Downloaded from by guest on October 16, 2026. Copyright 2021

The Minimum Wage, EITC, and Criminal Recidivism

Amanda Y. Agan
Michael D. Makowsky

ABSTRACT

For released prisoners, the minimum wage and earned income tax credits (EITCs) can influence their ability to find employment and their potential wages relative to illegal sources of income, affecting the probability they are reincarcerated. Using administrative prison release records we identify the effects of state variation in minimum wages and EITC policies on recidivism. We find that a minimum wage increase of $0.50 reduces the probability an individual returns to prison within three years by 2.15 percent; these reductions come mainly from returns for property and drug crimes. The availability of state EITCs also reduces recidivism, but only for women.

Amanda Y. Agan is an assistant professor of economics at Rutgers University–New Brunswick (amanda.agan@rutgers.edu). Michael D. Makowsky is an associate professor of Economics in the John E. Walker Department of Economics at Clemson University. *We thank Jeff Clemens, Jennifer Doleac, John Kennan, Benjamin Hansen, Justin McCrary, Jonathan Meer, Jeff Smith, Kevin Schnepel and Jacob Vigdor for helpful comments. We have also benefited greatly from workshop and conference participants at the AEA Annual Meetings, Atlanta Fed Employment Conference, ALEA Meetings, Brazilian Econometric Society Meetings, CUNY-Hunter, George Mason Law School, Georgetown Law School, Georgia State, NBER Labor Studies and Summer Institute, Transatlantic Workshop on the Economics of Crime, NYU Wagner, Princeton University, Stanford University, SOLE Annual Meeting, The Brookings Institution, University of Rochester, UBC, UCSD, University of Oregon, University of Washington, UT Austin, and UVA. The authors thank Crystal Yang for sharing her Stata code for cleaning the NCRP data and for initial discussions about this project and to Steve Mello for help with the LEOKA data. Yujie Deng, Shannon Graham, and Sarah Wilson provided valuable research assistance. Both authors received IRB approval for the analysis of restricted use secondary data from their respective institutions. The authors have no additional disclosures. Main data used in this article are available through NACJD, though require a restricted use application: https://doi.org/10.3886/ICPSR36373.v1; supplemental data sets are all publicly available. Additional replication materials are provided in the Online Appendix.*
[Submitted December 2020; accepted May 2021]; doi:10.3368/jhr.58.5.1220-11398R1
JEL Classification: J22, J23, J38, K14, and K40
ISSN 0022-166X E-ISSN 1548-8004 © 2023 by the Board of Regents of the University of Wisconsin System
Supplementary materials are available online at https://jhr.uwpress.org.
This open access article is distributed under the terms of the CC-BY-NC-ND license (http://creativecommons.org/licenses/by-nc-nd/4.0) and is freely available online at: http://jhr.uwpress.org
Amanda Y. Agan https://orcid.org/0000-0002-4759-8367
Michael D. Makowsky https://orcid.org/0000-0002-5558-0490

I. Introduction

Every year, more than 600,000 men and women are released from prison in the United States; nearly one-third will return to prison within three years (Yang 2017). The probability these individuals return to prison is in part determined by the labor market opportunities they face upon release (Raphael and Weiman 2007; Visher, Debus, and Yahner 2008; Yang 2017; Galbiati, Ouss, and Philippe 2021; Schnepel 2018). Recently released prisoners tend to have lower human capital and interrupted work histories (Waldfogel 1994; Pager 2008); they also carry the stigma of a criminal conviction and the associated risks to potential employers (Pager 2003; Agan and Starr 2018). In the market for low-skilled labor, released prisoners are likely to be at the very margin, their employment sensitive to even moderate changes in wage policies. Their outside option may also include criminal activity, implying recidivism may not just be influenced by whether they can find a job, but whether they can find a job that pays better than crime. Given the limited efficacy of past policies and the political obstacles to passing legislation supporting individuals with criminal records, understanding the impact of broader labor market policies on this population is critical to efforts towards reintegrating released prisoners back into the workforce and breaking the cycle of crime and imprisonment.

In this work, we estimate how two major low-wage labor market policies, the minimum wage and (state-level) earned income tax credits (EITCs), impact the probability that a recently released prisoner is reincarcerated. We exploit variation in the implementation and levels of these policies across states combined with microdata from nearly six million individual prison release records from 2000–2014 across 43 states to understand how these policies impact recidivism. A change in the minimum wage could impact the labor market prospects of released prisoners, and thus recidivism, through two competing channels: the wage they can expect to earn if they get a minimum wage job (the *wage effect*) and their likelihood of finding employment (the *disemployment effect*). An EITC is a (usually) refundable tax credit available to individuals with a job earning a low to moderate income, which are much more generous for individuals with children; some states offer additional top-ups to the federal EITC benefits for their residents. Additional EITC benefits, for those who are eligible, serve to increase incomes and subsidize the costs of labor in the legal market, potentially decreasing recidivism through both the wage and employment channels. Given the way EITCs are implemented, this is likely to have stronger impacts for women (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001).

We find that higher minimum wages are associated with a lower probability of returning to prison. That is, the increased incentive to substitute legal employment for criminal market activity, on average, appears to be greater than any disemployment effects of minimum wages for recently released prisoners. Our data do not allow for separate identification of the magnitude of disemployment or wage effects, but supplementary data from the Current Population Survey (CPS) (Flood et al. 2020) allow for some exploratory tests of the plausibility of the employment mechanism. When focusing on populations that have a relatively high likelihood of having a criminal record (low-education Black men and individuals who say they are not eligible to vote), we find evidence that higher minimum wages are associated with higher probabilities of employment. Using state and year variation in the availability of state EITC policies, we

Downloaded from by guest on October 16, 2026. Copyright 2021

find evidence that the availability of state top-ups to the federal EITC corresponds to a lower rate of longer-term (three-year) recidivism for women.[1] State EITCs have no discernible effect on men—if anything, our results offer the possibility of these policies corresponding to increases in returns to prison for men for some categories of crime.

Ours is the first paper to explore the effects of these low-wage policies on recidivism. There is a literature on the minimum wages and overall crime that is fairly mixed and predominantly focused on teenagers and young adults (Beauchamp and Chan 2014; Fone, Sabia, and Cesur 2023; Fernandez, Holman, and Pepper 2014; Hashimoto 1987).[2] We have reason to be narrowly interested in the effects of minimum wages on recidivism rather than crime rates in general. The minimum wage literature has predominately focused on *disemployment effects*—particularly for groups likely to be looking for low-wage work, such as teenagers, immigrants, and high-school dropouts—with mixed results (Card and Krueger 1994; Neumark and Wascher 1995; Dube, Lester, and Reich 2010; Allegretto, Dube, and Reich 2011; Gorry 2013; Dube and Zipperer 2015; Liu, Hyclak, and Regmi 2016; Slichter 2016; Powell 2017; Orrenius and Zavodny 2003; Deere, Murphy, and Welch 1995; Jardim et al. 2022; see Neumark 2017 for a recent review).[3] Released prisoners share attributes with these groups and also have the additional stigma of a criminal record, as well as employer reluctance to hire them (Pager 2003; Holzer, Raphael, and Stoll 2006; Agan and Starr 2018), making them vulnerable to disemployment or labor–labor substitution effects of minimum wages, even if higher minimum wages do not cause aggregate decreases in employment.[4]

A criminal record also increases the importance of considering supply-side effects of the minimum wage. For some released prisoners, their next best alternative to legal employment includes income-generating criminal endeavors. These criminal earnings function as a "reservation wage"—legal sector employment is only attractive if the individual expects to earn more than they could in the illegal market. For wages below

1. This effect is stronger for three-year recidivism rates than one-year; this delay in impact, relative to the minimum wage, is unsurprising. EITC refunds are subject to the lag of tax filing and the issuing of government refunds, while any increase in the minimum wage stands to appear in any individual's next paycheck. The earliest the IRS expects the EITC refunds to be available in taxpayer bank accounts or distributed as debit cards is the first week of March each year (https://www.irs.gov/individuals/refund-timing, accessed February 10, 2023). Actual receipt will depend on timing of tax filing.

2. Beauchamp and Chan (2014) find teenagers who had been working at around the minimum wage are more likely to report having committed crimes after an increase, while results for older ages (20–30) are mixed. Hashimoto (1987) and Fone, Sabia, and Cesur (2023) use arrest data to find that higher minimum wages are associated with increases in property crime arrests for teenagers or very young adults. Fernandez, Holman, and Pepper (2014) study living wages but also include minimum wages in their specifications and find suggestive evidence of *decreases* in reported UCR crime with higher minimum wages. Braun (2019) calibrates a model of crime and employment and predicts national crime rates initially decreasing with the federal minimum wage, but eventually generating a net increase via disemployment effects.

3. While direct employment effects of minimum wages have been the focus of much of the literature on minimum wages, our research also adds to the growing literature on impacts of minimum wages on other margins, which includes research on wage distributions and inequality (DiNardo, Fortin, and Lemieux 1996; Lee 1999; Autor, Manning, and Smith 2016), firm entry and exit (Luca and Luca 2017), and fringe benefits (Clemen, Kahn, and Meer 2018).

4. The opportunity cost of crime is likely lower for individuals already carrying criminal records than those who would be risking a first arrest or conviction, resulting in subpopulations of would-be criminals facing different demand- and supply-side effects of the minimum wage. This should be an important consideration for future research looking at similar policy effects on general crime rates.

Downloaded from by guest on October 16, 2026. Copyright 2021

this reservation wage, any reduced probability of legal employment from higher minimum wages is irrelevant. Minimum wages are a particularly salient indicator of the wage for legal work available to released prisoners, and prior estimates imply that criminal earnings are not far from typical minimum wages, so there is an opportunity for higher minimum wages to succeed in pulling individuals into the legal labor market (Viscusi 1986; Levitt and Venkatesh 2000; McCarthy and Hagan 2001; Uggen and Thompson 2003).[5] This *wage effect* implies that minimum wages could reduce recidivism, similar to higher market wages or better labor market opportunities (Yang 2017; Galbiati, Ouss, and Philippe 2021; Schnepel 2018). In estimating the impact of minimum wages on recidivism, we are in effect estimating the net of wage and disemployment effects. A more detailed exploration of this conceptual framework is included in Online Appendix A.

The predicted effects of EITCs for eligible individuals are less ambiguous (Leigh 2010; Rothstein 2010; Nichols and Rothstein 2016). The federal EITC is far more generous for people with minor children who live with them at least half the year—in 2016 the maximum federal EITC benefit for a household with no qualifying children was $506, whereas it was $3,373 for those with one qualifying child and $6,269 for households with at least three children.[6] Within our sample, 21 states offer an additional "top-up" to eligible filers in those states equal to 3.5–40 percent of the federal benefit they will receive depending on the state—15 always do so for the duration of our sample period, three states introduce an EITC top-up, and three states initially offered a top-up, only to later discontinue or fail to fund their program. It is within these latter six states that we are able to identify within-state effects of the EITC on criminal recidivism.[7]

The research on the EITC also constitutes a deep literature, but its connection to recidivism has similarly not been studied. As a wage subsidy for individuals reporting earned income, standard theory predicts that the wage and employment effects of the EITC should move in the same direction for eligible individuals, with both working towards reducing the probability an individual returns to prison. Given that the size of the credit is strongly contingent on number of children, impacts on recidivism are likely to be heterogeneous across genders. The Bureau of Justice Statistics reports that 41.7 percent of women reported being the single parent of their household before entering prison, compared to 19.2 percent of men (Glaze 2008).[8] Prior work has shown that, in

Downloaded from by guest on October 16, 2026. Copyright 2021

5. The existence of higher-paying jobs, if an individual is able to secure one, also increases the opportunity cost of returning to prison, presumably reducing the probability of recidivism, similar to the argued mechanisms behind minimum wages increasing high school dropouts as individuals exit education in favor of higher wages available in the job market (Chaplin, Turner, and Paper 2003; Neumark and Wascher 2003).

6. https://www.irs.gov/credits-deductions/individuals/earned-income-tax-credit/eitc-income-limits-maximum-credit-amounts (accessed February 10, 2023).

7. Introductions: North Carolina, Nebraska, and Oklahoma. Discontinuations: Colorado, Indiana, and Michigan. Federal changes during this time came in 2002 when the plateau region of the EITC was expanded for married couples and in 2009 when benefits were increased for three or more children. As we do not have access to information on the number of children offenders have or their marital status, we cannot exploit these changes and instead focus on the state-level top-ups.

8. In a survey of recently released prisoners in Chicago, Baltimore, and Cleveland performed by the Urban Institute, in the two to four months after release 28 percent of men and 49 percent of women report children under 18 living with them (authors' calculations from the "Returning Home Study" data provided by the Urban Institute). Further to this point, in exit interviews with released prisoners, women were found to consistently place a higher priority on maintaining and regaining custody of children than men (Spjeldnes and Goodkind

terms of labor market participation, single mothers are more responsive to the EITC than other comparable groups (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001; Bastian 2020), while the observed net effects on married and childless men are insignificant (Hoffman and Seidman 2003; Eissa and Hoynes 2004). Further, prior research has offered evidence that the EITC may have a sufficiently strong effect of pushing single mothers into the workforce that the additional competition for low-skilled jobs may actually increase male unemployment (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001).[9] These earlier findings are congruent with our conclusion that EITC benefits reduce recidivism for women, but have null, if not criminogenic, effects for men. The EITC is also likely to have stronger effects over time. While the prospects of a higher future refund should have the immediate effect of increasing legal labor force participation, an additional income effect should enter the following year upon filing taxes when the individual reaps the benefit of the tax credit.

Our findings have important implications for both labor and crime policy. Nearly half of the $125 million budget for the Second Chance Act Prisoner Reentry Initiative was earmarked for released prisoner employment programs (GAO 2011). Summarizing the "flurry of community-based employment interventions, generally involving some combination of job readiness, job training, and job placement services" implemented between 1971 and 1994, Visher, Winterfield, and Coggeshall (2005) identify eight programs for individuals with criminal records that were designed with an eye towards testable outcomes. While these programs likely have a range of genuine benefit to their participants, none produced statistically identifiable reductions in recidivism rates. Further, the intent of these and similar policies not withstanding, the balance of U.S. labor policy is very much against those with criminal records.[10] We find evidence that minimum wages and wage subsidies have socially beneficial second-order effects on criminal recidivism, while also demonstrating a potential additional benefit to expanding the EITC to individuals who do not have custodial custody of young children. These observed outcomes suggest that broader labor policies targeted towards helping low-skilled workers may serve as an alternative to policies specifically targeting released prisoners.

Downloaded from by guest on October 16, 2026. Copyright 2021

II. Data

A. National Corrections Reporting Program

Data on prison spells were obtained from the National Corrections Reporting Program (NCRP).[11] The data were constructed using administrative data voluntarily provided

2009). There also exists the possibility of second-order effects driven by this gender heterogeneity in EITC benefit accrual if women are put in a position to outcompete men in the market for lower wage labor. On net, our simple model unambiguously predicts that the EITC will reduce criminal recidivism *for women*. For male recidivism, however, the predicted effect of the EITC is weaker and less certain.

9. Blank, Gelbach, and Ford (2006) test this "crowd-out" hypothesis. While they find no significant evidence of crowd-out, any potential effects would nonetheless be predicted to be stronger for males with a criminal record.

10. As of 2016, there were 6,392 separate state restrictions on employment eligibility for those with felony records (Fredericksen and Omli 2016). Criminal records as a major barrier to employment would appear, in many ways, to be not just an outcome, but an explicit policy *goal* (Pager 2003; Agan and Starr 2018).

11. United States Department of Justice, Office of Justice Programs, Bureau of Justice Statistics (2016).

by states to the Bureau of Justice Statistics (BJS) on prison admissions and releases from 2000–2014. Forty-four states have provided data into this system at some point during this time period. The BJS data are linked using inmate ID numbers, allowing the matching of individuals across prison spells within a state.[12]

The data include the admission and release month for each prison spell. Observed demographics for each offender include age, race, Hispanic ethnicity, education (highest grade completed), gender, and whether the individual has previously been convicted of and incarcerated for a felony. For each prison spell, we observe the type of facility the prisoner was placed into, the reason why the offender entered into the custody of the correctional facility, and why the prisoner was released. For each prison spell we also observe up to three conviction offenses, the sentence imposed for each offense, and the total sentence imposed. Because we observe the prison admission and release date for each period of incarceration, we can calculate the total time served for each period of incarceration. Actual time served can differ from the sentence imposed because of early release via parole or time credited.

The data do not include specific information on state or county of residence after incarceration. They do include the state that incarcerated the offender, and the state (and county) of conviction. For the most part, recently released offenders are released into the state and county of most recent legal residence prior to incarceration. For most of our analyses, we will use the state of conviction as a proxy for the state the offender was living in after release. In some analyses, we will also use information on the county of conviction as a proxy for the county the offender lived in after release, but this is a more noisy proxy.[13] For complete details and background on this decision see Online Appendix C.

From the 2000–2014 NCRP data we drop individuals who have not yet been released, which is 13 percent of the sample; who are missing gender, as this is a major component of our analysis (0.03 percent of the sample); or those who were "released" from prison because of death (0.4 percent of the sample). We also drop individuals who have a different state of conviction than the state that incarcerated them—these are cases where it appears the offender was sent out of state to serve their sentence, though no reason is given why. In these cases, we are less clear about the state the prisoners will return to (0.04 percent of the sample).

The broadest exclusion of data from our analysis comes from the state of California. In 2011, California enacted the Public Safety Realignment Act (PSRA), an attempt to reduce overcrowding in California prisons, and as a result many convicts served their time in county jail rather than state prison post-PSRA. Given our data's reliance on state prison records (county jail admissions are unobserved), for the purposes of our analysis this completely changes the definition of recidivism in California in 2011 and after. This can be seen clearly in Figure 1, where California recidivism rates drop precipitously around 2011, a far outlier from other large states or the state with the largest drop in

12. For a description of how prison term records were created, see http://www.icpsr.umich.edu/files/NACJD/ncrp/white-paper-computing-code.pdf (accessed February 10, 2023).

13. The 2015 NCRP data include, for some offenders, the last known state and county for the offender. Among those with the requisite data, 95 percent lived in the state of conviction prior to incarceration, but only 70 percent lived in the *county* of conviction prior to incarceration.

Downloaded from by guest on October 16, 2026. Copyright 2021

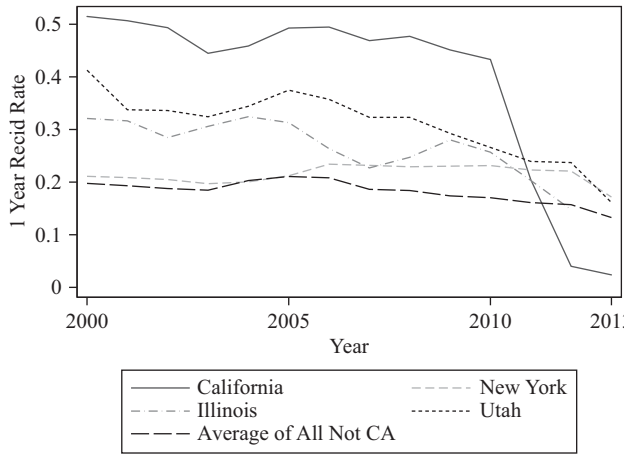

Figure 1

One-Year Recidivism Rates for California versus Other States

recidivism around that time, Utah. Taking what we believe to be the most conservative approach, we opt to exclude California entirely from our analysis.[14]

After all restrictions are imposed, including the exclusion of California, our sample includes nearly 5.8 million prison releases from four million unique offenders in 43 states when one-year recidivism rates are the outcome. When three-year recidivism rates are the outcome, the sample includes nearly 4.8 million prison releases from three million individuals.

There are two main limitations to the data for our purposes. First, the NCRP data only link prison spells within a state, so any reoffending in a different state is not captured and is indistinguishable from an individual who is not recommitted in the same state.[15] Second, the data only capture a return to custody in state prison, not rearrest or prosecution.

Our sample of prisoner data is summarized in Tables 1 and 2. Table 1 gives demographic characteristics of the sample, and Table 2 shows recidivism rates. As expected, a vast majority of our sample, 88.2 percent, are male. Minorities make up a larger share of our sample of prisoners than in the U.S. population (54.5 percent of our sample are Black or Hispanic). The average prisoner is in their mid-30s upon release, which makes sense, as our data concern prisons and not local or county jails, and thus most people are

14. The data from California prior to 2011 are compromised as well. In 2006, California declared a "prison overcrowding state of emergency," authorizing the involuntary transfer of thousands of prisoners to out-of-state prisons. This state of emergency was not repealed until 2013, at which point 8,900 prisoners sentenced in California were still serving their sentences in out-of-state prisons https://www.ca.gov/archive/gov39/2013/01/08/news17885/index.html (accessed February 10, 2023).

15. One condition of parole is often that one stays within a small geographic area after release. In addition, according to data collected by the Bureau of Justice Statistics on prisoners released from prison into 30 states in 2005, 3 percent were arrested out-of-state within one year, and 7 percent were arrested out-of-state within three years (Durose, Snyder, and Cooper 2015). Thus, while out-of-state migration could clearly be an issue, we believe it will not have large effects on our results.

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 1
Summary Statistics: Characteristics of Sample

	All	Recidivate 1 Year	Recidivate 3 Years
Male	0.882	0.903	0.905
White (not Hispanic)	0.427	0.415	0.402
Black (not Hispanic)	0.425	0.438	0.464
Hispanic	0.120	0.109	0.103
Less than HS degree	0.388	0.401	0.421
HS degree	0.315	0.325	0.312
College degree	0.007	0.005	0.005
Prior felony incarceration	0.290	0.334	0.330
Age at release	35.063	33.642	33.507
Time served (days)	655.543	485.335	540.696
Prior offense violent	0.216	0.192	0.189
Prior offense property	0.289	0.341	0.334
Prior offense drug	0.294	0.273	0.293
Min. wage	6.405	6.313	6.093
State EITC	0.357	0.393	0.374
State EITC percent	5.229	5.962	5.532
Observations	5,786,062	999,321	1,645,055

Notes: "Recidivate 1 Year" indicates those released prisoners who returned to prison within one year of their release (analogous for "Recidivate 3 Year"). Violent, Property, Drug are indicators for the offense for which the offender initially went to prison. The final three rows represent the average value of those policy variables for the state and month in which the offender was released. State EITC is a dummy for whether the state had its own EITC in a given year; thus, that row represents a proportion. "State EITC percent" is the average percent of the federal EITC that the top-up represents.

Downloaded from by guest on October 16, 2026. Copyright 2021

incarcerated for a relatively serious crime. In addition, almost 30 percent have a previous incarceration for a felony. In Table 2 we see that just over 17 percent of offenders in our data set are returned to prison within one year, and 35 percent are returned within three years. Men recidivate at a higher rate than women (18 percent versus 14 percent in the first year). Property and drug crimes constitute the most common returning offenses.

B. Minimum Wage and EITC Data

We combine the NCRP data with data on minimum wages and the availability of state EITC top-ups. The minimum wage data are from Vaghul and Zipperer (2016), which include state and substate minimum wages for the entire period of our study from May 1974 to July 2016.[16] Table 3 and Figure 2 summarize state minimum wage and minimum wage changes during these years. The average state had four changes in their

16. In one specification, we use the minimum wage at admission as well. We have some individuals admitted before May 1974; to fill in pre-1974 minimum wages we use data from David Neumark for January 1960–April

Table 2
Summary Statistics: Recidivism Rates

	Recidivate 1 Year (1)	Recidivate 3 Years (2)
Overall	0.173	0.346
Men	0.177	0.355
Women	0.142	0.284
Black (not Hispanic)	0.178	0.370
White (not Hispanic)	0.168	0.331
Hispanic	0.156	0.303
Less than HS	0.179	0.363
HS	0.178	0.349
More than HS	0.148	0.299
Returning offense violent	0.032	0.062
Returning offense property	0.057	0.113
Returning offense drug	0.047	0.103
Observations	5,786,062	4,749,284

Notes: "Recidivate 1 Year" indicates those released prisoners who returned to prison within one year of their release (analogous for "Recidivate 3 Year"). Column 2 has fewer observations to allow everyone to have three years of post-release data, whereas Column 1 only requires one year of post-release data.

Table 3
Summary Statistics on Minimum Wages and EITCs by State–Year–Month, 2000–2014

	Mean	SD	Min.	Max.
Minimum wage	6.43	1.10	5.15	9.50
Number of minimum wage changes	4.73	2.54	2.00	13.00
Size of minimum wage change	0.51	0.33	0.04	1.80
Size of minimum wage change (%)	0.08	0.06	0.01	0.35
Has state EITC	0.39	0.49	0.00	1.00
State EITC percent	6.39	10.22	0.00	40.00

Notes: Each observation is a state–year–month, so there are 9,180 observations (51 states including DC × 15 years × 12 months). A change in the state minimum wage could come from a state-level law or a federal minimum wage change. Minimum wages are measured in real 2011 dollars. Note: 17 states had no changes other than the federal minimum wage increases. State EITC percent is the percent of the federal EITC that the state EITC represents.

Downloaded from by guest on October 16, 2026. Copyright 2021

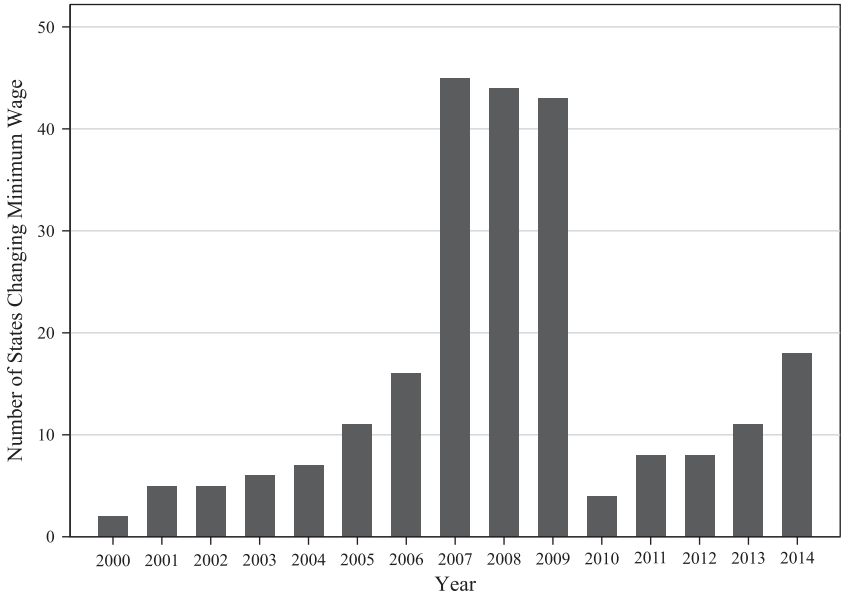

Figure 2

Number of States Changing Minimum Wage by Year

Notes: A 2007 amendment enacted federal minimum changes in 2007, 2008, and 2009 ($5.85 effective July 24, 2007; $6.55 effective July 24, 2008; and $7.25 effective July 24, 2009). If a state already had a minimum wage at or above these federal increases, then it would not register as having a minimum wage change; hence, the number changed in those years is not 51.

Downloaded from by guest on October 16, 2026. Copyright 2021

minimum wage during our time period, with no state changing less than twice. Many of these changes stemmed from the 2007 amendments to the Fair Labor Standard Act, which implemented federal minimum wage increases in 2007, 2008, and 2009 ($5.85 effective July 24, 2007; $6.55 effective July 24, 2008; and $7.25 effective July 24, 2009).[17] On average, states increased their minimum wage $0.50 (about 8 percent of the previous minimum wage), ranging from $0.05 to $1.70. At any given point in our window of study, as many as 31 states had minimum wages above the federal minimum. A number of substate minimum wage changes take place in our window of observation. These changes are mostly at the city level, which limits our opportunities for integrating substate minimum wages into our analysis because the lowest geographic level of identification in our data is at the county level, and our county proxy (courthouse of prison admittance) is less reliable than our state identifier. In light of these limitations, our main analyses focus only on state minimum wages, but county-level analysis of substate minimum wage changes is included as a supplemental robustness check.

1974 (http://www.economics.uci.edu/ dneumark/datasets.html, accessed February 10, 2023). For pre-1960 we assign the prevailing federal minimum wage at the month and year of admission.

17. If a state already had a minimum wage at or above these federal increases, then it would not register as having a minimum wage change; hence, the number changed in those years is not 51.

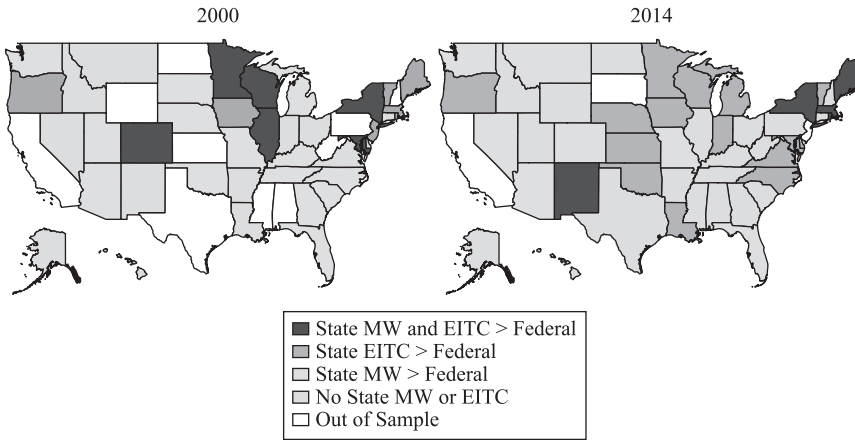

Figure 3

States with Minimum Wages Above Federal and EITC Top-Ups in January 2000 and January 2014

Notes: This map reports state minimum wage and EITC programs at the beginning and end of our sample. States not included in our sample are reported as "out of sample."

Downloaded from by guest on October 16, 2026. Copyright 2021

The state EITC data come from the Tax Policy Center.[18] As of 2016, 25 states and DC offer EITCs on top of the federal EITC, worth an additional 3.5–40 percent of the federal benefit. These EITC state "top-ups" were introduced in different states at different times, and many have expanded or contracted their EITC benefits over time, giving us both within-state and across-state variation in state EITC generosity and availability. Within the states and years included in our sample, 22 states never provide an EITC top-up, 15 always do so, three states introduced an EITC top-up (North Carolina, Nebraska, and Oklahoma), and three states initially offered a top-up, only to later discontinue or fail to fund their program (Colorado, Indiana, and Michigan).[19]

In Figure 3, we map the overlapping presence of state minimum wages and EITC programs at the beginning (2000) and end (2014) of the window within which we are able to observe prison release. The variation across states and over time in both the minimum wage and the EITC allows us to employ a difference-in-differences identification strategy in our analysis of the effect of low-wage labor market policies on individual recidivism. All minimum wage changes, state EITC provision, and state EITC

18. http://www.taxpolicycenter.org/statistics/state-eitc-based-federal-eitc (accessed February 10, 2023).

19. Colorado, for example, offered an EITC in 2000–2001 that was contingent upon the state having a budget surplus (Colorado subsequently voted in 2013 to reintroduce the state EITC once surplus funds were available again). Indiana discontinued their top-up in 2003, Michigan in 2008. This would, at face value, appear to provided sufficient variation for our identification strategy, but given the inclusion of state and year fixed effects in all of our specifications, a considerable amount of the identification of the impact of the availability, as opposed to the level of, a state EITC is dependent on the six states who change policies within our sample window. Additional analysis of the robustness of our results to concerns regarding the number of treated clusters is included in Table 6.

top-up amounts for each state in our sample period (2000–2014) can be found in Online Appendix Table B.1.

III. Empirical Model and Estimation

To estimate the effect of low-wage labor market policies on whether a released prisoner recidivates within a certain time period, we exploit the panel nature of our data and the fact that minimum wage changes and EITC top-ups were enacted and changed in different years and months across many states between 2000 and 2014. Our baseline specification is:

$$(1) \quad Recidivate_{isct} = \beta_1 MW_{st} + \beta_2 EITC_{st} + \mathbf{X}_{it} + \mathbf{K}_{ts} + \mathbf{J}_{tc} + \gamma_y + \delta_s + \varepsilon_{isct}$$

where $Recidivate_{isct}$ is an indicator variable for whether an offender i, imprisoned by state s, sentenced in a court in county c, released in year–month t, returned to prison in the same state within a certain time period (one or three years depending on the specification).[20] MW_{st} is included as either the (nominal) minimum wage or log minimum wage in the state and year–month into which the offender was released. $EITC_{st}$ is included in all specifications as either an indicator for the availability of a state top-up to the EITC or the percentage amount of the state top-up in the state and year–month into which the offender was released. Given that the EITC is mainly claimed by and impacts single mothers, however, for results focusing on the EITC, we run our analysis separately by reported gender. Results included in stratified analysis of specifications are otherwise identical to our main specification. This, in effect, fully interacts the controls and fixed effects with gender. To the extent that we think that any missing gender interactions are causing omitted variable bias, this is a useful exercise. However, there is also a trade-off with precision. In particular, we have a far smaller sample of women due to the nature of criminal justice interactions. We will report coefficients on β_1 in the pooled sample when discussing the minimum wage results, and mainly focuses on coefficients on β_2 when discussing EITC results in the gender stratified sample.[21]

$\mathbf{X}_{it}$ is a vector of characteristics about the individual offender, both time invariant and specific to the particular prison spell that ends in year–month t: race/ethnicity, gender, highest grade completed (at entry), age at release and its square, time served for this spell and its square, offense committed for this spell, number of counts convicted of for this spell, prior felony incarceration indicator, prison admission type (parole violation, new offense, etc.), and indicators for missing values for each of these variables. $\boldsymbol{\kappa}_{ts}$ are time-varying state characteristics: the housing price index, the percent of the state legislative bodies that are Democrats, the maximum TANF benefit for a family of three, and whether drug felons are banned from TANF benefits.[22] These variables are meant to pick up other state policies that may interact with recidivism or employment for offenders and

20. In Equation 1, we identify the effect of the low-wage labor market policy at the time the offender was released. There is a trade-off in using one- or three-year recidivism rates. The longer the recidivism period, the fewer observations we can use in our analysis. Three-year recidivism rates may more fully capture recidivism probabilities, however, as criminal cases often take a while to wind through the court system and into the prisons.

21. We also report gender stratified minimum wage results.

22. Housing price index is the all-transactions quarterly index from the Federal Housing Finance Agency. This is the same control used in Clemens and Wither (2016) to control for time-varying macroeconomics conditions

Downloaded from by guest on October 16, 2026. Copyright 2021

Downloaded from by guest on October 16, 2026. Copyright 2021

macroeconomic trends in the state. In robustness checks, we also add the unemployment rate as an additional control, though of course this may be directly impacted by the minimum wage. $\mathbf{J}_{tc}$ is a vector of demographic control variables from the census and American Community Survey (ACS) at the level of the county where the individual was convicted: median household income, percent age 15–24, percent Black, and percent Hispanic.[23] γ_y are year fixed effects, and δ_s are state fixed effects.

Standard errors are clustered at the state level. There are 43 states in our data during this time period, leaving us with 43 clusters. While there is some debate on the minimum number of clusters necessary to use standard clustering techniques, 30 is often cited as a threshold under which adjustment to standard cluster-robust standard errors are necessary to avoid overrejection (Cameron, Gelbach, and Miller 2008). While we are over this arbitrary threshold, it is nevertheless sufficiently close to warrant additional caution. As such, for all main coefficients of interest, we report both cluster-robust standard errors and *p*-values from the wild-cluster bootstrap procedure suggested by Cameron, Gelbach, and Miller (2008), which tend to be more conservative, particularly when clusters are asymmetric in size. The notes for Table 4 provide more details. The EITC presents an additional concern in the number of *treated* clusters. Within our sample, 22 states never offered an EITC top-up, while 15 have always offered a top-up. During the sample window, three states introduced an EITC top-up (North Carolina, Nebraska, and Oklahoma), while three states initially offered a top-up, only to later discontinue the program (Colorado, Indiana, and Michigan).[24] Given the inclusion of state and year fixed effects in all of our specifications, the EITC impact is identified enirely off of the within-state variation of six states. Further, given the smaller sample of women and their unequal distribution across states in our sample, some caution is warranted.[25] In their analysis of bootstrap inference, Roodman et al. (2019) note that the wild-cluster bootstrap can dramatically *underreject* the null hypothesis when the number of treated clusters is small. In these contexts, MacKinnon and Webb (2018) advise the use of a subcluster bootstrap. For the sake of additional robustness, estimates of the effect of the EITC include wild bootstrap standard errors estimated off of the state–year subcluster.

Our identification of the impact of the minimum wage or the EITC compares observably similar offenders, released into the same state, but who happen to be

---

across states without controlling for unemployment, which could be a direct result of minimum wage and EITC policies. State legislature data is from the National Conference of State Legislatures (for Washington, DC we use percent of the U.S. Congress that are Democrats. Nebraska's unicameral legislature is technically nonpartisan, but one can determine the political affiliation of its members by voter registration, party endorsements, or reporting in media or by nonprofits, which we did. TANF benefits and felony drug bans come from the Urban Institute's Welfare Rules Database.

23. These are meant to proxy for demographic characteristics of the county of release. For years and counties not represented in the ACS or census, values were either linearly interpolated or carried forward from the most recent census. As stated, our best approximation is that around 70 percent of individuals live in the same county that convicted them. In other robustness checks, we fully utilize this county proxy and include county fixed effects amongst other additional analyses. See Online Appendix D.

24. Colorado, for example, offered an EITC in 2000-–2001 that was contingent upon the state having a budget surplus (Colorado subsequently voted in 2013 to reintroduce the state EITC once surplus funds were available again). Indiana discontinued their top-up in 2003 and Michigan in 2008.

25. All of the states in our sample report both male and female prisoners, with the sole exception of Alaska, which only reported male prisoners in 2014.

Table 4
Minimum Wage and Recidivism Rates

	1 Year		3 Year	
	Linear (1)	Log (2)	Linear (3)	Log (4)
Min. wage	–0.0091**	–0.0517**	–0.0149***	–0.0869***
	(0.0040)	(0.0225)	(0.0044)	(0.0269)
Wild bootstrap *p*	0.078	0.048	0.009	0.013
Mean recid. rate	0.173		0.346	
Observations	5,786,062	5,786,062	4,749,284	4,749,284

Notes: The dependent variable is return to prison in the same state within one or three years of release (indicated in the column heading). Minimum wage is measured in nominal dollars in the state and month the offender was released. All specifications include state and year fixed effects, as well as the individual and time-varying state-level controls outlined in Section III. State EITC policy is included as a dummy variable, but its coefficient is not shown. See Table 6 for those result stratified by gender. Mean recidivism rates are the mean of the dependent variable for the respective column. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). *p*-values from 1,000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤30 (our analysis is near but not below this threshold). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$ (based on cluster-robust standard errors).

Downloaded from by guest on October 16, 2026. Copyright 2021

released under different minimum wages or EITC policy regimes. The coefficients of principal interest are identified off of the random variation in the month of release, whether that release occurred before or after a raise in the minimum wage or EITC, and how an individual's probability of recidivism compares to other prisoners with similar characteristics. These policies can obviously change after the offender is released, and this may affect their employment, wages, and potential recidivism. To account for this we also consider the average minimum wage in six months and 12 months following release. In the robustness checks, we also consider several alternative specifications to deal with potential endogeneity of minimum wage changes to trends in employment or other macroeconomic conditions in the state. We test specifications addressing state-specific macroeconomic time trends, closer geographic controls, exogenous shifts caused by the federal minimum wage changes, and potential dynamic effects.

IV. Results

We start by presenting the basic relationship between minimum wages or EITC top-up percentages and the probability of recidivism as binned scatterplots, controlling only for state and year fixed effects with a bivariate regression line fit to the plotted bins. Figure 4 is for the state minimum wages. We see a distinct downward trend: high minimum wages appear to be associated with a reduced risk of returning to prison, and while there are clear level differences, the slopes are almost identical for men and

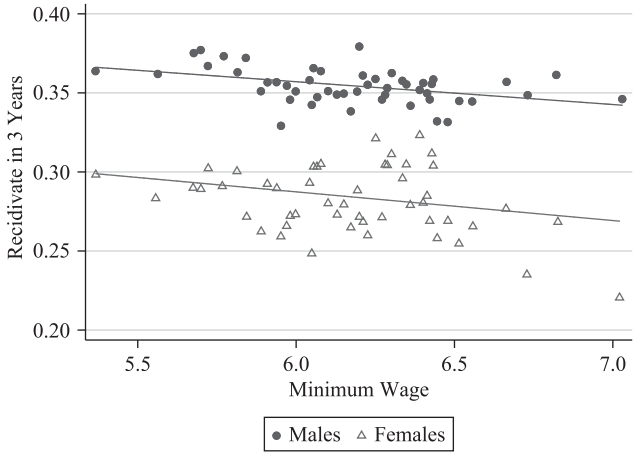

Figure 4

Correlation between Minimum Wage and Recidivism

Notes: To construct the binned scatter plots we plot the probability of recidivism within three years of release (*y*-axis) over the minimum wage (*x*-axis). Observations are demeaned for state and year effects. The observed state–year residual mean rates of recidivism are divided into 50 bins of equal numbers of observations and plotted over the mean minimum wage within each bin. Fifty bins were chosen for symmetry with the subsequent plot of EITC state top-ups (Figure 5). The figure is qualitatively identical plotted with 20 bins. The line shows the best linear fit.

Downloaded from by guest on October 16, 2026. Copyright 2021

women. Figure 5 similarly plots the relationship between recidivism and state EITC top-up percentages. Here we see a slightly different story—there is a negative association for women, but for men the slope of the relationship between the EITC top-up and recidivism is not so strong. While we will explore these relationships with more rigor and controls in later regression analysis, we see the initial story start to emerge. Higher minimum wages are associated with reduced recidivism for both men and women, and EITC top-ups are associated with reduced recidivism for women but not for men.

A. Minimum Wage Results

Table 4 presents our main results from Equation 1 focusing on the minimum wage. All specifications include state and year fixed effects and a vector of controls for both offender and state of release characteristics. The minimum wage is included as its nominal value in Columns 1 and 3, and as its logged nominal value in Columns 2 and 4. We show results for both one-year and three-year recidivism rates. We include EITC as a dummy variable, but its coefficient is not shown.[26]

26. Table 6 shows results for these same regressions run separately by gender, focusing on the EITC coefficient. While there is the possibility of complementarity between minimum wages and EITC policies (Neumark and Wascher 2011), we do not observe a statistically significant interaction effect between minimum wages and the existence of a state EITC top-up on recidivism in this analysis or in the gender stratified results. Results from specifications with an interaction term are not included in the table, but are available on request.

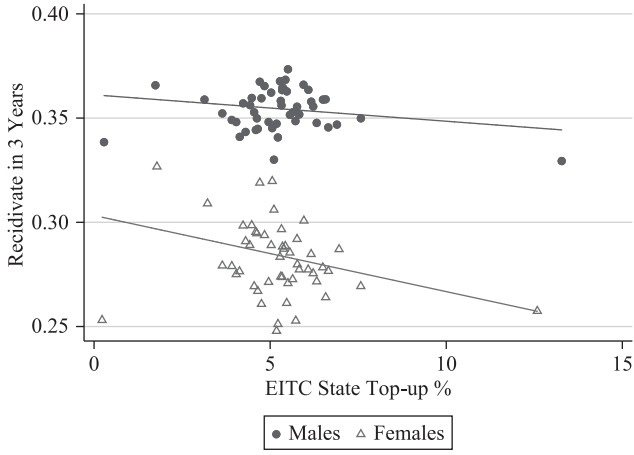

Figure 5

Correlation between State EITC Top-Ups and Recidivism

Notes: To construct the binned scatter plots, we plot the probability of recidivism within three years of release (*y*-axis) over the EITC top-up percent (*x*-axis). Observations are demeaned for state and year effects The observed state–year residual rates of recidivism are divided into 50 bins of equal numbers of observations and plotted over the mean EITC top-up within each bin. Fifty bins were chosen to so that the states without a state top-up (that is, zeros) were not overrepresented as bins. The figure is qualitatively identical plotted with twenty bins. The line shows the best linear fit.

Downloaded from by guest on October 16, 2026. Copyright 2021

The results from Column 1 show that a $1.00 increase in the minimum wage is associated with a 0.91 percentage point *decrease* in the probability an individual returns to prison within one year. The mean probability of returning to prison in one year is 17.3 percent, and the average minimum wage increase is $0.50, implying that the average minimum wage increase is associated with a 2.6 percent decrease in the probability of returning to prison in one year. Results are very similar in magnitude for three-year recidivism rates, with a $1.00 increase in the minimum wage reducing the probability of return to prison within three years by 1.49 percentage points (implying the average minimum wage increase decreases the probability of return to prison in three years by 2.15 percent).

These results imply an elasticity for the probability an individual returns to prison in three years with respect to the minimum wage of roughly –0.28.[27] Results using the log minimum wage (Column 4) imply a similar elasticity of –0.25. To put this in context, Yang (2017) estimates hazard models of return to prison within three years with local labor market conditions and finds an elasticity with respect to low-skill local wages in the county of release of about –0.45. Our estimates as such, are smaller, which makes

27. Using the estimate from Column 2 of Table 4, a $1.00 increase in the minimum wage is associated with a 1.49 percentage point reduction in the probability of return to prison. Over an average minimum wage of $6.50 and a 34.6 percent three-year recidivism rate, this implies that a 15 percent increase in the average minimum wage is associated with a 4.4 percent decrease in the probability of return to prison within three years, or an elasticity of about –0.28.

intuitive sense within our context, as we expect that minimum wages may still have a disemployment effect for some portion of the relevant population.

B. Robustness of Minimum Wage Results

The literature addressing the minimum wage, like much of contemporary labor economics, emphasizes the robustness and specification sensitivity of results. Taking our cues from some of the most prominent papers in the recent literature, in this section we address a variety of plausible concerns regarding the interpretation of our main results on the minimum wage. One concern for our identification strategy is that there are unobserved trends in recidivism rates that are correlated with the implementation of a minimum wage that could bias our results. We employ a variety of specifications to assess potential bias and the sensitivity of our results. We present results these results in Table 5, using both one-year and three-year recidivism rates as an outcome.

For example, if states that implement minimum wage increases are also states that are experiencing decreases in recidivism rates, then this could explain our negative result. If this were the case, this would imply that that *future* minimum wage changes would appear to have a significant association with current recidivism rates (Meer and West 2015). In Column 2 of Table 5 we test this idea by adding in the maximum minimum wage 13–24 months after release minus the minimum wage 12 months after release. This gives us the value of a change in the minimum wage that happens in the second year after release but *not* during the first year after release, so that one-year recidivism is not contaminated by the change.[28] We call this a "future minimum wage change." We see that the coefficient on the future minimum wage change is negative, though small and not statistically significant (and its confidence interval does not contain our main estimate), and the coefficient on the current minimum wage is qualitatively unchanged (if not slightly larger) when adding in the future minimum wage change.

Minimum wage changes could also be endogenous to state-specific trends that also affect recidivism—that is, if states tend to change their minimum wages when economic conditions are are on an up- or a downswing, this could bias our results.[29] The potential for time-varying state or local geography-specific differential trends in economic conditions and how to control for them is the thrust of much of the recent debate in the minimum wage and employment literature (see, for example, Dube et al. 2010; Neumark, Salas, and Wascher 2014; Allegretto et al. 2017; Neumark and Wascher 2017).

In Columns 3–5 of Table 5, as in much of the applied microeconomic literature concerned about differential trends in unobservables across states, we add in state-specific time trends. We consider linear trends, as well as trends with higher-order polynomials. The higher-order polynomials follow the suggestion of Neumark, Salas, and

28. Hence, we only report this in the one-year recidivism results, as with the three-year results it could potentially be contaminated by this change. However, ignoring that concern when we *do* include the future minimum wage change 13–24 months ahead in the three-year recidivism results, we see similarly a small, insignificant coefficient that does not change our main effect.

29. Though the main contention in some of the previous literature regarding minimum wages and teen employment is that state minimum wages have increased when low-skill labor markets were in decline and that this was causing the negative relationship that some papers estimate (Allegretto, Dube, and Reich 2011; Allegretto et al. 2017).

Downloaded from by guest on October 16, 2026. Copyright 2021

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 5
Trend Analysis: One- and Three- Year Recidivism Rates

	Main (1)	Future Minimum Wage (2)	State-Specific Time Trend Polynomials: Linear (3)	2nd (4)	3rd (5)	Division × Year FE (6)	Binding Changes (7)
Panel A: 1 Year Recidivism							
Min. wage	–0.0091**	–0.0109**	–0.0042	–0.0067**	–0.0065**	–0.0082*	–0.0097**
	(0.0040)	(0.0046)	(0.0039)	(0.0029)	(0.0026)	(0.0047)	(0.0048)
Future minimum wage change		–0.0041					
		(0.0028)					
Bound minimum wage							0.0006
							(0.0339)
Min. wage × bound							–0.0006
							(0.0053)
Wild bootstrap *p*	0.059	0.061	0.335	0.053	0.010	0.141	0.071
Observations	5,786,062	5,786,062	5,786,062	5,786,062	5,786,062	5,786,062	5,786,062

(*continued*)

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 5 (*continued*)

	Main (1)	Future Minimum Wage (2)	State-Specific Time Trend Polynomials: Linear (3)	2nd (4)	3rd (5)	Division × Year FE (6)	Binding Changes (7)
Panel B: Three-Year Recidivism							
Min. wage	–0.0149***		–0.0049	–0.0044	–0.0069**	–0.0149***	–0.0162***
	(0.0044)		(0.0039)	(0.0039)	(0.0033)	(0.0054)	(0.0042)
Future minimum wage change							
Bound minimum wage							–0.0247
							(0.0328)
Min. wage × bound							0.0038
							(0.0049)
Wild bootstrap p	0.010		0.231	0.266	0.067	0.028	0.001
Observations	4,749,284		4,749,284	4,749,284	4,749,284	4,749,284	4,749,284

Notes: The dependent variable is return to prison in the same state within one or three years of release as indicated by the panel title. Minimum wage is measured in nominal dollars and is measured in the state and month the offender was released. All specifications include state and year fixed effects, as well as the individual and time-varying state-level controls outlined in Section III. State EITC policy is included as a dummy variable, but its coefficient is not shown. For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1,000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient, as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤30 (our analysis is near but not below this threshold). Results are based off of Table 4 Columns 1 and 3, which are repeated in Column 1 "Main." Additions to these regressions are indicated in the column headers. Future minimum wage change is the maximum minimum wage within the window 13–24 months after release minus the minimum wage 12 months after release; it is meant to measure a future minimum wage change that does not occur within one year of release—hence it is only included in the one-year recidivism results. Bound minimum wage is an indicator for a state–year that was bound by one of the three federal changes in 2007, 2008, or 2009—that is, had a state minimum wage below the new federal level at the beginning of the year. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$ (based on cluster-robust standard errors).

Wascher (2014), who note that recessionary periods can lead to cross-state deviations in employment and labor market conditions that could cause linear time trends to be biased, and that higher-order polynomials may pick up this variation better.[30] Columns 3–5 of Table 5 add in first- through third-order state-specific polynomial time trends. In our analysis of one-year recidivism with linear time trends, the coefficient is smaller and not statistically significant. However, accounting for the potential *nonlinear* time trends via second- and third-order state-specific time trend polynomials, the coefficients are statistically significant and imply that the average minimum wage increase is associated with about a 2 percent decrease in recidivism, smaller than the main results (2.7 percent), but still economically significant.[31] These coefficients are statistically significant when using either cluster-robust and wild bootstrap p-values to account for the potential for too few clusters. The period under study saw two recessionary periods: one very early in the period in 2001 and the Great Recession, which implies that nonlinear trends may be necessary to pick employment and labor market trends appropriately. Our identical analysis of three-year recidivism with time trends is hampered by the limited data window. The coefficients remain negative with linear and second-order time trends, but their precision only achieves statisitical significance when the trend is specified as a third-order polynomial.

In Column 6 we we attempt to control for potential unobserved regional heterogeneity by including census-division-by-year fixed effects, as is common in the panel difference-in-differences literature. This specification implies that geographically close states may be better controls as they are (potentially) hit by similar shocks. Our results with these division-by-year fixed effects are very similar in magnitude. The one-year recidivism results are only significant at the 10 percent level, however (wild bootstrap $p = 0.14$), while the three-year results are significant at the 1 percent level (wild bootstrap $p = 0.03$).

The general concerns addressed above are that the minimum wage changes we see are not exogenous to conditions that may also be affecting recidivism. However, the federal minimum wage changes in 2007, 2008, and 2009 are more plausibly exogenous to state-specific trends and policies. As such, we take a nod from Clemens and Wither (2016) and focus on these federal changes. Some states were bound by these minimum wage increases by virtue of having minimum wages below the new federal levels, and others were not (by having minimum wages already at or above the new federal minimums).[32] While our variable of interest (individual recidivism rates) does not permit an identical triple-difference design, we can similarly treat the the binding status of federal minimum wage laws as a source of variation in state minimum wages that are exogenous to the macroeconomic conditions, trends, and policies in any particular state. We identify state–years

30. There is particular debate in the minimum wage and employment literature about the correct specification for the state-specific time trends; as such we report several versions (Dube, Lester, and Reich 2010; Neumark, Salas, and Wascher 2014; Allegretto et al. 2017; Neumark and Wascher 2017).

31. This pattern mirrors the pattern in the minimum wage and employment literature; see Neumark, Salas, and Wascher (2014).

32. These changes happened during the Great Recession, which also differentially affected states. While state minimum wage increases tend to be correlated with weak economic conditions, the federally bound states in this window, on average, were less negatively impacted by the Great Recession (Clemens and Wither 2016). They control for these differences using an index of housing market prices, a control variable that we employ as well.

Downloaded from by guest on October 16, 2026. Copyright 2021

that experienced minimum wage increases *caused* by the federal increases and compare those to other changes via the following specification:

$$(2)\quad Recidivate_{isct} = \alpha + \beta_1 MinWage_{st} + \beta_2 Bound_{st} + \beta_3 MinWage_{st} \times Bound_{st} + \mathbf{X}_{it} + \mathbf{K}_{ts} + \mathbf{J}_{tc} + \gamma_y + \delta_s + \varepsilon_{isct}$$

Online Appendix E describes how we define *Bound* and identifies the states and years that are bound by the federal increases. In Column 7 of Table 5, we show that states that experienced bound and unbound minimum wage changes experienced similar decreases in recidivism. The coefficient on the interaction term ($MinWage_{st} \times Bound_{st}$) is small and not statistically significant in our analysis of both our one- and three-year recidivism. This indicates that states that experienced plausibly exogenous increases in their minimum wages also experienced similar decreases in recidivism.

Online Appendix Table G.1 employs some additional robustness checks on our results. For our main analyses, we controlled for the general macroeconomic conditions in a state via the housing price index. A more natural control might be state unemployment rates, but this could be directly impacted by the minimum wage. Nevertheless, in Online Appendix Table G.1, Column 1, we add the state unemployment rate to our main specification and see that this does not change our conclusions about the association of minimum wage changes with reduced recidivism. Individuals may have different probabilities of committing a crime conditional on the minimum wage when they committed the crime associated with their first observed incarceration. Further, minimum wage changes may be correlated across time. This opens the possibility of selection into who we see in prison based on initial minimum wages that we are picking up as an effect of minimum wages at release. Instead, to test, and better control for, this possibility, in Column 2 of Online Appendix Table G.1 we control for the minimum wage at prison entry and the minimum wage one year before prison entry.[33] The coefficients on the minimum wages at admission and the year before admission are small and statistically insignificant, while the coefficients on the minimum wage and EITC at time of release remain qualitatively unchanged. Finally, in Columns 3 and 4 of Online Appendix Table G.1 we consider average minimum wages six and 12 months after release to account for the fact that the minimum wage can change after an offender is released. We see qualitatively similar, if not slightly larger, coefficients when using this alternative independent variable.

1. Dynamic Labor Market Effects

While focusing on recidivism relieves our model of some of the difficulties of precisely measuring the elasticities of labor demand, there are considerations in the minimum wage literature that merit attention. In addressing the dynamics of low-skill labor markets, Meer and West (2015) note that the disemployment effects of minimum wage policies are less likely to show up immediately in the form of job separations, more likely to show up as foregone growth, and that narrow post-policy change treatment windows can obfuscate underlying disemployment effects. To address this, we include a set of

33. Unfortunately, the data do not have the date the crime was committed, so these dates are used to proxy the date the crime was committed.

Downloaded from by guest on October 16, 2026. Copyright 2021

one-, two-, and three-year lags of the state minimum wages to the model specification in Online Appendix Table G.2. When included as singular right-hand-side variables, the coefficients on the one-, two-, and three-year lags are also negative, though not statistically significant. When all are included with the concurrent minimum wage, the coefficient on the concurrent minimum wage remains negative and of a similar magnitude ($p < 0.10$), consistent with our primary analysis. While simple inclusion of lagged minimum wage covariates does not replicate the nuance of the Meer–West model, it does suggest that our observed dominance of wage effects over disemployment effects is unlikely to be an artifact of unaccounted for future growth stagnation.

We similarly include the lagged availability of a state EITC in Online Appendix Table G.2. One of the benefits of this specification is that it allows us to better accommodate the delayed benefit structure of any tax credit system. We observe not only large and significant effects of the one-, two-, and three-year lags of the EITC, but also their dominance of the current EITC in the estimation of one-year recidivism rates. A similar pattern is observed for the three-year recidivism rate, though the impact of the current EITC retains greater impact alongside the three lagged covariates (Online Appendix Table G.3). Each of the lagged EITC covariates produced coefficients very similar to the coefficients on the current values in our main specification.

Downloaded from by guest on October 16, 2026. Copyright 2021

2. Additional County-Level Analysis

While county of conviction is a less precise proxy for county of residence post-incarceration than state of conviction is for state of residence, it is nonetheless sufficiently reliable to merit use in additional robustness tests that leverage this proxy (see Online Appendix C for details). Results for these additional robustness test are in Online Appendix Table D.1, showing both one- and three-year recidivism results. In Column 1, we recreate our main results from Table 4 with only observations that are not missing county of conviction (96.4 percent of the sample), which look broadly similar to our main estimates. In Column 2, we add in the county unemployment rate as a way of controlling for local economic conditions at the time of release, and in Column 3, we add in county fixed effects, both showing broadly similar results.

Between 2000 and 2014 there are 18 municipalities that have a minimum wage that is above the state's minimum wage.[34] Using counties rather than simply states could allow us to leverage these variations. However, this represents only 0.2 percent of our data, about 14,000 observations.[35] Recall that we have data at the county level—some substate minimum wages are in fact those of subcounty municipalities and below our level of data granularity. In Online Appendix Table D.1, we try two different strategies for dealing with this. Column 4 drops any jurisdiction with a substate minimum wage. In Column 5, the substate minimum wage is assigned to everyone in the county, even if only one city within the county had this. The resulting coefficients are unchanged from

34. Data on substate minimum wages are also from Vaghul and Zipperer (2016). Data are only available for 2004–2015 as 2004 is the earliest substate minimum wage above a state that the authors documents. These localities are Albuquerque, NM; Bangor, ME; Berkeley, CA; Fayette County, KY; Jefferson county, KY; Johnson County, IA; Las Cruces, NM; Montgomery County, MD; Portland, ME; Prince George's County, MD; San Francisco County, CA; San Jose, CA; Santa Fe, NM; Seatac, WA; Seattle, WA; and Tacoma, WA.

35. Note also that several of these municipalities are in California, which is not included in our results.

Downloaded from by guest on October 16, 2026. Copyright 2021

our primary specification, perhaps unsurprisingly given how few observations these localities represent.

The state-specific time trend analysis, discussed earlier, requires parametric assumptions about the trajectory of unobservable conditions across states. A less parametrically reliant approach to control for time-varying area-specific shocks is to use geographically close controls. For example, the approach used in Dube, Lester, and Reich (2010) takes advantage of discontinuities offered by state borders to compare outcomes of minimum wage increases in one county to another county just across a state border that does not experience such an increase. This approach relies on the assumption that geographically close counties experience very similar shocks and only experience different minimum wages due to the state border between them.[36] This approach also seriously taxes our power, since we have to subset to only individuals convicted in counties that are near a state border (which are also potentially counties where county of conviction is a less reliable predictor for county of residence). Nonetheless, in Column 6 of Online Appendix Table D.1, we restrict our analysis to pairs and triads of counties that straddle state borders and include county-cluster fixed effects to control for shocks common to three counties in a cluster straddling a state border.[37] For both the one-year and three-year recidivism result, the coefficient implies that a $1.00 increase in the minimum wage is associated with an approximately 0.7 percentage point decrease in recidivism and is marginally statistically significant with the cluster-robust standard errors even in the limited data we have in border counties.

C. EITC Results

In Table 6, we show results for the EITC (and minimum wage) fully stratified by gender. Analysis within the gender subsamples, rather than with the addition of a gender interaction term, ensures that the regression specification accounts for how gender relates to not just our primary variables of interest, but also the entire vector of policy, cultural, and political contexts accounted for as controlling covariates. Perhaps most importantly, stratifying by gender ensures that coefficient on the EITC indicator is identified solely off of within-state policy variation and not off of the between-state differences in outcomes for women or men that may reflect countless gender-related determinants of recidivism. Panel A uses the existence of a state top-up to the federal EITC as the treatment; Panel B uses the level of the top-up. The results imply that for *women* the existence of a state EITC decreases recidivism, though this total effect is only significant for three-year recidivism. We interpret this delay in impact to be a function of the lag of tax filing and issuing of government refunds, as the earliest the IRS expects EITC refunds to be available is the first week of March each year.[38]

For three-year recidivism, the results indicate that the availability of a state top-up to the federal EITC decreases the probability a woman returns to prison in three years by

36. We note that Neumark and Wascher (2017) critique this approach, arguing that the data does not always support that contiguous counties across state borders are the best controls and that other policies that affect outcomes may also vary across the border.

37. See Online Appendix D for details about the construction of these county clusters.

38. Actual receipt will depend on timing of tax filing. https://www.irs.gov/individuals/refund-timing (accessed February 10, 2023).

Table 6
State EITC and Minimum Wage Effects on Recidivism Rates by Gender

	Female		Male	
	1 Year (1)	3 Year (2)	1 Year (3)	3 Year (4)
Panel A: Any State EITC				
State EITC	0.0003	–0.0203***	0.0070	–0.0017
	(0.0055)	(0.0064)	(0.0055)	(0.0071)
Min. wage	–0.0105*	–0.0161**	–0.0091**	–0.0149***
	(0.0057)	(0.0065)	(0.0039)	(0.0044)
Min. wage coeff. wild bootstrap p	0.131	0.047	0.062	0.011
EITC coeff. wild bootstrap p[a]	0.957	0.054	0.282	0.842
Panel B: State EITC Percent				
State EITC percent	–0.0006	–0.0017	0.0008	–0.0002
	(0.0010)	(0.0011)	(0.0007)	(0.0008)
Min. wage	–0.0105*	–0.0160**	–0.0091**	–0.0149***
	(0.0056)	(0.0064)	(0.0039)	(0.0044)
Min. wage coeff. wild bootstrap p	0.117	0.047	0.061	0.004
EITC coeff. wild bootstrap p[a]	0.650	0.261	0.350	0.843
Mean recid. rate	0.142	0.284	0.177	0.355
Observations	680,826	551,211	5,105,236	4,198,073

Notes: The dependent variable is return to prison in the same state within one or three years of release (indicated in the column heading). Minimum wage is measured in nominal dollars. State EITC is an indicator for the existence of a state top-up. State EITC percent is the percent of the federal EITC available to those in that state measured in percentage points. All are measured in the state and month the offender was released. All specifications include state and year fixed effects, as well as the individual and time-varying state-level controls outlined in Section III. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1,000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the EITC coefficients, as suggested by Cameron et al. (2008) when the number of clusters is small. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

[a]To address the smaller number of treated clusters for EITC estimates, the EITC wild boostrap errors are estimated at the state–year subcluster (MacKinnon and Webb 2018; Roodman et al. 2019).

about two percentage points (7 percent), and this is statistically significant at the 1 percent level (wild bootstrap $p < 0.05$). Panel B uses the magnitude of the state EITC top-up into Equation 1 instead of simply whether a state top-up exists. The results indicate that a one percentage point increase in the EITC top-up reduces three-year recidivism by 0.17 percentage point for women (0.55 percent). Within the context of our sample (mean state top-up = 5.2 percent, SD = 9.3), a one standard deviation increase in the state EITC corresponds to a 5.1 percent reduction in the expected recidivism rate for

Downloaded from by guest on October 16, 2026. Copyright 2021

women. It is important to note, however, that the precision of this coefficient is below the threshold of statistical significance. The estimated effect of the EITC on three-year recidivism by men is zero, albeit with considerable imprecision (Column 4). In Column 3, there is suggestive evidence that the existence of an EITC top-up is increasing one-year recidivism for men by around 3 percent based on the point estimate, though this is not statistically significant and is difficult to interpret given the delay in EITC refund receipt.

D. Subcategories: Returning Offense, Education, and Race

In Tables 7 and 8, we show results for the minimum wage and availability of a state EITC (for women only, given results above) by return crime type, education level, and race/ethnicity.[39]

One potential mechanism for the reduction in recidivism is that increased wages lead returning prisoners to commit fewer crimes associated with income generation, such as property crimes or selling illegal drugs. Table 7 reports results for our main minimum wage results by the type of crime an offender was returned to prison for committing: violent, property, drug, or other crime for both one- and three-year recidivism.[40] We see here that higher minimum wages are significantly associated with decreases in property and drug crimes, but not violent and other crimes. These decreases are between 5–9 percent of the baseline means. These results continue to support the conclusion that the wage effects of the minimum wage dominate any employment effects—high minimum wages do not reduce "crimes of passion" but do reduce potentially income-generating crimes.

We similarly observe mixed results with the EITC. For women, the availability of a state EITC is associated with declines in property crimes (by around 10 percent, marginally significant with cluster-robust p-values but not with the wild bootstrap p) and "other" crimes (13 percent). We note, however, that we also see significant declines in re-incarceration for "other" crimes for men within three years in Online Appendix Table G.4. We also see a suggestive positive increase in returns for drug crimes within one year, though the lags on EITC refund receipt advise caution when interpreting effects one-year recidivism. While the results for men in Online Appendix Table G.4 imply the EITC is associated with an increase in violent for men in their first year after release, this result largely disappears for three-year recidivism. A similarly positive effect on reincarceration for drug crimes, however, is observed in both the one- and three-year results for men. This could suggest that crowd-out effects of the EITC on males, or, given that the threshold quantity of narcotics possession sufficient to result in a return to prison is smaller for those on early-release, could simply reflect greater cash on hand for consumption purchasing.

Given that minimum wages and EITCs affect mainly low-skill labor, we may expect these results to be stronger for returning prisoners with less education. Table 7 reports minimum wage results from different subsamples based on the highest level of education completed by the released prisoner, for both one- and three-year recidivism rates.

39. Online Appendix Table G.4 reports results for men.

40. Other crimes are primarily composed of weapons violations and DUI/DWI, as well as missing offenses and various other types of crimes.

Downloaded from by guest on October 16, 2026. Copyright 2021

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 7
Outcome Differentials by Returning Crime, Race and Ethnicity, and Education

	1 Year				3 Year			
	Violent (1)	Property (2)	Drug (3)	Other (4)	Violent (5)	Property (6)	Drug (7)	Other (8)
Min. wage	–0.0004	–0.0049***	–0.0032**	–0.0007	–0.0009	–0.0064***	–0.0057***	–0.0019
	(0.0011)	(0.0012)	(0.0015)	(0.0013)	(0.0015)	(0.0017)	(0.0018)	(0.0015)
Wild bootstrap *p*	0.784	0.002	0.075	0.616	0.613	0.003	0.027	0.227
Mean recid. rate	0.032	0.057	0.047	0.036	0.062	0.113	0.103	0.069
Observations	5,786,062	5,786,062	5,786,062	5,786,062	4,749,284	4,749,284	4,749,284	4,749,284
	Education							
	<HS (9)	HS (10)	>HS (11)		<HS (12)	HS (13)	>HS (14)	
Min. wage	–0.0150***	–0.0094*	–0.0104		–0.0210***	–0.0156**	–0.0141	
	(0.0049)	(0.0054)	(0.0079)		(0.0054)	(0.0061)	(0.0088)	
Observations	2,245,904	1,824,652	317,609		1,907,947	1,472,633	258,196	
Wild bootstrap *p*	0.032	0.184	0.317		0.005	0.048	0.228	
Mean recid. rate	0.179	0.178	0.148		0.363	0.349	0.299	

(*continued*)

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 7 (*continued*)

	Race/Ethnicity					
	Black (15)	White (16)	Hispanic (17)	Black (18)	White (19)	Hispanic (20)
Min. wage	–0.0114**	–0.0080*	–0.0050	–0.0151**	–0.0122**	–0.0117**
	(0.0051)	(0.0041)	(0.0046)	(0.0060)	(0.0048)	(0.0055)
Observations	2,457,794	2,471,327	696,338	2,062,432	1,998,305	561,803
Wild bootstrap *p*	0.059	0.097	0.441	0.034	0.033	0.200
Mean recid. rate	0.178	0.168	0.156	0.370	0.331	0.303

Notes: In Columns 1–8 the dependent variable is return to prison for a certain crime type within one or three years of release (as indicated by column headers). Columns 9–20 are one- and three-year rates of returning to prison within subsamples based on either education level or race/ethnicity. >HS means any college, not necessarily a college degree. Minimum wage is measured in nominal dollars in the state and month the offender was released. Mean recidivism rates are the mean of the dependent variable for the respective column. All specifications include state and year fixed effects, as well as the individual and time-varying state-level controls outlined in Section III. State EITC policy is included as a dummy variable but its coefficient is not shown. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). *p*-values from 1,000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient, as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤30 (our analysis is near but not below this threshold). Outcome heterogeneity by gender is examined in Table 6. *$p<0.10$, **$p<0.05$, ***$p<0.01$ (based on cluster-robust standard errors).

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 8
State EITCs and Outcome Differentials for Female Offenders by Returning Crime, Race and Ethnicity, and Education

	1 Year				3 Year			
	Violent (1)	Property (2)	Drug (3)	Other (4)	Violent (5)	Property (6)	Drug (7)	Other (8)
State EITC	0.0008	–0.0010	0.0034*	–0.0029*	–0.0007	–0.0104*	–0.0019	–0.0073***
	(0.0013)	(0.0044)	(0.0019)	(0.0015)	(0.0015)	(0.0058)	(0.0039)	(0.0019)
Wild bootstrap p[a]	0.590	0.825	0.129	0.180	0.747	0.188	0.729	0.048
Mean recid. rate	0.013	0.051	0.047	0.031	0.025	0.105	0.098	0.055
Observations	680,826	680,826	680,826	680,826	551,211	551,211	551,211	551,211
	Education							
	<HS (9)	HS (10)	>HS (11)		<HS (12)	HS (13)	>HS (14)	
State EITC	0.0002	–0.0025	–0.0277**		–0.0237**	–0.0228**	–0.0355***	
	(0.0072)	(0.0078)	(0.0107)		(0.0109)	(0.0105)	(0.0120)	
Wild bootstrap p[a]	0.987	0.794	0.055		0.197	0.151	0.045	
Mean recid. rate	0.151	0.137	0.115		0.303	0.275	0.241	
Observations	247,801	211,701	48,929		211,210	168,840	38,925	

(*continued*)

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 8 (*continued*)

	Race/Ethnicity					
	Black (15)	White (16)	Hispanic (17)	Black (18)	White (19)	Hispanic (20)
State EITC	0.0014	–0.0002	0.0137	–0.0101	–0.0268***	–0.0147
	(0.0071)	(0.0076)	(0.0227)	(0.0079)	(0.0095)	(0.0290)
Wild bootstrap p[a]	0.876	0.977	0.679	0.396	0.092	0.712
Mean recid. rate	0.135	0.141	0.149	0.284	0.278	0.289
Observations	219,083	380,130	53,340	187,962	299,333	41,998

Notes: Only female offenders are included in this table. In Columns 1–8 the dependent variable is return to prison for a certain crime type within one or three years of release (as indicated by column headers). Remaining columns the dependent variable is return to prison within one or three years of release (as indicated in column headers). Columns 9–20 are subsample results based on either education level or race/ethnicity. >HS means any college, not necessarily a college degree. State EITC is an indicator for the existence of a state top-up and is measured in the state and month the offender was released. Mean recidivism rates are the mean of the dependent variable for the respective column. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1,000 wild-cluster bootstrap iterations are shown as well, as suggested by Cameron et al. (2008) when the number of clusters is small. *$p<0.10$, **$p<0.05$, ***$p<0.01$ (based on cluster-robust standard errors).

[a]To address the smaller number of treated clusters for EITC estimates, the EITC wild boostrap errors are estimated at the state–year subcluster (MacKinnon and Webb 2018; Roodman et al. 2019). Results for men can be found in Online Appendix Table G.4.

We find that the observed reduction in recidivism under higher minimum wages is similar across education subsamples, particularly for men, though the effect is not statistically significant for those with more than a high school education (who constitute a much smaller subsample). This may not be surprising—returning prisoners with higher education are still entering the labor market with limited or interrupted work experience, and their educational credentials may not carry the same strong labor market signal value. Total effects of the minimum wage are similarly negative for women, though it is worth noting that the effect diminishes with education faster than it does for men. The negative impacts of the EITC on three-year recidivism for women are present and consistent across education levels.

Finally, we consider whether our results vary by race. Table 7 shows results that are qualitatively similar to our base results, but with some variation by race and ethnicity. The negative impact of the minimum wage on returns to prison exists across race and ethnicity though is not statistically significant at conventional levels for Hispanic released prisoners when considering one-year recidivism rates. The negative impact of the EITC for women when considering three-year recidivism rates is broadly consistent across race and ethnicity as well, though only statistically significant for white females. This pattern may be simply a product of differing sample sizes; our sample includes 380,130 white women versus only 53,340 Hispanic women. Similar to our observation about violent crime, we observe a positive relationship between state EITC top-ups and one-year returns to prison for Black and Hispanic male released prisoners, though the effect drops out for three-year returns of Black males, as shown in Online Appendix Table G.4. This could suggest that crowd-out effects of the EITC are more substantive for minority males.

V. Exploratory Tests of Employment Mechanisms

If the employment prospects of individuals with criminal records are the main mechanism through which the minimum wage impacts recidivism, our results imply that the wage effect (drawing individuals into the labor market) dominates the disemployment effects of the minimum wage for this group. Optimal assessment of this hypothesis demands that we not just observe effects on recidivism, but their employment status as well. Unfortunately, we do not have labor market outcomes for the individuals in the NCRP.[41]

An alternative avenue for exploring the plausibility of the employment mechanism is to turn to a larger survey and explore the impact of the minimum wage on subgroups of individuals with a higher likelihood of having served time in prison or otherwise having a felony criminal record. The previous minimum wage literature has principally focused on estimating impacts on employment (and wages and hours) for teenagers in the Current Population Survey (CPS). We similarly use the CPS to understand how these effects vary for subgroups likely to have a criminal record. Data on exactly the proportion of people who have criminal records by various demographic characteristics is

41. Data linking labor market outcomes to criminal records are not yet readily accessible, particularly at a national scale.

Downloaded from by guest on October 16, 2026. Copyright 2021

Downloaded from by guest on October 16, 2026. Copyright 2021

somewhat difficult to come by, though several authors have tried. Shannon et al. (2017) estimate that 15 percent of the Black, male population in the United States has been to prison and 33 percent of them have a felony conviction. Bucknor and Barber (2016) estimate that 39–44 percent of Black males have a conviction, whereas only 9–10 percent of white males do, and only about 1 percent of all females do. Among males they estimate that only around 4 percent of the male population with any college has a conviction, whereas 16–18 percent of those with a high school degree and 60–68 percent of those with no high school degree do. So to pinpoint a population likely to have a criminal conviction, we consider the broader category of prime-age Black men with no postsecondary education and estimate the effect of minimum wages on their employment in analysis similar to the previous literature on teenagers. We then use the November supplement of the CPS to try to pinpoint a smaller subgroup even more likely to contain individuals with records: those who say they did not register to vote due to being "ineligible."

To most closely adhere to the previous literature, we start by replicating analysis in Allegretto, Dube, and Reich (2011) and Neumark, Salas, and Wascher (2014) for teenagers. We use data from the CPS 2020 outgoing rotation groups from 1990–2016 and merge in unemployment rates by state and month from the Local Area Unemployment Statistics (LAUS) (nonseasonally adjusted) and minimum wages by state and month from Vaghul and Zipperer (2016). We estimate linear probability models of employment with controls for age, education, gender, race, marital status, state, and year–quarter fixed effects. We then add in the time trends and other fixed effects that have been central to the debate in this literature.

The results of replicating their specifications for teenagers in the CPS data are presented in Panel A of Table 9, generating qualitatively similar results—the Allegretto, Dube, and Reich (2011) specifications (Columns 2–4) report effects not statistically distinguishable from zero, while the Neumark, Salas, and Wascher (2014) specifications (Columns 5–7) with polynomial time trends report a negative effect on employment.[42] Confident that we are using similar specifications, in Panel B, we run the same regressions but restricting the sample to low-skill (high school degree or less) Black men ages 24–55, a group with a relatively high probability of having a criminal record.[43] Both the Allegretto, Dube, and Reich (2011) and Neumark, Salas, and Wascher (2014) specifications yield consistently *positive* effects of minimum wages on employment. Four of the seven coefficient estimates are statistically significant (as with the teen employment literature, the level of statistical significance is sensitive to the specification). When we run the same regressions for a group that is similar, but much less likely to have a criminal record, low-skill Black women age 24–55, the estimated effects are noisier, have inconsistent signs, and fall well short of statistical significance. In Online Appendix H we re-run these specifications with other groups not likely to have a criminal record, such as college-educated Black men, low-skill white men, and low-skill white women. For all groups the elasticities are noisy, and often negative. This gives us greater confidence that the positive results in Panel B are not due to spurious correlations.

42. For a direct comparison with this previous literature, see Online Appendix H.1.

43. Twenty-four and 55 represent the tenth and 90th percentile, respectively, of age at release in the NCRP data.

Downloaded from by guest on October 16, 2026. Copyright 2021

Table 9

Minimum Wage Effects on Employment for Different Subpopulations: CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Teenagers 15–19, CPS 1990–2009							
ln(MW) coeff.	–0.050**	–0.041	–0.039	–0.028	–0.019	–0.090***	–0.086***
	(0.021)	(0.034)	(0.031)	(0.025)	(0.037)	(0.027)	(0.026)
Elasticity	–0.121	–0.100	–0.094	–0.068	–0.045	–0.218	–0.209
Observations	447,719	447,719	447,719	447,719	447,719	447,719	447,719
Panel B: Low-Skill Black Men 25–54, CPS 1990–2016							
ln(MW) coeff.	0.106**	0.028	0.093**	0.053	0.115**	0.075*	0.050
	(0.045)	(0.049)	(0.039)	(0.050)	(0.046)	(0.043)	(0.042)
Elasticity	0.156	0.042	0.136	0.078	0.169	0.110	0.074
Observations	104,020	104,020	104,020	104,020	104,020	104,020	104,020
Panel C: Low-Skill Black Women 25–54, CPS 1990–2016							
ln(MW) coeff.	0.098**	0.034	0.025	–0.016	0.043	0.051	–0.003
	(0.046)	(0.032)	(0.048)	(0.043)	(0.056)	(0.055)	(0.065)
Elasticity	0.164	0.056	0.041	–0.027	0.071	0.086	–0.004
Observations	129,329	129,329	129,329	129,329	129,329	129,329	129,329
Div. × Quarter FE	N	Y	N	Y	N	N	N
Linear trends	N	N	Y	Y	N	N	N
Quadratic trends	N	N	N	N	Y	N	N
Cubic trends	N	N	N	N	N	Y	N
Quartic trends	N	N	N	N	N	N	Y

Notes: Data from the Current Population Survey Outgoing Rotation Groups, population stratification, and years are indicated in the panel titles. Panel A focuses on teenagers (15–19 years old), the targeting population from Allegretto et al. (2011) and Neumark et al. (2014). Panels B and C focus on "low-skill" Black men and women, with low skill here indicating the absence of postsecondary education. Each cell is a different regression. Elasticities are calculated by dividing the coefficient by the mean employment rate for the relevant population. Controls included in all regressions: age, nonseasonally adjusted unemployment rate, marital status, education, race/ethnicity, gender, quarter FE, state FE, and additional trends or FE as noted (in Panel A all specifications also include proportion of population aged 15–19 as in Allegretto et al. 2011). Column 1 provides a baseline estimation. Columns 2–4 replicate the key specifications of Allegretto et al. (2011). Columns 5–7 include different polynomial time trends, replicating the key specifications from Neumark et al. (2014). Regressions are weighted using the person-level weight *wtfinl*. Standard errors clustered on state in parentheses. *$p<0.10$, **$p<0.05$, ***$p<0.01$.

Downloaded from by guest on October 16, 2026. Copyright 2021

An alternative means for restricting the analysis to individuals likely to have a criminal record comes from the CPS November Supplement on Voting and Registration, run every in two years from 2004–2016. For those who respond that they did not register to vote, there is a question about the primary reason an individual did not register and one option is "not eligible to vote." For U.S. citizens of voting age, there are few reasons that would render one ineligible beyond criminal record status. Thus, we use this response as a proxy for having a criminal record.[44] For 2004–2016, there are 5,934 individuals who say they are citizens over the age of 18 where the primary reason they did not register is ineligibility, and thus they likely have a record of some kind.[45] The November supplement also includes questions about employment and basic demographic characteristics.

Table 10 reports results from specifications similar to our previous CPS analysis using only the voter supplement subsample. Panel A again recreates an analysis in the style of of Allegretto, Dube, and Reich (2011) to verify that the November supplement data are not dramatically different from the main CPS data.[46] There are some differences again likely due to this not being a strict replication, using a different time period and only November data.[47] Panel A rather serves as a coarse proof-of-concept that the November CPS produces comparable effects for teens.[48]

Panel B of Table 10 reports estimates of effects of the minimum wage on employment for individuals with a high likelihood of having a criminal record as proxied by stating they are ineligible to vote. Here, as for low-skill Black men generally, we observe *positive* coefficients on the minimum wage: the coefficient in Column 1 implies that a 1 percent increase in the minimum wage is associated with a 0.0019 percentage point increase in the probability of employment for people with likely criminal records, an implied elasticity of 0.37. While none of the coefficients are significant at conventional levels, this analysis is quite under-powered, with only 5,000 individuals over ten years, and in Column 1 the p-value on the coefficient on log minimum wage is $p = 0.13$. The coefficients retain their magnitudes, but become less precise as we add in additional controls. This imprecision is unsurprising given that we are, again, estimating from 5,934 observations total. This result, nonetheless, remains consistent with our previous subsample analysis of the CPS.[49]

44. Note, after coming up with this idea we were alerted to a recent paper by Congdon-Hohman (2018) using this strategy to study the employment impacts of Ban the Box. In the interest of space, we direct the reader to his paper for summary statistics about this group, including the fact that the group that lists themselves as ineligible is disproportionately male and minority.

45. We use over age 18 to avoid any issues with those right around 17/18, indicating they did not register due to ineligibility due to age. Note the question asks for the *primary* reason, so even someone ineligible due to a criminal record may have listed something else.

46. We use year rather than quarter fixed effects since we only have data from November.

47. Our coefficient in Column 1 is bigger than theirs (–0.084 vs. –0.047). We also find that state-specific time trends reduce the coefficient significantly and render it statistically insignificant. Unlike Allegretto et al. (2011), however, the division-by-year fixed effects do not make the negative employment result go away.

48. Due to only having data from November every other year, we do not include the higher-order polynomial time trends as the data are already fairly sparse, and thus for this specific example we are unable to test whether the addition of these polynomials would bring back the negative and significant coefficient on employment as in Neumark et al. (2014).

49. For the sake of comparison and as an additional test of the concept, in Panel C of Table 10 we run the same regression as in Panel B on citizens of voting age but who do not indicate they are ineligible to vote. This is a group for which the minimum wage is unlikely to be directly salient (which is why the previous literature does not focus on them, but rather on teenagers). Moreover, as expected, we find very small, somewhat imprecise coefficients on the log minimum wage on employment.

Table 10
Minimum Wage Effects on Employment by Voting Eligibility: CPS Data

	(1)	(2)	(3)	(4)
Panel A: Teenagers 15–19, CPS November Supplement				
Ln(MW) coeff.	–0.084***	–0.084**	–0.015	0.012
	(0.028)	(0.039)	(0.036)	(0.045)
Elasticity	–0.229	–0.230	–0.040	0.032
Observations	101,105	101,105	101,105	101,105
Panel B: Ineligible to Vote				
Ln(MW) coeff.	0.191+	0.154	0.184	0.158
	(0.125)	(0.162)	(0.143)	(0.184)
Elasticity	0.363	0.292	0.350	0.301
Observations	5,934	5,934	5,934	5,934
Panel C: Not Ineligible to Vote				
Ln(MW) coeff.	–0.008	0.003	–0.013	–0.003
	(0.011)	(0.013)	(0.009)	(0.014)
Elasticity	–0.016	0.006	–0.024	–0.005
Observations	644,516	644,516	644,516	644,516
Div. × Year FE	N	Y	N	Y
Linear trends	N	N	Y	Y

Notes: Data from the November Current Population Survey (1990–2016; voting eligibility only available from in even years [federal election years] for 2004–2016). Dependent variable is employed (not including self-employment). Each cell is a different regression. Regressions in Panel A are meant to mimic Allegretto et al. 2011, including the sample restriction to teenagers (15–19 years old). In Panel B, we focus on citizens of voting age who say they are ineligible to vote; this is our proxy for having a criminal record. In Panel C, we run the same regression on citizens of voting age who do not say they are ineligible vote. Controls included but not shown are: gender, race, age, education, marital status, state unemployment rate, state fixed effects, year fixed effects, and time trends or division × year effects as indicated. Standard errors clustered on state in parentheses. $+p<0.15$, $*p<0.10$, $**p<0.05$, $***p<0.01$.

Downloaded from by guest on October 16, 2026. Copyright 2021

The results from these analyses provide suggestive evidence for the plausibility of a direct employment mechanism. Higher minimum wages can lead to a net increase in employment for people with records, the positive supply effects of higher wages sufficient to dominate the potential disemployment effects for this population, and in turn reducing the probability these individuals end up back in prison. There are, of course, alternative explanations for these observed relationships, particularly with regard to Black male employment. There is evidence that employers respond to higher minimum wages by substituting more experienced or skilled workers for young, novice workers (Jardim et al. 2022; Clemens, Kahn, and Meer 2021). This could explain, at least in part, the positive effect we are observing for employment of Black males closer to middle age

in our CPS sample. We are unable to run a "horse race" between these alternative explanations in either data set, given their respective lack of employment data (in the NCRP) or criminal histories (in the CPS). In the future, we hope that data linking people with criminal records to their locations and employment outcomes could be used to better strengthen these suggestive results.

VI. Conclusion

We exploit changes in minimum wage laws and state EITCs to estimate the impact of these policies on the probability recently released prisoners return to prison. Using records on nearly six million offenders released between 2000 and 2013 and admissions through the end of 2014, we find that, on net, higher minimum wages decrease recidivism. These results suggest that while increases in the minimum wage may potentially reduce labor demand among the population of individuals with criminal records, negative employment effects are dominated by the labor–crime substitution effects of increased wages relative to potential criminal earnings. Exploratory analysis of CPS data, replicating prior estimates of employment effects of the minimum wage from the literature but on respondents more likely to have a felony record, suggests the possibility of net positive employment effects of minimum wages on employment for those more likely carrying a criminal record.

We find some evidence that EITC wage subsidies reduce recidivism, as well, but only for women. For men, we have suggestive evidence that the EITC may be increasing recidivism, though the coefficients tend to be noisy, and we cannot rule out decreases as well. In light of the uniform effects of minimum wages across gender, we believe this gender-specificity of the EITC is a by-product of its emphasis on subsidizing the wages of custodial parents, an outcome analogous to the marked salience of the EITC to single mothers (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001). The exclusion of men without children (and fathers without custody) serves as a mechanism to exclude the bulk of men released from prison from the predicted positive (recidivism reducing) wage and employment effects. And labor market crowd-out effects could actually imply worse labor market outcomes for low-education men in response to higher EITCs. Disaggregating the underlying employment and wage effects of the EITC for men and women, as well as parents and nonparents, leaving prison will require future research and, likely, more detailed data on the familial standing of released prisoners.

Our main results from Table 4 allow us to make some back-of-the-envelope policy comparisons between the EITC and the minimum wage, at least for predicted reductions in female recidivism rates. Our models estimate that a $0.50 increase in the minimum wage corresponds to the same reduction in the rate of female three-year recidivism as a five percentage point increase in the state EITC top-up. While the typical minimum wage employee is both young and engaged in part-time employment, for the sake of comparison, a full-time employee working 2,000 hours per year would earn an additional $1,000 after a $0.50 wage increase. In 2015, a married couple with children and yearly household income between $14,000 and $23,750 could receive the maximum EITC benefit of $5,572, while the mean EITC recipient household received $3,186 (Center on Budget and Policy Priorities 2016). Given that it is awarded as a percent of

Downloaded from by guest on October 16, 2026. Copyright 2021

the federal credit, a state top-up of five percentage points would provide the average household with an additional $159 a year, up to a maximum of $279. In other words, $159 per year in additional income via a five percentage point increase in a state EITC corresponds to the same expected reduction in female three-year recidivism as $1,000 worth of additional (full-time) income via a $0.50 increase in the minimum wage.[50] Note, however, that the EITC operates through both an increase in available income (after filing taxes) but also as an incentive to join the legal labor market to reap these rewards. We can do little more than speculate on the differing wage policy effects on male recidivism, save that these results further speak to the potential gains to be had from expanding access to the EITC for individuals who are not custodial parents.

Our analysis and results are, of course, characterized by several important limitations. Importantly, we cannot directly link released prisoners to their labor market outcomes, hence we can only posit that our results reflect impacts of labor market opportunities. Exploratory results from the CPS, however, do find that higher minimum wages suggestively increase employment for a group likely to have records: prime-age, low-education Black men and U.S. citizens over age 18 who claim they are ineligible to vote. We are unable, however, to connect our results to characteristics of an individual's household, family, or social network. Minimum wages and EITCs offer the possibility for higher earnings for a subset of a household or community members, which, even if corresponding to a net decrease in aggregate income, could allow for greater household specialization. Such household production effects, and associated positive externalities within a community, are a potential mechanism operating in parallel to our simple income model that is unobservable within our data. In addition, our inability to connect convictions to prior prison time served in different states means that a portion of recidivating crimes are incorrectly identified as first-time incarcerations in our data. Finally, a limitation of growing importance is the 2000–2014 window of our data. Our highest observed minimum wage is $9.50. In recent years, a number of states and localities have adopted far higher minimum wages, several at $15 per hour. It is difficult to project from our results what the net effect of such policies will be.

The balance of U.S. labor policy is very much against those with criminal records, with little reason to believe reducing their labor market opportunities will lead to anything other than greater criminal activity (Pager 2003; Yang 2017; Schnepel 2018; Agan and Starr 2018). As of 2016, there were 6,392 separate state restrictions on employment eligibility for those with felony records, led by Louisiana's 389 and no state carrying fewer than 41 (Fredericksen and Omli 2016). Our results raise the possibility of significant second-order welfare benefits of broad wage policies. The minimum wage may serve as something of an efficiency wage that, while potentially paying more than the market estimate of released prisoners' marginal products, provides a public good in the form of reduced criminal activity. Similarly, the EITC can serve to push wages above those available from criminal activity, increasing the opportunity cost of crime without the potential disemployment effects associated with minimum wages.

50. This, of course, does not imply that the EITC is necessarily the more efficient policy. The deadweight losses of the two policies are beyond the scope of this analysis. Rather, it implies that given a fixed household income policy target, the comparable EITC increase corresponds to a larger expected decrease in female recidivism.

Downloaded from by guest on October 16, 2026. Copyright 2021

References

Agan, A.Y., and S.B. Starr. 2018. "Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment." *Quarterly Journal of Economics* 133(1):191–235.

Allegretto, S.A., A. Dube, and M. Reich. 2011. "Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data." *ILR Review* 50(2):205–40.

Allegretto, S., A. Dube, M. Reich, and B. Zipperer. 2017. "Credible Research Designs for Minimum Wage Studies." *ILR Review* 70:559–92.

Autor, D., A. Manning, and C.L. Smith. 2016. "The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment." *American Economic Journal: Applied Economics* 8(1):58–99.

Bastian, J. 2020. "The Rise of Working Mothers and the 1975 Earned Income Tax Credit." *American Economic Journal: Economic Policy* 12(3):44–75.

Beauchamp, A., and S. Chan. 2014. "The Minimum Wage and Crime." *BE Journal of Economic Analysis & Policy* 14(3):1213–35.

Blank, R., J. Gelbach, and G.R Ford. 2006. "Are Less Skilled Women Crowding out Labor Market Participation among Less Skilled Men?" In *Black Males Left Behind*, ed. R.B. Macy, 87–120. Washington, DC: Urban Institute Press.

Braun, C. 2019. "Crime and the Minimum Wage." *Review of Economic Dynamics* 32:122–52.

Bucknor, C., and A. Barber. 2016. "The Price We Pay: Economic Costs of Barriers to Employment for Former Prisoners and People Convicted of Felonies." CEPR Reports and Issue Briefs 2016-07. Washington, DC: CEPR.

Cameron, A.C., J.B. Gelbach, and D.L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90(3):414–27.

Card, D., and A.B. Krueger. 1994. Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84(4): 772–93.

Center on Budget and Policy Priorities. 2016. "The Earned Income Tax Credit." Washington, DC: Center on Budget and Policy Priorities.

Chaplin, D.D., M.D. Turner, and A.D. Paper. 2003. "Minimum Wages and School Enrollment of Teenagers: A Look at the 1990s." *Economics of Education Review* 22(1):11–21.

Clemens, J., L. Kahn, and J. Meer. 2018. "The Minimum Wage, Fringe Benefits, and Worker Welfare." NBER Working Paper 24635. Cambridge, MA: NBER.

———. 2021. "Dropouts Need Not Apply? The Minimum Wage and Skill Upgrading." *Journal of Labor Economics* 39(51):S107–S149.

Clemens, J., and M. Wither. 2016. "The Minimum Wage and the Great Recession: Evidence of Effects on the Employment and Income Trajectories of Low-Skilled Workers." NBER Working Paper 20724. Cambridge, MA: NBER.

Congdon-Hohman, J. 2018. "The Persistent Labor Market Effects of a Criminal Conviction and "Ban the Box" Reforms." Unpublished.

Deere, D., K.M. Murphy, and F. Welch. 1995. "Employment and the 1990–1991 Minimum-Wage Hike." *American Economic Review* 85(2):232–37.

DiNardo, J., N.M. Fortin, and T. Lemieux. 1996. "Labor Market Institutions and the Distribution of Wages 1973–1992: A Semiparametric Approach." *Econometrica* 64(5):1001–44.

Dube, A., T.W. Lester, and M. Reich. 2010. "Minimum Wage Effects across State Borders: Estimates Using Contiguous Counties." *Review of Economics and Statistics* 92(4):945–64.

Dube, A., and B. Zipperer. 2015. "Pooling Multiple Case Studies Using Synthetic Controls: An Application to Minimum Wage Policies. IZA Discussion Paper 8944. Bonn, Germany: IZA.

Durose, M.R., H.N. Snyder, and A.D. Cooper. 2015. "Multistate Criminal History Patterns of Prisoners Released in 30 States." Report NCJ 248942. Washington, DC: Office of Justice Programs, Bureau of Justice Statistics.

Downloaded from by guest on October 16, 2026. Copyright 2021

Eissa, N., and H.W. Hoynes. 2004. "Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit." *Journal of Public Economics* 88(9):1931–58.
Eissa, N., and J.B. Liebman. 1996. "Labor Supply Response to the Earned Income Tax Credit." *Quarterly Journal of Economics* 111(2):605–37.
Ellwood, D.T. 2000. "The Impact of the Earned Income Tax Credit and Social Policy Reforms on Work, Marriage, and Living Arrangements." *National Tax Journal* 53:1063–105.
Fernandez, J., T. Holman, and J.V. Pepper. 2014. "The Impact of Living-Wage Ordinances on Urban Crime." *Industrial Relations: A Journal of Economy and Society* 53(3):478–500.
Flood, S., M. King, R. Rodgers, S. Ruggles, and R.J. Warren. 2020. "Integrated Public Use Microdata Series, Current Population Survey: Version 8.0 [data set]." Technical report. Minneapolis, MN: IPUMS.
Fone, Z.S., J. J. Sabia, and R. Cesur. 2023. "The Unintended Effects of Minimum Wage Increases on Crime." *Journal of Public Economics* 219:104780.
Fredericksen, A., and D. Omli. 2016. "Jobs after Jail: Ending the Prison to Poverty Pipeline." Report. Seattle, WA: Alliance for a Just Society.
Galbiati, R., A. Ouss, and A. Philippe. 2021. "Jobs, News, and Re-offending after Incarceration." *Economic Journal* 131(633):247–70.
GAO. 2011. "Multiple Employment and Training Programs." Washington, DC: US. Government Printing Office.
Glaze, L.E. 2008. "Parents in Prison and Their Minor Children." Technical Report NCJ 222984. Washington, DC: Bureau of Justice Statistics.
Gorry, A. 2013. "Minimum Wages and Youth Unemployment." *European Economic Review* 64:57–75.
Hashimoto, M. 1987. "The Minimum Wage Law and Youth Crimes: Time-Series Evidence." *Journal of Law & Econonomics* 30(2):443–64.
Hoffman, S.D., and L.S. Seidman. 2003. *Helping Working Families: The Earned Income Tax Credit*. Kalamazoo, MI: WE Upjohn Institute.
Holzer, H.J., S. Raphael, and M.A. Stoll. 2006. "Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of employers." *Journal of Law & Economics* 49 (2):451–80.
Jardim, E., M.C. Long, R. Plotnick, E. Van Inwegen, J. Vigdor, and H. Wething. 2017. "Minimum-Wage Increases and Low-Wage Employment: Evidence from Seattle." *American Economic Journal: Economic Policy* 14(2):263–314.
Lee, D.S. 1999. "Wage Inequality in the United States during the 1980s: Rising Dispersion or Falling Minimum Wage?" *Quarterly Journal of Economics* 114(3):977–1023.
Leigh, A. 2010. "Who Benefits from the Earned Income Tax Credit? Incidence among Recipients, Coworkers and Firms." *BE Journal of Economic Analysis & Policy* 10(1):1–43.
Levitt, S.D., and S.A. Venkatesh. 2000. "An Economic Analysis of a Drug-Selling Gang's Finances." *Quarterly Journal of Economics* 115(3):755–89.
Liu, S., T.J. Hyclak, and K. Regmi. 2016. "Impact of the Minimum Wage on Youth Labor Markets." *Labour* 30(1):18–37.
Luca, D.L., and M. Luca. 2017. "Survival of the Fittest: The Impact of the Minimum Wage on Firm Exit." Working Paper 17-088. Cambridge, MA: Harvard Business School NOM Unit.
MacKinnon, J.G., and M.D. Webb. 2018. "The Wild Bootstrap for Few (Treated) Clusters." *Econometrics Journal* 21(2):114–35.
McCarthy, B., and J. Hagan. 2001. "When Crime Pays: Capital, Competence, and Criminal Success." *Social Forces* 79(3):1035–60.
Meer, J., and J. West. 2015. "Effects of the Minimum Wage on Employment Dynamics." *Journal of Human Resources* 51:500–522.
Meyer, B.D., and D.T. Rosenbaum. 2001. "Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers." *Quarterly Journal of Economics* 116(3):1063–114.

Downloaded from by guest on October 16, 2026. Copyright 2021

Neumark, D. 2017. "The Employment Effects of Minimum Wages: Some Questions We Need to Answer." NBER Working Paper 23584. Cambridge, MA: NBER.
Neumark, D., J.I. Salas, and W. Wascher. 2014. "Revisiting the Minimum Wage-Employment Debate: Throwing out the Baby with the Bathwater?" *ILR Review* 67(3):608–48.
Neumark, D., and W. Wascher. 1995. "Minimum Wage Effects on Employment and School Enrollment." *Journal of Business & Economic Statistics* 13(2):199–206.
———. 2003. "Minimum Wages and Skill Acquisition: Another Look at Schooling Effects." *Economics of Education Review 22*(1):1–10.
Neumark, D., and W. Wascher. 2011. "Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit?" *Industrial and Labor Relations Review* 64(4):712–46.
———. 2017. "Reply to 'Credible Research Designs for Minimum Wage Studies.'" *ILR Review* 70(3):593–609.
Nichols, A., and J. Rothstein. 2016. "The Earned Income Tax Credit. In *Economics of Means-Tested Transfer Programs in the United States*, Volume 1, ed. R. Moffitt, 137– 218. Chicago, IL: University of Chicago Press.
Orrenius, P.M., and M. Zavodny. 2003. "The Effect of Minimum Wages on Immigrants' Employment and Earnings." *American Journal of Sociology* 108(5):937–75.
Pager, D. 2003. "The Mark of a Criminal Record." *American Journal of Sociology* 108(5):937–75.
———. 2008. *Marked: Race, Crime, and Finding Work in an Era of Mass Incarceration*. Chicago, IL: University of Chicago Press.
Powell, D. 2017. "Synthetic Control Estimation Beyond Case Studies: Does the Minimum Wage Reduce Employment?" Working Paper WR-1142. Santa Monica, CA: RAND.
Raphael, S., and D.F. Weiman. 2007. "The Impact of Local Labor-Market Conditions on the Likelihood that Parolees Are Returned to Custody." In *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America*, ed. S.D. Bushway, M. Stoll, and D. Weiman, 304–32. New York: Russel Sage Foundation.
Roodman, D., M.Ø. Nielsen, J.G. MacKinnon, and M.D. Webb. 2019. "Fast and Wild: Bootstrap Inference in Stata Using Boottest." *Stata Journal* 19(1):4–60.
Rothstein, J. 2010. "Is the EITC as Good as an NIT? Conditional Cash Transfers and Tax Incidence." *American Economic Journal: Economic Policy 2*(1):177–208.
Schnepel, K.T. 2018. "Good Jobs and Recidivism." *Economic Journal* 128:447–69.
Shannon, S.K., C. Uggen, J. Schnittker, M. Thompson, S. Wakefield, and M. Massoglia. 2017. "The Growth, Scope, and Spatial Distribution of People with Felony Records in the United States 1948–2010. *Demography* 54(5):1795–818.
Slichter, D. 2016. "The Employment Effects of the Minimum Wage: A Selection Ratio Approach to Measuring Treatment Effects." Unpublished.
Spjeldnes, S., and S. Goodkind. 2009. "Gender Differences and Offender Reentry: A Review of the Literature." *Journal of Offender Rehabilitation* 48(4):314–35.
Uggen, C., and M. Thompson. 2003. "The Socioeconomic Determinants of Ill-Gotten Gains: Within-Person Changes in Drug Use and Illegal Earnings." *American Journal of Sociology* 109(1):146–85.
United States Department of Justice, Office of Justice Programs, Bureau of Justice Statistics. 2016. "National Corrections Reporting Program 2000–2014." http://dx.doi.org/10.3886/ICPSR36373.v1
Vaghul, K., and B. Zipperer. 2016. "Historical State and Sub-State Minimum Wage Data." Technical Report 2016-09. Washington, DC: Washington Center for Equitable Growth.
Viscusi, W.K. 1986. "The Risks and Rewards of Criminal Activity: A Comprehensive Test of Criminal Deterrence." *Journal of Labor Economics* 4(3, Part 1):317–40.
Visher, C.A., S. Debus, and J. Yahner. 2008. *Employment after Prison: A Longitudinal Study of Releases in Three States*. Washington, DC: Urban Institute, Justice Policy Center.

Downloaded from by guest on October 16, 2026. Copyright 2021

Visher, C.A., L. Winterfield, and M.B. Coggeshall. 2005. "Ex-Offender Employment Programs and Recidivism: A Meta-Analysis." *Journal of Experimental Criminology* 1(3):295–316.
Waldfogel, J. 1994. "Does Conviction Have a Persistent Effect on Income and Employment?" *International Review of Law and Economics* 14(1):103–119.
Yang, C.S. 2017. "Local Labor Markets and Criminal Recidivism." *Journal of Public Economics* 147:16–29.

Downloaded from by guest on October 16, 2026. Copyright 2021