The Minimum Wage, EITC, and Criminal Recidivism*

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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 return to prison. Using administrative prison release records from nearly six million offenders released between 2000 and 2014, we identify the effect of over two hundred minimum wage increases, as well as 21 state EITC programs, on recidivism. We find that a minimum wage increase of $0.50 reduces the probability an individual returns to prison within 1 year by 2.7%. This implies that on average the effect of higher wages, drawing at least some released prisoners into the legal labor market, dominates any disemployment effects. Reductions in returns to incarceration are observed for property and drug crimes; prison reentry for violent crimes are unchanged. The availability of state EITCs also reduces recidivism, but only for women.

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1 Introduction

Every year, more than 600,000 men and women are released from U.S. prisons; 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 et al. 2008; Yang 2017; Galbiati et al. 2017; 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.

In this paper, we estimate how two major low-wage labor market policies, the minimum wage and earned income tax credits (EITCs), impact the probability a recently released prisoner returns to prison. We exploit variations in the implementation and levels of these policies across states combined with microdata from nearly six million individual prison release records from 2000 to 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 succeed (the *wage effect*) and their likelihood of finding employment (the *disemployment effect*). Additional EITC benefits, on the other hand, serve to increase available income and incentives to work in the legal labor market, both channels potentially decreasing recidivism. Given the way EITCs are implemented, this is likely to have stronger impacts for women.

The *wage effect* of the minimum wage implies that higher minimum wages could reduce recidivism. Minimum wages are a particularly salient indicator of the wage for legal work available to released prisoners. Given that prior work estimating returns to remunerative criminal activity has found earnings not far from typical minimum wages, there is an opportunity for higher minimum wages to succeed in pulling individuals out of illegal income generating activities and into the legal job market (Viscusi 1986; Levitt and Venkatesh 2000; McCarthy and Hagan 2001; Uggen and Thompson 2003). The existence of higher paying jobs, if an individual is able to secure one, increases the opportunity cost of returning to prison, presumably reducing the probability of recidivism. This is similar to the argument that minimum wages increase high school dropouts as individuals exit education in favor of higher wages available in the job market (Chaplin et al. 2003; Neumark and Wascher 2003).

The *disemployment effect* pushes in the opposite direction. The potential for disemployment induced by higher minimum wages is at the heart of most economic studies of the minimum wage (e.g. Card and Krueger 1994; Allegretto et al. 2011; Neumark et al. 2014; Meer and West 2015; Allegretto et al. 2017; Neumark and Wascher 2017; Jardim et al. 2018; see Neumark 2017 for a recent review). To the extent that employers reduce overall labor demand or are able to substitute to more desirable labor pools due to higher minimum wages, this may increase the likelihood of non-employment and reduce the opportunity cost of further crime for recently released prisoners, increasing the probability of recidivism. Given employers' particular distaste for hiring individuals with criminal records and the potential for labor-labor substitution (Pager 2003; Holzer et al. 2006; Agan and Starr 2018), released prisoners are likely to be particularly vulnerable to any potential disemployment effects of the minimum wage, even if higher minimum wages do not cause aggregate decreases in employment.[1]

The theoretical wage effect is not substantively different from what we might expect from higher market wages or better labor market opportunities, as studied in the recent work of Yang (2017), Galbiati et al. (2017), and Schnepel (2018)—all of which show that better local labor market opportunities at the time of release decrease the probability of returning to prison. It is the possibility of a coincident disemployment effect from higher minimum wages that differentiates any predicted effects, and ensuing empirical content, of this question from this recent literature.

It is thus an empirical question whether, on net, the potential disemployment effects or wage effects of the minimum wage will dominate for the population of returning prisoners. Exploiting state and year variation in minimum wages over our time period, we find that higher minimum wages are associated with a *lower* probability of returning to prison, with no discernible difference in effect for men or women. That is, the increased incentive to substitute legal employment for criminal market activity, on net, appears to be greater than any disemployment effects of minimum wages for this group. However, our results do not allow for separate identification of the magnitude of disemployment or wage effects, only that wage effects dominate.

Of course the individual's *own* employment is not the only mechanism through which a minimum wage or EITC could impact their possibility of returning to prison. Minimum wages and EITCs may impact other members of the individuals household or community in ways that could increase or decrease the support availability to them. Our data do not allow

[1]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 et al. 1996; Lee 1999; Autor et al. 2016), firm entry and exit (Luca and Luca 2017), and fringe benefits (Clemens et al. 2018).

us to directly tease out these mechanisms and could be interpreted as the net effect of both individual and community level mechanisms. However, we do use supplementary data from the Current Population Survey (CPS) to do some exploratory tests of the plausibility of the employment mechanism. We focus on populations that have a relatively high likelihood of having a criminal record (low-skill black men and individuals who say they are not eligible to vote) and in these populations we find evidence that higher minimum wages *are* associated with higher probabilities of employment.

Evidence regarding the impact of minimum wages on overall crime within the literature is fairly mixed and predominantly focused on teenagers and young adults. Most closely related to our study are a set of papers that study the impact of minimum wages on overall crime rates.[2] Braun (2017) 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. Her estimates imply that the current federal minimum wage is close to the 'crime minimizing value'. Beauchamp and Chan (2014) use data from the NLSY97 to study the impact of minimum wage changes specifically on teenagers and young adults directly impacted by the change - those working at or below the new minimum wage before the change. They find that directly impacted teenagers (aged 17-19) are more likely to report having committed crimes after the increase, while results for older ages (20-30) are mixed.[3] Fone et al. (2019) use Uniform Crime Reporting arrest data to study the impact of minimum wages on arrests per capita by age group, finding that higher minimum wages are associated with increased property crime arrests for those aged 16-to-24.[4] They find no statistically significant impact on arrests for violent or drug crimes, and impacts on arrests at older ages are also not statistically significant.[5] Fernandez et al. (2014) studies living wages but also includes minimum wages in their specifications, and find suggestive evidence of *decreases* in reported UCR crime with higher minimum wages.

We have reason to be narrowly interested in the effects of minimum wages on recidivism, which has not yet been studied in relation to minimum wages, rather than crime rates in general. Individuals already bearing a criminal record are likely amongst the most vulnerable to potential disemployment effects. Minimum wage studies have broadly emphasized

[2]While not explicitly criminal, Muravyev and Oshchepkov (2016) found that a doubling of the Russian minimum wage in 2007 resulted in increased participation in the "informal" labor market.

[3]There is evidence of a *decline* in remunerative crimes for those aged 20-24 who were bound by a minimum wage change, though not for those aged 25-30.

[4]Hashimoto (1987) uses a time series analysis on arrest rates to come to a similar conclusion, higher minimum wages were associated with higher property crime arrests by teens, with no effect on other types of arrest or for younger adults.

[5]Fone et al. (2019) find no statistically significant impact of minimum wages on overall reported crime or aggregated arrests (though point estimates tend to be positive).

identifying effects of higher minimum wages on groups likely to be looking for low-skilled occupations, including (but not limited to) teenagers (Neumark and Wascher 1995; Dube et al. 2010; Allegretto et al. 2011; Gorry 2013; Dube and Zipperer 2015; Liu et al. 2016; Slichter et al. 2016; Powell 2017), immigrants (Orrenius and Zavodny 2003), and high school drop-outs (Deere et al. 1995).[6] Released prisoners share attributes with all of these groups, including the limited skill sets, education, and/or reference networks. They also tend to be older, with the average age of release in our data being 35. Additionally, they have the additional stigma of a criminal record, implying that few released prisoners are likely to enjoy the luxury of being inframarginal with regard to any nontrivial low-skill wage policy. For other participants in the low-skill labor market, the next best alternative to legal employment is often leisure or school; the prospect of a higher minimum wage pulling such individuals into employment is a private outcome with little external consequence. For some released prisoners, on the other hand, their next best alternative to legal employment may be criminal endeavor. As such, minimum wages stand to have a previously unconsidered impact on private decisions to participate in illegal labor markets and, in turn, criminal activities with potentially significant negative impacts on others.

The predicted effects of EITCs are less ambiguous, at least for those who are eligible (Leigh 2010; Rothstein 2010; Nichols and Rothstein 2016). An EITC is a (usually) refundable tax credit available to individuals with a job and low to moderate income (up to $53,505 depending on the number of children you have and whether you are married filing jointly or single). 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.[7] In addition to the federal EITC, some states also have their own additional EITCs—an additional percentage of the federal EITC offered to filers in those states. As an explicit 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, with both working towards reducing the probability an individual returns to prison.

Given that EITC magnitudes are strongly contingent on the number of children for which an individual holds custodial responsibility, however, their impacts on recidivism are likely to be heterogenous across individuals, particularly with regards to gender. The Bureau

[6] Again, the main thrust of Beauchamp and Chan (2014) and Fone et al. (2019) is that higher minimum wages cause increases in crime for teenagers or very young adults, a complement to the papers finding some evidence of decreases in employment for teenagers from higher minimum wages.

[7] https://www.irs.gov/credits-deductions/individuals/earned-income-tax-credit/eitc-income-limits-maximum-credit-amounts

of Justice Statistics reports that 53% of men and women in prison are parents of minor children before they entered prison; 41.7% of women reported being the single parent of their household, compared to 19.2% of men (Glaze 2008). In a survey of recently released prisoners in Chicago, Baltimore, and Cleveland performed by the Urban Institute, in the 2-4 months after release 28% of men and 49% of women report children under 18 living with them.[8] The EITC literature has shown that these subsidies tend to increase labor force participation of single women (Eissa and Liebman 1996). That fact, combined with the statistics on custodial responsibility, imply that EITCs may have stronger impacts on female recidivism. 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. Both of these are likely to have the effect of reducing recidivism.

Using state and year variation in the availability of state EITCs, we find some evidence that the availability of state top-ups to the federal EITC corresponds to a lower rate of longer-term (3-year) recidivism *for women*. This effect is stronger for 3-year recidivism rates than 1-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. State EITCs have no discernible effect on men, and, if anything, our results point to these policies corresponding to increases in returns to prison for some crimes.

The research on the EITC also constitutes a deep literature, but its connection to recidivism has similarly not been considered. Prior work has shown that, in 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), 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). Blank and Gelbach (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. These earlier findings are congruent with our conclusion that EITC benefits reduce recidivism for women, but have null, if not criminogenic, effects for men.

Our findings have important implications for both labor and crime policy. Nearly half

[8] Authors' calculations from the "Returning Home Study" data provided by the Urban Institute.

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 et al. (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.[9] Given the limited efficacy of past policies, and the political obstacles to passing legislation targeted to support individuals with criminal records, understanding the impact of broader labor policies is critical to efforts towards reintegrating released prisoners back into the workforce and breaking the cycle of crime and imprisonment. 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.

2 A Conceptual Framework

In this section, we outline a simple conceptual framework that models the behavior of individuals released from prison as a choice between legal and illegal activities and considers how low-wage labor market policies such as the minimum wage interact with this decision (Becker 1968; Ehrlich 1974; Grogger 1998).[10] Consider a simple model where a released prisoner can earn w_i^* in the legal market absent a minimum wage and w_i^{crime} in the criminal market.[11] The effect of a minimum wage, w^{min}, will depend on its size relative to w_i^* and w_i^{crime}. If we

[9] 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).

[10] This framework only considers the individual's choice between illegal and legal activities. Clearly, there are other aspects of an individual's life that could influence the probability they return to incarceration. As the focus of our research is on low-wage labor market policies, however, we believe this focus is relevant for us. We acknowledge, however, that it is not the only factor influencing recidivism.

[11] In our model we will consider a greatly simplified version of criminal wages, where the returns are reduced to a linear value inclusive of all associated uncertainty, risk aversion, or any non-pecuniary rewards (e.g. thrills or social signaling). The "wages" of different types of crimes will, of course, bear different compositions of these rewards, with some more akin to a black market occupation, while others a randomly encountered opportunistic gamble (Cook 1980). While not explicitly modeled here, we will include in our empirical analysis a breakdown of policy effects by different types of crime committed.

assume that the minimum wage is both salient in the market, i.e. that $w_i^* < w^{min}$, and that, as such, the probability of finding a job is decreasing with the minimum wage, then the net effect of the minimum wage on criminal activity (and recidivism) depends on the relative levels of w^{min}, w^*, and w^{crime}.[12]

If $w^{crime} < w^* < w^{min}$, then this individual would prefer to work in the legal labor market regardless of the minimum wage. Under these conditions, however, increases in the minimum wage may make it harder for them to find legal sector employment. An increase in the minimum wage will not induce any substitution of legal labor for crime at the margin given that the uncontrolled market wage was already sufficient to dominate the criminal wage. This increased unemployment, we posit, will increase the probability of recidivism. In this case the probability of recidivism will *increase* with the minimum wage w^{min}.

If $w^* < w^{crime}$, then this individual would prefer to "work" in the illegal labor market in the absence of a minimum wage. However, an increase in the minimum wage could push their potential wages high enough to dominate, such that $w^* < w^{crime} < w^{min}$. Recidivism is now tied to the minimum wage and the premium it offers relative to the wages of crime. Any increase in unemployment due to the minimum wage is irrelevant because the uncontrolled market wage is insufficient to dominate criminal endeavor. Then the probability of recidivism will *decrease* with w^{min}.[13]

The net effect of increasing minimum wages on recidivism is ambiguous and will depend on the underlying distributions of market and criminal wages. Increasing minimum wages could lead employers to reduce hiring, and released prisoners may be particularly susceptible to this loss, leading to increased recidivism. Higher minimum wages, however, could bring legal wages above potential "criminal" wages for some offenders, enticing them into the legal labor market. Given some probability of finding and securing this higher paid job, this wage effect should lead to reductions in recidivism. In the context of our simple framework,

[12] We could, alternatively, formalize our theoretical framework as an Ehrlich-Becker model of the market for criminal offenses where demand can be construed as the "tolerance of crime" and supply the emergent labor response to crime's costs and benefits (Ehrlich 1996). We will here treat demand as exogenous, focusing instead on supply and the net payoff to criminal activity, π, where $\pi = W_C - \Psi(W_L, e, W_{min}) - pF$. W_C and W_L are the expected wages of criminal and legitimate labor, F is the punishment if caught engaging in criminal labor, and p is the probability of being caught. $\Psi(W_L, e, W_{min})$ is the distribution of legitimate wages whose shape is a function of the the expected uncontrolled market wage, W_L, the "natural" employment rate within the hypothetically uncontrolled low-skill labor market, e, and the minimum wage, W_{min}. The minimum wage can affect both a released prisoner's probability of being employed and their expected wage if they find a job. As such, $\Psi()$ is not a particularly well-behaved distribution - a portion of it is equal to zero, and from these "zeros" there is a discontinuous jump to the legal minimum wage. In much the same way as our the minimalist conceptual framework, any predicted effect of the minimum wage on the expected payoff, π, will be ambiguous. The values of W_{min} for which $\int \Psi(W_L, e, W_{min}) > W_C - pF$ will depend on the underlying labor market conditions and the expected wages of crime.

[13] If $w^* < w^{min} < w^{crime}$, then the individual will opt for criminal activity, regardless of the market and minimum wages.

for individuals whose market wages are below the wages of crime (w_L^*), the wage effect will dominate. For individuals whose market wages are above the wages of crime (w_U^*), the employment effect will dominate. In estimating the impact of minimum wages on recidivism, we are in effect estimating the net of these two forces.[14]

This model simplifies the decision to an all-or-nothing choice between legal or criminal activities as the individual's source of income. The decisions predicted in our model are akin to the "first hour" of criminal income generation in the structural model in Grogger (1998), where individuals optimize their portfolio of legal and criminal income generating activities. Assuming monotonically decreasing benefits (and increasing costs, such as the probability of being apprehended) to criminal income opportunities, an individual's market wage serves as the reservation wage the highest criminal income opportunity must exceed for an individual to include any amount of criminal activity in their portfolio. Absent a minimum wage, the market wage would serve as an identical threshold for criminal activity in our model. The predicted disemployment and wage effects of the minimum wage in our model are qualitatively identical, and similarly ambiguous, to what would be predicted within Grogger's model: they depend on whether the marginal return to crime is ever equal to the marginal return of work in the legal market.[15]

Considered in this simple framework, the predicted impact of the EITC appears straightforward. The first order wage and employment effects of a pure wage subsidy push in the same direction—with larger subsidies leading to higher wages *and* higher probability of employment. The EITC, however, is not a universal subsidy program, with benefits almost entirely accruing to custodial parents of dependent children. This differential benefit to released prisoners stands to correspond to differential impact, particularly with regards to gender. Disproportionate accrual to single mothers is likely to be relevant to the 41.7% of women that entered prison as the only parent living with their child. 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 2009). On net, our simple model unambiguously predicts that the EITC will reduce criminal recidivism *for women*. With regards to male recidivism, however, the predicted effect of the EITC is far weaker.

[14] Braun (2017) builds a search-theoretic model of crime and employment under a minimum wage that is broadly compatible with our framework.

[15] While Grogger (1998) builds a structural model of portfolios of legal and illegal earnings observed in the NLSY 1979 data, we only observe whether an individual returns to prison and, as such, we simplify our conceptual framework to the individual's decision to earn her income legally or illegally.

3 Data

3.1 National Corrections Reporting Program

Data on prison spells were obtained from the National Corrections Reporting Program (NCRP).[16] The data were constructed using administrative data voluntarily provided by states to the Bureau of Justice Statistics (BJS) on prison admissions and releases from 2000-2014. 44 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.[17]

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, as well as why the prisoner was released. For each prison spell we also observe up to 3 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, however this is a more noisy proxy.[18] For complete details and background on this decision see Appendix B.

From the 2000-2014 NCRP data we drop individuals who have not yet been released, which is 13% of the sample; who are missing gender, as this is a major component of our analysis (0.03% of the sample); or those who were "released" from prison because of death (0.4% of the sample). We also drop individuals who have a different state of conviction than

[16] United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics 2016

[17] For a description of how prison term records were created, see `http://www.icpsr.umich.edu/files/NACJD/ncrp/white-paper-computing-code.pdf`.

[18] The 2015 NCRP data include, for some offenders, the last known state and county for the offender. Amongst those with the requisite data, 95% lived in the state of conviction prior to incarceration, however only 70% lived in the *county* of conviction prior to incarceration.

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% 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 CA 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 CA in 2011 and after. This can be seen clearly in Figure 2 where CA recidivism rates drop precipitously around 2011, a far outlier from other large states or the state with the largest drop in recidivism around that time, Utah. Taking what we believe to be the most conservative approach, we opt to entirely exclude California from our analysis.[19]

After all restrictions are imposed, including the exclusion of California, our sample includes nearly 5.8 million prison releases from 4 million unique offenders in 43 states when 1-year recidivism rates are the outcome. When 3-year recidivism rates are the outcome, the sample includes nearly 4.8 million prison releases from 3 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.[20] 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% are male. Minorities make up a larger share of our sample of prisoners than in the U.S. population (54.5% of our sample are black or Hispanic). The average prisoner is in their mid-thirties on release, which makes sense as our data concern prisons and not local or county jails, and thus most people are incarcerated for a relatively serious crime. In addition, almost 30% have a previous incarceration for a felony. In Table 2 we see that just over 17% of offenders in our dataset are returned to prison within 1 year, and 35%

[19]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.gov.ca.gov/news.php?id=4278

[20]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% were arrested out of state within one year and 7% were arrested out of state within 3 years (Durose et al. 2015). So while out of state migration could clearly be an issue, we believe it will not have large effects on our results.

are returned within 3 years. Men recidivate at a higher rate than women (18% versus 14% in the first year). Property and drug crimes constitute the most common returning offenses.

3.2 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 sub-state minimum wages for the entire period of our study from May 1974 to present.[21] Table 3 and Figure 1 summarize state minimum wage and minimum wage changes during these years. The average state had 4 changes in their 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).[22] On average, states increased their minimum wage $0.50 (about 8% 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.

The state EITC data come from the Tax Policy Center.[23] As of 2016, 25 states and D.C. offer EITCs on top of the federal EITC, worth an additional 3.5-40% 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, 3 states introduce an EITC top-up (North Carolina, Nebraska, and Oklahoma), and 3 states initially offer a top-up, only to later discontinue or fail to fund their program (Colorado, Indiana,

[21] 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 1974 available here: http://www.economics.uci.edu/~dneumark/datasets.html. For pre-1960 we assign the prevailing federal minimum wage at the month and year of admission.

[22] 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.

[23] Downloaded from: http://www.taxpolicycenter.org/statistics/state-eitc-based-federal-eitc

and Michigan).[24]

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

4 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 employ a difference-in-differences design. This design exploits 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:

$$\begin{aligned} Recidivate_{isct} = \beta_1 MW_{st} + \beta_2 MW_{st} \times Female_i + \beta_3 EITC_{st} + \beta_4 EITC_{st} \times Female \\ + \mathbf{X_{it}} + \mathbf{K_{ts}} + \mathbf{J_{tc}} + \gamma_y + \delta_s + \epsilon_{isct} \end{aligned} \tag{1}$$

where $Recidivate_{ist}$ is an indicator variable for whether an offender i, imprisoned by state s, released in year-month t, returned to prison in the same state within a certain time period (1 or 3 years depending on the specification). MW_{st} is the (nominal) minimum wage in the state and year-month into which the offender was released.[25] $EITC_{st}$ is either the existence 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 single mothers, we also interact our labor market policies with gender to see whether policy impact differs for men and women. There is a trade-off in using 1- or 3-year recidivism rates. The longer the recidivism period, the fewer observations we can use in our analysis. 3-year

[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, 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 E.1.

[25]Results are qualitatively similar if we use log minimum wages.

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. In Equation 3, we identify the effect of the low-wage labor market policy at the time the offender was released. For that reason, we mostly focus on recidivism within 1 year, although we also show results for a 3 year window. γ_y are year fixed effects, and δ_s are state fixed effects.

$\mathbf{X}_{it}$ 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. $\mathbf{K}_{ts}$ 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.[26] These variables are meant to pick up other state policies that may interact with recidivism or employment for offenders and 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 that the individual was convicted in: median household income, percent age 15-24, percent black and percent Hispanic.[27]

In an Appendix, we also present result from regressions similar to above but run separately for men and women. 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. In addition, the pooled tables make the comparisons of coefficients across gender

[26] 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 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 non-partisan, however one can determine the political affiliation of its members by voter registration, party endorsements, or reporting in media or by non-profits, which we did). TANF benefits and felony drug bans come from the Urban Institute's Welfare Rules Database.

[27] 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 early, our best approximation is that around 70% 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 Appendix C.

more straightforward for the reader.[28] In Appendix Table E.1 we show that our main results do not differ qualitatively when we split the sample by gender, implying there is not considerable omitted variable bias from eliminating the gendered interactions on the controls. Thus, for the main results in the text we focus on the pooled tables.

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 over-rejection (Cameron et al. 2008). While we are over this arbitrary threshold, it is nevertheless sufficiently close to warrant additional caution. As such, for all coefficients, we report cluster-robust standard errors with their respective stars to indicate statistical significance. For the main impacts of interest, β_1 (on MW_{st}) and $(\beta_3 + \beta_4)$ (the total effect of the EITC for women), we also present p-values from the Wild-cluster bootstrap procedure suggested by Cameron et al. (2008), which tend to be more conservative, particularly when clusters are asymmetric in size. The table notes for Table 4 provide more details.

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 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 6 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.

5 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

[28]Running the fully interacted models instead of separate regressions by gender, which would fix the problem of ease of comparison, has thus far been infeasible with the computer power we are currently able to access.

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 women. The relationship between recidivism and state EITC top-up percentages is similarly plotted in Figure 5. 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 these relationships will be explored 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.

Table 4 presents our main results from Equation 3 for the minimum wage and state EITC. All specifications include state and year fixed effects and a vector of controls for both offender and state of release characteristics. In Columns 1 and 2, EITC is defined as a binary variable for whether the state has a top-up available; in Columns 3 and 4 we use the magnitude of the state top-up as a percent of the federal EITC. We show results for both 1-year and 3-year recidivism rates.[29] The results from Column 1 show that a $1.00 increase in the minimum wage is associated with a 0.94 percentage point *decrease* in the probability an individual returns to prison within one year. The mean probability of returning to prison in 1 year is 17.3%, and the average minimum wage increase is $0.50, implying that the average minimum wage increase is associated with a 2.7% decrease in the probability of returning to prison in 1 year. Consistent with the results from the binned scatter plots, we find that the effect of the minimum wage does not differ significantly for women versus men. Results are very similar in magnitude for 3-year recidivism rates, with a $1.00 increase in the minimum wage reducing the probability of return to prison within 3 years by 1.53pp (implying the average minimum wage increase decreases the probability of return to prison in 3 years by 2.25%). The main minimum wage results are statistically significant at the 5% level with the cluster-robust standard errors reported in parentheses. Using the more conservative wild bootstrap p-values, reported at the bottom of the table, the coefficient is significant at the 10% level for 1-year recidivism and 1% level for 3-year recidivism.

Columns 1 and 2 also consider the availability of a state top-up to the federal EITC. The results imply some evidence that for *women* the existence of a state EITC decreases recidivism, though this total effect is only significant for 3-year recidivism. For 3-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 3 years by about 3 percentage points (11%), and

[29] Appendix E shows results for these same regressions run separately by gender

this is statistically significant at the 5% level even with bootstrap p-values. Columns 3 and 4 of Table 4 substitute the magnitude of the state EITC top-up into Equation 3 instead of simply whether a state top-up exists. The results indicate that a 1pp increase in the EITC top-up reduces 3-year recidivism by 0.15pp for women (0.55%). Within the context of our sample (mean state top-up = 5.2%, Std. Dev. = 9.3), a one standard deviation increase in the state EITC corresponds to a 5.1% reduction in the expected recidivism rate for women. It is important to note, however, that this result is only marginally significant with the cluster-robust standard errors (p=0.067), and the wild bootstrap p-value is 0.25.[30]

These results imply an elasticity for the probability an individual returns to prison in 3 years with respect to the minimum wage of roughly -0.29.[31]. To put this in context, Yang (2017) estimates hazard models of return to prison within 3 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 are, as such, are smaller, which makes intuitive sense within our conceptual framework as we expect that minimum wages may still have a disemployment effect for some portion of the relevant population.

5.1 Robustness of Main Result

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 here using 1-year recidivism rates as an outcome; for space, results using 3-year recidivism rates as an outcome are in Appendix G.

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 Columns 1 and 2 of Table 5 we test this idea by adding in leading values of the minimum wage 1 and 2 years in advance. We see that the coefficients on the leading values are negative, though small and not statistically significant, and the coefficient on the current minimum wage is qualitatively unchanged when adding in the leading minimum wages.

[30] 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 our analysis. Results are available on request.

[31] Using the estimate from Column 2 of Table 4, a \$1.00 increase in the minimum wage is associated with a 1.53 pp reduction in the probability of return to prison. Over an average minimum wage of \$6.50 and a 34.6% 3-year recidivism rate, this implies that a 15% increase in the average minimum is associated with a 4.4% decrease in the probability of return to prison within 3 years, or an elasticity of about -0.29.

Minimum wage changes could also be endogenous to state-specific trends that could also affect recidivism, i.e. if states tend to change their minimum wages when economic conditions are are on an up- or a downswing, this could bias our results.[32] 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 et al. 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 et al. (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.[33] Columns 3-5 of Table 5 add in first- through third-order state-specific polynomial time trends.[34] With linear time trends, the coefficient is smaller and not statistically significant. However, accounting for the potential *non-linear* 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% decrease in recidivism, smaller than the main results (2.7%), but still economically significant. 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 non-linear trends may be necessary to pick employment and labor-market trends appropriately.

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. They are only significant at the 10% level

[32] 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 et al. 2011; Allegretto et al. 2017).

[33] 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 et al. 2010; Neumark et al. 2014; Allegretto et al. 2017; Neumark and Wascher 2017).

[34] Adding in the interactions with female does not change the conclusion and is not presented in the interest of space.

however, and the wild bootstrap p-value is 0.15.

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

$$\begin{aligned} Recidivate_{isct} = \alpha + \beta_1 MinWage_{st} + \beta_2 Bound_{st} + \beta_3 MinWage_{st} \times Bound_{st} \\ + \mathbf{X_{it}} + \mathbf{K_{ts}} + \mathbf{J_{tc}} + \gamma_y + \delta_s + \epsilon_{isct} \end{aligned} \tag{2}$$

Appendix D 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 (-0.0010, cluster-robust se=0.0052). This indicates that states that experienced plausibly exogenous increases in their minimum wages also experienced similar decreases in recidivism.

Table 6 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, however this could be directly impacted by the minimum wage. Nevertheless, in Table 6, 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

[35] 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 which we employ as well.

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 Table 6 we control for the minimum wage at prison entry and the minimum wage one year before prison entry.[36] 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 Table 6 we consider average minimum wages 6- 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.

Concerns over the potential endogeneity of minimum wages to trends or policies have been the focus of the recent minimum wage and employment research agenda, and are germane to our analysis as well. Our minimum wage variable demonstrates broad robustness to a variety of specifications meant to deal with similar omitted variables that could also be impacting recidivism. The total EITC effects for women are also broadly qualitatively similar to our main results across these specifications. Our main results for 1-year recidivism implied potentially negative associations between state EITCs and recidivism for women, but were only marginally statistically significant; for 3-year recidivism our main results showed statistically significant negative associations ($p < 0.01$), implying that the existence of a state EITC top-up was associated with an 11% decrease in recidivism. The robustness results in Tables 5 and 6, as well as the 3-year recidivism results in Appendix G, broadly echo this. For 3-year recidivism rates, all the total EITC effects for women are negative, of a similar magnitude, and are significant at the 10% level across specifications, with many significant at 5% even with the more conservative wild bootstrap p-values.

5.1.1 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 disemploy-

[36] 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.

ment effects. To address this, we include a set of one-, two-, and three-year lags of the state minimum wages to the model specification in Table 7. When included as singular right-hand side variables, the coefficients on the 1-, 2-, and 3-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.

5.1.2 Additional County-Level Analysis

While we feel that 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 Appendix B for details). Results for these additional robustness test are in Appendix Tables C.1 and C.2 showing 1- and 3-year recidivism results respectively. In Column 1 we recreate our main results from Table 4 with only observations that are not missing county of conviction (96.4% of the sample), which look broadly similar. 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.[37] Using counties rather than simply states could allow us to leverage these variations. However, this represents only 0.2% of our data, about 14,000 observations.[38] Recall that we have data at the county level—some substate minimum wages are in fact those of sub-county municipalities and below our level of data granularity. In Tables C.1 and C.2 we try two different strategies for dealing with this. Column 5 drops any jurisdiction with a substate minimum wage. In Column 6 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 our primary specification, perhaps unsurprisingly given how few observations these localities represent.

The state-specific time trend analysis, discussed earlier, requires parametric assumptions

[37] Data on substate minimum wages are also from Vaghul and Zipperer (2016): data only available from 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; Tacoma, WA

[38] Note also that several of these municipalities are in CA which is not included in our results.

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 et al. (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.[39] 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 Appendix Tables C.1 we restrict our analysis to pairs and triads of counties that straddle state borders and include state fixed effects to control for shocks common to 3 counties in a cluster straddling a state border.[40] For the 1-year recidivism result, the coefficient implies that a $1.00 increase in the minimum wage is associated with a 0.72pp decrease in recidivism, and is only marginally statistically significant even in the limited data we have in border counties.[41]

5.2 Subcategories: Returning Offense, Education, and Race

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 8 reports results by the type of crime an offender was returned to prison for: violent, property, drug, or other crime for both 1- and 3-year recidivism.[42] 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 6-7% of the baseline means, and are true for both men and women. 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.

[39] 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.

[40] See Appendix C.1 for details about the construction of these county clusters.

[41] The 3-year recidivism results are similarly negative implying a 0.68pp decrease in the probability of returning to prison in 3 years, though the p-value of 0.11 is not significant at conventional levels. This is perhaps unsurprising as the 3-year results include less data, and the county-border analysis already limits the sample to only border counties.

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

We do observe an increase in female violent crime recidivism with higher minimum wages, however. We similarly observe mixed results with the EITC. The EITC is associated with an increase in violent and drug crimes for men, thought violent crime result largely disappears for 3-year recidivism. It is significantly associated with a decline in violent crimes for women in 3-years. These results have less of a direct connection to our theoretical framework and may be related to domestic violence and how it is processed in the criminal justice system. Van Wormer and Bartollas (2000) suggest that the growing rate of female incarceration for violent crimes (Spjeldnes and Goodkind 2009) is a result of domestic violence mandatory arrest laws. Combined with prior evidence that married couples are less likely to remain married under large increases in the EITC (Dickert-Conlin and Houser 2002), these small violent crime effects may be an unexpected byproduct of domestic violence arrests that, within our data, cannot be distinguished from assault and other violent offenses. The association with increased violent and drug crimes for men could be a byproduct of the same marriage effects, or could represent a labor market crowd-out effect from increased female participation (Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2001). Our data do not contain sufficient information to tease out a mechanism behind these results.

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 9 reports results from different subsamples based on the highest level of education completed by the released prisoner, for both 1- and 3-year recidivism rates. 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 3-year recidivism for women are present and consistent across education levels.

Finally, we consider whether our results vary by race. Table 10 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 1-year recidivism rates. The negative impact of the EITC for women when considering 3-year recidivism rates is broadly consistent across race and ethnicity as well. While the coefficient on $EITC_{st} \times Female_i$ is statistically significant and considerably larger for Hispanics, the cumulative effect of the EITC for female released prisoners is only statistically significant for white female prisoners. 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 earlier, we observe a positive relationship between state EITC top-ups and 1-year returns to prison for black and Hispanic male released prisoners, though the effect drops out for 3-year returns of black males.

6 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.[43]

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 that have criminal records by various demographic characteristics is somewhat difficult to come by, though several authors have tried. Shannon et al. (2017) estimate that 15% of the black, male population in the United States has been to prison and 33% have a felony conviction. Bucknor et al. (2016) estimate that 39-44% of black males have a conviction, whereas only 9-10% of white males do and only about 1% of all females do; amongst males they estimate that only around 4% of the male population with any college has a conviction whereas 16-18% of those with a high school degree and 60-68% 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 post-secondary 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

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

register to vote due to being "ineligible".

To most closely adhere to the previous literature, we start by replicating analysis in Allegretto et al. (2011) (ADR) and Neumark et al. (2014) (NSW) for teenagers. We use data from the CPS outgoing rotation groups from 1990-2016 and, merging in unemployment rates by state and month from the Local Area Unemployment Statistics (LAUS) (non-seasonally 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 is presented in Panel A Table 11, generating qualitatively similar results—the ADR specifications (columns 2-4) report effects not statistically distinguishable from 0, while the NSW specifications (columns 5-7) with polynomial time-trends report a negative effect on employment.[44]. 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 between the ages of 24-55, a group with a relatively high probability of having a criminal record.[45] Both the ADR and NLW 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 Appendix H we re-run these specifications with other groups not likely to have a criminal record, such as college-education black men, low-skill white men, 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.

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.[46] From 2004-2016, there are 5,934 individuals who say they are citizens

[44] For a direct comparison with this previous literature, see Appendix H)

[45] 24 and 55 represent the 10th and 90th percentile, respectively, of age at release in the NCRP data.

[46] 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

over the age of 18 where the primary reason they did not register is ineligibility, and thus likely have a record of some kind.[47] The November supplement also includes questions about employment and basic demographic characteristics.

Table 12 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 et al. 2011 to verify that the November supplement data are not dramatically different from the main CPS data. [48] There are some differences again likely due to this not being a strict replication, using a different time period and only November data.[49] Panel A rather serves as a coarse proof-of-concept that the November CPS produces comparable effects for teens.[50]

Panel B of Table 12 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, like for low-skill black men generally, we observe *positive* coefficients on the minimum wage: the coefficient in column 1 implies that a 1% increase in the minimum wage is associated with a 0.0019 pp 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 5000 individuals over 10 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 results, nonetheless, remains consistent with our previous subsample analysis of the CPS.[51]

The results from these analyses provide suggestive evidence for the plausibility of a direct

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.

[47] 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.

[48] We use year- rather than quarter-fixed effects since we only have data from November.

[49] 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.

[50] Due to only having data from November every-other-year, we do not include the higher-order polynomial time trends as the data is 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.

[51] For the sake of comparison and as an additional test of the concept, in Panel C of Table 12 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). And as expected we find very small, somewhat imprecise coefficients on the log minimum wage on employment.

employment mechanism within our conceptual framework. 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. In their analysis of the 2nd phase in of the Seattle Minimum Wage Ordinance, Jardim et al. (2017) found evidence that employers responded to higher minimum wages by substituting more experienced workers for young, novice workers. 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.

7 Conclusion

In this paper, 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 to 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. This observed reduction, within our theoretical framework, implies that, on net, there are more individuals for whom their wages of crime are higher than their uncontrolled market wage—the higher minimum wage draws them into the legal labor market, a phenomenon determined on the supply side of the labor market. Exploratory analysis of CPS data, replicating from the literature prior estimates of employment effects of the minimum wage on respondents more likely to have a felony record, suggests the possibility of net positive employment effects of minimum wages on employment for those carrying a criminal record.

We find some evidence that EITC wage subsidies reduce recidivism, as well, but only for women. In light of the uniform effects of minimum wages across gender, we believe this gender-specificity of the EITC is a byproduct 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. Disaggregating the underlying employment and wage effects of the EITC for men and women, as well as parents and non-parents, 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 3-year recidivism as a 5 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 the federal credit, a state top-up of 5 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 5 percentage point increase in a state EITC corresponds to the same expected reduction in female 3-year recidivism as \$1,000 worth of additional (full-time) income via a \$0.50 increase in the minimum wage.[52] 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-skill black men and U.S. citizens over age 18 who claim they are ineligible to vote. In particular, we are unable to connect our results to characteristics of an individual's household, family, or social network.

[52]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.

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 to 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 with the ambition of moving to $15 an 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; 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 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.

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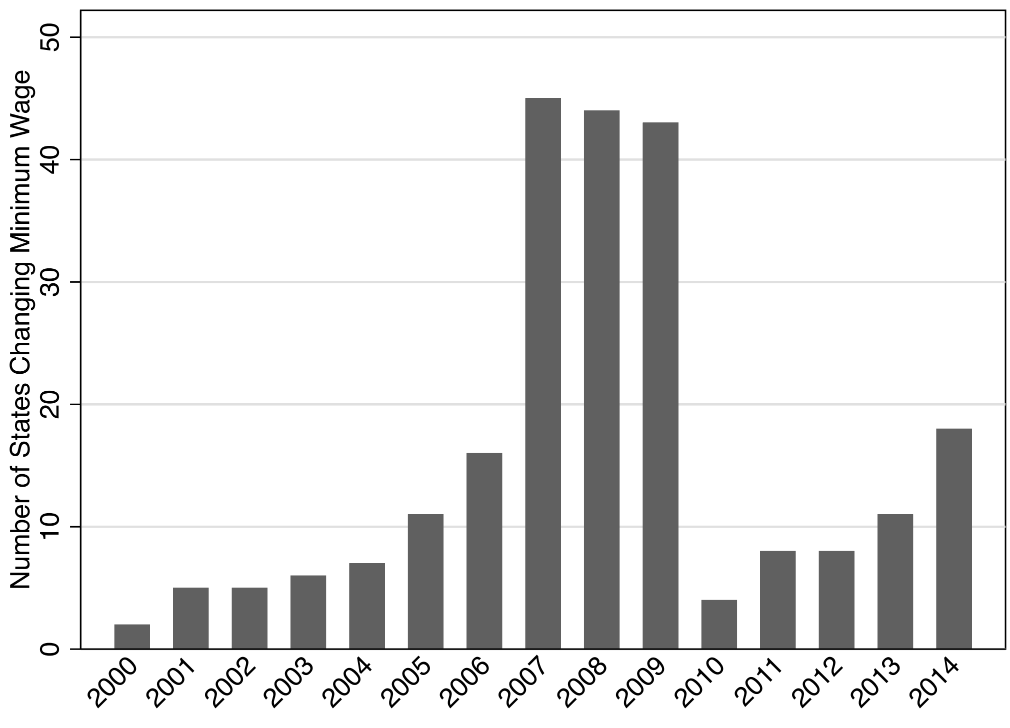
Figure 1: Number of States Changing Minimum Wage by Year



Note: 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.

Figure 2: 1 Year Recidivism Rates for CA versus Other States

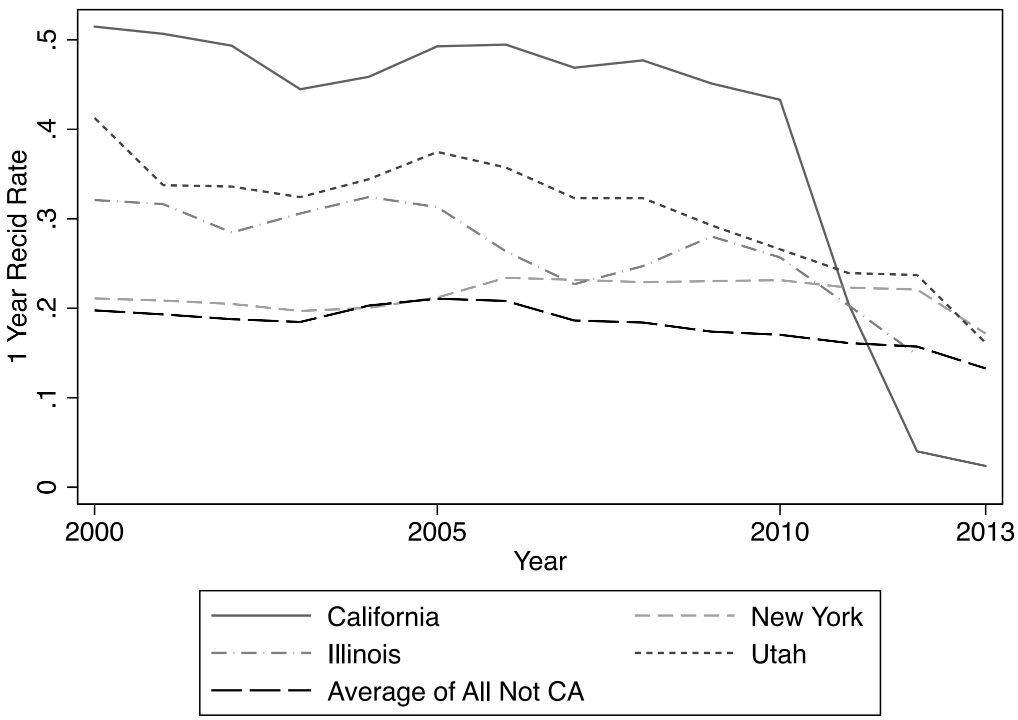
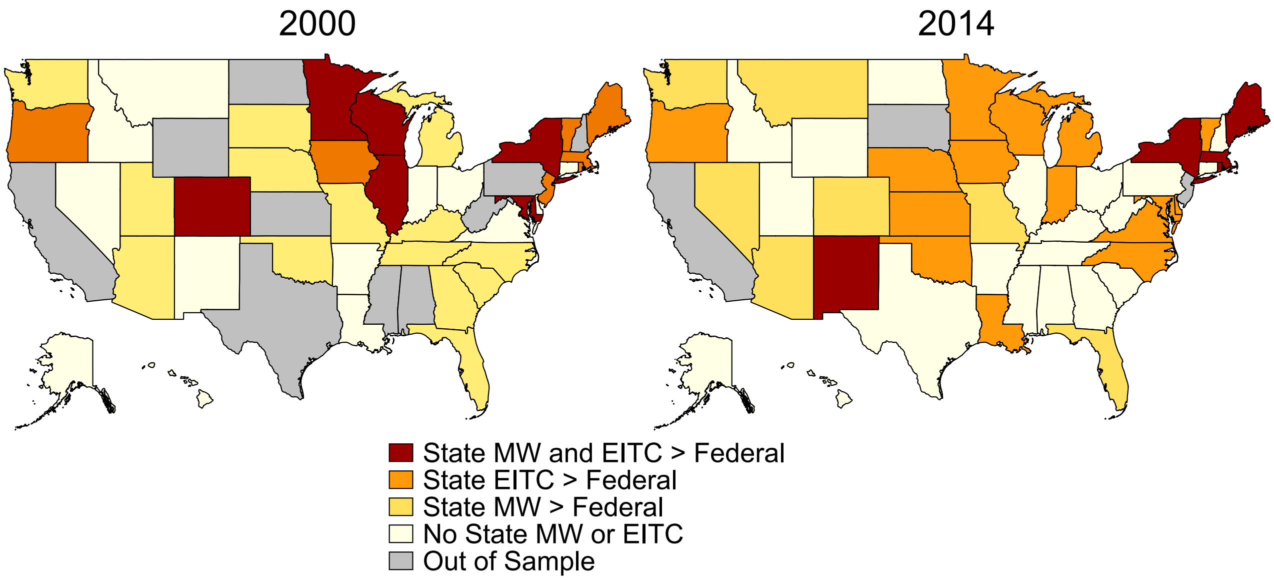
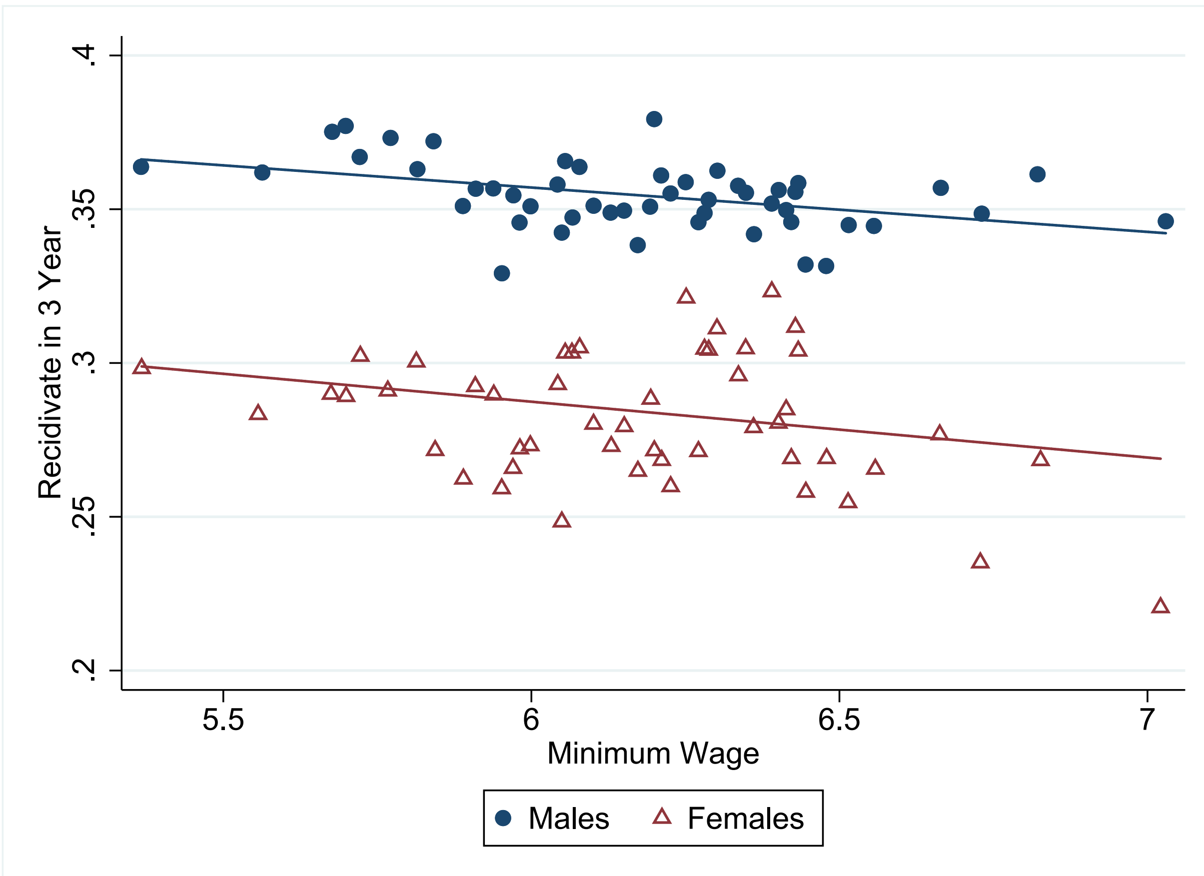


Figure 3: States with Minimum Wages Above Federal and EITC top-ups in January 2000 and January 2014



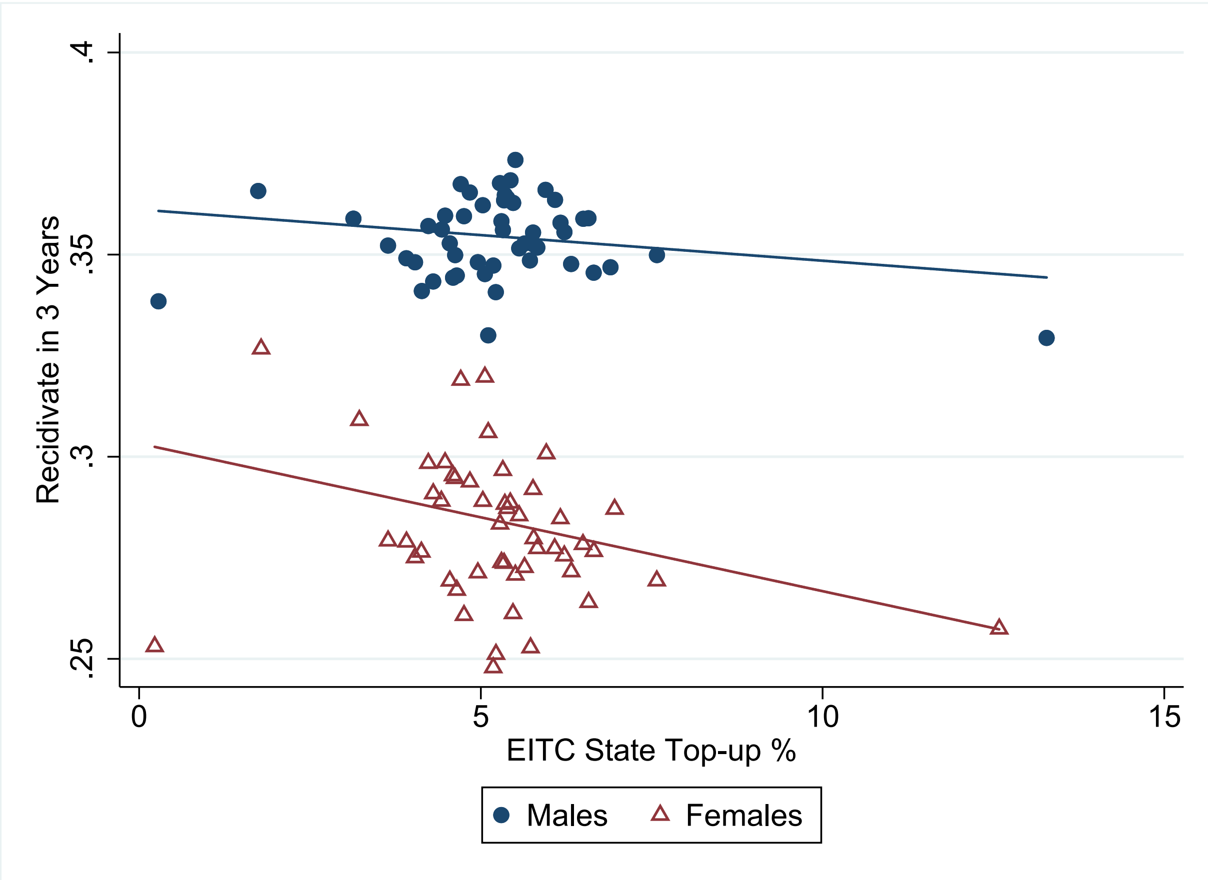
Note: This map reports state Minimum Wage and EITC programs at the beginning and end of our sample. States not included in our sample are reported as “out of sample”.

Figure 4: Correlation between Minimum Wage and Recidivism



Note: To construct the binned scatter plots we plot the mean offender probability of recidivism within 3-years of release (y-axis) over the minimum wage (x-axis) for each of the 442 state-year combinations in the data, demeaning for state and year fixed effects. The observed state-year residual rates of recidivism are divided into fifty equal-sized bins and plotted over the mean minimum wage for each bin. Fifty bins were chosen for symmetry with the subsequent plot of EITC state topups (Figure 5). The figure is qualitatively identical plotted with twenty bins. The line shows the best linear fit.

Figure 5: Correlation between State EITC Top-Ups and Recidivism



Note: To construct the binned scatter plots we plot the mean offender probability of recidivism within 3-years of release (y-axis) over the state EITC top-up percent (x-axis), for the 442 state-year combinations in the data, demeaning for state and year fixed effects. The observed state-year residual rates of recidivism are divided into fifty equal-sized bins and plotted over the mean EITC from each bin. Fifty bins were chosen to so that the states without a state top-up (i.e. zeros) were not over-represented as bins. The figure is qualitatively identical plotted with twenty bins, but far sparser. The line shows the best linear fit.

Table 1: Summary Statistics: Characteristics of Sample

	All	Recidivate 1 Year	Recidivate 3 Years
Male	0.882	0.903	0.905
White (Not Hisp)	0.427	0.415	0.402
Black (Not Hisp)	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 Off. - Violent	0.216	0.192	0.189
Prior Off. - Property	0.289	0.341	0.334
Prio Off. - 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 Perc	5.229	5.962	5.532
Observations	5786062	999321	1645055

Note: "Recidivate 1 Year" indicates those released prisoners who returned to prison within 1 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 3 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 it's own EITC, and thus that row represents a proportion; State EITC Perc is the average percent of the federal EITC that the top-up represents.

Table 2: Summary Statistics: Recidivism Rates

	(1) Recidivate 1 Year	(2) Recidivate 3 Years
Overall	0.173	0.346
Men	0.177	0.355
Women	0.142	0.284
Black (Not Hisp)	0.178	0.370
White (Not Hisp)	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 Off. - Violent	0.032	0.062
Returning Off. - Property	0.057	0.113
Returning Off. - Drug	0.047	0.103
Observations	5786062	4749284

Note: "Recidivate 1 Year" indicates those released prisoners who returned to prison within 1 year of their release (analogous for "Recidivate 3 Year"). Column 2 has fewer observations to allow everyone to have 3 years of post-release data, where as Column 1 only requires 1 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 MW Changes	4.73	2.54	2.00	13.00
Size of MW Change	0.51	0.33	0.04	1.80
Size of MW Change (Perc)	0.08	0.06	0.01	0.35
Has State EITC	0.39	0.49	0.00	1.00
State EITC Perc	6.39	10.22	0.00	40.00

Note: Each observation is a state-year-month, hence there are 9180 observations (51 states including DC x 15 years x 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 Perc is the percent of the federal EITC that the State EITC represents.

Table 4: Minimum Wage and State EITC Availability on Recidivism Rates

	Any State EITC		State EITC Percent	
	(1)	(2)	(3)	(4)
	1 year	3 year	1 year	3 year
Min Wage	-0.0094**	-0.0153***	-0.0094**	-0.0152***
	(0.0040)	(0.0045)	(0.0040)	(0.0045)
Min Wage x Female	0.0023	0.0029	0.0020	0.0026
	(0.0024)	(0.0037)	(0.0025)	(0.0038)
State EITC	0.0090	0.0001	0.0008	-0.0002
	(0.0056)	(0.0069)	(0.0007)	(0.0008)
State EITC x Female	-0.0225*	-0.0316***	-0.0009**	-0.0015***
	(0.0115)	(0.0106)	(0.0004)	(0.0005)
Min Wage Coef:				
wild bootstrap p	0.078	0.008	0.056	0.012
Female EITC Effect:				
Total	-0.0135	-0.0314	-0.0001	-0.0017
cluster-robust p	0.264	0.017	0.882	0.067
wild bootstrap p	0.343	0.039	0.936	0.252
Mean Recid Rate:				
All	0.173	0.346	0.173	0.346
Male	0.177	0.355	0.177	0.355
Female	0.142	0.284	0.142	0.284
Observations	5786062	4749284	5786062	4749284

Note: The dependent variable is return to prison in the same state within 1 or 3 years of release (indicated in the column heading). Minimum wage is measured in dollars, State EITC is an indicator for the existence of a state top-up in Columns 1 and 2 and is the percent of the federal EITC available to those in that state in Columns 3 and 4, measured in percentage points; all are measured in the state and month the offender was released. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. 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 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 5: Trend Analysis - 1 Year Recidivism Rates

	Leading MWs		State-Specific Time Trend Polynomials			Division X Year FE	Binding Changes
	(1) t+1	(2) t+2	(3) Linear	(4) 2nd	(5) 3rd	(6)	(7)
Min Wage	-0.0081**	-0.0090**	-0.0044	-0.0069**	-0.0068**	-0.0085*	-0.0095*
	(0.0040)	(0.0038)	(0.0039)	(0.0029)	(0.0027)	(0.0047)	(0.0047)
Min Wage x Female	0.0023	0.0023	0.0022	0.0022	0.0023	0.0026	
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	
State EITC	0.0089	0.0084	0.0009	0.0067	-0.0071	0.0119*	
	(0.0057)	(0.0058)	(0.0057)	(0.0047)	(0.0077)	(0.0060)	
State EITC x Female	-0.0225*	-0.0225*	-0.0234*	-0.0232*	-0.0233*	-0.0226*	
	(0.0115)	(0.0115)	(0.0117)	(0.0116)	(0.0116)	(0.0116)	
Min Wage Lead	-0.0024	-0.0046					
	(0.0063)	(0.0055)					
Bound MW							0.0034
							(0.0331)
Min Wage X Bound							-0.0010
							(0.0052)
Min Wage Coef:							
wild bootstrap p	0.084	0.039	0.327	0.028	0.019	0.147	0.082
Female EITC Effect:							
Total	-0.0137	-0.0141	-0.0225	-0.0165	-0.0304	-0.0107	
cluster-robust p	0.260	0.251	0.097	0.229	0.056	0.314	
wild bootstrap p	0.357	0.374	0.134	0.322	0.108	0.464	
Observations	5786062	5786062	5786062	5786062	5786062	5786062	5786062

Note: The dependent variable is return to prison in the same state within 1-year of release. Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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 Column 1, with additions as indicated in the column headers. Bound MW is an indicator for a state-year that was bound by one of the 3 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. Results with 3-year recidivism as dependent variable can be found in Appendix G Table G.1.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 6: Robustness - Unemployment Rate and Substate Minimum Wages

	(1) State Unemp	(2) MW at Admit	(3) Avg MW 6 Months	(4) Avg MW 12 Months
Min Wage	-0.0107**	-0.0086**	-0.0121**	-0.0119*
	(0.0043)	(0.0039)	(0.0052)	(0.0061)
Min Wage x Female	0.0023	0.0024	0.0020	0.0018
	(0.0024)	(0.0025)	(0.0025)	(0.0025)
State EITC	0.0076	0.0091	0.0089	0.0087
	(0.0050)	(0.0056)	(0.0055)	(0.0057)
State EITC x Female	-0.0225*	-0.0225*	-0.0225*	-0.0225*
	(0.0115)	(0.0115)	(0.0115)	(0.0115)
State Unemp Rate	0.0034**			
	(0.0017)			
MW Admit		-0.0005		
		(0.0021)		
MW 1 Yr Bef Admit		-0.0015		
		(0.0023)		
Min Wage Coef:				
wild bootstrap p	0.042	0.075	0.061	0.128
Female EITC Effect:				
Total	-0.0149	-0.0134	-0.0136	-0.0138
cluster-robust p	0.219	0.269	0.260	0.258
wild bootstrap p	0.305	0.334	0.373	0.354
Observations	5786062	5786036	5786062	5786062

Note: The dependent variable is return to prison in the same state within 1-year of release. Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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 Column 1, with additions as indicated in the column headers. MW at Admit includes controls for the minimum wage in the state-year of admission or 1-year before. Columns 3-4 use the average minimum wage 6- or 12-months after release, respectively, as the main independent variable. Results with 3-year recidivism as dependent variable can be found in Appendix G Table G.2.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 7: Lagged Minimum Wages - 1 Year Recidivism Rates

	(1)	(2)	(3)	(4)
	-1	-2	-3	0, (incl t= -1,-2,-3)
Min Wage at t=	-0.0063	-0.0047	-0.0024	-0.0087*
	(0.0045)	(0.0042)	(0.0048)	(0.0045)
Min Wage x Female	0.0025	0.0037	0.0040	-0.0041
	(0.0024)	(0.0026)	(0.0027)	(0.0043)
Female	-0.0496***	-0.0566***	-0.0575***	-0.0520***
	(0.0140)	(0.0152)	(0.0155)	(0.0163)
Min Wage Coef: *wild bootstrap p*	0.283	0.394	0.694	0.106
Observations	5786062	5786062	5786062	5786062

Note: The dependent variable is return to prison in the same state within 1-year of release. This table estimates the impact of lagged minimum wages on probability of returning to p in an effort to identify potential disemployment effects through slower economic growth. Columns 1-3 use 1-, 2-, and 3-year lags of the minimum wage respectively as the main independent variable of interest. Column (4) includes contemporaneous minimum wage at release, as in previous specifications, *and* all 3 lags. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficients, as suggested by Cameron et al. (2008) for analysis of a small number of clusters, typically ≤ 30 (our analysis is near, but never below this threshold).
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 8: Minimum Wage and State EITCs on Recidivism: by Return Crime Type

	1 Year				3 Year			
	(1) Violent	(2) Property	(3) Drug	(4) Other	(5) Violent	(6) Property	(7) Drug	(8) Other
Min Wage	-0.0006	-0.0049***	-0.0033**	-0.0006	-0.0014	-0.0063***	-0.0058***	-0.0018
	(0.0011)	(0.0012)	(0.0015)	(0.0014)	(0.0015)	(0.0017)	(0.0019)	(0.0016)
Min Wage x Female	0.0019*	0.0006	0.0009	-0.0011	0.0040***	-0.0008	0.0007	-0.0010
	(0.0010)	(0.0006)	(0.0009)	(0.0016)	(0.0014)	(0.0015)	(0.0013)	(0.0025)
State EITC	0.0049**	0.0023	0.0046***	-0.0028	0.0022	0.0007	0.0057**	-0.0084**
	(0.0020)	(0.0038)	(0.0014)	(0.0019)	(0.0019)	(0.0054)	(0.0023)	(0.0037)
State EITC x Female	-0.0081**	-0.0033	-0.0087	-0.0025	-0.0124***	-0.0012	-0.0141**	-0.0038
	(0.0034)	(0.0035)	(0.0056)	(0.0042)	(0.0043)	(0.0034)	(0.0065)	(0.0065)
Min Wage Coef:								
wild bootstrap p	0.600	0.001	0.068	0.716	0.434	0.004	0.018	0.277
Female EITC Effect:								
Total	-0.0031	-0.0010	-0.0041	-0.0053	-0.0103	-0.0005	-0.0084	-0.0122
cluster-robust p	0.397	0.840	0.406	0.206	0.028	0.933	0.157	0.109
wild bootstrap p	0.430	0.871	0.718	0.304	0.053	0.943	0.283	0.210
Mean Recid Rate:								
All	0.032	0.057	0.047	0.036	0.062	0.113	0.103	0.069
Male	0.034	0.058	0.047	0.037	0.067	0.114	0.103	0.071
Female	0.013	0.051	0.047	0.031	0.025	0.105	0.098	0.055
Observations	5786062	5786062	5786062	5786062	4749284	4749284	4749284	4749284

Note: The dependent variable is return to prison for a certain crime type within 1- or 3-years of release (as indicated in column headers). Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. 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 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 9: Minimum Wage and State EITCs on Recidivism: by Education

	1 Year			3 Year		
	(1) < HS	(2) HS	(3) > HS	(4) < HS	(5) HS	(6) > HS
Min Wage	-0.0152***	-0.0099*	-0.0116	-0.0212***	-0.0165**	-0.0149
	(0.0048)	(0.0053)	(0.0079)	(0.0054)	(0.0062)	(0.0088)
Min Wage x Female	0.0011	0.0035*	0.0073**	-0.0001	0.0059*	0.0045
	(0.0019)	(0.0017)	(0.0032)	(0.0041)	(0.0030)	(0.0033)
State EITC	0.0099*	0.0053	0.0066	-0.0023	-0.0013	-0.0020
	(0.0052)	(0.0075)	(0.0074)	(0.0052)	(0.0110)	(0.0060)
State EITC x Female	-0.0302	-0.0226*	-0.0327**	-0.0359**	-0.0349***	-0.0342**
	(0.0182)	(0.0124)	(0.0152)	(0.0150)	(0.0119)	(0.0137)
Min Wage Coef:						
wild bootstrap p	0.025	0.129	0.273	0.005	0.050	0.198
Female EITC Effect:						
Total	-0.0203	-0.0172	-0.0260	-0.0382	-0.0362	-0.0362
cluster-robust p	0.249	0.261	0.145	0.017	0.054	0.020
wild bootstrap p	0.517	0.334	0.306	0.056	0.122	0.067
Mean Recid Rate:						
All	0.179	0.178	0.148	0.363	0.349	0.299
Male	0.182	0.183	0.154	0.370	0.358	0.309
Female	0.151	0.137	0.115	0.303	0.275	0.241
Observations	2245904	1824652	317609	1907947	1472633	258196

Note: The dependent variable is return to prison in the same state within 1 or 3 years of release by education level at prison entry (indicated in the column heading). Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up in Columns 1 and 2 and is the percent of the federal EITC available to those in that state in Columns 3 and 4 (measured in percentage points); all are measured in the state and month the offender was released. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. 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 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 10: Minimum Wage and State EITCs on Recidivism: by Race/Ethnicity

	1 Year			3 Year		
	(1) Black	(2) White	(3) Hispanic	(4) Black	(5) White	(6) Hispanic
Min Wage	-0.0117**	-0.0082*	-0.0049	-0.0153**	-0.0122**	-0.0120**
	(0.0052)	(0.0041)	(0.0046)	(0.0062)	(0.0050)	(0.0055)
Min Wage x Female	0.0026	0.0013	-0.0008	0.0010	-0.0003	0.0069
	(0.0039)	(0.0019)	(0.0023)	(0.0067)	(0.0033)	(0.0047)
State EITC	0.0156***	0.0044	0.0244**	0.0057	-0.0049	0.0286**
	(0.0051)	(0.0074)	(0.0101)	(0.0067)	(0.0092)	(0.0105)
State EITC x Female	-0.0341**	-0.0116	-0.0302***	-0.0281*	-0.0234***	-0.0571***
	(0.0164)	(0.0080)	(0.0087)	(0.0145)	(0.0085)	(0.0123)
Min Wage Coef:						
wild bootstrap p	0.082	0.085	0.463	0.038	0.051	0.204
Female EITC Effect:						
Total	-0.0185	-0.0072	-0.0058	-0.0225	-0.0282	-0.0285
cluster-robust p	0.256	0.493	0.714	0.147	0.042	0.124
wild bootstrap p	0.444	0.540	0.783	0.239	0.075	0.266
Mean Recid Rate:						
All	0.178	0.168	0.156	0.370	0.331	0.303
Male	0.182	0.173	0.157	0.379	0.340	0.304
Female	0.135	0.141	0.149	0.284	0.278	0.289
Observations	2457794	2471327	696338	2062432	1998305	561803

Note: The dependent variable is return to prison in the same state within 1 or 3 years of release by race/ethnicity (indicated in the column heading). The three categories are mutually exclusive: Black is non-Hispanic Black, White is non-Hispanic White. Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up in Columns 1 and 2 and is the percent of the federal EITC available to those in that state in Columns 3 and 4 - measured in percentage points; all are measured in the state and month the offender was released. A female indicator is included in all regressions but not shown for space. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. 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 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table 11: 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) Coef.	-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	447719	447719	447719	447719	447719	447719	447719
Panel B: Low-skill Black Men 25-54, CPS 1990-2016							
ln(MW) Coef.	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	104020	104020	104020	104020	104020	104020	104020
Panel C: Low-skill Black Women 25-54, CPS 1990-2016							
ln(MW) Coef.	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	129329	129329	129329	129329	129329	129329	129329
DivXQuarterFE	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

Note: Data from the Current Population Survey Outgoing Rotation Groups, population stratification and years are indicated in the Panel titles. Panel A focuses on teenagers (15-19yo), 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, low skill here indicating the absence of post-secondary 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, non-seasonally 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 baselines 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. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$

Table 12: Minimum Wage Effects on Employment by Voting Eligibility: CPS Data

	(1)	(2)	(3)	(4)
Panel A: Teenagers 15-19, CPS November Supplement				
Ln(MW) Coef.	-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	101105	101105	101105	101105
Panel B: Ineligible to Vote				
Ln(MW) Coef.	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	5934	5934	5934	5934
Panel C: Not Ineligible to Vote				
Ln(MW) Coef.	-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	644516	644516	644516	644516
DivXYearFE	N	Y	N	Y
Linear Trends	N	N	Y	Y

Note: Data from the November Current Population Survey (1990-2016; voting eligibility only available from in even years (Federal Election Years) from 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 (15-19yo). 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 x year effects as indicated. Standard errors clustered on state in parentheses.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$, $+p < 0.15$

A Minimum Wages and EITC Values by State

Table A.1: Minimum Wages and EITC Top-Ups by State: 2000-2014

	2000		2001		2002		2003		2004		2005		2006		2007	
	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC
AL	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	
AK	5.65		5.65		5.65		7.15		7.15		7.15		7.15		7.15	
AZ	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.75	
AR	5.15		5.15		5.15		5.15		5.15		5.15		5.43		6.25	
CA	5.75		6.25		6.75		6.75		6.75		6.75		6.75		7.50	
CO	5.15	10%	5.15	10%	5.15		5.15		5.15		5.15		5.15		6.85	
CT	6.15		6.40		6.70		6.90		7.10		7.10		7.40		7.65	
DE	5.78		6.15		6.15		6.15		6.15		6.15		6.15	20%	6.65	20%
DC	6.15	10%	6.15	25%	6.15	25%	6.15	25%	6.15	25%	6.60	35%	7.00	35%	7.00	35%
FL	5.15		5.15		5.15		5.15		5.15		5.82		6.40		6.67	
GA	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	
HI	5.25		5.25		5.75		6.25		6.25		6.25		6.75		7.25	
ID	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	
IL	5.15	5%	5.15	5%	5.15	5%	5.15	5%	5.50	5%	6.50	5%	6.50	5%	7.00	5%
IN	5.15		5.15		5.15		5.15	6%	5.15	6%	5.15	6%	5.15	6%	5.50	6%
IA	5.15	6.5%	5.15	6.5%	5.15	6.5%	5.15	6.5%	5.15	6.5%	5.15	6.5%	5.15	6.5%	5.94	7%
KS	5.15	10%	5.15	10%	5.15	15%	5.15	15%	5.15	15%	5.15	15%	5.15	15%	5.50	17%
KY	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.56	
LA	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	
ME	5.15	5%	5.15	5%	5.75	5%	6.25	4.92%	6.28	4.92%	6.39	4.92%	6.56	5%	6.81	5%
MD	5.15	15%	5.15	16%	5.15	16%	5.15	16%	5.15	20%	5.15	20%	5.15	20%	6.15	20%
MA	6.00	10%	6.75	15%	6.75	15%	6.75	15%	6.75	15%	6.75	15%	6.75	15%	7.50	15%
MI	5.15		5.15		5.15		5.15		5.15		5.15		5.60		7.05	
MN[a]	5.15	33%	5.15	33%	5.15	33%	5.15	33%	5.15	33%	5.57	33%	6.15	33%	6.15	33%
MS	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	
MO	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.50	
MT	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.15	
NE	5.15		5.15		5.15		5.15	8%	5.15	8%	5.15	8%	5.15	8%	5.50	8%
NV	5.15		5.15		5.15		5.15		5.15		5.15		5.32		6.24	
NH	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.72	
NJ	5.15	10%	5.15	15%	5.15	17.5%	5.15	20%	5.15	20%	5.40	20%	6.40	20%	7.15	20%
NM	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	8%

	2008		2009		2010		2011		2012		2013		2014	
	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC
AL	6.20		6.90		7.25		7.25		7.25		7.25		7.25	
AK	7.15		7.20		7.75		7.75		7.75		7.75		7.75	
AZ	6.90		7.25		7.25		7.35		7.65		7.80		7.90	
AR	6.40		6.90		7.25		7.25		7.25		7.25		7.25	
CA	8.00		8.00		8.00		8.00		8.00		8.00		8.50	
CO	7.02		7.28		7.25		7.36		7.64		7.78		8.00	10%
CT	7.65		8.00		8.25		8.25	30%	8.25	30%	8.25	30%	8.70	27.5%
DE	7.15	20%	7.20	20%	7.25	20%	7.25	20%	7.25	20%	7.25	20%	7.54	20%
DC	7.28	40%	7.90	40%	8.25	40%	8.25	40%	8.25	40%	8.25	40%	8.88	40%
FL	6.79		7.23		7.25		7.29		7.67		7.79		7.93	
GA	6.20		6.90		7.25		7.25		7.25		7.25		7.25	
HI	7.25		7.25		7.25		7.25		7.25		7.25		7.25	
ID	6.20		6.90		7.25		7.25		7.25		7.25		7.25	
IL	7.63	5%	7.88	5%	8.13	5%	8.25	5%	8.25	5%	8.25	5%	8.25	10%
IN	6.20	6%	6.30	9%	7.25	9%	7.25	9%	7.25	9%	7.25	6%	7.25	9%
IA	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	14%
KS	6.20	17%	6.90	17%	7.25	18%	7.25	18%	7.25	18%	7.25	18%	7.25	17%
KY	6.20		6.90		7.25		7.25		7.25		7.25		7.25	
LA	6.20	3.5%	6.90	3.5%	7.25	3.5%	7.25	3.5%	7.25	3.5%	7.25	3.5%	7.25	3.5%
ME	7.06	5%	7.31	5%	7.50	5%	7.50	5%	7.50	5%	7.50	5%	7.50	5%
MD	6.35	20%	6.90	20%	7.25	25%	7.25	25%	7.25	25%	7.25	25%	7.25	25%
MA	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%
MI	7.28	10%	7.40	10%	7.40	20%	7.40	20%	7.40	6%	7.40	6%	7.65	6%
MN[a]	6.35	33%	6.90	33%	7.25	33%	7.25	33%	7.25	33%	7.25	33%	7.56	33%
MS	6.20		6.90		7.25		7.25		7.25		7.25		7.25	
MO	6.65		7.15		7.25		7.25		7.25		7.35		7.50	
MT	6.40		7.08		7.25		7.35		7.65		7.80		7.90	
NE	6.20	10%	6.90	10%	7.25	10%	7.25	10%	7.25	10%	7.25	10%	7.25	10%
NV	6.59		7.20		7.90		8.25		8.25		8.25		8.25	
NH	6.76		7.25		7.25		7.25		7.25		7.25		7.25	
NJ	7.15	22.5%	7.20	22.5%	7.25	20%	7.25	20%	7.25	20%	7.25	20%	8.25	20%
NM	6.53	10%	7.50	10%	7.50	10%	7.50	10%	7.50	10%	7.50	10%	7.50	10%

NY	5.15 22.5%	5.15 25.0%	5.15 27.5%	5.15 30%	5.15 30%	6.00 30%	6.75 30%	7.15 30%	7.15 30%	7.20 30%	7.25 30%	7.25 30%	7.25 30%	7.31 30%	8.06 30%
NC	5.15	5.15	5.15	5.15	5.15	5.15	5.15	6.15	6.35 3.5%	6.90 3.5%	7.25 5%	7.25 5%	7.25 5%	7.25 5%	7.25 5%
ND	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
OH	5.15	5.15	5.15	5.15	5.15	5.15	5.15	6.85	7.00	7.30	7.30	7.40	7.70	7.85	7.95
OK	5.15	5.15	5.15 5%	5.15 5%	5.15 5%	5.15 5%	5.15 5%	5.50 5%	6.20 5%	6.90 5%	7.25 5%	7.25 5%	7.25 5%	7.25 5%	7.25 5%
OR	6.50 5%	6.50 5%	6.50 5%	6.90 5%	7.05 5%	7.25 5%	7.50 5%	7.80 5%	7.95 6%	8.40 6%	8.40 6%	8.50 6%	8.80 6%	8.95 6%	9.10 6%
PA	5.15	5.15	5.15	5.15	5.15	5.15	5.15	6.70	7.15	7.20	7.25	7.25	7.25	7.25	7.25
RI	5.82 26%	6.15 25.5%	6.15 25%	6.15 25%	6.75 25%	6.75 25%	7.04 25%	7.40 25%	7.40 25%	7.40 25%	7.40 25%	7.40 25%	7.40 25%	7.75 25%	8.00 25%
SC	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
SD	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
TN	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
TX	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
UT	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
VT	5.75 32%	6.25 32%	6.25 32%	6.25 32%	6.75 32%	7.00 32%	7.25 32%	7.53 32%	7.68 32%	8.06 32%	8.06 32%	8.15 32%	8.46 32%	8.60 32%	8.73 32%
VA	5.15	5.15	5.15	5.15	5.15	5.15	5.15 20%	5.50 20%	6.20 20%	6.90 20%	7.25 20%	7.25 20%	7.25 20%	7.25 20%	7.25 20%
WA	6.50	6.72	6.90	7.01	7.16	7.35	7.63	7.93	8.07	8.55	8.55	8.67	9.04	9.19 10%	9.32 10%
WV	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25	7.25
WI[b]	5.15 4/13/44%	5.15 4/13/44%	5.15 4/13/44%	5.15 4/13/44%	5.15 4/13/44%	5.47 4/13/44%	6.17 4/13/44%	6.50 4/13/44%	6.53 4/13/44%	6.90 4/13/44%	7.25 4/13/44%	7.25 4/11/44%	7.25 4/11/44%	7.25 4/11/44%	7.25 4/11/44%
WY	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25
Fed	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.20	6.90	7.25	7.25	7.25	7.25	7.25

Notes: Minimum wages shown are the average for the year, in the actual analysis we have the year AND month and thus can be more exact. EITC is represented as the percent of the federal EITC. CO and WA state EITCs are on the books but currently unfunded and are coded in our data as 0s for the EITC tops ups in 2013 and 2014.

[a] MN's EITC is not structured as a percentage of the federal. Depending on income it represents 25-45%, what is shown is the average

[b] WI's EITC varies based on number of children, shown are for 1/2/3 children

B State and County Identification

The NCRP data, unfortunately, do not include information on the state or county the individual lived in after incarceration.

Our research into processes for different jurisdictions indicates that individuals, for the most part, are released into:

1. the county they lived in prior to incarceration or
2. the county that convicted them (if no prior address can be established)

unless there are pressing safety or rehabilitation reasons not to do either of the above.[53] The offender can also petition to be released into a different county/state - usually because they can show that they have family there that can support them, they have proof of employment in that area, or because they need treatment that is only available in that county.[54]

The 2000-2014 NCRP data do not contain address of residence prior to incarceration. They do, however contain state and county of conviction. Using data from California, Raphael and Weiman (2007) show that 90% of parolees are returned to the county they were convicted in. Thus, both Raphael and Weiman (2007) and Schnepel (2018), which also analyzes NCRP data from California, use the county of conviction as an approximation of the county they are released into.

Starting in 2015, the NCRP has begun to collect data on State and County of last known address. We obtained this data, initially for the purposes of expanding our analysis. Unfortunately, in this version of the data, according to the codebook: "Due to a data-processing error, all county codes for [12 states] are set to missing" (National Corrections Reporting Program, 2000-2015 Codebook p. 122).[55] However, these data include 37,312 prison releases that include both the last state of known address and the state of conviction;

[53] In the Federal Prison system: "In most instances, a parolee will be released to the Judicial District in which he or she was convicted or the Judicial District of legal residence" (https://www.justice.gov/uspc/frequently-asked-questions). In California,"an inmate who is released on parole shall be returned to the county that was the last legal residence of the inmate prior to his or her incarceration" (https://www.cdcr.ca.gov/Parole/Parole_Requirements/index.html). In New York State, if a family member does not pick up a recently released prisoner and they don't have transportation available, a bus ticket will be provided to the county where they were convicted. For example, if the conviction occurred in Nassau County, a bus ticket will be provided to Nassau County (http://www.doccs.ny.gov/FamilyGuide/cominghomebrochure.html). In Oregon: "the board shall order as a condition of parole that the person reside for the first six months in the county where the person resided at the time of the offense that resulted in the imprisonment, and if no identifiable address is found then they are returned to county where offense occurred" (https://www.oregon.gov/doc/CC/docs/pdf/County_of_Record.pdf).

[54] See for example documents from Wisconsin: https://appsdoc.wi.gov/public/faq and Oregon: https://www.oregon.gov/doc/CC/docs/pdf/County_of_Record.pdf).

[55] E-mail correspondence with NACJD and the BJS did not clarify what the error was, and thus we decided not to rely on the 2000-2015 version of the data but use the 2000-2014 in our analysis.

and 31,840 that include both last known county of address and county of conviction. Using these data, we see that 94.5% of offenders lived in the same state they were convicted in. This makes us pretty confident that state of conviction is a good proxy for state of previous residence, which by the above is a good proxy for state of release. However, only 69.6% of releasees for whom we have the requisite data lived in the *same county* they were convicted in. So while that is still a majority, there is significantly more measurement error when trying to approximate county of release with county of convicted if most people are actually sent back to county of previous residence. Therefore, for a majority of our analysis, we rely on the state of conviction variable to define state of release and focus on state variation. In one table, we use the county of conviction as a proxy for county of release to allow us finer geographic variation but with less reliability.

Approximately 2% of our observations are missing county/state of conviction. Amongst the observations not missing county/state of conviction, the state of conviction is the same as the state providing the data for 99.9% of the sample. The few thousand observations where this variable differs seem to be cases where the person was sent out of state to serve their sentence (the reason is not given). This could be due to prison overcrowding in the state of conviction, request by the prisoner to be imprisoned in a different state, request of either state system to house the prisoner in a different state—we have no way of knowing. In those cases, we are less clear about the state the prisoner will return to. If anything, it seems more likely to be the state of conviction rather than the state that imprisoned them. However, our recidivism variable indicates if there is a return to prison in the same state that imprisoned an individual, so if they are more likely to return to the state of conviction, it is unlikely we will see them recidivating even if they did. Thus, due to higher uncertainty about return state for these individuals and the issue with defining recidivism for them, we drop this 0.01% of our sample in our analysis. Because the state of conviction and the state providing the data are so highly correlated, for the 2% of the sample missing county/state of conviction, we impute state of conviction with the state that provided the data to the NCRP.

C County Level Analysis

As outlined in Appendix B, our main analyses focus on the state of incarceration as a proxy for state of residence post-release. We also have county of conviction which we can use as a proxy for county of residence post-release, though this is less reliable as it appears empirically some fraction of individuals are likely to live in a different county. They may even make a choice of which county to live in based on local labor market conditions, making the measurement error not purely selection-free. Still, focusing on county allows us a finer set of controls for the local experience of the individual, so below we present results using the county data.

C.1 Identifying and Defining Shared State Border Counties

Pairs of neighboring counties, each on opposite sides of a state border, are attractive as controls. Predicated on the intuition that adjacent counties are similar, changes in the minimum wage or EITC policy on one side of a state border separating two counties offers a compelling identification strategy. Allegretto et al. (2017) use contiguous pairs of counties along state borders as a spatial control, including a county pair by state-period fixed effect in their regression models. We use their set of identified ”county pairs” to construct our own set of fixed effects.

Our data presents a slightly different challenge, however. Where the Allegretto et al. analysis is looking at unemployment across county pairs, our unit of observation is the individual who lives in a given county at a given time. A county may, of course, share borders with more than one county across a state line. If we include only individuals who live in counties who share a cross-state border with a single county, we are forced to drop >90% of our observations. To better cope with the irregular patterning of shared county we borders, we construct “clusters” of counties who share borders with one or two other counties on the other side of a border. These clusters will include triplets–one with two border counties, and two counties with one (and, very rarely, quadruplets where two counties both share a single state border with two others). Expanding the our identification of border-sharing counties to a ”county cluster” allows us to recover a considerably larger number of observations.

Table C.1: County - 1 Year Recidivism

	(1) Baseline With County	(2) County Unemp	(3) County FE	(4) No Substate	(5) Substate All	(6) County Border Cluster
Min Wage	-0.0079**	-0.0078**	-0.0077**	-0.0080**	-0.0079**	-0.0072*
	(0.0038)	(0.0038)	(0.0038)	(0.0038)	(0.0038)	(0.0039)
Min Wage x Female	0.0023	0.0023	0.0022	0.0023	0.0023	0.0009
	(0.0025)	(0.0025)	(0.0025)	(0.0025)	(0.0025)	(0.0028)
State EITC	0.0083	0.0085	0.0093	0.0082	0.0083	0.0192**
	(0.0058)	(0.0059)	(0.0066)	(0.0058)	(0.0058)	(0.0077)
State EITC x Female	-0.0235*	-0.0235*	-0.0233*	-0.0235*	-0.0235*	-0.0343**
	(0.0118)	(0.0118)	(0.0115)	(0.0118)	(0.0118)	(0.0146)
Female	-0.0419***	-0.0419***	-0.0419***	-0.0419***	-0.0419***	-0.0307
	(0.0153)	(0.0153)	(0.0155)	(0.0153)	(0.0153)	(0.0201)
Min Wage Coef:						
wild bootstrap p	0.085	0.091	0.083	0.088	0.068	0.130
Female EITC Effect:						
Total	-0.0152	-0.0150	-0.0140	-0.0152	-0.0152	-0.0150
cluster-robust p	0.228	0.239	0.257	0.227	0.228	0.369
wild bootstrap p	0.305	0.328	0.350	0.311	0.313	0.568
Observations	5579060	5579060	5579060	5565953	5579060	1437042

Note: The dependent variable is return to prison in the same state within 1-year of release. Column 1 recreates Column 1 of Table 4, for the sample of individuals for whom we have County of conviction. Column 2 adds in the county unemployment rate. Column 3 adds in County fixed effects in addition to state- and year- fixed effects. Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).

Table C.2: County - 3 Year Recidivism

	(1) Baseline With County	(2) County Unemp	(3) County FE	(4) No Substate	(5) Substate All	(6) County Border Cluster
Min Wage	-0.0136***	-0.0130***	-0.0132***	-0.0138***	-0.0136***	-0.0068
	(0.0043)	(0.0041)	(0.0042)	(0.0043)	(0.0043)	(0.0041)
Min Wage x Female	0.0033	0.0033	0.0032	0.0034	0.0033	-0.0020
	(0.0039)	(0.0039)	(0.0039)	(0.0038)	(0.0039)	(0.0050)
State EITC	-0.0004	0.0002	-0.0003	-0.0005	-0.0004	0.0182**
	(0.0072)	(0.0072)	(0.0077)	(0.0072)	(0.0072)	(0.0083)
State EITC x Female	-0.0323***	-0.0323***	-0.0318***	-0.0324***	-0.0323***	-0.0335***
	(0.0107)	(0.0107)	(0.0104)	(0.0107)	(0.0107)	(0.0094)
Female	-0.0778***	-0.0778***	-0.0789***	-0.0784***	-0.0778***	-0.0464
	(0.0234)	(0.0234)	(0.0233)	(0.0234)	(0.0234)	(0.0338)
Min Wage Coef:						
wild bootstrap p	0.012	0.019	0.017	0.012	0.013	0.122
Female EITC Effect:						
Total	-0.0327	-0.0322	-0.0321	-0.0329	-0.0327	-0.0153
cluster-robust p	0.017	0.020	0.019	0.017	0.017	0.208
wild bootstrap p	0.044	0.024	0.045	0.049	0.040	0.196
Observations	4580355	4580355	4580355	4569953	4580355	1204908

Note: The dependent variable is return to prison in the same state within 3-year of release. Column 1 recreates Column 2 Table 4 for the sample of individuals for whom County of conviction is not missing. Column 2 adds in the county unemployment rate. Column 3 adds in County fixed effects in addition to state- and year- fixed effects. Column 4 drops any county that includes a jurisdiction with it's own minimum wage different from the States. Column 5 instead assigns any sub-state minimum wage to all individuals convicted in that county even if only a fraction of the county is covered by the minimum wage. Minimum wage is measured in dollars. State EITC is an indicator for the existence of a state top-up. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 4. Total Female EITC Effect=β_3 (on EITC) + β_4 (on EITC $\times$ Female) is the total impact of the EITC for women. For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, 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).

D Defining Bound Minimum Wage Changes

Taking a cue from Clemens and Wither (2016), we focus some of our analysis on states that were bound by federal minimum wage increases. There were 3 increases in the federal minimum wage during our time period: from \$5.15 to \$5.85 on 7/24/2007, to \$6.55 on 7/24/2008, and to \$7.25 on 7/24/2009. We define a state-year pair as being bound by these federal minimum wage changes if, as of January 1 of that year, the state had a minimum wage *below* what would become the federal level in July of that year.

Table D.1 below lists the states considered bound for each of the federal minimum wage change years: 2007, 2008, and 2009.

Table D.1: States Bound by Federal Changes

2007		2008		2009	
State	Jan 1 MW	State	Jan 1 MW	State	Jan 1 MW
				Alaska	$7.15
Alabama	N/A	Alabama	N/A	Alabama	N/A
		Arkansas	$6.25	Arkansas	$6.25
				Delaware	$7.15
				Florida	$7.21
Georgia	$5.15	Georgia	$5.15	Georgia	$5.15
Idaho	$5.15	Idaho	$5.85	Idaho	$6.55
Indiana	$5.15	Indiana	$5.85	Indiana	$6.55
Iowa	$5.15				
Kansas	$2.65	Kansas	$2.65	Kansas	$2.65
Kentucky	$5.15	Kentucky	$5.85	Kentucky	$6.55
Louisiana	N/A	Louisiana	N/A	Louisiana	$6.55
		Maryland	$6.15	Maryland	$6.55
		Minnesota	$6.15	Minnesota	$6.15
Mississippi	N/A	Mississippi	N/A	Mississippi	$6.55
				Missouri	$7.05
		Montana	$6.25	Montana	$6.90
Nebraska	$5.15	Nebraska	$5.85	Nebraska	$6.55
		Nevada	$6.33	Nevada	$6.85
				New Jersey	$7.15
				New York	$7.15
New Hampshire	$5.15	New Hampshire	$6.50		
New Mexico	$5.15	New Mexico	$6.50		
		North Carolina	$6.15	North Carolina	$6.55
North Dakota	$5.15	North Dakota	$5.85	North Dakota	$6.55
Oklahoma	$5.15	Oklahoma	$5.85	Oklahoma	$6.55
				Pennsylvania	$7.15
South Carolina	N/A	South Carolina	N/A	South Carolina	$6.55
South Dakota	$5.15	South Dakota	$5.85	South Dakota	$6.55
Tennessee	N/A	Tennessee	N/A	Tennessee	$6.55
Texas	$5.15	Texas	$5.85	Texas	$6.55
Utah	$5.15	Utah	$5.85	Utah	$6.55
Virginia	$5.15	Virginia	$5.85	Virginia	$6.55
		Wisconsin	$6.50	Wisconsin	$6.50
Wyoming	$5.15	Wyoming	$5.15	Wyoming	$5.15

Note: Data on state minimum wages from the U.S. Department of Labor "Changes in Basic Minimum Wages in Non-Farm Employment Under State Law: Selected Years 1968-2016" available at: https://www.dol.gov/whd/state/stateMinWageHis.htm. Federal minimum wages increased on July 24 of 2007 (to $5.85), of 2008 (to $6.55), and of 2009 (to $7.25). N/A indicates the state did not set a minimum wage, and thus minimum wages in that state are dictated by prevailing federal minimums.

E Main Results Estimated Separately by Gender

Table E.1 presents results from regression specifications identical to Table 4, save that the data is stratified by gender. The availability of a state EITC top-up is of particular interest in the analysis of female recidivism. As noted earlier, we include wild-cluster bootstrap standard errors as a conservative precaution given our relatively small number of state clusters (42). 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, 3 states introduced an EITC top-up (North Carolina, Nebraska, and Oklahoma), while 3 states initially offered a top-up, only to later discontinue the program (Colorado, Indiana, and Michigan).[56] This would, at face value, appear to provide sufficient variation, but given the inclusion of state and year fixed-effects in all of our specifications, a considerable amount of the identification of EITC impact is dependent on six states. Further, given the smaller sample of women and their unequal distribution across states in our sample, some caution is warranted.[57] In their analysis of bootstrap inference, Roodman et al. (2019) note that the wild cluster bootstrap can dramatically *under-reject* 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, the estimates Table E.1 also include errors estimated via bootstrap, where the underlying regression remains clustered by state, but the wild bootstrap is clustered at the both i) state and ii) state and year.[58] The standard errors are largely unchanged, save becoming notably smaller ($p < 0.10$) for the EITC on 3-year female recidivism.

[56] 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

[57] 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.

[58] We have far too many observations for it to be practical to bootstrap at the individual level.

Table E.1: Minimum Wage and State EITC Availability on Recidivism Rates By Gender

	Any State EITC		State EITC Percent	
	(1) 1 year	(2) 3 year	(3) 1 year	(4) 3 year
Panel A: Male				
Min Wage	-0.0091**	-0.0149***	-0.0091**	-0.0149***
	(0.0039)	(0.0044)	(0.0039)	(0.0044)
State EITC	0.0070	-0.0017	0.0008	-0.0002
	(0.0055)	(0.0071)	(0.0007)	(0.0008)
Min Wage Coef:				
wild bootstrap p	0.053	0.006	0.051	0.009
state-year wild bootstrap p	.061	.005	.042	.004
EITC Coef:				
wild bootstrap p	0.400	0.829	0.191	0.852
state-year wild bootstrap p	0.315	0.848	0.359	0.842
Mean Recid Rate:				
male	0.177	0.355	0.177	0.355
Observations	5105236	4198073	5105236	4198073
Panel B: Female				
Min Wage	-0.0105*	-0.0161**	-0.0105*	-0.0160**
	(0.0057)	(0.0065)	(0.0056)	(0.0064)
State EITC	0.0003	-0.0203***	-0.0006	-0.0017
	(0.0055)	(0.0064)	(0.0010)	(0.0011)
Min Wage Coef:				
wild bootstrap p	0.115	0.050	0.113	0.043
state-year wild bootstrap p	.108	.028	.098	.027
EITC Coef:				
wild bootstrap p	0.944	0.175	0.671	0.327
state-year wild bootstrap p	0.938	0.073	0.630	0.258
Mean Recid Rate:				
female	0.142	0.284	0.142	0.284
Observations	680826	551211	680826	551211

Note: The dependent variable is return to prison in the same state within 1 or 3 years of release (indicated in the column heading). Minimum wage is measured in dollars, State EITC is an indicator for the existence of a state top-up in Columns 1 and 2 and is the percent of the federal EITC available to those in that state in Columns 3 and 4, 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 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient and the total Female EITC effect, as suggested by Cameron et al. (2008) when the number of clusters is small. To address the potentially small number of treated clusters for EITC estimates, we also include a state-year subcluster wild bootstrap out of concern for the number of treated clusters (MacKinnon and Webb, 2018; Roodman et al., 2019). $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$

F Event Study Identification Strategy

Taking a cue from Fone et al. (2019), we can apply an event study approach to our analysis. Collapsing to a state-year panel, we estimate

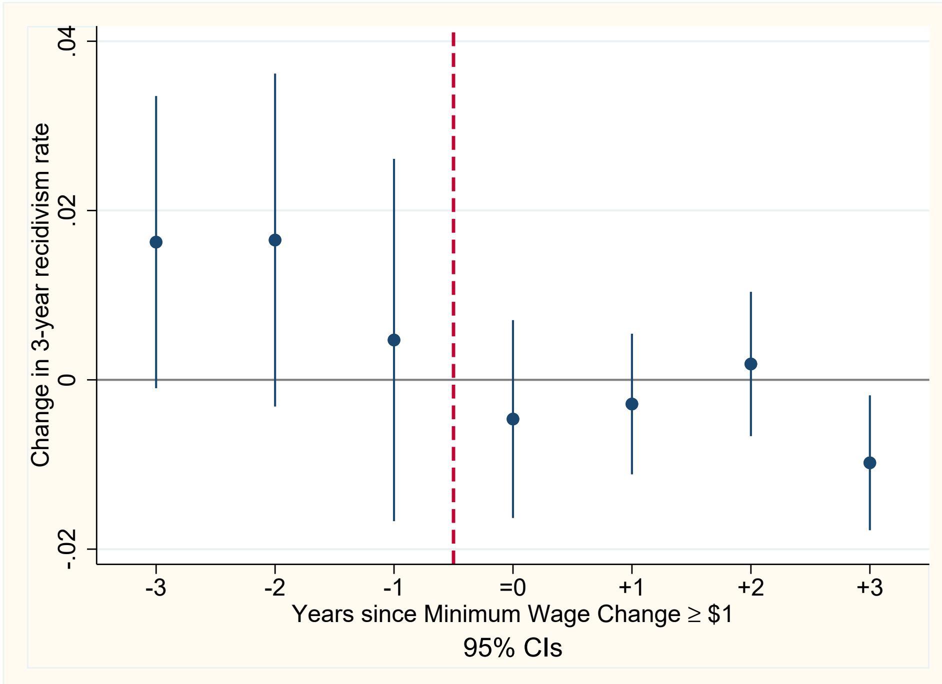
$$FractionRecidivate_{st} = \Sigma_{k=-3}^{3}\beta_j V_{st}^{j} + \beta_3 EITC_{st} + \mathbf{K_{ts}} + \gamma_y + \delta_s + \epsilon_{st}, \tag{3}$$

where $FractionRecidivate_{st}$ is the fraction of prisoners within state s released in year t who recidivated within 3 years of release and V_{st}^{j} is a set of indicators that equal 1 if the minimum wage was increased by $\geq$ \$1 within j years of t. The regression also includes the an indicator for whether the state has an EITC top-up and the battery of state-level control covariates $\mathbf{K_{ts}}$ identical to those found in our main regressions.

The principal benefit of this event study approach is that it allows for the a test of the common pre-treatment assumption (Goodman-Bacon, 2018; Athey and Imbens, 2018). This approach, however, is best suited for situations where the treatment is "fully absorbing" i.e. that the shift from untreated occurs once and is permanent. In our context, the minimum wage within states changes an average of 4 times between 2000 and 2014. While these changes are never reversed (states do not reduce their minimum wages), they are cumulative. While the event studies approach serves as a useful robustness exercise, particularly as an examination of the importance of pre-trends and whether we can observe noted differences pre- and post- "treatment", we do not believe this to be optimal method of identifying the effects of minimum wages on recidivism, and advise against placing too much weight on these results. Our principal concern is that in aggregating our data into a panel, we diminish the primary advantages of our data, sacrificing the within-subject variation of our microdata and the within-state variation from cumulative changes in the minimum wage. Unlike Fone et al. (2019), which focuses on broader aggregated crime rates within city and county panels (which are more-often characterized by one-time, fully absorbing sub-state changes in minimum wage policies), our focus remains on how individual decision-making changes in response to the current level of the minimum wage and EITC top-ups.

Figure F.6 displays the coefficients on each of the indicators within $\Sigma_{k=-3}^{3}\beta_j V_{st}^{j}$. In the pre-treatment window, the effects of the future minimum wage change is positive, but not significant, with large confidence intervals. This is consistent with an "untreated" assumption. Post treatment, we observe some negative effects, but none of the observed changes in 3-year recidivism rates are statistically significant until 3 years after the initial change. This lag in effect is not inconceivable, and the broader pre- to post-treatment trend is consistent with our main results.

Figure F.6: Event Study Approach: $1 Increases in the State Minmium Wage, 3-year window



Note: $FractionRecidivate_{st}$ is the fraction of prisoners within state s released in year t who recidivated within 3 years of release and V_{st}^{j} is a set of indicators that equal 1 if the minimum wage was increased by $\geq$ $1 within j years of t. $\mathbf{K_{ts}}$ are a vector of control covariates identical to those found in the main regressions. The dashed vertical line separates the pre- and post-treatment windows.

G Additional Tables

G.1 3-Year Results

Table G.1: Trend Analysis - 3 Year Recidivism Rates

	Leading MWs		State-Specific Time Trend Polynomials			Division X Year FE	Binding Changes
	(1) t+1	(2) t+2	(3) Linear	(4) 2nd	(5) 3rd	(6)	(7)
Min Wage	-0.0113**	-0.0143***	-0.0053	-0.0048	-0.0073**	-0.0154***	-0.0164***
	(0.0043)	(0.0043)	(0.0039)	(0.0040)	(0.0034)	(0.0054)	(0.0042)
Min Wage x Female	0.0030	0.0031	0.0031	0.0033	0.0034	0.0034	
	(0.0037)	(0.0037)	(0.0038)	(0.0037)	(0.0037)	(0.0037)	
State EITC	-0.0001	-0.0008	0.0040	0.0126*	0.0034	0.0059	
	(0.0070)	(0.0070)	(0.0065)	(0.0069)	(0.0074)	(0.0088)	
State EITC x Female	-0.0316***	-0.0316***	-0.0323***	-0.0322***	-0.0323***	-0.0314***	
	(0.0106)	(0.0106)	(0.0106)	(0.0106)	(0.0105)	(0.0105)	
Min Wage Lead	-0.0077	-0.0109*					
	(0.0052)	(0.0060)					
Bound MW							-0.0263
							(0.0331)
Min Wage X Bound							0.0040
							(0.0049)
Min Wage Coef:							
wild bootstrap p	0.030	0.005	0.193	0.244	0.047	0.027	0.000
Female EITC Effect:							
Total	-0.0317	-0.0324	-0.0283	-0.0196	-0.0289	-0.0255	
cluster-robust p	0.017	0.016	0.026	0.127	0.023	0.061	
wild bootstrap p	0.030	0.034	0.066	0.186	0.054	0.114	
Observations	4749284	4749284	4749284	4749284	4749284	4749284	4749284

Note: The dependent variable is return to prison in the same state within 3 years of release. See notes to Table 5.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table G.2: Robustness - Unemployment Rate and Substate Minimum Wages

	(1) State Unemp	(2) MW at Admit	(3) Avg MW 6 Months	(4) Avg MW 12 Months
Min Wage	-0.0144***	-0.0153***	-0.0195***	-0.0206***
	(0.0045)	(0.0043)	(0.0054)	(0.0061)
Min Wage x Female	0.0029	0.0030	0.0025	0.0021
	(0.0037)	(0.0038)	(0.0037)	(0.0037)
State EITC	0.0010	0.0001	0.0001	-0.0002
	(0.0066)	(0.0070)	(0.0068)	(0.0069)
State EITC x Female	-0.0316***	-0.0316***	-0.0315***	-0.0314***
	(0.0105)	(0.0106)	(0.0106)	(0.0105)
State Unemp Rate	-0.0018			
	(0.0019)			
MW Admit		0.0007		
		(0.0031)		
MW 1 Yr Bef Admit		-0.0009		
		(0.0031)		
Min Wage Coef:				
wild bootstrap p	0.006	0.006	0.006	0.013
Female EITC Effect:				
Total	-0.0306	-0.0314	-0.0314	-0.0316
cluster-robust p	0.021	0.018	0.017	0.017
wild bootstrap p	0.048	0.046	0.045	0.043
Observations	4749284	4749259	4749284	4749284

Note: The dependent variable is return to prison in the same state within 3 years of release. See notes to Table 6.
***$p < 0.01$,**$p < 0.05$,*$p < 0.10$ (based on cluster-robust standard errors)

H CPS Analysis: Additional Details and Tables

We start our CPS analysis using the same sample as Allegretto et al. (2011): the 1990-2009 CPS outgoing rotation groups, to ensure we are using a similar specification as the previous literature. Panel A of Table 11 and recreated below in Table H.1 serves as the closest replication as could be created with the information from the published paper. For comparison, Panel B reports results from Allegretto et al. (2011). While qualitatively similar, there are some small discrepancies in the calculated elasticities. Differences may due to updates in the data since their analysis was performed: their sample consists of 447,091 teenagers versus 447,719 in ours. We downloaded data from the LAUS on non-seasonally adjusted unemployment rates by state/month, however our summary statistics on these numbers do not exactly match theirs which could also cause some discrepancies. The results remain sufficiently similar to compare results across our specifications in the context of their prior results. The addition of the polynomial time trends comes from Neumark et al. (2014), who argued for their importance in controlling for underlying economic trends. They use data from CPS 1990-2011, aggregating the data into a state-quarter panel. For comparison, their results are reported in Panel C. While our analysis remains at the individual level, we nonetheless estimate elasticities very similar to those reported in their paper.

Table H.2 recreates Table 11 for other sub-populations in the CPS data, organized by skill-level, gender, or race, less likely to contain individuals with criminal records. We do not observe similarly positive employment effects from the minimum wage for any other subgroup. This increases our confidence that the positive result for low-skill black males was not due to spurious correlations.

Table H.1: Employment Elasticities w.r.t to Ln(MW) for Teenagers Aged 15-19 Compared to Previous Literature

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Our Analysis Individual-level CPS ORG 1990-2009							
Elasticity	-0.121**	-0.100	-0.094	-0.068	-0.045	-0.218***	-0.209***
Observations	447719	447719	447719	447719	447719	447719	447719
Panel B: ADR (2011) Individual-level CPS ORG 1990-2009							
Elasticity	-0.118**	-0.036	-0.034	0.047	-	-	-
Observations	447091	447091	447091	447091	-	-	-
Panel C: NSW (2014) Aggregated CPS ORG 1990-2011:Q2							
Elasticity	-0.165***	-0.074	-0.098	0.009	-0.051	-0.230***	-0.180**
Observations	4386	4386	4386	4386	4386	4386	4386
DivXQuarterFE	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

Note: Panel A recreates the elasticities from our Table 11 Panel A. Panel B shows elasticities from Allegretto et al. (2011) Table 3, Panel B "Employment, All Teens". Panel C shows elasticities from Neumark et al. (2014) Table 1 Panels A and B. $***p < 0.01$,$**p < 0.05$,$*p < 0.10$

Table H.2: Minimum Wage Effects on Employment for Different Subpopulations: CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: High-skill Black Men 25-54, CPS 1990-2016							
ln(MW) Coef.	-0.029	-0.010	-0.019	-0.045	-0.023	-0.012	-0.031
	(0.023)	(0.045)	(0.026)	(0.045)	(0.028)	(0.032)	(0.035)
Elasticity	-0.034	-0.012	-0.023	-0.055	-0.028	-0.014	-0.037
Observations	88224	88224	88224	88224	88224	88224	88224
Panel B: Low-skill White Men 25-54, CPS 1990-2016							
ln(MW) Coef.	0.020	-0.006	0.006	-0.014	-0.003	-0.003	-0.010
	(0.021)	(0.016)	(0.011)	(0.016)	(0.013)	(0.013)	(0.014)
Elasticity	0.024	-0.007	0.008	-0.016	-0.004	-0.004	-0.012
Observations	800677	800677	800677	800677	800677	800677	800677
Panel C: Low-skill White Women 25-54, CPS 1990-2016							
ln(MW) Coef.	0.050***	0.036	0.004	-0.013	0.004	0.006	0.008
	(0.017)	(0.025)	(0.014)	(0.020)	(0.015)	(0.014)	(0.017)
Elasticity	0.078	0.056	0.006	-0.020	0.007	0.009	0.012
Observations	778034	778034	778034	778034	778034	778034	778034
Panel D: High-skill White Men 25-54, CPS 1990-2016							
ln(MW) Coef.	-0.020**	-0.020**	-0.012	-0.012	-0.011	-0.007	-0.011
	(0.008)	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)	(0.011)
Elasticity	-0.022	-0.022	-0.013	-0.013	-0.012	-0.007	-0.012
Observations	1031507	1031507	1031507	1031507	1031507	1031507	1031507
DivXQuarterFE	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

Note: Data from the Current Population Survey Outgoing Rotation Groups, population stratification and years are indicated in the Panel titles. Low skill here indicating the absence of post-secondary education; "high skill" indicates at least some post-secondary 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, non-seasonally adjusted unemployment rate, marital status, education, race/ethnicity, gender, quarter FE, state FE, and additional trends or FE as noted. Column 1 provides a baselines 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*.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$