ONLINE APPENDIX:

The Minimum Wage, EITC, and Criminal Recidivism

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A A Conceptual Framework

In this appendix, 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).[51]

Consider a released prisoner who can earn w^* in the legal labor market absent a minimum wage and w^{crime} in the criminal market. Criminal wages are simple, and analagous to legal waegs, reduced to a linear value inclusive of all associated uncertainty, risk aversion, or any non-pecuniary rewards (e.g. thrills or social signaling). The effect of a minimum wage, w^{min}, will depend on its size relative to w^* and w^{crime}. If we assume that the minimum wage is binding in the market, i.e. that $w^* < w^{min}$, and that the probability of finding a job is decreasing under larger minimum wages, 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}. If $w^{crime} < w^* < w^{min}$, then workers would prefer employment in the legal labor market regardless of the minimum wage. An increase in the minimum wage, however, may reduce demand, making it harder for them to find legal employment while, at the same time, failing to 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. In this scenario, with negative demand effects absent any countervailing effects substitution of legal labor for crime, we expect that reduced employment from a larger minimum wage w^{min} will drive an *increase* in the probability of recidivism.

If $w^* < w^{crime}$, then workers would prefer "employment" 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 criminal earnings, 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

[51] 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.

increase in unemployment due to the minimum wage is irrelevant because workers prefer criminal income generating activities to legal employment—even if they were offered a job in the legal market, they wouldn't accept it. In this secenario, the probability of recidivism will *decrease* with w^{min}.[52]

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.

In either theoretical framing, the net effect of increasing minimum wages on recidivism is left ambiguous and will depend on the underlying distributions of market and criminal wages. Increasing minimum wages could lead employers to reduce hiring, leading to increased recidivism amongst released prisoners who are especially vulnerable to job loss. Higher minimum wages, however, could bring legal wages above potential "criminal" wages for some offenders, enticing them to exit the criminal labor market. Given some positive probability of finding and securing this higher paid legal job, this wage effect should lead to reductions in recidivism. For individuals

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

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.[53]

The all-or-nothing choice between legal or criminal activities predicted in our model is 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.[54]

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

[54] 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.

B Minimum Wages and EITC Values by State

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

	2000		2001		2002		2003		2004		2005		2006		2007		2008		2009		2010		2011		2012		2013		2014	
	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	MW	EITC	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		6.20		6.90		7.25		7.25		7.25		7.25		7.25	
AK	5.65		5.65		5.65		7.15		7.15		7.15		7.15		7.15		7.15		7.20		7.75		7.75		7.75		7.75		7.75	
AZ	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.75		6.90		7.25		7.25		7.35		7.65		7.80		7.90	
AR	5.15		5.15		5.15		5.15		5.15		5.15		5.43		6.25		6.40		6.90		7.25		7.25		7.25		7.25		7.25	
CA	5.75		6.25		6.75		6.75		6.75		6.75		6.75		7.50		8.00		8.00		8.00		8.00		8.00		8.00		8.50	
CO	5.15	10%	5.15	10%	5.15		5.15		5.15		5.15		5.15		6.85		7.02		7.28		7.25		7.36		7.64		7.78		8.00	10%
CT	6.15		6.40		6.70		6.90		7.10		7.10		7.40		7.65		7.65		8.00		8.25		8.25	30%	8.25	30%	8.25	30%	8.70	27.5%
DE	5.78		6.15		6.15		6.15		6.15		6.15		6.15	20%	6.65	20%	7.15	20%	7.20	20%	7.25	20%	7.25	20%	7.25	20%	7.25	20%	7.54	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%	7.28	40%	7.90	40%	8.25	40%	8.25	40%	8.25	40%	8.25	40%	8.88	40%
FL	5.15		5.15		5.15		5.15		5.15		5.82		6.40		6.67		6.79		7.23		7.25		7.29		7.67		7.79		7.93	
GA	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	
HI	5.25		5.25		5.75		6.25		6.25		6.25		6.75		7.25		7.25		7.25		7.25		7.25		7.25		7.25		7.25	
ID	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	
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%	7.63	5%	7.88	5%	8.13	5%	8.25	5%	8.25	5%	8.25	5%	8.25	10%
IN	5.15		5.15		5.15		5.15	6%	5.15	6%	5.15	6%	5.15	6%	5.50	6%	6.20	6%	6.90	9%	7.25	9%	7.25	9%	7.25	9%	7.25	6%	7.25	9%
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%	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	7%	7.25	14%
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%	6.20	17%	6.90	17%	7.25	18%	7.25	18%	7.25	18%	7.25	18%	7.25	17%
KY	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.56		6.20		6.90		7.25		7.25		7.25		7.25		7.25	
LA	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50		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	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%	7.06	5%	7.31	5%	7.50	5%	7.50	5%	7.50	5%	7.50	5%	7.50	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%	6.35	20%	6.90	20%	7.25	25%	7.25	25%	7.25	25%	7.25	25%	7.25	25%
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%	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%	8.00	15%
MI	5.15		5.15		5.15		5.15		5.15		5.15		5.60		7.05		7.28	10%	7.40	10%	7.40	20%	7.40	20%	7.40	6%	7.40	6%	7.65	6%
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%	6.35	33%	6.90	33%	7.25	33%	7.25	33%	7.25	33%	7.25	33%	7.56	33%
MS	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	
MO	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.50		6.65		7.15		7.25		7.25		7.25		7.35		7.50	
MT	5.15		5.15		5.15		5.15		5.15		5.15		5.15		6.15		6.40		7.08		7.25		7.35		7.65		7.80		7.90	
NE	5.15		5.15		5.15		5.15	8%	5.15	8%	5.15	8%	5.15	8%	5.50	8%	6.20	10%	6.90	10%	7.25	10%	7.25	10%	7.25	10%	7.25	10%	7.25	10%
NV	5.15		5.15		5.15		5.15		5.15		5.15		5.32		6.24		6.59		7.20		7.90		8.25		8.25		8.25		8.25	
NH	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.72		6.76		7.25		7.25		7.25		7.25		7.25		7.25	
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%	7.15	22.5%	7.20	22.5%	7.25	20%	7.25	20%	7.25	20%	7.25	20%	8.25	20%
NM	5.15		5.15		5.15		5.15		5.15		5.15		5.15		5.50	8%	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

C 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.[55] 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.[56]

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,

[55] 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).

[56] 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).

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).[57] However, these data include 37,312 prison releases that include both the last state of known address and the state of conviction; 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

[57] 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.

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.

D County Level Analysis

As outlined in Appendix C, 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 will 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.

D.1 Identifying and Defining Shared State Border Counties

Pairs of neighboring counties 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. (2017) 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 D.1: County Results

	(1) Baseline With County	(2) County Unemp	(3) County FE	(4) No Substate	(5) Substate All	(6) County Border Cluster
Panel A: 1 Year Recidivism						
Min Wage	-0.0076*	-0.0075*	-0.0074*	-0.0076*	-0.0079**	-0.0070*
	(0.0038)	(0.0038)	(0.0038)	(0.0038)	(0.0038)	(0.0039)
wild bootstrap p	0.071	0.113	0.097	0.097	0.062	0.169
Mean Recid Rate:	0.173					
Observations	5579060	5579060	5579060	5565953	5579060	1437042
Panel B: 3 Year Recidivism						
Min Wage	-0.0131***	-0.0126***	-0.0128***	-0.0133***	-0.0136***	-0.0069*
	(0.0042)	(0.0040)	(0.0041)	(0.0042)	(0.0043)	(0.0040)
wild bootstrap p	0.016	0.017	0.014	0.016	0.018	0.128
Mean Recid Rate:	0.348					
Observations	4580355	4580355	4580355	4569953	4580355	1204908

Note: The dependent variable is return to prison wihtin 1 or 3 years as indicated in panel title. Column 1 recreates Columns 1 and 3 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. Column 4 drops any observations from states that had any substate minimum wages. Column 5 applies substate minimum wages to everyone within a county, even if the substate minimum wage only applies to a city within a county. Column 6 includes fixed effects for clusters of counties that share a state border, see subsection D.1 below. Minimum wage is measured in dollars. All specifications include individual and time-varying state level controls outlined in Section 3; State EITC policy is included as a dummy variable but its coefficient is not shown. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient, as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤ 30 (our analysis is near but not below this threshold).

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

Table E.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.

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

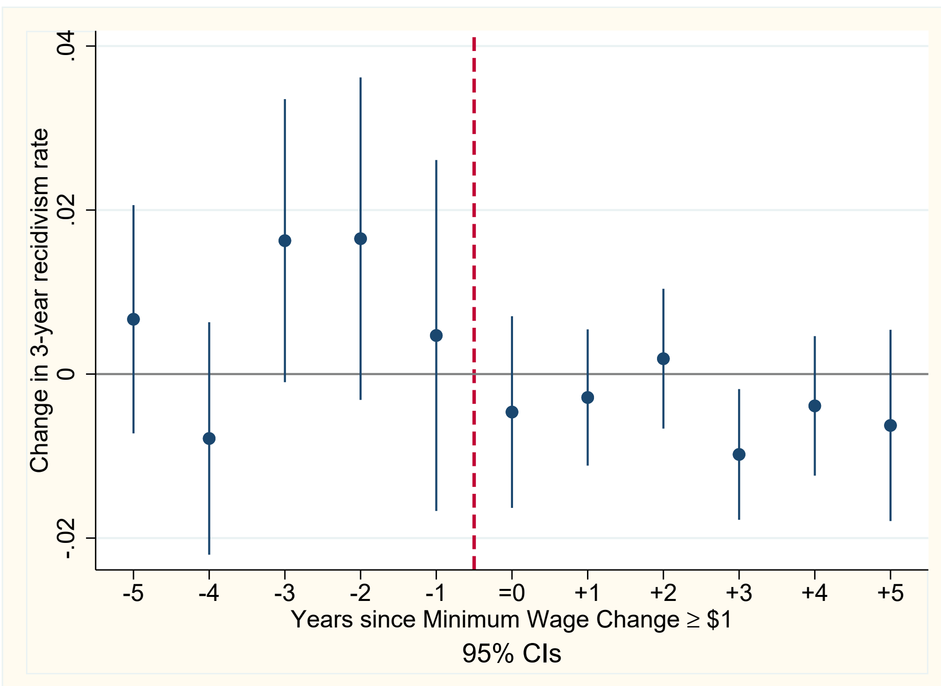
$$FractionRecidivate_{st} = \Sigma_{k=-3}^{3} \beta_j V_{st}^{j} + \beta_3 EITC_{st} + \mathbf{K_{ts}} + \gamma_y + \delta_s + \varepsilon_{st}, \tag{3}$$

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. Further, the events are not perfectly comparable in the traditional sense—not only is there variation in magnitudes of changes, but federal increases, for example, are potentially different events than state increases. 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 youth 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, 5-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

Table G.1: Robustness - Unemployment Rate and Substate Minimum Wages

	(1) Main	(2) State Unemp	(3) MW at Admit	(4) Avg MW 6 Months	(5) Avg MW 12 Months
Panel A: 1 Year Recidivism					
Min Wage	-0.0091**	-0.0104**	-0.0083**	-0.0118**	-0.0117*
	(0.0040)	(0.0043)	(0.0039)	(0.0052)	(0.0061)
State Unemp Rate		0.0034**			
		(0.0017)			
MW Admit			-0.0004		
			(0.0021)		
MW 1 Yr Bef Admit			-0.0015		
			(0.0023)		
wild bootstrap p	0.051	0.046	0.083	0.073	0.128
Observations	5786062	5786062	5786036	5786062	5786062
Panel B: 3 Year Recidivism					
Min Wage	-0.0149***	-0.0140***	-0.0149***	-0.0192***	-0.0203***
	(0.0044)	(0.0044)	(0.0042)	(0.0053)	(0.0060)
State Unemp Rate		-0.0018			
		(0.0019)			
MW Admit			0.0008		
			(0.0031)		
MW 1 Yr Bef Admit			-0.0009		
			(0.0031)		
wild bootstrap p	0.009	0.012	0.003	0.007	0.006
Observations	4749284	4749284	4749259	4749284	4749284

Note: The dependent variable is return to prison in the same state within 1 or 3 years of release as indicated by panel title. Minimum wage is measured in dollars and is measured in the state and month the offender is released. All specifications include state- and year-fixed effects, as well as the individual and time-varying state level controls outlined in Section 3; State EITC policy is included as a dummy variable but its coefficient is not shown. . For baseline means, see Table 4. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). p-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient , as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤ 30 (our analysis is near but not below this threshold). Results are based off of Table 4 Columns 1 and 3, which are repeated in Column 1. Additions to these regressions are 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 4-5 use the average minimum wage 6- or 12-months after release, respectively, as the main independent variable.
$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$ (based on cluster-robust standard errors)

Table G.2: Lagged Minimum Wages - 1 Year Recidivism Rates

	(1)	(2)	(3)	(4)
	-1	-2	-3	0, (incl t= -1,-2,-3)
Panel A: 1 Year Recidivism				
Min Wage at t=	-0.0064	-0.0045	-0.0019	-0.0089*
	(0.0045)	(0.0042)	(0.0047)	(0.0044)
wild bootstrap p	0.290	0.403	0.732	0.076
Observations	5786062	5786062	5786062	5786062
Panel B: 3 Year Recidivism				
Min Wage at t=	-0.0138***	-0.0133**	-0.0082	-0.0142***
	(0.0035)	(0.0049)	(0.0069)	(0.0048)
wild bootstrap p	0.002	0.023	0.334	0.019
Observations	4749284	4749284	4749284	4749284

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 3. 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 G.3: Lagged Minimum Wages - 3 Year Recidivism Rates

	(1)	(2)	(3)	(4)
	-1	-2	-3	0, (incl t= -1,-2,-3)
Min Wage at t=	-0.0145***	-0.0142***	-0.0084	-0.0131**
	(0.0035)	(0.0049)	(0.0069)	(0.0051)
Min Wage x Female	0.0051	0.0062	0.0040	-0.0099**
	(0.0040)	(0.0049)	(0.0059)	(0.0040)
EITC at t=	-0.0033	-0.0013	0.0064	0.0062
	(0.0077)	(0.0089)	(0.0096)	(0.0089)
State EITC x Female	-0.0327***	-0.0346***	-0.0369***	-0.0143
	(0.0111)	(0.0118)	(0.0117)	(0.0103)
Female	-0.0889***	-0.0946***	-0.0814**	-0.0690**
	(0.0225)	(0.0264)	(0.0316)	(0.0322)
Min Wage Coef:				
wild bootstrap p	0.001	0.013	0.332	0.036
Female EITC Effect:				
Total	-0.036	-0.036	-0.030	-0.008
cluster-robust p	0.022	0.031	0.048	0.609
wild bootstrap p	0.079	0.108	0.132	0.683
Observations	4749284	4749284	4749284	4749284

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 3. 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 G.4: State EITCs and Outcome Differentials for Male Offenders by Returning Crime, Race & Ethnicity, and Education

	1 Year				3 Year			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Return Crime Type:	Violent	Property	Drug	Other	Violent	Property	Drug	Other
State EITC	0.0044**	0.0022	0.0036***	-0.0032	0.0008	0.0018	0.0048**	-0.0091**
	(0.0021)	(0.0038)	(0.0012)	(0.0020)	(0.0019)	(0.0057)	(0.0021)	(0.0041)
wild bootstrap p	.113	.646	.021	.23	.692	.804	.073	.14
Mean Recid Rate:	0.0344	0.0582	0.0474	0.0367	0.0667	0.1138	0.1033	0.0708
Observations	5105236	5105236	5105236	5105236	4198073	4198073	4198073	4198073
	(9)	(10)	(11)		(12)	(13)	(14)	
Education:	$<$ HS	HS	$>$ HS		$<$ HS	HS	$>$ HS	
State EITC	0.0076	0.0027	0.0067		-0.0041	-0.0039	-0.0020	
	(0.0049)	(0.0074)	(0.0081)		(0.0055)	(0.0113)	(0.0059)	
wild bootstrap p	0.196	0.720	0.471		0.527	0.760	0.775	
Mean Recid Rate:	0.182	0.183	0.154		0.370	0.358	0.309	
Observations	1998103	1612951	268680		1696737	1303793	219271	
	(15)	(16)	(17)		(18)	(19)	(20)	
Race/Ethnicity:	Black	White	Hispanic		Black	White	Hispanic	
State EITC	0.0139***	0.0027	0.0232**		0.0045	-0.0059	0.0279***	
	(0.0049)	(0.0075)	(0.0097)		(0.0072)	(0.0098)	(0.0101)	
wild bootstrap p	0.041	0.760	0.079		0.609	0.631	0.064	
Mean Recid Rate:	0.182	0.173	0.157		0.379	0.340	0.304	
Observations	2238711	2091197	642998		1874470	1698972	519805	

Note: Only male offenders are included in this table. In Columns (1)-(8) the dependent variable is return to prison *for a certain crime type* within 1 or 3 years of release (as indicated by super-column headers). Remaining columns the dependent variable is return to prison within 1 or 3 of release (as indicated in super-column headers). Columns (9)-(20) are subsample results based on either education level or race/ethnicity. >HS means any college, not necessarily a college degree. State EITC is an indicator for the existence of a state top-up and is measured in the state and month the offender was released. Mean recidivism rates are the mean of the dependent variable for the respective column. Robust standard errors clustered at the state level are shown in parentheses (43 clusters). *p*-values from 1000 wild-cluster bootstrap iterations are shown for the main minimum wage coefficient, as suggested by Cameron et al. (2008) in cases with a small number of clusters, typically ≤ 30 (our analysis is near but not below this threshold). To address the potentially small number of treated clusters for EITC estimates, the EITC coefficients are estimated with subcluster wild bootstrap at the state-year level out of concern for the number of treated clusters.(MacKinnon and Webb, 2018; Roodman et al., 2019). ***$p < 0.01$,**$p < 0.05$,*$p < 0.10$ (based on cluster-robust standard errors) ***$p < 0.01$,**$p < 0.05$,*$p < 0.10$ (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 9 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 9 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 9 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*.
$***p < 0.01$, $**p < 0.05$, $*p < 0.10$