \beta_2 Black_i + \beta_3 ConvAge_i + \beta_4 Felony_i$$
$$+ \beta_4 Incarcerated_i + \gamma_c CrimeType_{ci} + \delta_y ConvYear_{yi}$$
$$+ \mu_s County_{si} + \theta EmplPrevQtr_{it} + \pi WagePrevQtr_{it}$$
$$+ \rho TimeElapsed_{it} + \epsilon_{it}.$$

The specifications in Column 4 and 5 are similar to the previous columns, but also include variables that change over time for a given individual. Column 4 includes whether an individual was employed in the previous quarter (that is, whether *any* wage is reported) and her reported wages the previous quarter, as well as the number of years that have elapsed since the conviction. We focus on the *previous* quarter in part because there is likely to be at least a one-quarter lag between set-aside applications and receipt, and we are interested in understanding the motivation for the application. In addition, lagging this variable avoids the problem of reverse causation: wages and employment in the quarter of a set-aside could be affected by the set-aside itself.

In Column 5, we focus more specifically on whether the individual has experienced a recent employment setback, which we proxy for with an indicator for whether the individual's wages have dropped by 20% in a year as of the preceding quarter. Our framework allows us to assess the effects of a wage loss in a way that is not confounded by the recession timing. The whole sample is affected by the same economic trends, and the controls for year of conviction and years since conviction (which together produce the calendar year) also absorb those effects. The regression focuses on whether, among individuals subjected to the same broader economic conditions, personal employment setbacks affect set-aside probability.

We find, on the one hand, that being employed is a very strong positive predictor of set-aside receipt in a particular quarter, increasing the odds by a factor of 1.78 (Col. 4). On the other hand, a recent wage loss is an even stronger predictor, increasing the odds of set-aside receipt by a factor of 2.29 (Col. 5). Although these results seem in tension, they each have plausible explanations: employed people may generally be more likely to have the resources, information, and money to pursue a set-aside, but people who have experienced a recent employment setback may have more motivation to pursue a new job. These results also help to inform our interpretation of the wage and employment *effects* of set-asides; we refer back to them in Part IV.

C. *What Explains the Uptake Gap?*

Given the many life disadvantages that come with a criminal record, one might expect that most people who have the chance to get rid of their records would jump at that chance. Yet strikingly few do so. Why? Our quantitative data shed a little bit of light this question, but cannot really answer it. To complement the data, we sought out the insights of experienced Michigan set-aside lawyers and other advocates for people with records, many of whom are actively involved with outreach efforts (such as "expungement fairs")

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to try to encourage set-aside applications.[103]   we sought to identify the underlying mechanisms of limited uptake. Our discussions pointed clearly to a set of likely explanations.

1. Lack of information. Every advocate we spoke to mentioned this concern, and many thought it was the single most important uptake barrier. Most people with records, even if they are eligible for set-asides, lack the information they need to pursue them. As Prof. Tracey Brame, who runs a law clinic that handles set-asides, explained: "A lot of people have absolutely no idea that they can do this."[104] Many do not know that the set-aside law exists at all. Others may have a vague idea that set-asides are possible, but do not know that they are eligible, or do not know what they need to do to pursue it (or how to find out). The law is complicated and not easy for a layperson to read.[105] Many people do not understand their own records—for example, they may not know that a traffic offense they pled to was a criminal conviction. Several advocates told us that when they run set-aside fairs, even though their materials promoting the fairs identify the key eligibility requirements, a substantial majority of those who turn up learn when they are there that they are not eligible, and walk out frustrated. It is certainly plausible that there are conversely many people who *are* eligible who assume that they are not. After all, the great majority of people with records are *not* eligible for set-asides, and in communities where many people have records, to the extent that people know about the set-aside law at all, frustration with the its stringent restrictions is common. [106] The shared impression that "nobody is eligible" may be entrenched.

2. Administrative hassle and time constraints. Obtaining a set-aside requires a nontrivial amount of organization, effort, and time. The process is drawn out, and requires patience and ongoing resolve. A would-be applicant, after overcoming the basic informational hurdles described above, must track down the official application form, which is available on the website of the State Courts Administrative Office. Occupying two-thirds of the second page of that form, in fairly small font, is the following set of instructions:

> 1. Determine whether you are eligible to apply to have your conviction set aside according to MCL 780.621. You must complete a separate application for each conviction if you are applying to have more than one conviction set aside [as is allowed under the current statute].
>
> 2. Find out the exact date of conviction and the charge from the court. Get a certified copy of the adjudication and attach it to your application.

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[103] *See* Interview with Miriam Aukerman, ACLU of Mich., by phone (Feb. 23, 2019); Interview with Michael Kiehne, *supra* note 99; Email from Josh Hoe, Co-Chair, Policy and Education Committee, Nation Outside, to recipient, title, org. (Feb. 13, 2019) (on file with authors); Interview with Chioke Mose-Telesford, *supra* note 99; Interview with [Name Redacted], Legal Aid Practitioner, by phone (Feb. 22, 2019); Email from John Shea, private practioner in Ann Arbor, MI, to Sonja Starr (Feb. 15, 2019) (on file with authors); Email from Kim Thomas, *supra* note 83; Interview with Tracey Brame, *supra* note 91.

[104] Interview with Tracey Brame, *supra* note 91.

[105] This is particularly so given that the target population includes many people with significant socioeconomic challenges, including with literacy. Interview with Michael Kiehne, *supra* note 99.

[106] *See* Email from Josh Hoe, *supra* note 103; Interview with Chioke Mose-Telesford, *supra* note 99.

3. Swear to the truth of the statements in this application and then sign it in the presence of the court clerk or a notary public.

4. Make four copies of all attachments and this application. Take all copies to the court clerk.

5. Depending on local practice, the clerk of the court may set a hearing date at the time of filing. If a hearing date is set at the time of filing, the clerk of the court will complete the Notice of Hearing.

6. Go to the local law enforcement agency for a fingerprint card and get fingerprinted on the applicant card (RI-8) . . . .

7. Make out a money order or check to the State of Michigan for the application…

8. Mail a copy of the application packet, application fee, and the fingerprint card to the Michigan State Police . . . .

9. Mail a copy of the application packet to the Attorney General of the State of Michigan . . . .

10. Mail a copy of the application packet to the correct prosecuting official where the conviction occurred (county, city, or township) . . . .

11. On both copies of the application, fill in the Proof of Service on the back of the form. After you fill out and sign the Proof of Service, mail or take one of the remaining application packets with the completed Proof of Service to the court. Keep the other copy for your records . . . .

It is easy to see how this list would be daunting and potentially confusing to potential applicants (and these steps do not end the process; a court hearing follows, usually many weeks later). Advocates confirm that this is so; everyone we interviewed independently mentioned the administrative burdens facing applicants. As many pointed out, people with records are usually struggling with a variety of life challenges. Taking time away from work and childcare responsibilities to go to a police station to get fingerprinted, to make several separate trips to a courthouse, to find a notary, and to mail all these materials may be simply impossible, or at least difficult enough to be strongly discouraging.[107] This is especially so if the applicant does not live near the court that convicted her, where her set-aside must be processed. One very experienced set-aside lawyer at a public interest organization pointed out that many of her clients are overwhelmed already with paperwork, such as that associated with receiving public benefits.[108]

3. Fees. Every advocate we spoke to emphasized the barriers imposed by fees and other associated costs. The $50 application fee cannot be waived, and it is not the only cost. Michael Kiehne of Michigan Legal Help and Michigan Works estimated that the

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[107] This is consistent with the larger access-to-justice literature. The use of procedures that requires travel to courthouses, for instance, can dramatically reduce participation. *See, e.g.*, Maximilian A. Bulinski & J.J. Prescott, *Online Case Resolution Systems: Enhancing Access, Fairness, Accuracy, and Efficiency*, 21 MICH. J. RACE & L. 205 (2016); J.J. Prescott, *Assessing Access-to-Justice Outreach Strategies*, 174 J. INST. & THEORETICAL ECON. 34 (2018).

[108] Interview with [Name Redacted], *supra* note 103.

total cost usually is close to $100, including fingerprinting ($10-$20 at most local police stations), notary fees (up to $10), obtaining a certified record of conviction ($10-$12 if it is only one page), and photocopies.[109] This does not include the costs of transportation and possible loss of wages for time taken off work; Chioke Mose-Telesford, of Detroit's Project Clean Slate, emphasized that especially in a city with poor public transit and low car ownership, transportation was a serious hurdle.[110] Especially for applicants living in poverty, these accumulated costs pose a serious financial barrier—a point consistent with our finding that the unemployed are much less likely to apply. In principle, one could see the cost as an investment in future wages; in Part III, we document average expected gains that very quickly would cover the cost. But those without cash on hand may not have the liquidity or ability to make such an investment or may be reluctant to do so when the long-term benefits are speculative.

4. Distrust and fear of the criminal justice system. For many potential set-aside applicants, their prior experience with the criminal justice system may well have been among the worst experiences of their lives. According to several advocates, this often amplifies the daunting nature of the set-aside process; individuals with records, at least five years removed from their own justice processes, may be strongly averse to returning to court or to a police station for any reason. This may be especially so if they expect the prosecutor or a crime victim to come to court to contest the set-aside, which the law allows (and which sometimes happens). In addition to fear of the process itself, potential applicants may be pessimistic about the outcome, even though most set-aside applications are in fact granted. As Kiehne explained:

> Expungements are discretionary, and when you let people know that, they tend to be pessimistic. We always try to tell people that many judges are excited to grant these . . . . But their interaction with the law enforcement system has been all very negative—for them and their family and friends as well—so it is hard to get this information through.[111]

A potential applicant who fears that the application will be denied may be particularly reluctant to undertake the effort, cost, and stress of pursuing a set-aside.

5. Lack of access to counsel. Some of the obstacles above could be overcome or rendered less daunting with legal assistance. Although set-aside applications can be filed *pro se*, the process is far less difficult to navigate for an experienced attorney. Unfortunately, often, none is available. Criminal defense lawyers are typically long since out of touch with their clients by the time that they become eligible, and in most cases they do not advise their clients about the possible prospect of a set-aside five years in advance, either. Paid attorneys are out of reach for most people with records. And legal aid or pro bono attorneys have often not been available. Although there have been some recent improvements on that front (which we discuss below), one legal aid lawyer with extensive set-aside experience told us that many legal aid organizations are simply

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[109] Interview with Michael Kiehne, *supra* note 99.

[110] Interview with Chioke Mose-Telesford, *supra* note 99.

[111] Interview with Michael Kiehne, *supra* note 99.

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overwhelmed with the many other needs they are asked to serve, which makes it "hard to ask them to take on a whole new area."[112]

6. *Insufficient motivation to remove conviction.* Notwithstanding the various hardships that tend to be associated with criminal records, not everybody with a record is affected the same way by it. Virtually everyone with a criminal record would presumably prefer not to have it, but not everybody necessarily experiences the record as a significant enough burden to motivate them to take on the above-described investment of time, money, and energy. This may be especially true for people with minor convictions—a theory supported by our finding that those with misdemeanors are less than half as likely to get set-asides as those with felonies. It seems impossible that judges would on average be less willing to *grant* set-asides for misdemeanors, so the only plausible explanation is that people with misdemeanors are less likely to apply. This is intuitively sensible. While misdemeanor records can certainly have nontrivial consequences,[113] many collateral legal consequences apply only to felonies, and many employment applications ask only about felony records. It is not surprising that those with felonies appear to be particularly motivated to pursue relief. In addition, motivation may affect the timing of set-asides; as Tracey Brame explained: "A lot of people don't prioritize it until [the conviction] is a problem," such as when they are trying to pursue an occupational license.[114]

Over the past 15 years, as the challenges facing people with records have drawn more attention nationally and in Michigan, there have been some significant efforts in Michigan to increase support for people seeking set-asides. The City of Detroit, for example, recently adopted an initiative called Project Clean Slate to support set-aside efforts, and Michigan Works, a statewide workforce development program, has recently begun to get involved in the issue. Michigan Legal Help, a nonprofit that provides DIY tools to pro se litigants, has developed an online tool that allows applicants to assess their eligibility and to fill out the application form, although they must still go through the various steps to file it.

Many of these organizations have sponsored expungement fairs, which have become perhaps the central outreach tool in this space. Expungement fairs are designed at a minimum to provide information, and many also provide concrete assistance: applicants have been encouraged to fill out applications on site, and some fairs have had sheriffs on site to do the fingerprinting, as well as notaries. Depending on the sponsoring organization's funding, these fairs have sometimes been able to defray the costs for applicants—for example, getting the fingerprinting done for free and paying for the certified record of conviction. Expungement fairs in Detroit have reportedly been quite large, on the order of a thousand attendees, although most attendees have proven to be ineligible. Fairs elsewhere in the state have reportedly not had anywhere close to this kind of attendance.

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[112] Interview with [Name Redacted], *supra* note 103.

[113] Charlie Gerstein & J.J. Prescott, *Process Costs and Police Discretion*, 128 HARV. L. REV. F. 268 (2015); Issa Kohler-Hausmann, *Misdemeanor Justice: Control Without Conviction*, 119 AM. J. SOC. 351 (2013).

[114] Interview with Tracey Brame, *supra* note 91.

Miriam Aukerman, who worked on set-aside cases at Western Michigan Legal Aid until 2010, told us:

> When I started doing this work back in 2003 or 2004, nobody was doing it. People didn't think it was important. When I left in 2010, it was a thing. Legal services offices had started doing it; there were self-help packets available…. Expungement fairs were starting to happen. Advocacy organizations started spreading the word.

Aukerman's recollection is borne out by data. In 2010, the last full year for which we have complete data, the number of known-date set-asides granted in Michigan was 2044, which is 64% higher than the number in 2004 (1224). MSP reports to us that 2594 set-asides were granted in 2017; this number (although down slightly from 2016) represents another 27% growth since 2010. These changes suggest that efforts to help people get past the various barriers to access to set-asides can successfully encourage applications.

That said, one should not be too sanguine; even if uptake doubled from what we measured (for five-year periods ending between 2009 and 2011), it would still only be 13%. Set-aside lawyers emphasized to us the frustration that many of their clients feel with the process, even *with* legal help. Rather than expect a massive influx of aid to help people navigate the process, substantially closing the uptake gap may require legal changes to simplify that process. We return to this point in the Conclusion.

Finally, it bears noting that, although our interviews focused mainly on the problem of low uptake among *eligible* offenders, every advocate that we spoke to also emphasized the stringency of the eligibility requirements, which in their view exclude a great many worthy candidates. Reforms in 2011 and 2015 slightly softened the requirements regarding the number of convictions, but they are still stringent and in many ways quite arbitrary. Anyone with two felony convictions is still excluded, even if they are low-level felonies and even if they are two counts arising from the same incident. Inexplicably, people with three misdemeanors cannot seek set-asides at all, even though people with one felony plus two misdemeanors can seek to set aside the felony.[115] Traffic misdemeanors cannot be set aside themselves but count toward the limit and thus may disqualify an individual from setting aside another conviction. These offenses are very common in Michigan, accounting for more than half of all misdemeanor convictions.[116] Petty misdemeanors, including violations of hunting and fishing laws and dog-leash laws, likewise count toward the limit. Meanwhile, the waiting period now runs five years from the completion of all components of the sentence, which means that people with lengthy probation or parole terms might actually have to wait out eight or ten recidivism-free years in the community, not five as was required under the old law.

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[115] MICH. COMP. LAWS § 780.621. This seems like a clear drafting error, but it is apparently enforced. Interview with Michael Kiehne, *supra* note 99.

[116] MICH. COURTS, *supra* note 95. An experienced set-aside attorney told us that it is common for her clients to believe they are eligible for a set-aside because they do not understand that a prior traffic offense was a misdemeanor and because it does not come up in ICHAT, the free background-check tool that some use to check their eligibility (but does ultimately come up in the official background check). Interview with [Name Redacted], *supra* note 103.

All of these restrictions mean that the low uptake rate we estimated is even starker when viewed in context: it is a very small fraction of a very small fraction. For the past decade about two thousand set asides per year have been granted in Michigan. Meanwhile, each year the Michigan state courts add about 300,000 new criminal convictions. On balance, the population of people living with criminal records is continuing to grow quickly; the set-aside law is like a bucket removing water from an ever-rising ocean.[117]

III. RECIDIVISM OUTCOMES

Set-asides are not common—but what happens when they do occur? Are there significant public safety costs—i.e., does expungement increase recidivism risk, or conceal substantial risks from the public? Here, our data paint a sunnier picture. Expungement opponents routinely cite public safety concerns as their primary objection, arguing that the public—especially people with particular interests at stake, such as employers and landlords—have a right to know when people have records because of their heightened risk of committing future crimes. To address this concern, in Section A, we estimate recidivism rates for set-aside recipients, finding that they are strikingly low. In Section B, we consider the interpretation and policy implications of our findings. We draw on criminological literature to offer some informed speculation on what the *effect* of set-asides on crime might be—a question our data do not allow us to answer directly.

A. Recidivism Among Set-Aside Recipients

In Table 4, we report two-year and five-year re-arrest and reconviction rates for set-aside recipients, starting from the date of receipt of the set-aside. We rely for this analysis on an MSP criminal history dataset that ends on December 6, 2012. To allow a sufficient follow-up period, the analysis sample accordingly includes nearly all persons receiving set-asides in Michigan with known dates up through December 6, 2010 (for the two-year rates) and December 6, 2007 (for the five-year rates). The only individuals dropped are 232 persons identified in the data as having out-of-state driver's licenses, to reduce any bias that might result from unobserved out-of-state recidivism. The resulting numbers of observations for our analysis are 20,955 for the two-year sample and 15,256 for the five-year sample.

We analyze the criminal histories of the full universe of set-aside recipients during these periods: these are not really "estimates," and there is no sampling error in the results that we report for these populations. We need not worry about the representativeness of the sample, and most of the data concerns raised with regard to the "eligible" sample discussed in Part II do not arise here. Our only significant limitation is our inability to observe out-of-state and federal arrests and convictions, which may mean that we slightly understate true recidivism rates. We do not believe this omission has a large effect, however, for reasons already discussed in detail in Part II: federal arrests and

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[117] *See* Riley, *supra* note 95 (estimating that tens of thousands of Michigan residents receive first-time felony convictions each year).

convictions are rare, while interstate moves and interstate travel to commit crimes are not very common either.[118]

Table 4. Re-Arrest and Reconviction Rates for Set-Aside Recipients

	2-Yr Arr.	2-Yr Conv.	5-Yr Arr.	5-Yr Conv.
A. Full Sample				
Overall Arrest/Conviction Rates	3.4%	1.8%	7.1%	4.2%
Violent Arrest/Conviction Rates	1.0%	0.2%	2.6%	0.6%
Felony Arrest/Conviction Rates	1.2%	0.3%	2.7%	1.0%
B. Subsamples (Overall Rate Unless Specified)				
Set-Aside <6 Years from Conv./Release	3.8%	2.1%	8.1%	4.9%
Set-Aside 10-11 Years from Conv./Release	2.6%	1.6%	6.1%	3.4%
Set-Aside Conviction Was Felony	4.0%	1.9%	8.1%	4.6%
Set-Aside Conviction Was Misdemeanor	3.1%	1.8%	6.4%	3.8%
Incarcerated for Set-Aside Offense	3.2%	1.7%	6.7%	3.9%
Not Incarcerated for Set-Aside Offense	3.5%	1.8%	7.2%	4.3%
Set-Aside Offense Was Violent	4.4%	2.2%	8.4%	4.4%
Set-Aside Offense Was Violent: Violent Rate	1.6%	0.3%	4.0%	0.8%

In Row 1 of Table 4, Panel A, we report the *overall* 2-year and 5-year re-arrest and reconviction rates, while in the remaining rows we give specific rates for certain types of crime that may be of particular public concern: felony offenses and violent offenses. All of these rates paint a consistent picture: recidivism among set-aside recipients is low. Overall, 3.4% are re-arrested and 1.8% are reconvicted for crimes within two years; 7.1% are re-arrested and 4.2% are reconvicted within five years. The numbers are much lower yet when we focus on the types of crimes that worry people most. For example, within five years, only 2.6% are re-arrested and 0.6% are reconvicted for violent crimes; 2.7% are re-arrested and 1% are reconvicted for felonies.

These rates are very low—much lower than those found in most studies of criminal recidivism. Indeed, they suggest that set-aside recipients pose a lower crime risk than the general population of Michigan as a whole. Although no general-population data are available that directly parallel the figures in Table 4, annual arrest figures are publicly reported. For comparison purposes, in the two-year period from 2009–2010, Michigan

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[118] Applying the assumption used in Part II that 1.8% of individuals move per year and 4% of crime is committed across state borders, we expect we might be missing about 5–6% of the rearrests and reconvictions for our two-year outcome period and perhaps 7–8% in the five-year period (when more people have had time to move). Given the very low rates we report in Table 4, the effect of this problem is negligible in percentage-point terms.

police made about 6.6 arrests per 100 adults in the population.[119] In contrast, using cases from approximately the same time period, within two years of receiving a set-aside, there are only 4.7 arrests per 100 set-aside recipients.[120] This comparison is particularly striking given that most set-aside recipients (like most people with records generally) face socioeconomic disadvantages that are typically associated with elevated crime risk relative to the general population.

In Panel B of Table 4, we replicate the main results from Panel A, Row 1 (overall arrest and reconviction rates) for subsets of the set-aside population of particular interest. In Row 1, we look at individuals who received their set-asides early, in their first year of eligibility (by the end of Year 6 since sentencing or release). The waiting period for set-asides in Michigan is five years, but the actual average elapsed time is nearly 10 years after sentencing or release. Because recidivism risk declines over time, this delay should be expected to reduce their rates. But policymakers might wonder what would happen if people *didn't* wait those extra years—which would be the consequence of a policy that made set-asides automatic after five years. The Table 4.B, Row 1 estimate is designed to inform the question whether five years is enough—and it suggests that it is. Recidivism in all columns is only very slightly higher than the main-sample numbers, suggesting that this subgroup remains low-risk relative to the general public.

Meanwhile, Table 4.B, Row 2 shows re-arrest rates for people who receive set-asides in Years 11, 12, or 13 after sentencing or release—a good proxy for the effect of giving set-asides after 10 clean years, as Pennsylvania's new law does automatically for minor offenders. These recidivism rates are (unsurprisingly given the individuals' long clean records) even lower than average set-aside recipient rates. All in all, though, the differences among these different cohorts are quite small, suggesting that these waiting-period differences might not matter very much.

In Rows 3 and 4 of Panel 4.B, we show recidivism numbers for those whose set-aside conviction was a felony and a misdemeanor, respectively. Recidivism rates are low for both groups, although slightly higher for the felony group. In Rows 5 and 6 respectively, we compare set-aside recipients who did and did not serve any time behind bars. One might expect recidivism rates for the formerly incarcerated to be higher for two reasons: they have more serious prior convictions, and they may face additional socioeconomic disadvantages arising from the period of incarceration. These influences might be

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[119] Arrest figures are summed across "total arrests" for persons 18 and up in MICH. STATE POLICE, 2009 STATEWIDE ARREST TOTALS (2010), https://www.michigan.gov/documents/msp/2009Annual_StatewideArrests_332334_7.pdf, and MICH. STATE POLICE, 2010 STATEWIDE ARREST TOTALS (2011), https://www.michigan.gov/documents/msp/2010_Annual_StatewideArrests_358704_7.pdf. Population figures come from the 2010 Census. *QuickFacts: Michigan*, U.S. CENSUS BUREAU, https://www.census.gov/quickfacts/fact/table/mi/POP010210 (last visited Mar. 9, 2019).

[120] This number is calculated for the cohort of those receiving set-asides between December 2006 and December 2010, running for two years from each individual's set-aside date, and excluding those known to have out-of-state licenses; this is the most recent complete set-aside receiving cohort for which we have two years of subsequent data. Total arrests per capita over two years are higher than the two-year arrest rates for the same population (like those reported in Table 4), because some people are arrested more than once. There are no public data on the number of unique individuals arrested in Michigan in any given period, so the number of arrests is the best metric to use for comparison. We do not have a similar authoritative source for number of convictions statewide.

counterbalanced by the fact that during the five-year period we observe them, the formerly incarcerated persons are farther beyond their conviction dates (because the clock runs from release for them) and are older on average. On balance, we observe very slightly lower recidivism rates for those who have been incarcerated.

Finally, in Rows 7 and 8 of Panel 4.B, we show recidivism numbers for people who have had violent offense convictions set aside. In Row 7, we show that the overall re-arrest and reconviction numbers for this subsample are slightly higher than the average for the entire sample, although still quite low. In Row 8, we report the re-arrest and reconviction numbers for violent crimes alone (as we did in Panel 4.A, Row 3). This comparison addresses the specific fear that employers, landlords, and policymakers might have about people with a past violent offense: that they will commit another violent crime. Although the rates of violent reoffenses are very slightly higher in the subsample with violent crime convictions, they are still extremely low in absolute terms. Only 0.3% of those who have had a violent offense set aside are reconvicted of another violent offense within 2 years, and only 0.8% within five years.

B. Interpretation and Implications

Our recidivism analysis should, at the very least, dispel any notion that current set-aside recipients pose any particular crime risk. Employers and landlords in Michigan should rest assured that set-asides are unlikely to be keeping information from them that they need to protect themselves. Moreover, the numbers provide reason for optimism for other states considering expungement laws; it would be surprising if the story were much different elsewhere, at least assuming roughly similar waiting periods and eligibility restrictions.

But what are the broader implications of this analysis for expungement policy, for Michigan and beyond? In particular, can we say anything about the effects of broadening the availability of expungement—either procedurally (for example, by rendering it automatic) or substantively (by loosening eligibility requirements)? Set-aside recipients are, again, a highly selected sample. Primarily, they are self-selected—they had the motivation to apply, the belief that their case could be convincing to a judge, and the energy, money, and organization to complete all of the application process. They are also filtered by judges' discretion, and judges presumably look for candidates who seem to them to be at low risk of future crime. So we cannot assume that subsequent crime rates for people who are *not* selected in that way would be as low. And if they were not as low, would the public safety objection be legitimate?

To address this question, we must consider two separate influences on post-expungement crime rates. The first is *baseline risk*: the risk, absent expungement, that would be posed by the same individuals. The second is the *causal effect of expungement* on these individuals' crime rates. Our analysis in Section A does not allow us to disentangle these two plausible explanations for set-aside recipients' low recidivism rates.[121] Both may play a role, and both theories find strong support in criminological literature more broadly.

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[121] We cannot meaningfully compare set-aside recipients before and after set-asides because, per the eligibility rules, recipients will necessarily have no subsequent convictions as of the time of the set-aside.

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As to baseline risk, a key finding of this research is that most people who commit crimes do not continue to do so forever. Within the large criminological literature on recidivism and patterns of desistance from crime, a small but important subset focuses on the extent to which older criminal records remain usefully predictive of future offending.[122] In particular, it asks: when people have remained crime-free for a given period of time, at what point do they become no riskier, or at least not notably riskier, than the general population or people without criminal records? Alfred Blumstein and Kiminori Nakamura have labeled this concept "redemption"—suggesting that the passage of some number of recidivism-free years should be understood to "redeem" the individual in the eyes of society.[123] Estimated times-to-redemption have varied across samples, crime types, age cohorts, and methods, but have usually been in the range of four to ten years, at least when the comparison point is the same age cohort within the general population.[124]

One could easily interpret the low recidivism rates we find in Section A—and the favorable comparisons to general-population arrest rates—as indications of redemption. Set-aside recipients in our study all went at least five years without a subsequent conviction; the median is nearly ten years. Moreover, in addition to the "redemption" effects of the passage of time, many set-aside recipients are probably quite low-risk even before such time passes. In addition to the subtler selection factors discussed above (motivation, ability to impress a judge, etc.), they are all first-time offenders—and recidivism studies consistently find that the length of one's existing criminal record is a strong predictor of subsequent recidivism.[125] If the explanation for the low rates we observe is simply low baseline risk, then for offenders with this set of underlying characteristics, five years is plenty (likely more than enough) to achieve "redemption" in the sense of posing risk no higher than the general population. And if they are redeemed in the Blumstein and Nakamura sense, choosing *not* to expunge their records comes at a cost to them with no countervailing public-safety justification.

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[122] ALFRED BLUMSTEIN & KIMINORI NAKAMURA, EXTENSION OF CURRENT ESTIMATES OF REDEMPTION TIMES: ROBUSTNESS TESTING, OUT-OF-STATE ARRESTS, AND RACIAL DIFFERENCES (2012), https://www. ncjrs.gov/pdffiles1/nij/grants/240100.pdf; Alfred Blumstein & Kiminori Nakamura, *Redemption in the Presence of Widespread Criminal Background Checks*, 47 CRIMINOLOGY 327 (2009); Shawn Bushway et al., *The Predictive Value of Criminal Background Checks: Do Age and Criminal History Affect Time to Redemption?*, 49 CRIMINOLOGY 27 (2011); Megan C. Kurlychek, *Enduring Risk: Old Criminal Records and Prediction of Future Criminal Involvement*, 53 CRIME & DELINQ. 64 (2007); Megan C. Kurlychek et al., *Scarlet Letters and Recidivism: Does and Old Criminal Record Predict Future Offending*, 5 CRIMINOLOGY & PUB. POL'Y 483 (2006).

[123] Blumstein & Nakamura, *supra* note 122. This religiously derived language has potential connotations (for example, the idea that those with records are "fallen") that are probably not intended by any of the scholars using it and that we do not endorse; still, we use the term here in deference to the leading work.

[124] *See generally id.* If the comparison point is only people who have *no* prior record (instead of the general population, which includes people with records), then redemption studies tend to find that the risk posed by people with records never converges completely with the comparison group; however, it does eventually come close enough that a reasonable observable conclude that the old record provides no useful or actionable information. *Id.*

[125] *See, e.g.*, U.S. SENTENCING COMM'N, MEASURING RECIDIVISM: THE CRIMINAL HISTORY COMPUTATION OF THE FEDERAL SENTENCING GUIDELINES (2004), https://www.ussc.gov/sites/default/ files/pdf/research-and-publications/research-publications/2004/200405_Recidivism_Criminal_History.pdf.

It is, however, also possible that receiving a set-aside reduced recipients' crime risk below their previous baseline. We cannot test this theory, but there are good reasons in criminological literature to believe that set-asides *do* reduce crime risk.[126] Recidivism-related benefits of set-asides are likely to be primarily mediated by their effect on employment and wage levels—which we assess below in Part IV. Unemployment is a moderately strong predictor of recidivism.[127] Likewise, higher weekly wages significantly reduce recidivism.[128] In a study of female offenders, Holtfreter et al. found that "poverty status increases the odds of rearrest by a factor of 4.6," and is a stronger predictor of re-arrest than is a recidivism risk index commonly used in corrections.[129] Homeless offenders are especially likely to recidivate,[130] as are less educated offenders.[131] To the extent that criminal records limit access to housing, student loans, employment, and wage increases, expungement should reduce recidivism by mitigating each of these socioeconomic contributors to criminal behavior.

Another possibility is that expungement reduces recidivism by alleviating social exclusion associated with criminal records. Former offenders face significant social stigma,[132] which is exacerbated by the existence of public records that many states make

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[126] Indeed, although the empirical evidence has been lacking, this has often been part of the policy case made for expungement laws. *See, e.g.*, Jeremy Travis et al., *Prisoner Reentry: Issues for Practice and Policy*, CRIM. JUST., Spring 2002, at 12, 17; Andrew von Hirsch & Martin Wasik, *Civil Disqualifications Attending Conviction: A Suggested Conceptual Framework*, 56 CAMBRIDGE L.J. 599, 605 (1997); AM. BAR ASSOC., *Collateral Standards and the Discretionary Disqualification of Convicted Persons*, *in* ABA STANDARDS FOR CRIMINAL JUSTICE 13 (3d ed. 2004), https://www.americanbar.org/content/dam/ aba/publishing/criminal_justice_section_newsletter/crimjust_standards_collateralsanctionwithcommentary. pdf; Deborah N. Archer & Kele S. Williams, *Making America the Land of Second Chances*, 30 N.Y.U. REV. L. & SOC. CHANGE 527, 527–30 (2006).

[127] *See, e.g.*, CHI. MAYORAL POLICY CAUCUS, REBUILDING LIVES. RESTORING HOPE. STRENGTHENING COMMUNITIES. BREAKING THE CYCLE OF INCARCERATION AND BUILDING BRIGHTER FUTURES IN CHICAGO: FINAL REPORT OF THE MAYORAL POLICY CAUCUS ON PRISONER REENTRY (2006), https://www.mayors innovation.org/images/uploads/pdf/Article7_HC.pdf; PAUL GENDREAU ET AL., CORR. SERV. OF CAN., CASE NEEDS REVIEW: EMPLOYMENT DOMAIN, FORUM ON CORRECTIONS RESEARCH (2001), https://www.csc-scc.gc.ca/research/092/r90_e.pdf; JOAN PETERSILIA, WHEN PRISONERS COME HOME: PAROLE AND PRISONER REENTRY (2003); JEREMY TRAVIS ET AL., URBAN INST., FROM PRISON TO HOME: THE DIMENSIONS AND CONSEQUENCES OF PRISONER REENTRY (2001), https://webarchive.urban.org/ UploadedPDF/from_prison_to_home.pdf.; Nicholas Freudenberg et al., *Coming Home from Jail: The Social and Health Consequences of Community Reentry for Women, Male Adolescents, and Their Families and Communities*, 95 AM. J. PUB. HEALTH 1725, 1729 (2005).

[128] Samuel L. Myers, *Estimating the Economic Model of Crime: Employment Versus Punishment Effects*, 98 Q.J. ECON. 157 (1983).

[129] Kristy Holtfreter et al., *Poverty, State Capital, and Recidivism Among Women Offenders*, 3 CRIMINOLOGY & PUB. POL'Y 185, 189, 198 (2004).

[130] Stephen Metraux & Dennis P. Culhane, *Homeless Shelter Use and Reincarceration Following Prison Release: Assessing the Risk*, 3 CRIMINOLOGY & PUB. POL'Y 139 (2004).

[131] John Nuttall et al., *The Effect of Earning a GED on Recidivism Rates*, J. CORRECTIONAL EDUC., Sep. 2003, at 90 (2003); STEPHEN J. STEURER & LINDA G. SMITH, CORR. EDUC. ASS'N, EDUCATION REDUCES CRIME: A THREE-STATE RECIDIVISM STUDY (2003), https://files.eric.ed.gov/fulltext/ ED478452.pdf; Kristen M. Zgoba et al., *The Influence of GED Obtainment on Inmate Release Outcome*, 35 CRIM. JUST. & BEHAV. 375 (2008).

[132] Marc A. Franklin & Diane Johnsen, *Expunging Criminal Records: Concealment and Dishonesty in an Open Society*, 9 HOFSTRA L. REV. 733, 737 (1981).

available on the Internet.[133] Sociologists and criminologists have long argued that social stigma and exclusion can contribute to criminal recidivism.[134] Although little empirical support has been found for the strongest claims of 1970s-era labeling theorists (e.g., that labeling made criminal justice interventions actually counterproductive on net), stigma and exclusion may still be an important part of the dynamic underlying recidivism.[135] All in all, our evidence of the public safety benefits of set-asides is only suggestive but highly plausible, given what we know about recidivism patterns.

Moreover, *no* similarly plausible empirical support exists for the opposite claim: that sealing records increases recidivism risk. To our knowledge, those raising this objection have never presented evidence supporting it and its rationale is not obvious. The intuition seems to be that those who know a person's criminal background can take steps to protect themselves—for example, by choosing not to hire her. And as the empirical evidence discussed in Part I.A indicates, many employers do exactly that. But while such a step might be in an individual employer or landlord's interest, it is hard to see how it would be in society's interest, given that lack of employment, lack of stable housing, and social stigma are all risk factors for recidivism. Even if a person with a record is at an elevated crime risk, the employer who declines to hire her simply shifts risks to others, or even amplifies those risks. No empirical research suggests that the collective effect of decisions not to hire or rent to people with records makes society safer. Notably, the purported public-safety case against expungement follows the same logic as the purported public-safety case for sex offender registries with public notification requirements—yet empirical research has shown that these laws appear to *increase* recidivism.[136]

With that in mind, let's return to the hypothetical raised at the beginning of this Section. Suppose a state were considering making expungement more broadly available: making it automatic for some subset of offenders, shortening the waiting period, or loosening eligibility requirements. If new recipients have a higher baseline risk than current recipients do, their post-set-aside recidivism rates may also be higher—maybe substantially so. But from a policy perspective, this point is a red herring. That higher recidivism is not *caused* by the expungement or by the law change that allowed it; it is simply a baseline risk, which would exist with or without expungements. A higher recidivism rate is not relevant to the cost-benefit analysis unless it is an effect of the law change or the resulting set-asides. But there is no credible evidence to indicate that that risk might *grow* because of the expanded availability of set-asides. Meanwhile, criminological research provides many good reason to believe that, if anything, recidivism would decline.

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[133] Megan C. Kurlychek, *Enduring Risk: Old Criminal Records and Prediction of Future Criminal Involvement*, 53 CRIME & DELINQ. 64, 68 (2007)

[134] *See, e.g.*, Chares R. Tittle, *Deterrents or Labeling*, 53 SOC. FORCES 399 (1975); William D. Payne, *Negative Labels: Passageways and Prisons*, 19 CRIME & DELINQ. 33 (1973).

[135] David R. Karp, *The New Debate About Shame in Criminal Justice: An Interactionist Account*, 21 JUST. SYS. J. 301 (2000).

[136] J.J. Prescott & Jonah E. Rockoff, *Do Sex Offender Registration and Notification Laws Affect Criminal Behavior?*, 54 J.L. & ECON. 161 (2011).

IV.   EMPLOYMENT OUTCOMES

Although expungement can simultaneously address a range of collateral consequences of criminal convictions, probably the most important motivation for most applicants—and the most important policy rationale for expungement laws—is to improve access to employment. As discussed in Part I, there are solid theoretical accounts for why expungement should have positive employment consequences, but the empirical evidence on the question remains extremely limited. This is a huge problem as advocates and policymakers negotiate the future of expungement policies, and in this part, we hope to begin to solve it. Section A presents regression analyses and figures comparing the pre- and post-set-aside wage and employment trajectories for set-aside recipients; these show large gains in both employment rates and wages. Section B focuses on the question whether these gains can be interpreted as causal effects of receiving a set-aside, offering additional analyses to help us come to grips with this important question of interpretation.

A.   *Employment and Wage Trajectories for Set-Aside Recipients*

The outcome data we use in these analyses is quarterly Unemployment Insurance Agency wage data collected for set-aside recipients. The data run from the third quarter of 1997 through the second quarter of 2013. The dataset for these analyses is structured as a panel—that is, we follow the same individuals before and after their receipt of set-asides, and each observation is a person-quarter. For each analysis, we use a balanced panel, meaning that for every individual (regardless of the date of the set-aside), we include the same number of quarters before and after the set-aside. Essentially, each individual is used as her own control; we compare the same individuals' post-set-aside employment trajectories with their pre-set-aside trajectories. The different columns of Tables 5 estimate these trend changes over different widths of the time window surrounding the set-aside.

The sample inclusion criteria are as follows:

- Set-aside received on a known date between January 1998 and May 2011. The sample for particular columns of Table 5 is further constrained depending on the time window for estimation, to ensure that outcome data exists for all quarters included in the analysis.
- The individual did not have a non-Michigan driver's license identified in our data.
- The individual matched to the UIA data in at least one quarter during the period 1997-2013.[137]
- The individual was of working age (18 to 64) for the entire time window over which his wages are observed for a particular regression.

Table 5 estimates changes in employment trends before and after set-asides. We estimate the following equation using ordinary least squares regression:

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[137] For those who do match, an absence of reported wage data for a particular quarter is interpreted as implying no employment that quarter. However, total absence from the wage data is more likely to imply a failure in the matching process (e.g., a missing or incorrect Social Security number) or a non-Michigan resident.

$$Employed_{it} = \alpha + \gamma Elapsed_{it} + \delta Elapsed_{it} \times Post_{it}$$
$$+ \theta TotalEmployment_t + \vartheta URate_t + \epsilon_{it}.$$

Our primary outcome of interest, $Employed$, is an indicator for whether an individual has any positive wages reported to UIA by any employer in the quarter.[138] $Elapsed$ is a linear measure of time, increasing by one for each quarter, and set to zero in the quarter before the set-aside is received.[139] The coefficient on $Elapsed$ represents the underlying linear trend in employment in the quarters prior to set-aside receipt—the average change per quarter in the employment rate for the sample. A negative coefficient on $Elapsed$ would indicate that the group's average employment rate was declining prior to set-aside receipt, and a positive coefficient would indicate that it was increasing. Our key variable of interest, $Elapsed \times Post$, represents the *change* in the linear employment-rate trend after set-aside receipt. A positive coefficient on $Elapsed \times Post$ implies that trend improves, and a negative coefficient means it gets worse.[140] We estimate changes in the employment *trend*, rather than a simple one-time change in the employment level, on the assumption that the effects of set-asides may accumulate over time rather than being instantaneous.[141] We estimate *linear* trends (as opposed to more complicated curves) for simplicity of interpretation. But estimating these trends over a variety of time windows can help us to detect nonlinear patterns in the actual trajectory.[142]

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[138] This is a very minimal threshold for deeming an individual "employed," as there is no minimum number of hours or total compensation requirement, and the wages could come only from one part of the quarter; the wage data are not further broken down temporally. On the other hand, absence of employment/wages as measured in this data does not necessarily mean that the individual is truly "unemployed" in the sense that economists use it—i.e., actively looking for a job, but so far unsuccessful. Being unemployed in our data simply means that the individual does not have a wage- or salary-paying job. Virtually all employers in Michigan are required to report wages, but the data do not include people who are self-employed, nor do they differentiate between unemployed people who are looking for work and people who are out of the workforce. If we our real variable of interest as total lawful employment (including self-employment) among the working-age population, then presence in the UIA data is a rough proxy for it. UIA-reported wages may also understate total earnings. That said, these issues affect the data throughout the study period, and we do not think they should substantially affect our estimation of trend changes from before the set-aside to after its receipt. If anything, classical measurement error like this tends generally to bias regression estimates toward zero—that is, it may cause us to understate set-asides' true effect size.

[139] Positive values of $Elapsed$ represent the number of quarters since the individual received her set-aside. We count the quarters prior to the set-aside, running from negative values up through quarter zero, which is the last quarter unaffected by the set-aside.

[140] The sum of the coefficients of $Elapsed$ and $Elapsed \times Post$ represent the post-set-aside trend in the employment rate for this group; a positive sum means employment levels are going up, and a negative sum means they are going down.

[141] In addition, especially if there is an underlying trend over time even before the set-aside, estimating changes in levels alone could be misleading. For example, if there is a continuous upward trend throughout the time period, the level would be higher in the post-period even if it has nothing to do with the set-aside; on the other hand, if there is a downward trend before the set-aside that reverses after the set-aside, the average employment levels might look the same before and after, even though there has been a dramatic change in trajectory.

[142] The results are substantively similar if we use logistic regression for the binary outcome variables, as we did in the Table 3 uptake analysis. Here, we prefer the linear probability model because our key

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Table 5. Changes in Employment and Wage Trends After Set-Asides

	(1)	(2)	(3)	(4)
A. Employment Rate (Any Wage)				
Elapsed × Post	0.0247***	0.0198***	0.00945***	0.00559***
	(0.004)	(0.002)	(0.001)	(0.001)
Elapsed	-0.00610*	-0.00649***	-0.00346***	-0.00247***
	(0.003)	(0.001)	(0.001)	(0.000)
Net Gain	0.049	0.079	0.076	0.067
Proportional Net Gain	8.0%	12.8%	12.3%	10.9%
No. of Observations	57,596	125,451	226,525	301,500
B. Employment Rate (>$100/week)				
Elapsed × Post	0.0367***	0.0308***	0.0149***	0.00838***
	(0.003)	(0.002)	(0.001)	(0.001)
Elapsed	-0.0144***	-0.0124***	-0.00562***	-0.00320***
	(0.002)	(0.001)	(0.001)	(0.000)
Net Gain	0.073	0.123	0.119	0.101
Proportional Net Gain	13.9%	23.1%	22.4%	18.9%
No. of Observations	71,899	125,451	226,525	301,089
C. Wages				
Elapsed × Post	314.5***	277.8***	154.2***	92.22***
	(36.77)	(18.12)	(10.38)	(7.80)
Elapsed	-160.5***	-113.5***	-43.57***	-14.95*
	(22.26)	(11.78)	(7.32)	(6.10)
Net Gain	$629.00	$1,111.20	$1,233.60	$1,106.64
Proportional Net Gain	12.7%	22.5%	24.9%	22.2%
No. of Observations	71,899	125,451	226,525	301,089
Controls				
BLS Total Employment	✓	✓	✓	✓
BLS Unemployment Rate	✓	✓	✓	✓
Window (Quarters)	+/- 2	+/- 4	+/- 8	+/- 12

Notes: Standard errors clustered on the person are reported in parentheses. *,**, *** represent significance at the 5%, 1%, and 0.1% level, respectively.

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variable of interest is an interaction term, and interactions are notoriously difficult to interpret in logistic regression. Linear probability models also tend to perform well in the middle of the probability distribution, a condition satisfied here (but not in the Table 3 analysis).

The remaining variables in the equation are indicators for economic conditions in Michigan during the calendar quarter of each observation. The time period of the study (1997–2013) includes considerable economic fluctuations, most importantly the 2008 financial crisis and subsequent deep recession; overall it was a period of economic decline and rising unemployment in Michigan.[143] To control for these changes in employment markets, we include Michigan's quarterly unemployment rate and the total number of people employed in the state reported by the Bureau of Labor Statistics. We control for these changing economic conditions because they could potentially affect the pre- and post-set-aside periods differently. In contrast, because of the balanced-panel structure, the fixed features of given individuals or their criminal cases can be safely ignored, since they are equally present before and after the receipt of the set-aside. The standard errors that we estimate are clustered on the person; this means that the reported precision of our effect estimate accounts for the fact that our quarterly observations of each person are not independent of one another.

The "Window" reported at the bottom of each table reflects the number of *quarters* before and after the set-aside that the linear trend is estimated over; for example, +/- 4 means that we observe the individuals for one year before (four quarters) and one year after the set-aside. In the bottom two rows, "Net Gain" and "Proportional Net Gain" represent the estimated percentage-point and proportional gains in employment experienced by the average set-aside recipient by the end of this window, relative to what would have occurred had they stayed on their pre-set-aside trajectory.[144]

In each version of the regression in Table 5, we estimate a substantial and statistically significant ($p<0.001$) upward turn in the employment-rate trajectory of set-aside recipients after they receive their set-asides. The coefficients, representing per-quarter gains, decline in magnitude as the window gets larger (although they remain significant), which intimates that the improvements are steepest in the first two quarters and slowly become more gradual; this can also be seen in the Net Gain and Proportional Net Gain calculations. By the end of one year, after controlling for the pre-receipt trend, set-aside recipients have gained nearly eight percentage points in the employment rate; proportionally, they are 1.13 times as likely to be employed. The net gain is about the same in the two-year observation periods and slightly lower in the three-year period, suggesting that the employment improvements following a set-aside come relatively quickly, but are largely sustained in the following years.

In Panel B of Table 5, we show the same analyses, but substitute a different, less minimalist definition of the employment outcome variable. Instead of requiring that *any* wage have been earned at any time in the quarter, *Employed*$100 requires individuals to have earned an average of at least $100 a week ($1300 total for the quarter) before counting as employed. This is still a threshold that falls well short of representing full-time employment. The minimum wage in Michigan was in the vicinity of $7 throughout the study, so this represents about 14 hours of work per week at minimum wage, or less

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[143] The various individuals in the sample are each only observed for much shorter parts of this period, so the fluctuations will affect them differently, which should reduce their aggregate effect on our estimates.

[144] "Net Gain" is obtained by multiplying the *Elapsed * Post* coefficient by the number of quarters observed in the post-period. "Proportional Net Gain" is obtained by dividing the "Net Gain" by the average employment level in the quarter before the set-aside (which is 61.7%).

at a higher wage; it is less than one-fifth of the median wage among persons with any wage in our sample. Even so, it at least excludes truly trivial earnings that may be causing people who are for all practical purposes unemployed to be coded as employed, and thus allows us to pick up on some differences that the main *Employed* coding misses. And indeed, the results in Panel B show substantially larger effects than we saw in Panel A. The general pattern is the same—steeper trend-changes in the narrower windows—but the effect sizes are bigger. By the end of one year, we now see a twelve-percentage-point net gain in employment relative to the pre-set-aside trajectory. This implies that set-aside recipients are 1.23 times as likely to be making at least $100/week as they would have been (62% instead of 53% clear this threshold). Again, the net gains are similar in the longer-term windows (one point lower in the three-year window), suggesting that the employment gains are sustained but do not continue to increase after the first year.

In Panel C of Table 5, we estimate changes in set-aside recipients' average wage trajectory. Here we see a similar pattern, with a steep gain in the first year followed by a subsequent plateau in gains. By the end of that first year, relative to the pre-receipt trend, recipients have gained an average of $1,111 in quarterly wages (i.e., $4,444 per year), which is a 23% improvement over the pre-set-aside average; the net gain increases to $1,234 in the two-year estimate, a 25% improvement. The proportional wage increase is much larger than the increase in the employment rate in Panel A, but very close to the gain in the alternative employment measure in Panel B. This implies that the average wage increase may substantially be explained by unemployed people gaining employment or by very minimally employed people gaining more hours or higher-paying work. In Figures 3a, 3b, and 3c, we demonstrate visually the employment and wage patterns that underlie these regression estimates.[145]

The results that we show here are very robust; that is, similar patterns consistently continue to appear even if we vary the details of the regression specification or the sample definition. For example, there are certain additional economic control variables that could potentially improve our ability to account for economic fluctuations, but which we did not add to the main specification because we do not have them for all years. These include the average quarterly earnings for Michigan from the Quarterly Workforce Indicators series (available from 2001), and average quarterly wages and employment rates for set-aside non-recipients in our own data (available from 2004). For the years that they are available, adding these variables does not change the results. In addition, one might worry that nonetheless, these controls might somehow not be fully accounting for the major swings introduced by the financial crisis and recession. But the same patterns persist if we simply drop that entire time period, such that the windows for each analysis end before 2008. The same substantive story also appears if we leave in the sample people who did not match to the wage data at all; for our main sample definition we assumed these were likely failures of the matching process, but they could also be

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[145] Here, we do not model changes in employment outcomes as linear trends during the pre- and post-periods; we simply show the employment rate and average wage for each quarter beginning two years before and ending two years after the set-aside (including Quarter 0, the quarter immediately preceding the set-aside quarter, which is marked with a vertical dotted line), and connect the dots. The sample used to create the graphs is the same that is used for Column 3 in Table 5, Panels A-C. The graphs all have similar patterns—a clear V-shape immediately surrounding the set-aside.

people who are not working for the entire period. If we include them and count them as zeroes on all the outcome variables, the overall employment and wage averages decline and our point estimates decline proportionally, but the Proportional Net Gain estimates remain the same.

Figure 3a. Employment Rate (Any Wages in Quarter) Before and After Set-Aside
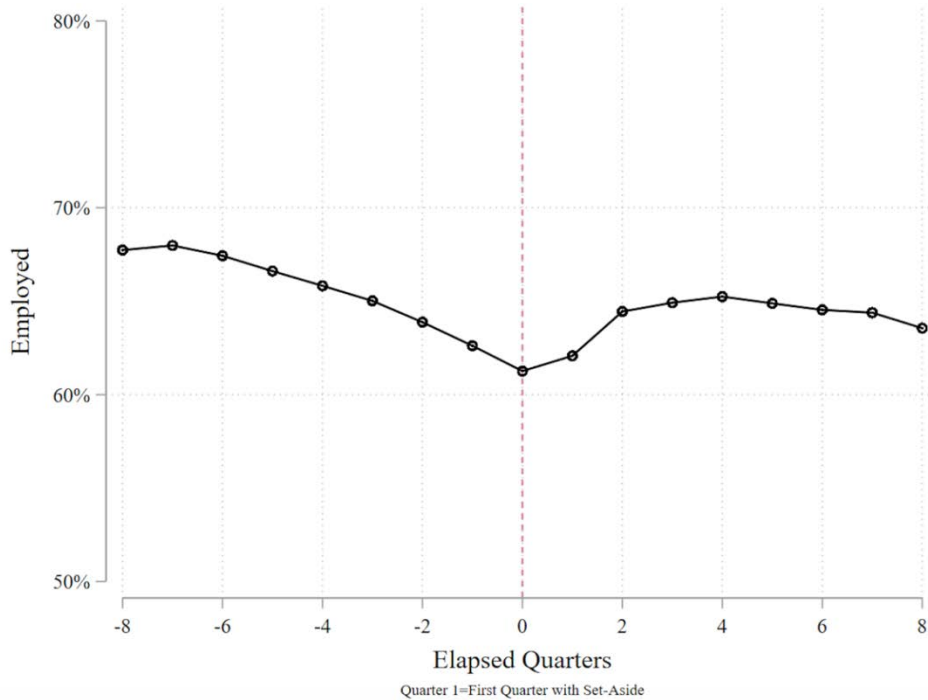

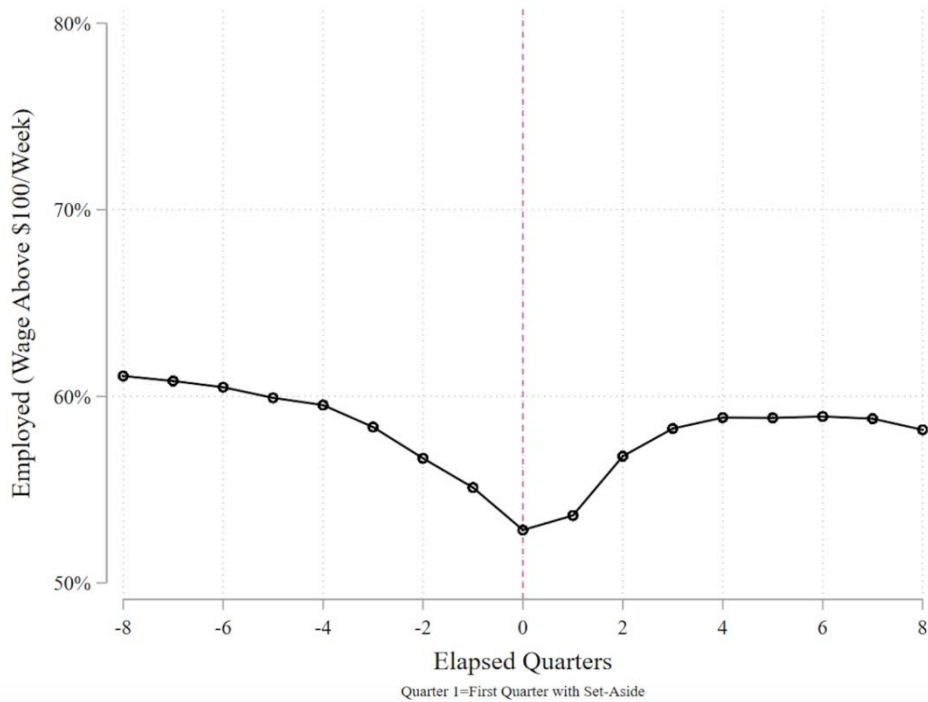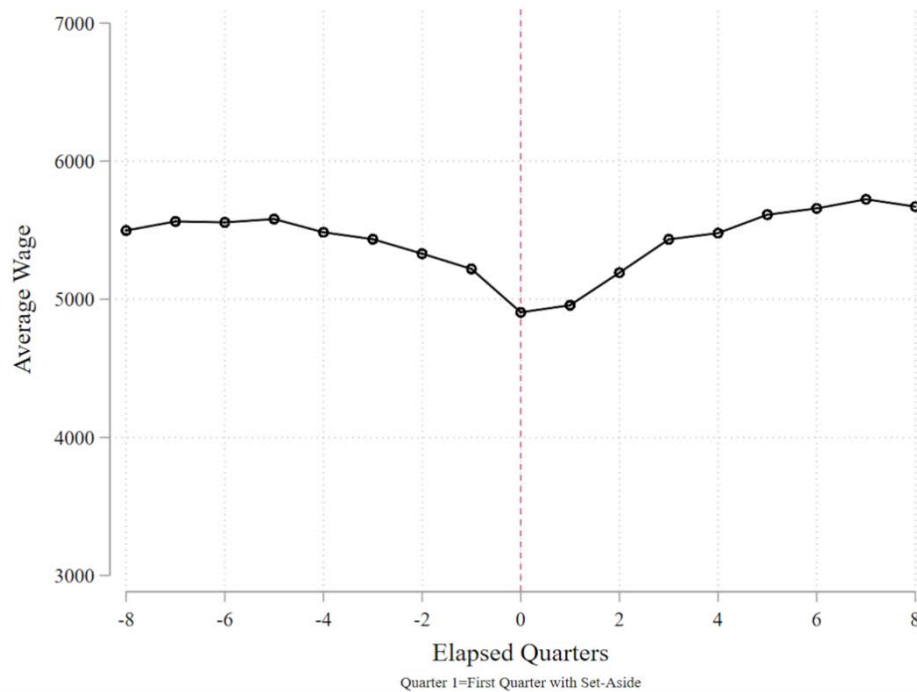**Figure 3b. Employment Rate (>$100/week) Before and After Set-Aside**

Figure 3c. Average Quarterly Wages Before and After Set-Aside

Quarter 1=First Quarter with Set-Aside

We also ran these analyses separately for different subpopulations to see if set-asides affect different groups differently. We find substantial, and largely similar, effects in every subgroup. The most striking pattern is that wage and employment gains are much bigger for women, at least in proportional terms (women have lower baseline wages). For example, in the one-year window, men's wages increase by 17%, while women's wages increase by 30%. We cannot really tell why this is, as there are many possible explanations; for example, perhaps women apply disproportionately to job types for which a criminal record is a particularly serious barrier. Studies of other kinds of job-access interventions, like job training, also generally find much larger effects for women, so this may be a byproduct of a more general phenomenon.[146] We also find larger effects for black set-aside recipients than white, but this difference is not as large (1-year wage gains of 25% versus 18%). Among both men and women, people who set aside a felony see only slightly larger gains than those who set aside a misdemeanor (1-year wage gains of 33% versus 29% among women, and 18% versus 16% for men). This difference seems surprisingly small, given that felonies carry more collateral consequences and presumably more labor market stigma. However, it is possible that, given that relatively few misdemeanors are set aside in the first place, they represent a highly selected subset—for example, perhaps the misdemeanors people choose to set aside tend to be particularly stigmatizing. In any case, the effects for all crime categories and severities, and for all categories of offenders, are large and significant.

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[146] *See infra* note 148 and accompanying text.

B. *Interpretation: Set-Aside Effect, Motivation, or Mean Regression?*

Our employment analysis demonstrates that a receipt of a set-aside is associated with large improvements in the employment rate and wages on average—and, in particular, a reversal of the pre-set-aside downward trend that we see for recipients as a group. Can we conclude from this that set-asides *cause* these improvements? The answer is not obvious. A causal interpretation of the trend-change estimates above depends on the assumption that, in the absence of these individuals receiving set-asides, their collective pre-set-aside employment and wage trajectories would have continued. But is that true? Absent the receipt of set-asides, would something else have reversed this group's downward slide at about the same time, and given rise to the same V-shaped pattern we see? The control variables in the regression already account for the role of changing labor-market trends, which would otherwise be one potential explanation. But there is still a possibility of *omitted variable bias*—there could be something else going on that we cannot measure or observe for that accounts for these patterns, other than set-aside receipt.

The most likely candidate is *motivation*. As discussed in Part II, it takes a lot of effort, as well as a financial investment, to obtain a set-aside. Moreover, most set-aside recipients don't go through this effort as soon as they become eligible—three-quarters wait more than a year, and 44% wait more than five years. So what motivates people who have waited several years to decide that *now* is the time to apply? From an inference perspective, it would be ideal if it were random, but it probably is not. In many cases, it may be that they are trying to get a job—perhaps because they have been laid off, or because they want to seek out better pay. This latter theory finds some support in our findings in Table 3, Column 5: people *are* more likely to get a set-aside within a year of a job loss or substantial wage decline. And a person who is motivated enough to improve their employment situation that they are willing to go through the burdensome set-aside process is probably doing other things too—like applying for jobs, obviously. So if we see a turnaround in their employment prospects just after the set-aside, could it be because they are doing those other things, and the set-aside is just incidental?

A related causal-inference concern is the possibility of *regression toward the mean*. Here's a classic example: Both very tall parents and very short parents tend, on average, to have children who are a little closer to the population average height than they are. Why? Well, height is substantially hereditary, but people don't turn out *exactly* like their parents; there's randomness involved in gene-mixing and other height influences. So parents who are extremely tall or short are likely to be that way partly because of that random noise—and their children may not get the same roll of the dice. Similar patterns are found in other real-world phenomena involving random fluctuations—extreme outcomes tend on average to be followed by less extreme ones (simply because most outcomes are, definitionally, not extreme).

Such a phenomenon could potentially explain some of our results. Suppose there is a certain amount of random variation in individuals' wages and employment status (or as good as random for our purposes—uncorrelated with the other variables of interest in our analysis). And suppose people tend to apply for set-asides when they have recently experienced an employment setback—so they are more likely to do so when they are on the downside of one of these random fluctuations. Then, even setting aside any broader job-search motivation that may be correlated with set-aside motivation, one might expect

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that downward trend to tend to reverse itself. The assumption of our analysis—that an existing downward trend observed across a sample should be expected to continue, absent some clear reason for a change like set-aside receipt—is sensible if you think that the underlying trend is probably driven by substantive factors that are likely to stay in place. But if you think it's not really a substantive trend at all—just the product of random noise, combined with a tendency to apply for set-asides when you're on the bottom end of that random trough—then you shouldn't have that expectation.

There is no totally clean way to disentangle these causal theories in our data. It is possible that *all* of these factors contribute to the large apparent effects that we find—that is, that set-asides do bring about employment gains, but the gains *caused* by set-aside receipt are not quite as large as those that we estimate. Indeed, the effects of motivation spillovers and set-asides might be expected to be mutually reinforcing: perhaps the motivation drives the job search, but the set-aside makes the search more likely to be successful (and that prospect in turn may increase motivation), suggesting an even more reticulated causal story.

Fortunately, our data do provide a couple of reasons to believe that causal effects of set-asides probably explain at least a large part of the upturn we observe. The first reason concerns the exact timing of that upturn. The motivation and mean regression theories both turn on the idea that people *apply* for set-asides at particular times: when they are motivated to seek work, and/or when they have recently experienced a job or wage loss. But there is a time lag between application and receipt, and our analysis focuses on the date of *receipt* of the set-aside. The date of the application is in the great majority of cases not known to us, and the time lag reportedly varies a fair amount. But our conversations with set-aside lawyers and with the Michigan State Police suggest that it is very hard to complete the process in less than two months, and that (while some jurisdictions have recently achieved efficiency improvements) three to six months was a more common lag during the years covered by our study. This constraint implies that, in our quarterly wage data, both motivation and mean-regression effects should begin to be visible even two quarters before the receipt of the set-aside, and definitely by one quarter before. And motivation effects might be expected to be visible even earlier, because the motivation to seek a job might often have been around for a while *before* the set-aside application.

But what we see in the data does not seem to match this alternative story—or at least, it matches it at best only weakly. In Figures 3a, 3b, and 3c, the first quarter in which there is a change in trajectory—an upturn from the prior quarter—is quarter 1, the quarter when the set-aside is actually received. In that quarter there is a very slight rise from the previous quarter—which does represent a substantial change, stopping the previous downward trajectory—and a much steeper upward turn begins between quarters 1 and 2 for the first employment curve (3a), and between quarters 2 and 3 for Figures 3b and 3c. This sort of pattern is very much what we would expect to observe if set-aside *receipt* (and not other factors correlated with the *application*) is driving the change in trajectory. We only detect about half of the trend change in the quarter that is only partially post-set-aside (Quarter 1), and then it appears to be in full bloom beginning in the first quarters that fall fully in the post-set-aside period. If the trend change were instead triggered mainly by something happening a quarter or two earlier (i.e., the motivation

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accompanying the choice to apply for a set-aside), we would expect to see a similar pattern shifted a quarter or two to the left. But we see no trend change until quarter 1 in any of these graphs; indeed, in the wage graph, the decline between Quarters –1 and 0 is particularly steep. It *is* possible that what we see between Quarter 0 and Quarter 1 includes some component of mean-reversion or motivation effect—we still can't rule it out. But the pattern appears more consistent with effects at least quite substantially being driven by set-aside receipt.

Second, we provide a further test of this causal theory in Table 6. Here, we analyze the effects of set-asides on a specific subset of the sample: those who receive set-asides within one year of becoming eligible to apply. As discussed above, we consider these cases (accounting for 25% of all set-asides, and 19% of the wage-linked sample) to be primarily "pent-up demand" cases—people who were just waiting to become eligible. These individuals are obviously motivated to get a set-aside, but there is no reason to believe that motivation would have arisen just at the time of application, or as a consequence of some recent employment setback. Motivation that is longer-term rather than specific to the particular time around the set-aside is not likely to confound our results, because it would not explain a turnaround in the trend at the time of set-aside receipt. Likewise, we also worry less about mean regression when we look at this sample, because there is less reason to believe that their choice to apply at that time is motivated by being at the bottom end of a random fluctuation in their employment outcomes; rather, they are applying the first chance they get.

The regressions in Table 6, Panels A–C, directly parallel those in Table 5, Panels A–C; the same analyses are carried out in the smaller sample. Table 6 demonstrates very strong gains in employment and wages after receiving a set-aside—changes that remain statistically significant ($p<0.001$ for almost all coefficients) despite the much smaller sample. The timing of the biggest gains is slightly later (the curves are slightly less steep in the six-month window), but by the end of two years, the net gain in proportional terms is essentially identical to what we observe for the larger group in Table 5. Early set-aside recipients become, by two years after set-aside receipt, 1.12 times as likely to be employed at all (a 7 percentage-point gain) and 1.2 times as likely to be employed for at least $100/month (10 percentage points), and earn 1.24 times their prior wages on average.

The biggest difference between these results and the Table 5 results concerns the pre-set-aside underlying trend, which was negative in all of the Table 5 specifications. Here, for the early-set-aside group, there is no significant prior trend for most of the windows and outcome variables; some specifications have a small negative trend, but others actually produce positive coefficients. These results support our causal account of set-aside receipt. The flat prior trend, which is followed by a steeply positive post-set-aside trend, indicates that the early set-aside recipients do not tend to apply for set-asides in the wake of an employment downturn, and is consistent with our theory of pent-up demand. The arrival of the five-year threshold is effectively random; it does not coincide with any particular pattern in employment. On average this group has fairly steady employment rates and wages prior to their set-aside, and after receipt the graph turns sharply upward. This turn cannot be explained by mean regression; there is no prior downturn from which to regress. It is also hard to explain by a job-search motivation emerging at that time—

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Table 6. Changes in Employment and Wage Trends After Set-Asides

(Set-Aside in First Eligible Year Subsample)

	(1)	(2)	(3)	(4)
A. Employment Rate (Any Wage)				
Elapsed × Post	0.0191*	0.0173***	0.00884***	0.00563***
	(0.009)	(0.004)	(0.002)	(0.002)
Elapsed	0.00170	-0.00529*	-0.00217+	-0.00223*
	(0.007)	(0.002)	(0.001)	(0.001)
Net Gain	0.038	0.069	0.071	0.068
Proportional Net Gain	6.3%	11.4%	11.6%	11.1%
No. of Observations	10,724	23,184	42,075	55,375
B. Employment Rate (>$100/week)				
Elapsed × Post	0.0230**	0.0215***	0.0125***	0.00742***
	(0.007)	(0.004)	(0.002)	(0.002)
Elapsed	-0.00364	-0.00605**	-0.00217	-0.00132
	(0.004)	(0.002)	(0.001)	(0.001)
Net Gain	0.046	0.086	0.100	0.089
Proportional Net Gain	8.8%	16.5%	19.2%	17.1%
No. of Observations	13,386	23,184	42,075	55,293
C. Wages				
Elapsed × Post	227.8**	211.1***	128.6***	59.70***
	(70.00)	(39.96)	(23.11)	(17.24)
Elapsed	-17.76	-25.56	9.292	31.45*
	(41.44)	(24.46)	(15.95)	(13.08)
Net Gain	$455.60	$844.40	$1,028.80	$716.40
Proportional Net Gain	10.6%	19.6%	23.8%	16.6%
No. of Observations	13,386	23,184	42,075	55,293
Controls				
BLS Total Employment	✓	✓	✓	✓
BLS Unemployment Rate	✓	✓	✓	✓
Window (Quarters)	+/- 2	+/- 4	+/- 8	+/- 12

Notes: Standard errors clustered on the person are reported in parentheses. +,*,**, *** represent significance at the 10%, 5%, 1%, and 0.1% level, respectively.

there is no prior setback to explain the timing of that motivation, and moreover, the timing of the set-aside for this group seems to be driven mostly by the eligibility clock. Perhaps the absence of mean-regression and motivation effects slightly dampens the *immediate* effects estimated in the shortest time window, but by two years the gains are just as large. This result provides quite good reason to believe that the employment and wage gains are substantially *caused* by the set-aside.

The results from the early-set-aside sample are encouraging in another sense as well: they provide good reason to believe that similar employment benefits might exist in states that make set-asides automatic after a fixed period of time post-conviction. One might otherwise worry that the employment gains depend on the fact that set-aside recipients are a self-selected group who choose to apply just when they are hunting for a job. Even if the set-aside *does* help (i.e., has a causal effect) in obtaining that job, one would not necessarily expect a similar effect to emerge if the individual is not job hunting at all. But in the early set-aside sample, there's much less reason to believe that the timing of the set-aside is determined by a job hunt; rather, for most of this group it is determined by the arbitrary 5-year waiting period. And that perhaps explains why the gains for this group are slightly slower to arrive—but still, they arrive fully within two years of the set-aside. If expungement kicked in automatically after five years or some other time threshold, presumably most recipients would not happen to be looking for a job (or a better-paying job or one with more hours) at that very time or within moments of learning about their expungements. But almost everyone looks to improve their employment situation eventually—and probably more frequently among people with records, who tend to be in lower-paying jobs with higher turnover.

The causal interpretation of our results is consistent with what we should expect based on the extensive research showing that criminal records pose substantial hurdles to employment. The evidence from Michigan is consistent with the intuition that expungement of those records *should* help to reduce those hurdles. Notably, these results are based entirely on set-asides that took place in the age of the Internet.[147] This suggests that it *is* possible for expungement to have substantial benefits notwithstanding the search tools currently available to employers. And this, perhaps, should not be surprising. Many arrests and convictions are not especially newsworthy, and do not create a long trail of adverse Google hits. Employers, after all, overwhelmingly pay to carry out background checks using criminal records databases; they do so presumably because they think that the database will provide them much more comprehensive and reliable information than they could otherwise obtain for free. And Michigan's experience shows that expungement can, in fact, succeed in getting convictions out of those databases. This is probably to be expected. The companies that manage those databases obtain their records from the state and update them frequently; their legal obligation not to share expunged records is clear; and failing to observe that requirement could threaten the data access on which they expend and potentially expose them to criminal prosecution.

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[147] Dates range from 1998 to 2011, with a median of 2006. Internet usage was less ubiquitous early in this period, but the gains from set-asides are similar (if anything, larger) if you look only at dates after the median.

To be sure, our data do not establish that expungement works *perfectly* in all cases; it is likely that some individuals really cannot escape the digital trail of their records, and it is possible that mistakes persist for some individuals who have had their records cleared in some criminal history databases. Moreover, a clean record obviously does not automatically translate into a job; many other factors are involved, and not *every* set-aside recipient will end up experiencing employment benefits (although some will presumably experience benefits in other areas of life as well). The employment and wage gains that we found are averages for set-aside recipients as a group, and to the extent that they can be interpreted as causal effects, they represent gains that can be obtained even if expungement does not necessarily work perfectly.

CONCLUSION

As states throughout the country debate the adoption and expansion of expungement laws, it is important for their decision-making to be guided by empirical evidence. Prior to the work in this Article, policymakers have had little at their disposal. Fortunately, Michigan's lengthy experience with a fairly typical expungement law provides a great opportunity to evaluate how these laws work in practice and what their consequences are likely to be. The challenges faced by people with records in Michigan are fundamentally similar to those faced by their counterparts in other states. So we hope the findings presented in this study, the first of its kind, will provide helpful guidance for those crucial policy decisions.

Taken together, our findings strongly support the increased availability of expungement—and particularly efforts to make them automatic, or at least procedurally easy to obtain. Those whose records are expunged experience large gains in employment rates and wages—and while some of those gains may result from other factors such as underlying motivation, there is good reason to believe that at least a substantial portion of them are caused by the expungement. The effects we find are large enough to suggest that allowing people with records to expunge them may, as a strategy for improving their employment outcomes, compare favorably with other possible policy interventions. Take, for example, job training—a common public investment. A meta-analysis of 31 studies covering 15 different publicly funded job training programs found average gains in *annual* wages of $1,417 for women and $318 for men.[148] Like set-aside recipients in Michigan, participants in these programs were self-selected; they pursued the programs because they thought they would benefit. And they did benefit—but not nearly as much as set-aside recipients do (annualized, the wage gains for our sample come to $4594 for women and $4295 for men).[149] Meanwhile, the average cost of these job-training programs to the government was $6600.[150] Expungement, in contrast, has comparatively minimal costs (running a background check, holding a court hearing, processing the paperwork), and these could very likely be reduced much further if the process were

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[148] David H. Greenberg et al., *A Meta-Analysis of Government-Sponsored Job Training Programs*, 57 INDUSTRIAL & LABOR RELATIONS REV. 31 (2003).

[149] The job-training study reported results in 1999 dollars, *id.* at 33, whereas our figures are nominal gains over one-year periods falling between 1997 to 2001, with a median year of 2005. Adjusting the job-training averages to 2005 dollars comes to $1611 for women and $373 for men.

[150] *Id.* at 50.

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rendered simpler or automated. As an employment intervention, therefore, it compares very favorably to job training in terms of both effectiveness and cost-effectiveness.

The arguments of expungement appoints have not typically focused on program expenditures., of course, but rather on public safety. Fortunately, our findings on the crime front are equally encouraging. Subsequent offending rates after expungement are extremely low. *Ninety-nine percent* of those who receive set-asides in Michigan are not convicted of a felony anytime in the next five years; 99.4% are not convicted of any violent crime; and 96% are not convicted of any crime at all, even a petty misdemeanor. In fact, set-aside recipients appear to be lower-risk than the general public. To be sure, recidivism rates might not be quite as low if states made expungement available right away, removed judicial discretion, or extended it to a generally riskier pool, such as people with more extensive criminal records. But even in those scenarios, there is still no evidence to suggest that access to expungement would *increase* the recidivism risk of those groups; if anything, due to the benefits we find in employment (and possible benefits in other areas), one should probably expect their crime rates to decline.

The discouraging part of our findings is that, despite the apparent benefits of set-asides, very few people—even among those who are eligible—actually obtain them. Our best estimate is that 6.5% of people who meet the legal requirements for set-asides in Michigan obtain them within five years—a small fraction of what is already a small fraction of all those living with records, given the tight eligibility requirements. This low uptake rate is troubling, but not shocking, given the procedural hurdles and expenses involved, the lack of legal counsel, the lack of public information, and the fact that most people with records have limited resources for overcoming these challenges. Unfortunately, nearly every state with an expungement law requires individuals to apply for expungements and gives judges the discretion to deny them, so the situation is unlikely to be better in other states.

The policy upshot of our research is clear: the process of obtaining expungement should be made as simple as possible, or ideally made automatic once the legal requirements are met. The new Pennsylvania automatic expungement law, while still the only one of its kind, illustrates a way forward; with luck the next wave of Clean Slate legislation across the country will move in this direction. California's newly proposed automatic expungement legislation, which is expected to pass and would in that case likely affect millions, would be a further dramatic advance.

Pennsylvania's law is a watershed in terms of expungement *procedure*, but it is unfortunately quite limited in its substantive scope (i.e., eligibility): automatic expungement applies only to people with minor, nonviolent misdemeanors after 10 crime-free years. Presumably Pennsylvania clean-slate advocates concluded that the bill's substantive constraints were politically necessary to secure automatic process. Likely, this limitation was meant to defuse the concern that people with more substantial or more recent convictions posed a greater public safety risk. But our findings in Michigan indicate that these concerns are unfounded; people who received set-asides after just five years posed a very low recidivism risk, even though Michigan does *not* exclude people with felonies or violent offenses. For this reason, California's pending legislation—which appears likely to end in a law of much broader scope, at present contemplating allowing

some criminal convictions to be expunged after a single year, seems likely to produce significant social benefits with relatively few costs in terms of public safety.

In any case, the Pennsylvania law is an excellent first step that will, we hope, show other states that automatic expungement processes can work smoothly. Given that the bill there passed unanimously, advocates may have been overly cautious about its crafting; there was plenty of room to spare. With luck, the next wave of reforms around the country will bring automatic-expungement to a broader swath of people with records, just as appears to be happening in California in early 2019.

For the indefinite future, however, it is likely that most states will continue to require an application-based, discretionary procedure for at least *some* subsets of expungement applicants. Where this is so, policymakers should consider how to at least render that process as easy and accommodating as possible. Courts, public defender's offices, or prosecutor's offices could automatically notify individuals, after the requisite waiting period, that they may be eligible, and provide them with links to online tools for determining eligibility. Court hearings should not be necessary, especially when set-asides are not opposed by prosecutors or victims. Online applications should be permitted. Serving the application on multiple entities (the court, the prosecution, the police) should be unnecessary, given that the information could be passed along automatically via computerized processes. Fingerprinting may be essential for background checks (although this too perhaps could be done via touch-screen), but there is no reason an applicant should have to make a fingerprinting visit to a police station *and* multiple courthouse visits, not to mention a possible visit to a notary. For example, the court clerk's office should be set up to provide whatever records are necessary, provide the form or a computer terminal, take the fingerprints, and collect the forms all at once. Fees should be eliminated; they are minor in their impact on the public fisc (especially given the cost-effectiveness point made above), but they are substantial barriers for many people with records.

Long after they have served their sentences, tens of millions of Americans and their families face the serious challenges of life with a criminal conviction record, and this number increases daily. Collectively, these challenges contribute to many significant public policy concerns, making it harder for these families to avoid poverty and contributing to racial disparities in employment and other outcomes. Our empirical results suggest that expungement is a powerful policy lever for redressing these negative consequences, without risk (and possibly with benefits) to public safety. But expungement will only realize its full potential, and make a serious dent in these large-scale social problems, if it is made available much more broadly and much more easily. Legislatures throughout the country have been taking up the issue, which provides reason for optimism. They should now consider the empirical evidence, which makes a clear case for action.