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Working Paper 25647
<http://www.nber.org/papers/w25647>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2019, Revised October 2020

This research was supported by a grant received from the Employment Policies Institute. We also acknowledge support from the Center for Health Economics & Policy Studies (CHEPS) at San Diego State University. The authors thank Kevin Schnepel, Melinda Pitts, Dhaval Dave, Hope Corman, Tuan Nguyen, and participants at the 2018 Eastern Economic Association meetings and the 2018 Southern Economic Association meetings for useful comments and suggestions on earlier drafts of this paper. We thank Thanh Tam Nguyen for outstanding research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 25647
March 2019, Revised October 2020
JEL No. J01,J3

ABSTRACT

An April 2016 Council of Economic Advisers (CEA) report advocated raising the minimum wage to deter crime. This recommendation rests on the assumption that minimum wage hikes increase the returns to legitimate labor market work while generating minimal adverse employment effects. This study comprehensively assesses the impact of minimum wages on arrests using data from the 1998-2016 Uniform Crime Reports (UCR) and the 1998-2016 waves of the National Longitudinal Survey of Youth 1997 (NLSY97). In contrast to the CEA claim, our results provide no evidence that minimum wage increases reduce arrests. Instead, we find that raising the minimum wage increases property crime arrests among 16-to-24-year-olds, with an estimated elasticity of approximately 0.2. This result persists when we use longitudinal data to isolate workers for whom minimum wages bind. Auxiliary analyses using the Current Population Survey (CPS) suggest that our findings are likely driven by adverse labor demand effects of the minimum wage. Our estimates suggest that a \$15 Federal minimum wage could generate criminal externality costs of nearly \$2.5 billion.

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1. Introduction

“Raising the federal minimum wage to \$12 an hour could prevent as many as half a million crimes annually, according to a new report from the White House's Council of Economic Advisers...as fewer people would be forced to turn to illegal activity to make ends meet.”

-Washington Post, April 25, 2016

Increasing incarceration and police can be effective (Levitt 2004; Corman and Mocan 2005; Chalfin and McCrary 2018), but expensive (Kearney et al. 2014) policy strategies to fight crime. Expenditures on police and the criminal justice system are estimated to be on the order of \$296 billion per year (Bureau of Justice Statistics 2019).¹ An alternative set of policies to deter crime, which are often less costly to taxpayers, includes those that improve labor market conditions and incentivize greater human capital acquisition. Among those at the margin of crime commission, criminal behavior is negatively related to employment opportunities (Mustard 2010; Schnepel 2018), wages (Gould et al. 2002; Yang 2017), and educational attainment (Machin and Meghir 2004; Anderson 2014). An April 2016 report from the White House Council of Economic Advisers (CEA) contrasted the high public costs of deterring crime via the criminal justice system with lower cost alternatives and recommended a novel policy strategy for combating crime: raising the minimum wage.

The CEA argued that because minimum wage increases raise the hourly wages of low-skilled workers, the opportunity cost of engaging in criminal activity will rise, resulting in less crime. Using estimates of the crime elasticity with respect to wages from Gould et al. (2002), the CEA concluded that raising the Federal minimum wage from \$7.25 to \$12 per hour would decrease crime by 3 to 5 percent, or 250,000 to 510,000 crimes annually (CEA 2016), resulting in \$8 to \$17 billion dollars per year in cost savings (CEA 2016). Consistent with the CEA's prediction, recent work by Agan and Makowsky (2020) finds that minimum wage increases are negatively related to criminal recidivism.

¹ The \$296 billion estimate is comprised of \$142 billion for police, \$88 billion for corrections (e.g. prisons, jails, and staffing), and \$65 billion for the judicial system (Bureau of Justice Statistics 2019). Additionally, the FBI estimates that in 2016, the victims of property crime (excluding arson) suffered losses of \$15.6 billion (FBI 2017c).

While intriguing, the CEA's policy conclusion rests on the assumption that (i) minimum wage increases do not cause adverse labor demand effects that lead to more crime, or (ii) any adverse labor demand effects are sufficiently small to be swamped by wage gains (Agan and Makowsky 2020) or by enhanced expectations for higher-paying jobs. But there are important reasons to expect that the adverse labor demand effects from minimum wages may not always be small (Neumark 2018; Clemens and Wither 2019; Gittings and Schmutte 2016; Powell 2016; Baksaya and Rubenstein 2015; Sabia et al. 2012; 2016; Churchill and Sabia 2019). Minimum wage-induced job loss or hours reductions may lead to more property crime for economic reasons (Grogger 1998; Mustard 2010) and more violent crime for despair-related, emotionally expressive reasons (Wang et al. 2010; Nordin and Almen 2017).² Minimum wage increases could also affect crime through their human capital effects, including impacts on school enrollment (Neumark and Wascher 2003; Pacheco and Cruickshank 2007) and on-the-job training (Neumark and Wascher 2001; Acemoglu and Pischke 2003). Additionally, they may also affect crime via their impacts on expected labor market opportunities, conditional on actual opportunities (Galbiati et al. Forthcoming). The net effect of minimum wages on crime depends on (i) the magnitudes of wage, employment, schooling, and on-the-job training elasticities with respect to the minimum wage, (ii) the magnitudes of crime elasticities with respect to wages, employment, schooling, and on-the-job training, (iii) the distribution of labor market effects of the minimum wage across individuals with heterogeneous propensities for crime, and (iv) on how higher minimum wages impact future expectations of labor market opportunities.

The current study assesses the credibility of the CEA claim by comprehensively examining the relationship between minimum wages and crime. Using data from the 1998-2016 Uniform Crime Reports (UCR) and the 1998-2016 waves of the National Longitudinal Study of Youth 1997 (NLSY97), difference-in-differences estimates provide little evidence of crime-reducing effects of the minimum wage. Instead, we find robust evidence that minimum wage hikes increase property crime arrests among teenagers and young adults ages 16-to-24, a population for whom minimum wages are likely to bind (Bureau of Labor Statistics 2017). We

² Wang et al. (2010) find that male ex-offenders in Florida released into counties with higher levels of unemployment are more likely to commit violent crime, which the authors suggest may be due to despair created by the lack of employment opportunities. They suggest this may also lead to male ex-offenders seeking alternative ways to express their masculinity (through violent crime, as opposed to employment). Nordin and Almen (2017) find that long-term unemployment spells are associated with increases in violent crime, which they suggest may be due to the strain created by these spells resulting in violent behavior.

estimate a property arrest elasticity with respect to the minimum wage of 0.2. This result is consistent with adverse labor demand effects of the minimum wage, a result that we confirm using data from Current Population Survey Outgoing Rotation Groups (CPS-ORG). Our confidence in the common trends assumption underlying our identification strategy is bolstered by event-study analyses.

Furthermore, we find little evidence that minimum wage increases affect arrests for violent offenses, or net crime among older individuals, but do increase delinquency-related crimes related to teenage idleness (Jacob and Lefgren 2003; Luallen 2006; Anderson 2014). In contrast to Agan and Makowsky (2020), we find no evidence that increases in the minimum wage reduce net crime among working-age individuals, suggesting that different margins of criminal behavior may be differentially affected by minimum wages.

Finally, estimates of the effect of the minimum wage on “treated workers” in the NLSY97, those workers earning wages such that they are affected by minimum wage increases, add to our confidence in interpreting our UCR-based findings causally. Our findings in individual-level panel data suggest that minimum wages increase the probability of property crime commission among those bound by such hikes.

To put our findings in the context of the 2016 CEA report, increasing the Federal minimum wage to \$12 would represent a 66 percent increase in the current Federal minimum wage. Lower bound intent-to-treat (ITT) estimates from the UCR suggest that a \$12 minimum wage would result in approximately 231,000 additional property crimes, generating annual criminal externality costs of \$1.4 billion (in 2019\$) (McCollister et al. 2010). Moreover, the *Raise the Wage Act of 2019* (HR 582), endorsed by Democratic Presidential Candidate Joe Biden, would raise the Federal minimum wage by 107 percent to \$15 per hour.³ Our estimates suggest that this minimum wage hike would generate approximately 423,000 additional property crimes and \$2.5 billion per year in additional crime costs. We conclude that increasing the

³ House Resolution (HR) 582 was introduced by Congressman Bobby Scott (D-VA) on January 16, 2019 and endorsed by House Speaker Nancy Pelosi (D-CA). HR 582 passed the House on 7/18/2019 and is currently on the Senate calendar as General Order No. 156. The resolution proposes a seven-step (over seven years) increase in the Federal minimum wage until it reaches \$15 per hour, in which subsequent increases are indexed to median wage growth. The resolution also proposes increases in the tipped minimum wage (topping out at \$14.10 per hour, and indexed to median wage growth thereafter). Legislative updates on HR 582 can be found at the following link: <https://www.congress.gov/bill/116th-congress/house-bill/582>

minimum wage will at best be ineffective at deterring crime and at worst will have unintended consequences that increase property crime among young adults.

2. Background

2.1 Crime, the Labor Market, and Human Capital

Becker's theory of rational crime (1968) posits that criminal behavior is responsive to labor market conditions and human capital acquisition, and there is strong empirical evidence to support this theory. First, studies that have exploited changes in local employment conditions for populations on the margin of criminal behavior find that crime is positively related to unemployment rates (Raphael and Winter-Ebmer 2001; Gould et al. 2002; Machin and Meghir 2004; Levitt 2004; Oster and Agell 2007; Lin 2008; Mustard 2010) and business cycle contractions (Arvanites and Defina 2006; Rosenfeld and Fornango 2007). Recidivism also decreases when low-skilled job opportunities in construction and manufacturing rise (sectors more willing to hire ex-offenders) in the communities to which ex-offenders are released (Schnepel 2018). In addition, there is strong evidence that criminal behavior responds to wages. Gould et al. (2002) find that a 10 percent increase in the wages of non-college-educated men is associated with a 5.4 percent decrease in property crime and a 10.8 percent decrease in violent crime.⁴ Along the same lines, Yang (2017) finds that ex-offenders released in counties with higher low-skilled wages are less likely to recidivate, particularly in sectors more willing to hire ex-offenders.

Second, increases in educational attainment may reduce crime. Raising the minimum legal school dropout age leads to a decline in criminal behavior among affected students (Lochner and Moretti 2004; Machin et al. 2011; Anderson 2014). These schooling effects can be explained by incapacitation effects (Jacob and Lefgren 2003; Luallen 2006) as well as enhanced human capital acquisition (Lochner and Moretti 2004; Machin et al. 2011), the latter of which may change both the opportunity costs of crime as well as the tastes for crime.⁵

⁴ They also find that a one percentage point increase in the unemployment rate for non-college-educated working males ages 18 to 65 is associated with a 2.3 percent increase in property crime and a 1.3 percent increase in violent crime.

⁵ Anderson (2014) finds little evidence of displacement effects of crime in schools.

Crime-reducing effects of human capital acquisition can also be attained through on-the-job training (Lochner 2004), which is expected to increase workers' wages (Mincer 1962; Brown 1989). On-the-job training has an important impact on the wages of young adult workers without a college degree (Lynch 1992).

2.2 Effect of Minimum Wages on Labor Market Outcomes

Minimum wages may affect each of the labor market outcomes described in Section 2.1, thereby impacting crime. First, there is strong and uncontroversial evidence that minimum wage increases raise the wages of low-skilled teenage and young adult workers (Card and Krueger 1994; Neumark and Wascher 2008; Dube et al. 2010; Allegretto et al. 2011; Sabia et al. 2012; Belman and Wolfson 2014; Neumark et al. 2014a,b). Estimated wage elasticities from this literature are around 0.1 to 0.3.⁶

In contrast, the literature on the employment effects of U.S. minimum wages is far more controversial. Quasi-experimental studies have taken a number of approaches to identify employment effects, including methods that exploit, (i) temporal variation across jurisdictions in minimum wage levels (Cengiz et al. 2019; Neumark et al. 2014a,b)⁷, (ii) heterogeneity in bindingness of minimum wage increases across jurisdictions with heterogeneous pre-treatment shares of low-wage workers (Stewart 2004; Thompson 2009), and (iii) jurisdiction-level differences in bindingness of a Federal minimum wage change due to pre-treatment differences in minimum wage levels (Clemens and Wither 2019; Currie and Fallick 1996). In addition, recent work has attempted to overcome the endogeneity of minimum wages by randomly assigning minimum wages to firms that post job openings online (Horton 2018).

The debate on the minimum wage's employment effects is unlikely to be settled for some time, owed, in part, to disagreements about the appropriateness of "close controls" (Dube et al. 2010; Neumark et al. 2014a) and whether partialling out geographic-specific time trends reduces or exacerbates bias in estimates of the minimum wage's employment effects (Allegretto et al.

⁶ These estimated wage elasticities are "intent-to-treat" estimates that are often far less than one, often closer to 0.1 to 0.2. This is because not all low-skilled workers earn wages such that they are affected by minimum wage increases and those that are may earn a wage between the old and new minimum wage.

⁷ See also, Card and Krueger 1994; 1995; Burkhauser et al. 2000; Couch and Wittenburg 2001; Neumark and Wascher 2008; Sabia 2009; Dube et al. 2010; Allegretto et al. 2011; Sabia et al. 2012, 2016; Addison et al. 2013; Meer and West 2016; Allegretto et al. 2017; Sabia et al. 2019; Powell 2016; Jardim et al. 2018.

2011; Neumark et al. 2014a). Moreover, differences in studies' findings may be explained by heterogeneous treatment effects of the minimum wage across low-skilled sub-groups (within- and across-industries), jurisdictions, and time periods (Neumark 2019). If there are adverse employment and hours effects of the minimum wage, they are likely concentrated among the least-experienced low-wage workers. In summary, Neumark (2019) concludes that "the preponderance of evidence indicates that minimum wages reduce employment of the least-skilled workers" and suggests that "there is a great deal of uncertainty about the employment effects of a \$15 minimum wage" (pp. 323-324).⁸

Minimum Wages and Human Capital Acquisition. Raising the minimum wage may also affect crime via its effects on schooling and job training. Evidence on the schooling effects of minimum wages are somewhat mixed. Early studies find heterogeneity in schooling effects across income groups, reducing enrollment for teenagers in low-income families while increasing enrollment for those in high-income families (Ehrenberg and Marcus 1980; 1982). More recent studies find adverse school enrollment effects of minimum wages (Neumark and Wascher 2003; Pacheco and Cruickshank 2007) and little impacts on overall educational attainment (Card 1992; Campolieti et al. 2005; Warren and Hammock 2010; Sabia 2012).⁹ There is stronger evidence that minimum wages reduce on-the-job training (Neumark and Wascher 2001; Acemoglu and Pischke 2003), a finding consistent with the hypothesis that wage

⁸ While most studies have focused on the effects of state and Federal minimum wage changes, others have examined the effects of local minimum and living wages on labor market outcomes, with mixed results. In a study of Seattle's minimum wage, Jardim et al. (2018) find that the increase from \$11 to \$13 per hour resulted in a 3.2 percent increase in wages of low-skilled workers, but a 6.9 percent decrease in hours worked and 5.9 percent decrease in employment (see Tables 5 and 6 in Jardim et al. 2018). In contrast, studies of local minimum wages in San Francisco (Schmitt and Rosnick 2011), Santa Fe (Schmitt and Rosnick 2011), and San Jose (Allegretto and Reich 2018) find evidence of minimum wage-induced wage gains, but no adverse employment effects; whereas, Luca and Luca (2019) find that the minimum wage increases in the San Francisco Bay Area were associated with restaurant closures. For living wages, studies find increases in wages (Neumark and Adams 2003a,b; 2005b; Brenner 2005; Fairris 2005; Reich et al. 2005; Neumark et al. 2012), yet there is also evidence of adverse employment effects (Neumark and Adams 2003a,b; 2005b; Fairris 2005; Neumark et al. 2012). Furthermore, Neumark and Adams (2003a) find that living wages which cover businesses receiving financial assistance bind the most for low-wage workers (compared to living wages covering other forms of employment: municipal employees, public contract workers, or hotel workers), with Neumark and Adams (2005a) finding that living wages covering these businesses generate larger wage increases and employment reductions.

⁹ Warren and Hamrock (2010) find some modest evidence that larger minimum wage increases may have small negative effects on high school completion rates in states where students are permitted to drop out before age 17.

floors reduce an employer's flexibility to finance job training out of workers' wages (Rosen 1972).

2.3 Minimum Wages and Crime

The literature on the crime effects of minimum wages is recent and small and was unmentioned in the April 2016 CEA report. Estimates obtained in this literature are sensitive to the (i) low-skilled population, (ii) time period, and (iii) margin of criminal behavior examined.¹⁰

Hansen and Machin (2002) examine the introduction of a new national minimum wage law in the United Kingdom and find that crime declines more in localities with larger shares of low-wage workers. Fernandez et al. (2014) use a clever identification strategy to estimate the effect of living wage ordinances enacted between 1990 and 2010 on overall crime rates for 239 large U.S. cities. They identify "control" cities as those that narrowly defeated living wage ordinances or passed such ordinances, but had them enjoined or repealed by state courts, and find that living wage increases are associated with reductions in both property and violent crimes. Although not emphasized in their paper, Fernandez et al. (2014) also find that minimum wage increases are associated with reductions in property and violent crimes, though these associations are sensitive to the inclusion of observable controls and city-specific time trends. In contrast, Beauchamp and Chan (2014) use individual-level panel data over a comparable period, and focusing on low-wage workers for whom minimum wages are more likely to bind, find that increases in the minimum wage increase property and violent crimes among teenagers, but often find the opposite effect for young adults.

Finally, a new working paper by Agan and Makowsky (2020) explores the impact of minimum wage increases on recidivism. They examine nearly six million prison releases (four million unique offenders) across 43 states from 2000 to 2014 and find that minimum wage increases are associated with a decline in recidivism, primarily through reduced property and

¹⁰ Most studies in this small literature have used a difference-in-differences identification strategy that exploits variation in minimum wages across jurisdictions and over time. There are exceptions to this approach. For instance, Hashimoto (1987) uses national data between 1947 and 1982 to estimate a time series regression and finds that Federal minimum wage increases are positively related to property crimes for youths ages 15-to-19, with estimated elasticities of 0.1 to 0.5.

drug crime.¹¹ The authors posit that (i) wage gains from minimum wage increases may dominate any negative employment effects, and (ii) some ex-offenders may see *increases* in employment following minimum wage increases if employers respond to minimum wage hikes by substituting away from less experienced workers and toward more experienced workers with a felony record. Their implied property and drug crime elasticities are quite large, ranging from -0.451 to -0.553, which would suggest that large shares of ex-offenders are affected by minimum wage increases or, perhaps, that media coverage about minimum wage hikes substantially changed ex-offenders' expectations about higher-paying jobs in the future (Galbiati et al. Forthcoming).¹²

We contribute to this literature by using two large national datasets over a two-decade period to comprehensively examine the impact of Federal, state, and local minimum wages on crime. In contrast with many prior papers, our study (i) focuses on younger, lower-skilled individuals for whom minimum wages are most likely to bind (Bureau of Labor Statistics 2017) and are more prone to crime (FBI 2017a), (ii) explicitly examines employment, hours, and human capital effects of minimum wages over the same time period (and occasionally for the same people for whom) we measure crime, (iii) explores the sensitivity of our findings to tests of the common trends assumption, including event studies, controls for jurisdiction-level time trends, and pseudo-falsification tests on demographic groups that should be less affected by minimum wages, and (iv) examines overall crime rates that include first-time arrests as well as criminal arrests that do not result in incarceration and release. Finally, we attempt to understand and reconcile sometimes conflicting results across the existing small minimum wage-crime literature.

¹¹ In addition, they find that expansions in the state Earned Income Tax Credit (EITC) reduces recidivism among women, consistent with the EITC increasing returns to legitimate employment. The authors posit that this result is driven by eligibility rules that make the EITC bind most strongly for custodial parents.

¹² Estimated intent to treat estimates of wage elasticities with respect to the minimum wage for low-wage workers tend to range from 0.1 to 0.3 (Card and Krueger 1994; Neumark and Wascher 2008; Dube et al. 2010; Allegretto et al. 2011; Sabia et al. 2012; Belman and Wolfson 2014; Neumark et. al. 2014a,b; Jardim et al. 2018).

3. Data and Methods

3.1 Data

The primary data source for our crime analysis is the Uniform Crime Reports (UCR), supplemented by data from the National Longitudinal Survey of Youth 1997 (NLSY97). Each dataset has advantages and disadvantages, which we discuss below.¹³

From the UCR, we generate county-by-year criminal arrest rates from 1998 to 2016 by the age of the offender. Our primary “treatment group” is comprised of teenagers and young adults ages 16-to-24, an age cohort for whom minimum wages are most likely to bind (Bureau of Labor Statistics 2017). Arrest data are collected for property crimes (larceny, burglary, motor vehicle theft, and arson), violent crimes (homicide, rape, robbery, and aggravated assault), and other minor crimes often linked to idleness and delinquency (vandalism, liquor law violations, drunkenness, disorderly conduct, and drug crimes). To assure data quality, we drop county-year arrest rates that are greater than two standard deviations from the county arrest rate mean, control for the number of agencies that report to a county each year, and limit our sample to counties where at least 65 percent of agencies report arrest data (see, for example, Anderson 2014).¹⁴ Alternate methods of ensuring consistent reporting, including requiring a balanced panel of agencies, generated a similar pattern of results.

Means of county-level arrests per 1,000 population are reported in Table 1. The average property crime arrest rate among 16-to-24 year-olds over the sample period is 15.83 per 1,000.

¹³ The UCR data used in this study are the *Arrests by Age, Sex, and Race* files (United States 2016). These data can be downloaded from the Inter-university Consortium for Political and Social Research (ICPSR): https://www.icpsr.umich.edu/web/NACJD/series/57?start=0&sort=TITLE_SORT%2520asc&SERIESQ=57&ARCFIVE=NACJD&PUBLISH_STATUS=PUBLISHED&rows=50

¹⁴ Explicitly, we utilize the “coverage indicator” sample criterion:

$$\text{Coverage Indicator}_{ct} = \left(1 - \sum_{i=1}^n \left\{ \left[\frac{\text{Agency Population}_{it}}{\text{County Population}_{ct}} \right] * \left[\frac{12 - \text{Months Reported}_{it}}{12} \right] \right\} \right) * 100$$

Where c denotes county, i denotes agency, and t denotes year. For a county with all agencies reporting 12 months of arrest data, the coverage indicator takes on the value of 100. For a county with none of the agencies reporting arrest data for any month, the coverage indicator takes on the value of zero. The coverage indicator measure was developed by the ICPSR (US DOJ 2017), and has been used by researchers as a sample criterion to assure data quality (see Freedman and Owens 2011; Thomas and Shihadeh 2013). Alternate cutoffs of the percentage of agencies reporting within the county (e.g. 60 percent, 75 percent, or 90 percent) generate a similar pattern of results.

For violent crime arrests, the mean is 5.04 per 1,000. As expected, arrest rates decline by age and are larger for men than women (see Appendix Table 1A).^{15, 16}

We add to the above analysis by using individual-level panel data from the National Longitudinal Survey of Youth 1997 (NLSY97) from 1998 through 2016. A key advantage of these longitudinal data is that we can identify low-wage workers who earn wages such that they are affected by future minimum wage increases. Thus, while the UCR-based analysis will permit us to identify intent-to-treat (ITT) estimates, the NLSY97 will permit estimates of the effect of treatment-on-the-treated (TOT). Moreover, the NLSY data permit us to measure crime that does not necessarily result in arrest, as well as jointly model labor market outcomes and criminal behavior.¹⁷ While Current Population Survey data permits us to examine net labor demand effects over the same period during which we measure crime, the NLSY97 actually permits us to examine crime and employment effects for the same *persons*.

Despite these advantages, the NLSY97 data have a number of limitations. Data collected as part of the NLSY97 survey are self-reported and hence the crime variables are likely to understate the true prevalence of crime. However, as long as such measurement error is orthogonal to minimum wage changes, estimated policy impacts in terms of percent changes (relative to mean reporting crime) should be unbiased. Second, as the original sample consists of 8,983 respondents, the sample is not designed to be representative of low wage workers at the jurisdiction-by-year level.¹⁸ Often, there are very small numbers of low wage workers bound by

¹⁵ Appendix Table 1B shows mean arrest rates for specific property, violent, and minor crimes in the UCR.

¹⁶ To supplement crime data from the UCR, we draw data from the National Incident-Based Reporting System (NIBRS) from 1998 through 2016. A key advantage of these data is that we can measure race/ethnicity-specific criminal incidents for 16-to-24 year-old arrestees, which is not possible with the UCR. This may be important if there are heterogeneous impacts of minimum wages by race or ethnicity. However, external validity using the NIBRS is limited. As of 2016, 38 states and the District of Columbia reported to the NIBRS (FBI 2017b), which represents 37.1 percent of the coverage in the UCR program (FBI 2017a) and smaller, more rural jurisdictions, are overrepresented (McCormack et al. 2017). Thus, if there are heterogeneous impacts of minimum wages by jurisdiction location and size, this could explain differences in results across the UCR and NIBRS.

¹⁷ These data also permit us to control for individual fixed effects to more effectively disentangle the effects of local minimum wages from difficult-to-measure time-invariant individual characteristics and examine person-specific changes in minimum wages, employment, and crime. In supplemental analysis, we take this tack, though results are somewhat less precisely estimated.

¹⁸ Moreover, the NLSY97 ceased asking crime questions to all the respondents starting in round 8 of the survey (2004), asking crime questions only to individuals who had reported being arrested at least once beginning 2004 in addition to about 10 percent of survey participants as a control group.

minimum wage increases, which might suggest that estimates may be imprecise and sensitive to model specification. Thus, estimates obtained from the NLSY97 should be treated as suggestive as opposed to being conclusive.

Our primary sample consists of approximately 38,000 person-years for individuals ages 16-to-24 for whom self-reported criminal engagement information is available. We generate five measures of crime using responses to seven questionnaire items.¹⁹ *Any Crime* is set equal to 1 if a survey participant reported committed a drug crime, a property crime (theft, damaging property, other property crime), or a violent crime (assault) since the date of the last interview, and is set equal to 0 otherwise; *Property Crime* is set equal to 1 for individuals who reported they had committed a theft or a property crime, or had damaged others' property since the date of the last interview, and is set equal to 0 otherwise; *Violent Crime* is set equal to 1 for individuals who reported committing assault since the date of the last interview, and is set equal to 0 otherwise,

¹⁹ The following are the survey questions used for the NLSY crime questions. For each survey question, the possible answers are "Yes" and "No."

Property Crime Items:

"Since the last interview on, have you stolen something from a store or something that did not belong to you worth less than 50 dollars?"

"Since the last interview on, have you stolen something from a store, person or house, or something that did not belong to you worth 50 dollars or more including stealing a car?"

"Since the last interview on, have you purposely damaged or destroyed property that did not belong to you?"

"Since the last interview on [date of last interview], have you committed other property crimes such as fencing, receiving, possessing or selling stolen property, or cheated someone by selling them something that was worthless or worth much less than what you said it was?"

Violent Crime Item:

"Since the last interview on, have you attacked someone with the idea of seriously hurting them or have had a situation end up in a serious fight or assault of some kind?"

Drug Crime Item:

"Since the last interview on, have you sold or helped to sell marijuana (pot, grass), hashish (hash) or other hard drugs such as heroin, cocaine or LSD?"

Arrest Item:

"Since the date of last interview on, have you been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)?"

Drug Crime is set equal to 1 for individuals who reported selling drugs and is set equal to 0 otherwise; and *Arrest*, set equal to 1 if respondents had been arrested and 0 otherwise.²⁰ Appendix Table 2 shows means of these crime outcomes from the NLSY.

3.2 Minimum Wages and Living Wages

Our main policy variable of interest for the UCR-based analysis is the higher of the Federal, state, or local minimum wage, *MW*. Federal and state-level minimum wages are collected from the United States Department of Labor, Wage and Hour Division. For county and city-level minimum wages, we use data compiled by Vaghul and Zipperer (2016) and update these data through 2016 via our own searches of local minimum wage ordinances. In addition, we measure living wage ordinances using effective dates compiled from the National Employment Law Project (2011) as well as our own individual contacts with local governments.

During the period from 1998 to 2016, there were 3 Federal minimum wage increases, 217 state minimum wage increases, 77 local minimum wage increases, and 116 living wage ordinances enacted. Figure 1 shows county-level variation in minimum wages over the period under study. The average state-legislated minimum wage hike over the 1998-2016 period was \$0.55 (in 2016\$) and 12 states indexed their minimum wages to inflation.

For the NLSY97-based analysis, our key treatment variable differs as we identify a treatment-on-the-treated (TOT) estimate. Following Currie and Fallick (1996), we define *Binding MW* as an indicator set equal to 1 if an individual is employed and earns a wage at year t that was no lower than the state or local minimum wage at year t and no higher than the state or local minimum wage at year $t+1$, and set equal to 0 if a worker earned a wage higher than the minimum wage at year $t+1$ or lower than the minimum wage at year t (i.e. because he or she was a tipped or informal worker not bound by the minimum wage). Thus, by construction, our estimation sample is limited to those who were employed in year t .

Given that wage spillovers are possible to those who earn wages at year t higher than the minimum wage at $t+1$ if firms engage in labor-labor substitution (or treat such laborers as complements), we experiment with dropping workers who earn hourly wages that are higher

²⁰ Following Beauchamp and Chan (2014), we assume that the absence of response is because of inactivity. Thus, we replace missing values for crime variables to zero for those who ever reported criminal behavior. Estimates without replacing missing crime variable observations produce similar results to those presented here.

than, but are within \$1 or \$2 of the next period's minimum wage. We also experiment with dropping sub-minimum wage workers (e.g. informal workers or tipped employees) from the analysis sample. The results were qualitatively similar to those presented below.²¹

3.3 Empirical Methods

We begin with data from the 1998-2016 UCR and estimate the following two-way fixed effects model via ordinary least squares (OLS):

$$Y_{cst} = \beta_0 + \beta_1 \ln(MW_{cst}) + X_{cst}'\alpha + E_{st}'\varphi + C_{st}'\theta + P_{st}'\omega + \tau_t + \delta_c + \varepsilon_{cst}, \quad (1a)$$

where Y_{cst} is the arrest rate per 1,000 population for those ages 16-to-24 in county c in state s in year t . The independent variable of interest $\ln(MW_{cst})$, is the natural log of the maximum of the city, county, state, or Federal minimum wage for a given county in year t , measured in 2016 dollars.^{22, 23} The vector X_{cst} includes demographic and crime reporting controls (the share of the county population that is African American, Hispanic, and male; the share of the state population ages 25 and older who have a Bachelor's degree or higher; and the number of agencies reporting arrests in the county); the vector E_{st} includes state-level economic controls (the natural log of the average hourly wage rate of 25-to-54 year-olds and the natural log of the male unemployment rate of 25-to-54 year-olds); the vector C_{st} includes state-level crime policy controls (shall issue concealed carry permit laws, the natural log of law enforcement employees per 1,000 population, and the natural log of police expenditures per 1,000 population); and the vector P_{st} includes state-level health and social welfare policies (whether the state has a refundable EITC, whether

²¹ Following Currie and Fallick (1996), we also experimented with *MW Gap*, set equal to 0 if a worker earned a wage higher than the minimum wage at year $t+1$, and equal to the difference between the minimum wage in period $t+1$ and the worker's wage in period t when the worker's wage is between the old and new minimum wages. Albeit less precise, the pattern of results is similar for regressions that replace *Binding MW* with *MW Gap*.

²² The county minimum wage is coded as the weighted average of the higher of the county/city minimum wage, where the weight depends on the share of the year the wage is in effect. We experiment with alternative coding of the minimum wage, including a weighted average of the prevailing county wage and the city wage, where the weight depends on the share of the year the wage is in effect and the share of the county population that the city represents as of the 2010 Census, and find similar patterns of results.

²³ In some specifications, we also include an indicator for the presence of living wage laws, following Fernandez et al. (2014).

the state Medicaid program has been expanded to include childless adults, whether all vehicles are exempt from an asset test for Supplemental Nutrition Assistance Program eligibility, whether the state minimum legal high school dropout age exceeds 17, whether the state has an E-Verify mandate, whether the state or county has a “ban-the-box” employment law, whether the state has a marijuana legalization or medical marijuana laws, and the natural log of the real beer tax).²⁴ In some specifications, we also include controls for state-specific time trends, both linear and higher-order trends:

$$Y_{cst} = \beta_0 + \beta_1 \ln(MW_{cst}) + X_{cst}'\alpha + E_{st}'\varphi + C_{st}'\theta + P_{st}'\omega + \tau_t + \delta_c + \mu_s \times t + \mu_s \times t^2 + \varepsilon_{cst} \quad (1b)$$

In equations (1a) and (1b), identification of β_1 comes from within-state, and occasionally within-county, variation in minimum wages. For our estimates to be interpreted causally, the common trends assumption must be satisfied. We take a number of tacks to address this concern. First, we carry out an event-study analysis, with particular attention to whether pre-treatment trends in arrests are similar between treatment and control jurisdictions. Our event-study approach accounts for the continuous and cumulative nature of minimum wage increases, namely that (i) the magnitudes of minimum wage increases vary over time, and (ii) jurisdictions may see multiple minimum wage increases over time. While specifying minimum wage events

²⁴ We compile the share of population ages 25 and older with a Bachelor’s degree, the prime-age (ages 25-to-54) average hourly wage and the prime-age male unemployment rate using the CPS Merged Outgoing Rotation Groups. Population data are collected from the Surveillance Epidemiology and End Results, U.S. Population Data (SEER). Police employment and expenditures are generated using data from the Bureau of Justice Statistics. Shall issue laws are updated using the sources available in Anderson and Sabia (2018). State EITC data are collected from the Tax Policy Center and E-verify data are collected from Churchill and Sabia (2019). Minimum legal dropout age data are collected through 2008 using Anderson (2014) and updated to 2016 from the National Center of Education Statistics. SNAP rules on vehicles are collected from U.S. Department of Agriculture, Food and Nutrition Service. Medicaid eligibility is compiled using various reports by the Henry J. Kaiser Family Foundation. Ban-the-box laws are updated from Doleac and Hansen (2020) using the National Employment Law Project (2017). Marijuana liberalization laws are updated using Sabia and Nguyen (2018). Beer taxes are collected from the Beer Institute. Population-weighted means and standard deviations of the main dependent and independent variables can be found in Table 1.

²⁵ The inclusion of controls for state-specific time trends is intended to mitigate bias in the estimates of minimum wage effects. However, the inclusion of these trends, particularly linear state time trends, may come at a cost of reduced precision, eliminating important dynamic labor market effects of the minimum wage (Meer and West 2016; Clemens 2019), or conflating minimum wage effects with the local business cycle (Neumark et al 2014a,b; Neumark and Wascher 2017). Thus, we also examine the robustness of estimated crime elasticities to the inclusion of state-specific higher-order polynomial trends.

in this manner removes the non-parametric appeal of dichotomous event study specifications, it aligns with the continuous minimum wage measure used in equations (1a) and (1b) and is most similar to a distributed lag model.²⁶ Following Schmidheiny and Siegloch (2019), we estimate:

$$Y_{cst} = \gamma_0 + \sum_{j \neq -3 \text{ to } -1} \gamma_j D_{cst}^j + X'_{cst} \alpha + E'_{st} \varphi + C'_{st} \theta + P'_{st} \omega + \tau_t + \delta_c + \mu_s \times t + \mu_s \times t^2 + \varepsilon_{cst}, \quad (2)$$

where j denotes event time and D_{cst}^j is a set of variables that captures the “intensity” of a minimum wage increase (i.e., the difference between the natural logs of new and old minimum wages) that occurred j periods from the time of the minimum wage change. Each γ_j is then a difference-in-differences estimator of the cumulative effect of minimum wage increases on arrests of 16-to-24 year-olds relative to the omitted time period $j(c,s,t) = -3$ to -1 .²⁷ We choose a multiple year reference period of 1 to 3 years prior to enactment (i) to guard against any idiosyncratic shock in a specific reference year driving our estimated contrasts, (ii) to avoid under-identification of the parameters γ_j in settings with dynamic treatment effects and jurisdiction-specific time trends (Borusyak and Jaravel 2017 suggest use of a “multiple period reference window” of up to 3 periods)²⁸, and (iii) to ensure that the reference period includes a time window prior to passage of the legislation (“pre-announcement period”).²⁹ This event-study framework will allow us to test for common trends prior to minimum wage enactment.

As an additional test of the common trends assumption, we explore heterogeneity in the impact of minimum wages across the age distribution. Older, more experienced individuals, may

²⁶ An alternative approach to the one we employ would be to estimate a traditional event study in which we dichotomize minimum wage increases by their magnitudes (Simon 2016; Fuest et al. 2018). In Appendix Figure 1, we show an event-study analysis of the top 25th percentile of real minimum wage increases over our sample period. The results are qualitatively similar to those shown in Figure 2.

²⁷ For our two “endpoints” D_{cst}^{-6} is the backward cumulated events that occurred six or more years into the sample period, and D_{cst}^2 is the forward cumulated events that occurred two or more years prior to the end of the sample period. Hence, the binned endpoints of D_{cst}^j capture the cumulative nature of minimum wage increases. See Schmidheiny and Siegloch (2019) for a formal derivation of this event study approach, and for multiple numerical examples.

²⁸ Borusyak and Jaravel (2017) illustrate that fully-dynamic event study specifications (which include both unit and time fixed effects) in staggered treatment adoption settings suffer from under-identification when only one pre-treatment period is omitted. Additionally, when unit-specific time trends are included, it requires another pre-treatment period to be omitted to achieve identification.

²⁹ We also experiment with a reference window several years prior to enactment (4 to 6 years prior to enactment) with a qualitatively similar pattern of results.

be less likely to be bound by the minimum wage or, may serve as labor-labor substitutes (or complements) for younger, less experienced workers. Thus, to ensure that any “post-treatment” trends we observe for 16-to-24 year-olds are not driven by differential trends unrelated to the minimum wage, we explore whether effects differ for older workers. While these are imperfect placebo tests, we expect smaller spillover effects to these workers.

Finally, our use of the NLSY97 will permit us to examine minimum wage effects for those for whom minimum wages bind. Specifically, in the spirit of Beauchamp and Chan (2014), we pool data from the 1998-2016 NLSY97 and estimate:

$$Y_{aist} = \beta_0^a + \beta_1^a \text{Binding} MW_{aist} + X'_{ist} \vartheta^a + C'_{st} \theta + E'_{st} \varphi + P'_{st} \omega + \tau_t + \delta_s + \varepsilon_{aist}, \quad (3)$$

where Y_{aist} is an indicator for the type of crime we are observing for respondent i , in age group a (ages 16-to-24 vs. 25 and older), in state s , during year t . Our primary coefficient of interest, β_1^a , captures the effect of minimum wage increases on criminal behavior for respondents (in age group a) who are bound by such increases compared to those who are not. The vector X_{ist} includes individual-level controls for race/ethnicity, age, math PIAT (Peabody Individual Achievement Test) scores, maternal education, and family income. The remainder of controls are identical to those in equation (1). Here, identification comes from changes in workers’ wages and/or changes in minimum wage policies that affect the bindingness of the minimum wage for a teen or young adult worker.

4. Results

4.1 UCR Results

Tables 2 through 5 show estimates of β_1 from the UCR. All models are weighted by the county population and standard errors are clustered on the state (Bertrand et al. 2004). Our primary focus is on property and violent offenses (Part I offenses).

In Table 2, we present estimates of β_1 from equation (1).³⁰ Column (1) presents findings from the most parsimonious specification, including only socio-demographic controls, while column (2) adds economic controls, column (3) adds crime policy controls, column (4) adds

³⁰ Estimates on the control variables present in Table 2 (column 4) appear in Appendix Table 3.

social welfare and health policy controls, column (5) adds state-specific linear time trends, and column (6) adds state-specific quadratic time trends.³¹ Across specifications in Panel I, we find consistent evidence that minimum wage increases are associated with increases in property crime arrests for 16-to-24 year olds. The estimated arrest elasticity with respect to the minimum wage is relatively stable across specifications, remaining around 0.2. We find no evidence that minimum wage increases had a statistically significant effect on teen and young adults violent crime arrests, though the estimated elasticities are positive (Panel II). These results suggest that minimum wage increases induce income-generating crimes among young adults.³²

To explore whether the effects we observe in Panel I of Table 2 can be explained by pre-treatment trends in property crime arrests, we next report results from the event study analyses described by equation (2). Panel (a) of Figure 2 shows that pre-treatment property arrests 16-to-24 year-olds were similar for “treatment” and “control” counties. Following enactment of minimum wage increases, we see substantial increases in property crime, with the largest increases occurring one year after a minimum wage increase, after which there is a slight decline. This pattern of pre- and post-treatment trends is consistent with minimum wage-induced increases in property offense arrests. While we find that pre-treatment trends in violent offense arrests were similar, we fail to detect any evidence that minimum wages impact violent crime (Panel b).³³

As a further test of the common trends assumption, we explore whether there are heterogeneous crime effects of minimum wage hikes across the age distribution.³⁴ Older, more

³¹ In Appendix Table 4, we present estimates from specifications that include higher-order polynomial trends and census region-specific year effects. In the main, the results are qualitatively similar to the findings shown in Table 2.

³² In results available upon request, we supplement our UCR-based analyses with analyses using the NIBRS. In the main, these results suggest that the property crime effects we find are largest in counties with populations of 100,000 or greater.

³³ This finding is not sensitive to the functional form of the state-specific time trends included as controls. In Panels (a) and (b) of Appendix Figure 2, we show very similar event-studies when using state-specific linear time trends as opposed to quadratic time trends as controls.

³⁴ For models that estimate crime effects for individuals ages 25 and older, we do not control for prime-age male unemployment rates, prime-age wage rates, or the share of individuals ages 25 and older with a college degree, as these measures may capture mechanisms through which minimum wages affect crime. Instead, following Clemens and Wither (2019) and Agan and Makowsky (2020), we control for the state-level housing price index (available from: <https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index.aspx>). This approach is designed to control for macroeconomic conditions that are not directly affected by minimum wage changes.

experienced individuals are less likely to be bound by minimum wages and hence any crime effects should likely be smaller. On the other hand, older individuals on the margin of crime commission may be more likely than the average older individual to be bound. The results in Panel I of Table 3 show that minimum wage hikes increase property crime arrests among teenagers ages 16-to-19 (Panel I, column 1) and young adults ages 20-to-24 (Panel I, column 2), with estimated elasticities of 0.162 to 0.270. There is little evidence of minimum wage-induced increases in property crime arrests for older individuals, which adds to our confidence that estimates of β_1 for those ages 16-to-24 are not capturing differential jurisdiction-level time trends. Finally, we find no evidence that minimum wage increases affect violent arrests among younger or older individuals (Panel II of Table 3).^{35, 36}

Finally, in column (8) of Table 3 we present estimates of the effect of minimum wages on net crime among all working age individuals (16-to-64 year-olds). Our estimates are positive, although statistically indistinguishable from zero, with estimated arrest elasticities around 0.08.

We next explore whether these effects differ by gender (Table 4) and offense type (Table 5). With regard to gender, we find that both male and female property crime rise similarly in response to minimum wage increases, with estimated elasticities of 0.215 to 0.343 (Table 4).

Given that larcenies comprise 72 percent of all property offenses for 16-to-24 year-olds during our sample period, we unsurprisingly find that the increase in property crime arrests for teens and young adults is driven by larcenies (Panel I, Table 5), where the estimated arrest elasticity is 0.241. There is no evidence that minimum wages affect other types of property (burglary, motor vehicle theft, and arson) or violent (homicide, robbery, rape, and aggravated assault) offenses (Panel II, Table 5). Finally, in Panel III of Table 5, we examine more minor

³⁵ In Appendix Table 5, we explore finer age groups for property crime arrests and probe the sensitivity of estimated arrest elasticities with respect to the minimum wage to the inclusion of state-specific time trends. We find estimates similar to Table 3, with the most consistent evidence of minimum wage-induced increases in property crime concentrated among 16-to-19 and 20-to-24 year-olds.

³⁶ In an alternate specification, we separate out the effects of state legislative minimum wage increases as compared to state minimum wages caused by changes in the Federal minimum wage over the time period (the 2007-2009 Federal increase from \$5.15 to \$7.25 per hour). We define an indicator variable *Bound*, that is set equal to one for a state-year in which a state experiences a minimum wage increase that is due to one of the Federal minimum wage increases between 2007-2009, and zero otherwise. We then re-estimate equation (1), including the *Bound* indicator and the $\text{Ln}(\text{MW}) * \text{Bound}$ interaction term (as well as the minimum wage main effect). In Appendix Table 6, we present these estimates for 16-to-24 year-olds. While the coefficient on the interaction is positive, we do not detect any evidence that state minimum wage increases caused by the 2007-2009 Federal increase were significantly different from state legislative increases.

offenses (Part II offenses) and find that minimum wage hikes increase disorderly conduct arrests, consistent with job-loss induced idleness among teens and young adults (Jacob and Lefgren 2003; Luallen 2006; Anderson 2014).³⁷

4.2 Mechanisms: CPS Results

An important mechanism for minimum wage increases to influence property crime is through their effects on the labor market outcomes of low-skilled workers. To explore this possibility, we draw data from the 1998-2016 Current Population Survey Outgoing Rotation Groups. We restrict our sample to individuals ages 16-to-24 with less than a high school diploma, as they are more likely to be bound by minimum wages and be on the margins of crime commission.³⁸ Table 6 shows these results.

Consistent with the prior literature, we find that minimum wage increases raise the hourly wages of teen and young adult workers, with estimated wage elasticities of 0.168 to 0.199 (Table 6, Panel I). However, we also find evidence that minimum wage increases lead to a reduction in employment, with estimated elasticities of -0.156 to -0.224 (Panel II). On the intensive margin, we find a reduction in usual weekly hours worked (conditional on employment), with estimated elasticities of -0.071 to -0.114 (Panel III). For unconditional weekly hours, we estimate elasticities of -0.222 to -0.343 (Panel IV). These findings suggest that adverse labor demand effects may be an important mechanism through which minimum wage increases lead to more property crime. Finally, we find that minimum wage increases are negatively (though

³⁷ To gauge whether crime effects differ across minimum wages as compared to living wage laws, in Appendix Table 7, we include an indicator for living wage laws in our model, as well as an interaction term for whether a living wage law applies to employers who receive financial assistance from the state or local government. For the financial assistance living wage provision, we find that laws including them are associated with a statistically significant 9.1 percent increase in property crimes. This result is consistent with evidence from the living wage-employment literature, which finds that living wage laws covering financial assistance recipients generates stronger adverse employment effects (Neumark and Adams 2005a). We also find some evidence that living wage ordinances are associated with increases in violent crime.

³⁸ Means of labor market outcomes and school enrollment used in the CPS are available in Appendix Table 8.

imprecisely) related to weekly earnings (Panel V), but have relatively little effect on school enrollment (Panel VI).^{39, 40}

Event-study analyses are consistent with a causal interpretation of these results.⁴¹ In specifications that exclude (Appendix Table 10) and include (Figure 3) state-specific time trends, we find that the adverse labor demand effects we detect are not driven by differential pre-treatment trends and are triggered following the enactment of the minimum wage hike. Moreover, property offense arrests are largest in the post-treatment event year ($j = 1$) precisely when employment and hours effects are largest, and both decline (in absolute magnitude) two years following the minimum wage increase. Additionally, when we examine higher-skilled, more experienced individuals, those ages 25-to-64 with a college degree or more, we find little evidence of wage or employment effects following minimum wage increases (see Appendix Figure 3).

4.3 NLSY97 Results

In Table 7A, we turn to individual-level panel data from the NLSY97 to explore whether minimum wages have the biggest bite on those 16-to-24 year-olds for whom the minimum wage is binding. That is, we move from the intent-to-treat framework of our UCR-based analysis to a treatment-on-the-treated framework in the NLSY97, following an approach similar to Currie and Fallick (1996) and Beauchamp and Chan (2014). We find that 16-to-24 year-olds bound by the minimum wage are 1.8 percentage-points (12.9 percent) more likely to engage in criminal activity, driven by a 1.7 percentage-point (21.3 percent) increase in property crime. For minimum wage bound individuals ages 25 and older, we also find evidence of minimum wage-induced increases in property crime, as well as an increase in the probability of being arrested.

³⁹ Using data from the CPS's October Supplement from 1998-2016, we estimate the effects of minimum wage increases on school enrollment via a probit model (Panel VI).

⁴⁰ In Appendix Table 9, we present estimates that control for the state-level housing price index in place of controls for the state-level prime-age male unemployment rate and prime-age wage rate. The estimates are qualitatively similar to Table 6, actually showing stronger evidence of adverse labor demand effects. Estimates with these alternate economic controls are also presented for arrests (Appendix Table 9, columns 1 and 2), with results quantitatively similar to those presented in Table 2.

⁴¹ The adverse labor demand effects of the minimum wage are not sensitive to the functional form of the state-specific time trend, as shown in panels (c) and (d) of Appendix Figure 2

Thus, we find no evidence to support the claim that minimum wage hikes reduce crime among those who are directly affected by it.⁴²

As our CPS-based estimates suggest, the lack of any crime-reducing effects of minimum wages may be explained by adverse labor demand effects borne by individuals bound by them. Our findings in Table 7B provide strong evidence that minimum wage increases negatively affect both the intensive and extensive margins of work, the likely mechanism at work. Furthermore, our results show stronger property crime effects for those below the median annual hours worked (Table 8, Panel I) as compared to individuals above the median (Table 8, Panel II), consistent with labor demand effects being an important mechanism at work.

4.4 Comparisons with Prior Estimates

Two relatively recent papers produce some evidence of crime reducing effects of minimum wages. We attempt to explain differences in our results from this prior work. First, while focusing largely on living wages, estimates shown in Tables 3A-B of Fernandez et al. (2014; pp. 488-489) show that minimum wages enacted between 1990 and 2010 reduced *overall* property and violent crime in large cities. First, to replicate their specification, we generate total arrest rates per 100,000 population for the 239 largest cities (as of 1990) from the UCR, collect data on their controls, and use their preferred log-log specification to estimate the effect of minimum wage increases on overall property (Panel I) and violent (Panel II) crime arrests from 1990-2010.⁴³ The results in column (1) of Table 9 are consistent with their results: minimum wage increases enacted between 1990 and 2010 resulted in large, statistically significant reductions in aggregate city-level arrests. The inclusion of a set of observable demographic and macroeconomic controls used by Fernandez et al. (2014) (column 2) produces an elasticity of -0.094 for property crime arrests and -0.237 for violent crime arrests, though both estimates are statistically indistinguishable from zero at conventional levels. In columns (3) through (5), we

⁴² We also experimented with including controls for individual fixed effects, which would require individual-specific changes in the bindingness of minimum wages over time for identification. Estimated property crime effects continue to be positive in these models, though the magnitudes of the estimated treatment effects are somewhat smaller and only marginally significantly different from zero for bound workers.

⁴³ Following Fernandez et al. (2014), we gather data for this replication using <https://www.ucrdatatool.gov/>, where the FBI uses an imputation procedure to estimate crime rates for agencies with poor reporting. These data are only available through 2014.

include controls for city-specific time trends; in these specifications, estimated property crime elasticities become small and positive and violent crime arrests elasticities fall to near zero. These results are largely consistent with our UCR-based findings nationwide, where we find little evidence of net effects on arrests for working-age individuals.

Second, Agan and Makowsky (2020) find that minimum wage increases enacted between 2000 and 2014 are associated with a reduction in recidivism rates, mainly through reduced property and drug crime. We explore whether our findings may differ due to (i) differences in the states and years comprising the analysis sample, (ii) demographic composition of arrestees, and (iii) margin of criminal behavior examined (recidivism versus overall crime). We collect data on the controls used by Agan and Makowsky (2020) to rule out observable controls as an explanation for differences in results.

In column (1) of Table 10, we estimate the effect of minimum wage increases on total arrests for all ages using the sample of states and years that were available in the National Corrections Reporting Program (NCRP), the data source used by Agan and Makowsky (2020).⁴⁴ We also use a specification similar to that employed by the authors. Our results show no evidence that minimum wage increases affected net property, violent, or drug crime. Estimated elasticities with respect to the minimum wage are 0.155 for property crime, 0.090 for violent crime, and -0.137 for drug crime arrests, each statistically indistinguishable from zero at conventional levels. These estimated effects are far more positive than the implied recidivism elasticities obtained by Agan and Makowsky (2020; Table 8, columns 1-3), -0.553, -0.121, and -0.451, respectively. Adding the full set of states and years available in the UCR imputed crime data files from 2000-2014 (column 2), we continue to find no evidence of minimum wage-induced declines in net crime.

To gauge whether results may differ across demographic groups, we also examine adults ages 18 and older (columns 3-4), adult males (columns 5-6), and African American adults

⁴⁴ In the sample of arrestees of all ages, we use imputed UCR crime compiled by the ICPSR. These data include imputed crime counts for jurisdictions which have poor reporting, and are available from: <https://www.ojjdp.gov/OJSTATBB/ezauct/asp/methods.asp>

⁴⁵ We identify the state-years available in the National Corrections Reporting Program (NCRP) public use files by state and year of release from prison, approximating the analysis sample used by Agan and Makowsky (2020). Additionally, we drop California from the analysis sample, as does Agan and Makowsky (2020). The NCRP public use files may be obtained from: <https://www.icpsr.umich.edu/icpsrweb/NACJD/studies/37021>

(columns 7-8). Across each of these demographic groups, we find no evidence of crime-reducing effects of minimum wages. Expanding the sample period through 2015 and 2016 (columns 4, 6, and 8) produces a similar pattern of results.^{46,47} Together with the findings of Agan and Makowsky (2020), our results in Table 10 suggest that minimum wage increases may have heterogeneous effects on different margins of criminal behavior, including first-time arrests as well as arrests that do not result in incarceration and release.

5. Conclusion

An April 2016 report from the White House Council of Economic Advisers claimed that raising the Federal minimum wage from \$7.25 to \$12 per hour could reduce crime by 3 to 5 percent, generating substantial social benefits. However, this conclusion rested on the assumption that minimum wage increases would only generate wage gains with no offsetting employment or human capital effects. This study comprehensively examines the effects of recent changes to Federal, state, and local minimum wages on crime. Our results suggest that minimum wage increases enacted from 1998 to 2016 led to increases in property crime arrests for those between the ages of 16-to-24, with an estimated elasticity of around 0.2. This finding is robust to the inclusion of controls for state-specific time trends, survive falsification tests on policy leads, and generally persist for workers who earn wages such that minimum wage changes bind. Increases in property crime appear to be driven by adverse labor demand effects of minimum wages. We find little evidence that minimum wage increases affect arrests for violent or minor offenses.

Our back-of-the-envelope calculation suggests that a 10 percent increase in the minimum wage between 1998 and 2016 led to nearly 80,000 additional property crimes committed by 16-to-24 year-olds, generating annual crime costs of \$467 million (2019\$) (McCollister et al. 2010).⁴⁸ Moreover, if our estimated crime elasticities are used to make predictions of future

⁴⁶ In Appendix Table 11, we present estimates using the Agan and Makowsky (2020) specification for 16-to-24 year-olds over the 2000-2014 and 2000-2016 period, which are consistent with our main results from Table 2.

⁴⁷ In results available upon request, we also examine the effect of minimum wage increases on arrests among 25-to-54 year-old males, an age demographic that comprises around 80 percent of the sample in Agan and Makowsky (2020). We find little evidence of arrest-reducing effects of the minimum wage among this demographic group.

⁴⁸ To generate this cost estimate, we first gather Part I property and violent crimes committed over the 1998-2016 period using the FBI's *Crime in the United States* reports (available from: <https://ucr.fbi.gov/crime-in-the->

policy changes, raising the Federal minimum wage to \$15 per hour, as the *Raise the Wage Act of 2019* proposes and is endorsed by Democratic Presidential Candidate Joe Biden⁴⁹, would generate approximately 423,000 additional property crimes and \$2.5 billion in additional crime costs.⁵⁰ These could be lower bound estimates if living wage ordinances also have the unintended consequence of increasing crime or if there are modest increases in delinquency-related crimes. Together, the findings from this study suggest that, in contrast to the CEA claim, higher minimum wages are unlikely to be an effective tool to fight net crime.

[u.s./2016/crime-in-the-u.s.-2016/topic-pages/tables/table-1](https://ucr.fbi.gov/crime-in-the-u.s.-2016/topic-pages/tables/table-1)). We then use the UCR's *Arrests by Age, Sex, and Race* files from 1998-2016 to calculate the share of property crimes committed by 16-to-24 year-olds. To generate an estimate of the number of crimes committed by 16-to-24 year-olds, we calculate the product of the average crime counts over the 1998-2016 period from the FBI's *Crime in the United States* report and the share of crimes committed by 16-to-24 year-olds from the UCR's *Arrests by Age, Sex, and Race* files. Using the estimated crime elasticity with respect to the minimum wage of 0.210 for property crime (Table 2, Panel I, column 4), we estimate 79,902 additional property crimes would be generated by a 10 percent increase in the minimum wage. Then, we use the per crime cost of a property offense of \$5,844 (in 2019USD) from McCollister et al. (2010) to estimate the total additional crime cost from a 10 percent increase in the minimum wage. We obtain an estimate of \$467 million for property crime.

⁴⁹ Vice President Biden pledges to increase the Federal minimum wage to \$15 as well as eliminate the Federal tipped minimum wage. For more information, see: <https://joebiden.com/empowerworkers/>

⁵⁰ To generate this cost estimate, we first gather state-specific Part I property crimes committed in 2016 using the FBI's *Crime in the United States* report (available from: <https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-3>). We then use the UCR's *Arrests by Age, Sex, and Race* files from 2016 to calculate the state-specific share of property crime committed by 16-to-24 year-olds. To generate the state-specific estimate of the number of crimes committed by 16-to-24 year-olds, we calculate the product of state-level crime counts from FBI's *Crime in the United States* 2016 report and the share of crimes committed by 16-to-24 year-olds from the UCR's *Arrests by Age, Sex, and Race* files. Then, to calculate the state-specific percentage change in the minimum wage caused by a \$15 Federal minimum wage, we use the higher of the state or Federal minimum wage in July 2018 to calculate a state-specific measure of the increase in the minimum wage they would experience from a \$15 minimum wage. Using our estimated property crime elasticity with respect to the minimum wage of 0.210 from column (4) of Table 2, we estimate the number of additional property crimes that would be generated in each state. Summing across states, we estimate 422,742 additional property crimes would be generated by a \$15 minimum wage. Then, we use the per crime cost of a property offense of \$5,844 (in 2019USD) from McCollister et al. (2010) to estimate the total additional crime cost from a \$15 minimum wage of \$2.5 billion.

6. References

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Figure 1. Identifying Variation in Real Minimum Wage Increases from UCR Sampled Counties, 1998-2016

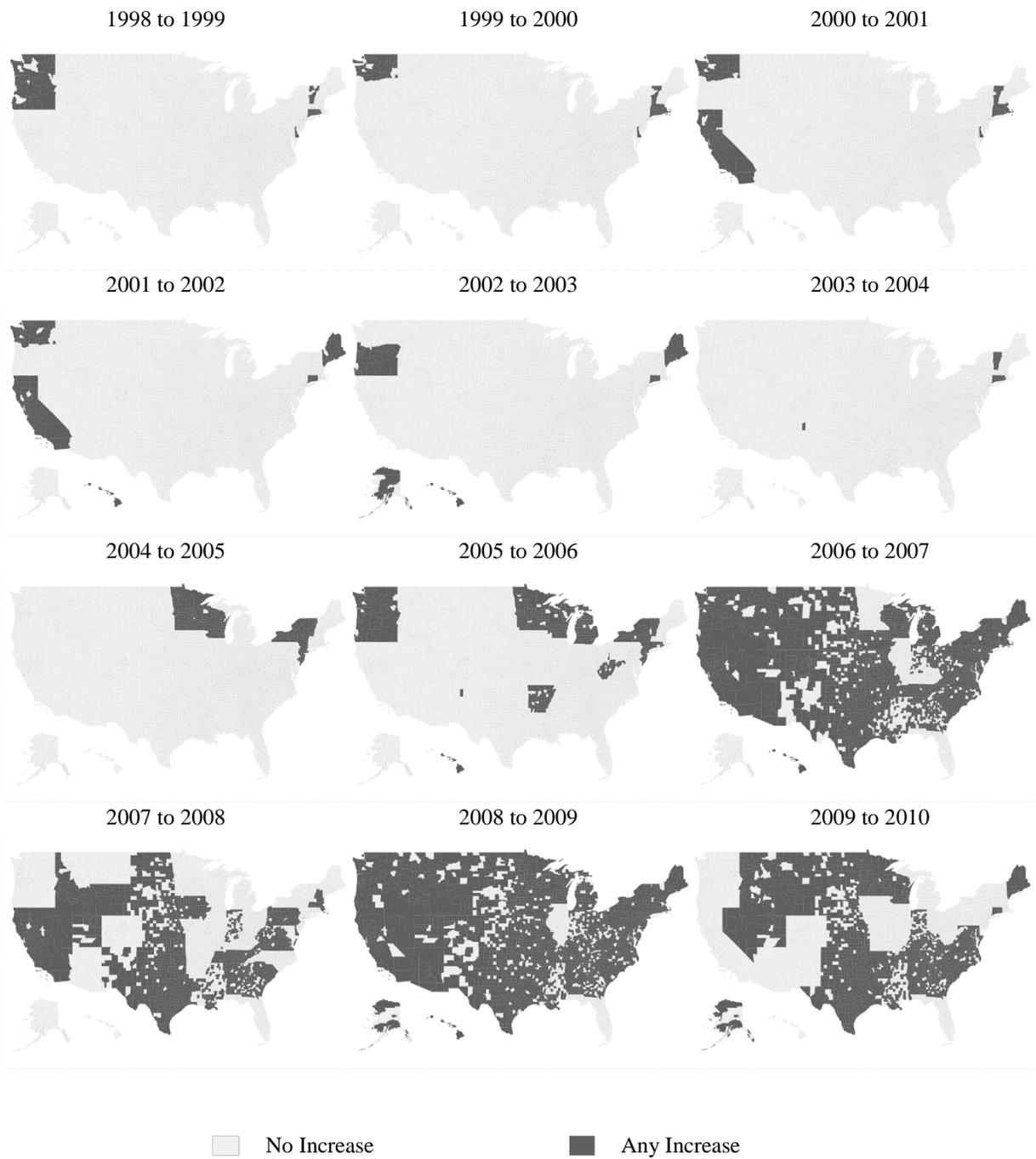
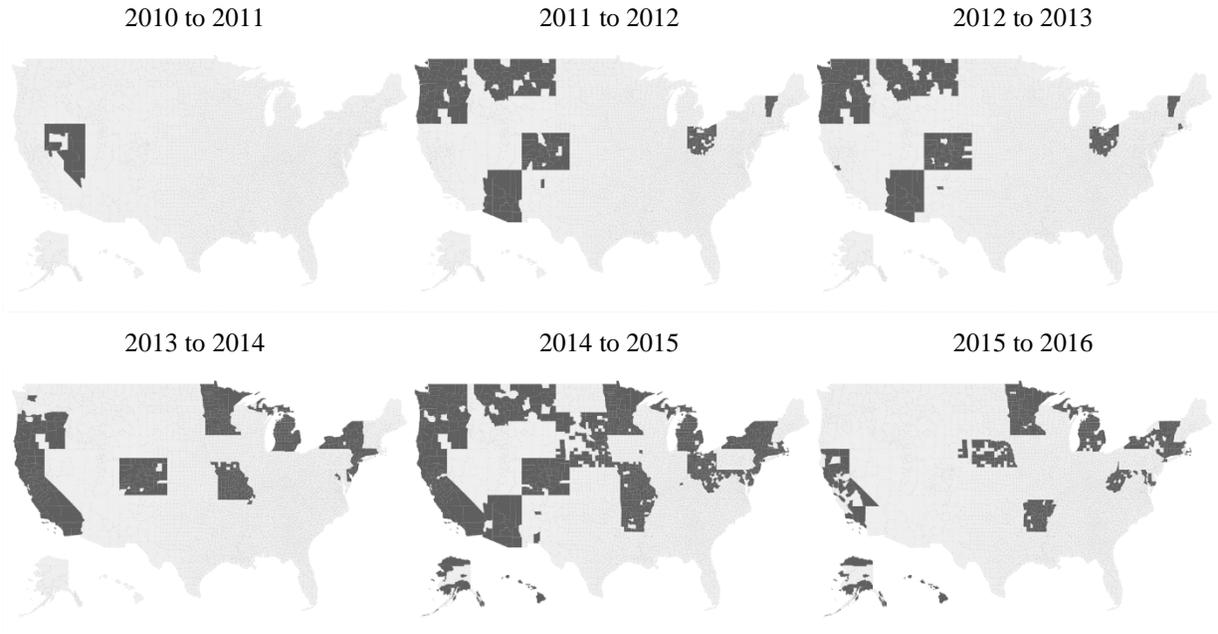
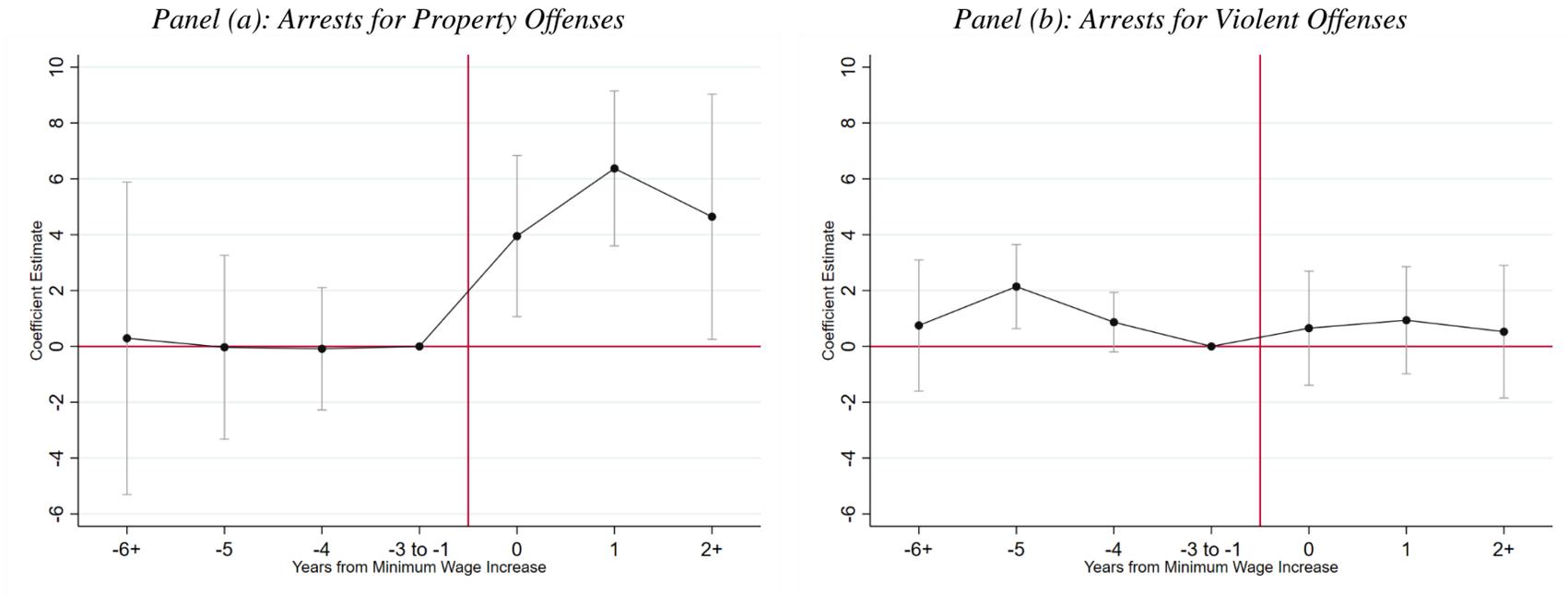


Figure 1 (Continued)



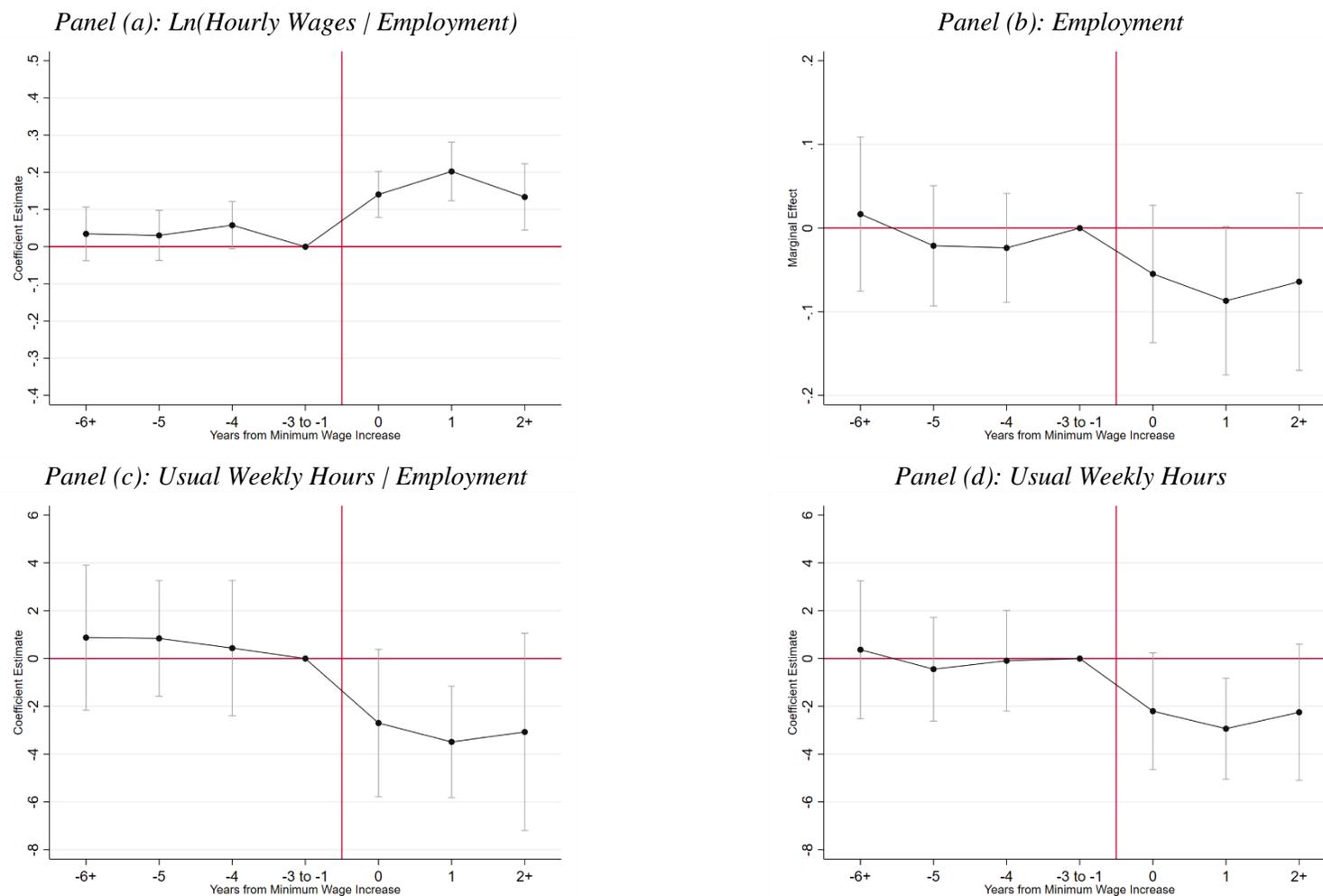
Notes: Real minimum wage (2016\$) increases among counties in the 1998-2016 UCR estimation sample. Counties with minimum increases are shaded in dark gray. Counties not in the estimation sample or counties without a real minimum wage increase are shaded in light gray.

Figure 2. Event Study Analysis of Estimated Relationship between Minimum Wages and Crime Arrest Rates for those ages 16-to-24, UCR, 1998-2016



Notes: Event study coefficients plotted from the regression outlined in equation (2). Error bars are 95 percent confidence intervals. Following Borusyak and Jaravel (2017), we use a multiperiod reference period of 1 to 3 years prior to enactment given that the model includes jurisdiction-specific time trends. Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, state-specific linear and quadratic time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Samples are restricted to counties with crime arrest rates within two standard deviations of the county’s average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Figure 3. Event Study Analysis of Estimated Relationship between Minimum Wages and Labor Market Outcomes for those ages 16-to-24 without a High School Diploma, CPS, 1998-2016



Notes: Event study coefficients plotted from the regression outlined in equation (2) (modified for CPS analysis). Following Borusyak and Jaravel (2017), we use a multiperiod reference period of 1 to 3 years prior to enactment given that the model includes jurisdiction-specific time trends. Data are drawn from the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG). All regressions include age, state, year, and month fixed effects, as well as state-specific linear and quadratic time trends. The list of socio-demographic controls, economic controls, crime policy controls and social welfare & health controls are available in Table 1. Panels (a), (c), and (d), are estimated via OLS; Panel (b) is estimated via probit. Estimates are weighted using the sample weights provided by the CPS, and standard errors are clustered at the state level.

Table 1. Descriptive Statistics, Uniform Crime Reports, 1998-2016

	Mean (Standard Deviation)
<i>Dependent Variables</i>	
Property Crime Arrest Rate, Ages 16-to-24	15.83 (9.80) [44,259]
Violent Crime Arrest Rate, Ages 16-to-24	5.04 (6.38) [44,203]
<i>Independent Variables</i>	
Minimum Wage (2016\$)	7.65 (0.87)
Socio-demographic controls	
Number of reporting agencies	23.073 (25.398)
Shares of males	0.492 (0.128)
Shares of African American	0.132 (0.136)
Shares of Hispanic	0.162 (0.171)
Shares of individuals ages 25+ with a BA degree	0.293 (0.052)
Economic controls	
Average hourly wages for adults ages 25-54 (2016\$)	22.92 (2.25)
Unemployment rates for males ages 25-54	0.051 (0.023)
Crime policy controls	
Shall issue laws	0.626 (0.484)
Police expenditures per capita (2016\$)	312.02 (84.24)
Police employment per capita	2.291 (0.599)
Health and Social Welfare Policies	
State refundable EITC	0.299 (0.458)
Presence of Medicaid for childless adults	0.110 (0.313)
SNAP all-vehicles exemption	0.600 (0.482)
Minimum dropout age of 18+	0.486 (0.500)
E-verify	0.166 (0.369)
Ban the box laws	0.163 (0.360)
Marijuana decriminalization	0.178 (0.382)
Medical marijuana laws	0.296 (0.452)
Beer taxes (2016\$)	0.28 (0.23)

Notes: Weighted means are generated using data from the 1998-2016 Uniform Crime Reports (UCR). Arrest rates are arrests per 1,000 people ages 16-to-24. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Standard deviations are in parentheses and number of observation in brackets. Means and standard deviations are weighted by the county population ages 16-to-24.

Table 2. Estimated Relationship between Minimum Wages and Crime Arrest Rates for those ages 16-to-24, UCR, 1998-2016

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel I: Property Crime Arrest Rates</i>						
Ln(MW)	4.138** (1.771)	4.068** (2.013)	3.983* (2.163)	3.317** (1.617)	5.182*** (1.135)	5.230*** (1.076)
<i>Elasticity</i>	<i>0.261</i>	<i>0.257</i>	<i>0.252</i>	<i>0.210</i>	<i>0.327</i>	<i>0.330</i>
N	44,259	44,259	44,259	44,259	44,259	44,259
<i>Panel II: Violent Crime Arrest Rates</i>						
Ln(MW)	0.772 (0.667)	0.812 (0.639)	0.861 (0.628)	0.551 (0.740)	0.481 (0.648)	0.678 (0.803)
<i>Elasticity</i>	<i>0.153</i>	<i>0.161</i>	<i>0.171</i>	<i>0.109</i>	<i>0.096</i>	<i>0.135</i>
N	44,203	44,203	44,203	44,203	44,203	44,203
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic controls	No	Yes	Yes	Yes	Yes	Yes
Crime policy controls	No	No	Yes	Yes	Yes	Yes
Social welfare & health policies	No	No	No	Yes	Yes	Yes
State-Specific Linear Time Trends	No	No	No	No	Yes	Yes
State-Specific Quadratic Time Trends	No	No	No	No	No	Yes

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include county fixed effects and year fixed effects. The list of socio-demographic controls, economic controls, crime policy controls and social welfare & health controls are available in Table 1. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Table 3. Heterogeneity in Relationship between Minimum Wages and Crime Arrest Rates by Age Group, UCR, 1998-2016

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages	Ages	Ages	Ages	Ages	Ages
	16-to-19	20-to-24	25-to-34	35-to-49	50+	16-to-64
<i>Panel I: Property Crime Arrest Rates</i>						
Ln(MW)	3.261*	3.310**	0.398	-0.902	-0.106	0.564
	(1.904)	(1.619)	(1.092)	(0.765)	(0.107)	(0.772)
<i>Elasticity</i>	<i>0.162</i>	<i>0.270</i>	<i>0.051</i>	<i>-0.191</i>	<i>-0.110</i>	<i>0.084</i>
N	44,203	44,325	44,391	44,290	44,240	44,526
<i>Panel II: Violent Crime Arrest Rates</i>						
Ln(MW)	-0.038	0.779	0.310	-0.001	0.041	0.208
	(0.940)	(0.660)	(0.680)	(0.376)	(0.069)	(0.465)
<i>Elasticity</i>	<i>-0.007</i>	<i>0.157</i>	<i>0.088</i>	<i>-0.000</i>	<i>0.098</i>	<i>0.081</i>
N	44,157	44,117	44,187	44,194	44,001	44,357

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people of the specified age group. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. Regressions in columns (3)-(6) do not control for prime-age male unemployment rates, prime-age wage rates, or the share of individuals with a college degree to avoid controlling for mechanisms; instead, they control for the state-level housing price index. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population of the specified age group. Standard errors are clustered at the state level.

Table 4. Heterogeneity in Relationship between Minimum Wages and Crime Arrest Rates for those ages 16-to-24 by Gender, UCR, 1998-2016

	(1)	(2)	(3)	(4)
	Males		Females	
<i>Panel I: Property Crime Arrest Rates</i>				
Ln(MW)	4.394*	7.036***	2.528***	3.307***
	(2.485)	(1.646)	(0.940)	(0.738)
<i>Elasticity</i>	<i>0.215</i>	<i>0.343</i>	<i>0.236</i>	<i>0.309</i>
N	44,200	44,200	44,201	44,201
<i>Panel II: Violent Crime Arrest Rates</i>				
Ln(MW)	0.633	1.016	0.435	0.273
	(1.172)	(1.266)	(0.312)	(0.305)
<i>Elasticity</i>	<i>0.077</i>	<i>0.124</i>	<i>0.251</i>	<i>0.157</i>
N	44,231	44,231	43,932	43,932
State-Specific Time Trends	No	Yes	No	Yes

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Columns (2) and (4) include state-specific linear and quadratic time trends. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population of the specified gender ages 16-to-24. Standard errors are clustered at the state level.

Table 5. Heterogeneity in Relationship between Minimum Wages and Crime Arrest Rates for those ages 16-to-24 by Offense Type, UCR, 1998-2016

<i>Panel I: Property Crime Offenses</i>					
	(1)	(2)	(3)	(4)	
	<i>Burglary</i>	<i>Larceny</i>	<i>Motor Vehicle Theft</i>	<i>Arson</i>	
Ln(MW)	-0.087 (0.372)	2.764*** (0.939)	0.746 (0.595)	0.016 (0.018)	
<i>Elasticity</i>	-0.029	0.241	0.664	0.198	
N	44,023	44,272	43,826	43,708	
<i>Panel II: Violent Crime Offenses</i>					
	(5)	(6)	(7)	(8)	
	<i>Aggravated Assault</i>	<i>Robbery</i>	<i>Homicide</i>	<i>Rape</i>	
Ln(MW)	0.338 (0.539)	0.160 (0.247)	0.026 (0.030)	0.013 (0.046)	
<i>Elasticity</i>	0.103	0.114	0.184	0.069	
N	44,161	44,002	43,839	43,803	
<i>Panel III: Minor Crime Offenses</i>					
	(9)	(10)	(11)	(12)	(13)
	<i>Vandalism</i>	<i>Liquor Law Violations</i>	<i>Drunkenness</i>	<i>Disorderly Conduct</i>	<i>Drug</i>
Ln(MW)	0.493 (0.367)	3.481 (3.181)	-0.715 (1.040)	3.389* (1.794)	0.655 (2.417)
<i>Elasticity</i>	0.190	0.382	-0.209	0.619	0.040
N	44,019	44,105	44,495	44,183	44,397

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Table 6. Estimated Relationship between Minimum Wages and Labor Market Outcomes and School Enrollment for those ages 16-to-24 without a HS Diploma, CPS, 1998-2016

	(1)	(2)	(3)
<i>Panel I: Ln(Hourly Wages / Employment)</i>			
Ln(MW)	0.199*** (0.025)	0.196*** (0.029)	0.168*** (0.025)
<i>Elasticity</i>	<i>0.199</i>	<i>0.196</i>	<i>0.168</i>
N	78,071	78,071	78,071
<i>Panel II: Employment</i>			
Ln(MW)	-0.042 (0.034)	-0.057** (0.029)	-0.061 (0.039)
<i>Elasticity</i>	<i>-0.156</i>	<i>-0.211</i>	<i>-0.224</i>
N	293,216	293,216	293,216
<i>Panel III: Usual Weekly Hours / Employment</i>			
Ln(MW)	-1.807* (1.071)	-1.842* (1.049)	-2.903** (1.130)
<i>Elasticity</i>	<i>-0.071</i>	<i>-0.072</i>	<i>-0.114</i>
N	83,731	83,731	83,731
<i>Panel IV: Usual Weekly Hours</i>			
Ln(MW)	-1.535* (0.813)	-1.891** (0.810)	-2.371** (0.979)
<i>Elasticity</i>	<i>-0.222</i>	<i>-0.274</i>	<i>-0.343</i>
N	293,216	293,216	293,216
<i>Panel V: Usual Weekly Earnings</i>			
Ln(MW)	-1.088 (9.266)	-5.224 (9.591)	-9.443 (10.748)
<i>Elasticity</i>	<i>-0.016</i>	<i>-0.076</i>	<i>-0.137</i>
N	293,216	293,216	293,216
<i>Panel VI: School Enrollment</i>			
Ln(MW)	-0.028 (0.035)	-0.032 (0.034)	-0.020 (0.037)
<i>Elasticity</i>	<i>-0.037</i>	<i>-0.042</i>	<i>-0.026</i>
N	95,674	95,674	95,674
Socio-demographic controls	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes
Crime policy controls	No	Yes	Yes
Social welfare & health policies	No	Yes	Yes
State-specific time trends	No	No	Yes

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Data are drawn from the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG) for Panels I-V and the CPS October Supplement for Panel VI. All regressions include age, state, year, and month fixed effects. Column (3) includes state-specific linear and quadratic time trends. The list of socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls are available in Table 1. Panels I, III, IV, and V are estimated via OLS;

Panels II and VI are estimated via probit. Estimates are weighted using the sample weights provided by the CPS, and standard errors are clustered at the state level.

Table 7A. Estimated Relationship between Minimum Wage and Crime, NLSY97, 1998-2016

	(1)	(2)	(3)	(4)	(5)
	<i>Any Crime</i>	<i>Property</i>	<i>Violent</i>	<i>Sold Drugs</i>	<i>Arrest</i>
Ages 16-to-24	0.018** (0.008)	0.017*** (0.006)	-0.003 (0.007)	-0.000 (0.008)	0.004 (0.005)
Ages 25+	0.019** (0.009)	0.026*** (0.007)	0.006 (0.007)	0.003 (0.009)	0.020*** (0.007)
N	51,067	51,066	45,730	51,069	71,880

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted estimates are generated using data from the 1998-2016 National Longitudinal Survey of Youth 1997 (NLSY97). All regressions include controls for state fixed effects, age dummies, individual characteristics (available in Appendix Table 2), socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Standard errors are clustered at the state level.

Table 7B. Estimated Relationship between Minimum Wage and Labor Market Outcomes, NLSY97, 1998-2016

	(1)	(2)	(3)	(4)
	<i>Total Weeks Worked</i>	<i>Hours Per Week</i>	<i>Total Hours Worked</i>	<i>Employed</i>
Ages 16-to-24	-1.264* (0.731)	-1.619*** (0.427)	-114.047*** (30.151)	0.008 (0.006)
Ages 25+	-6.320*** (0.849)	-4.461*** (0.476)	-401.732*** (40.948)	-0.035*** (0.009)
N	71,741	67,331	67,331	72,365

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted estimates are generated using data from the 1998-2016 National Longitudinal Survey of Youth 1997 (NLSY97). All regressions include controls for state fixed effects, age dummies, individual characteristics (available in Appendix Table 2), socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Standard errors are clustered at the state level.

Table 8. Estimated Relationship between Minimum Wage and Crime, Below and Above Median Annual Hours Worked Samples, NLSY97, 1998-2016

	(1)	(2)	(3)	(4)	(5)
	<i>Any Crime</i>	<i>Property Crime</i>	<i>Assault</i>	<i>Sold Drugs</i>	<i>Arrest</i>
<i>Panel I: Below Median Annual Hours Worked</i>					
Ages 16 to 24	0.032** (0.013)	0.026*** (0.008)	0.001 (0.010)	0.002 (0.013)	0.007 (0.006)
Ages 25+	0.015 (0.016)	0.033* (0.017)	0.000 (0.014)	-0.002 (0.012)	0.000 (0.012)
N	23,662	23,662	22,922	23,661	30,278
<i>Panel II: Above Median Annual Hours Worked</i>					
Ages 16 to 24	-0.009 (0.016)	-0.002 (0.013)	-0.011 (0.010)	-0.004 (0.009)	-0.004 (0.010)
Ages 25+	0.016 (0.013)	0.018** (0.009)	0.007 (0.009)	0.003 (0.010)	0.029*** (0.010)
N	27,405	27,404	22,808	27,408	41,602

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted estimates are generated using data from the 1998-2016 National Longitudinal Survey of Youth 1997 (NLSY97). All regressions include controls for state fixed effects, age dummies, individual characteristics (available in Appendix Table 2), socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Standard errors are clustered at the state level.

Table 9. Replication and Extension of Fernandez et al. (2014) Minimum Wage Findings, Aggregate City Crime Rates for Large U.S. Cities, UCR, 1990-2010

	(1)	(2)	(3)	(4)	(5)
<i>Panel I: Ln(Overall Property Crime Rates)</i>					
Ln(MW)	-0.500*** (0.118)	-0.094 (0.133)	-0.024 (0.054)	0.026 (0.062)	0.030 (0.109)
N	4,710	4,710	4,710	4,710	4,710
<i>Panel II: Ln(Overall Violent Crime Rates)</i>					
Ln(MW)	-0.802*** (0.102)	-0.237 (0.142)	-0.092 (0.071)	-0.092 (0.098)	-0.011 (0.120)
N	4,560	4,560	4,560	4,560	4,560
City & Year FEs	Y	Y	Y	Y	Y
Controls	N	Y	Y	Y	Y
City Linear Time Trends	N	N	Y	Y	Y
City Quadratic Time Trends	N	N	N	Y	Y
City 4th-Order Time Trends	N	N	N	N	Y

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using imputed UCR city crime data over the 1990-2010 period, which include imputed crime counts for jurisdictions that have poor reporting. Controls from Fernandez et al. (2014) include the percent of the county population that is African American, white, female, ages 0-19, 20-29, 30-39, 40-49, and 50-64, as well city-level log of police per capita, the state-level log of imprisonment rates, adult unemployment rates, and per capita personal income. Estimates are weighted by city population, and standard errors are clustered at the state level.

Table 10. Sensitivity of Results to Aggregate Crime Rates for States and Years in NCRP Data Examined by Agan and Makowsky (2020)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Ages ^a		Ages 18+ ^b		Males Ages 18+ ^b		African Americans Ages 18+ ^b	
<i>Panel I: Property Crime Arrest Rates</i>								
MW	0.118 (0.075)	0.083 (0.106)	0.024 (0.078)	0.059 (0.066)	0.001 (0.119)	0.076 (0.099)	-0.031 (0.202)	0.140 (0.164)
Elasticity	0.155	0.117	0.036	0.090	0.001	0.086	-0.019	0.090
N	24,287	35,586	35,492	40,584	35,448	40,542	34,957	40,005
<i>Panel II: Violent Crime Arrest Rates</i>								
MW	0.021 (0.026)	0.083 (0.053)	0.057 (0.045)	0.062 (0.037)	0.091 (0.075)	0.089 (0.062)	0.044 (0.169)	0.141 (0.140)
Elasticity	0.090	0.326	0.211	0.237	0.201	0.204	0.052	0.176
N	24,283	35,599	35,411	40,437	35,365	40,398	34,970	39,999
<i>Panel III: Drug Crime Arrest Rates</i>								
MW	-0.098 (0.064)	0.095 (0.097)	0.085 (0.103)	0.040 (0.113)	0.152 (0.178)	0.094 (0.190)	0.310 (0.731)	0.145 (0.596)
Elasticity	-0.137	0.133	0.108	0.052	0.117	0.075	0.143	0.071
N	24,287	35,621	35,470	40,451	35,452	40,464	34,952	39,950
Years	2000-2014	2000-2014	2000-2014	2000-2016	2000-2014	2000-2016	2000-2014	2000-2016
States	NCRP States	UCR States	UCR States	UCR States	UCR States	UCR States	UCR States	UCR States

***Significant at 1% level **Significant at 5% level *Significant at 10% level

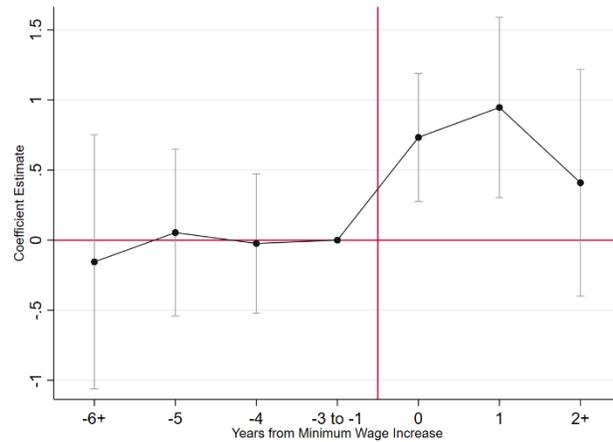
^a Estimates from columns (1)-(2) use imputed UCR crime, which include imputed crime counts for jurisdictions that have poor reporting.

^b Estimates from columns (3)-(8) use data from the UCR's *Arrests by Age, Sex, and Race* files, the data which we have used for Tables 1-5.

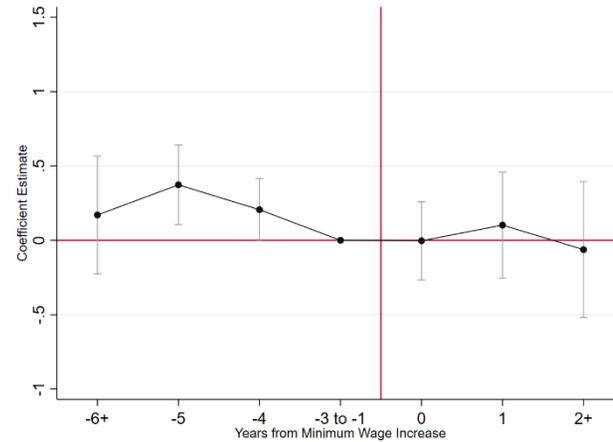
Notes: Weighted OLS estimates are generated using data from the 2000-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people of the specified age group. All regressions include controls for county fixed effects, year fixed effects, the number of agencies reporting to a county, county level percent male, African American, white, ages 0-19, 20-29, 30-39, 40-49, and 50-64, as well as state level police per capita, state level housing price index, the share of individuals with a college degree, an indicator for whether the governor is democrat, whether drug convicts are eligible for TANF benefits, whether parolees are eligible to vote, and the presence of a state EITC top-up. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population of the specified age group. Standard errors are clustered at the state level.

Appendix Figure 1. Event Study Analysis of Estimated Relationship between Large Minimum Wage Increases and Crime Arrest Rates and Labor Market Outcomes for those ages 16-to-24, UCR and CPS, 1998-2016

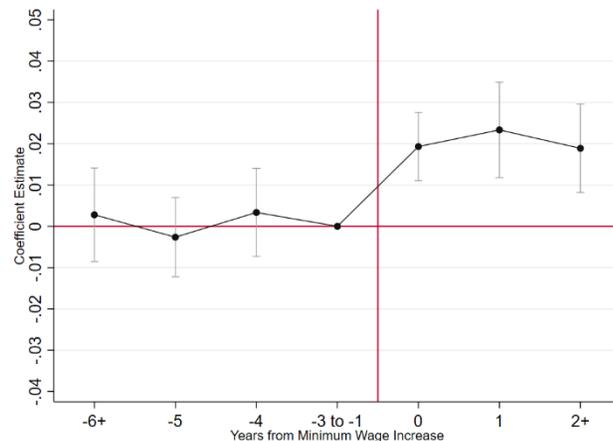
Panel (a): Arrests for Property Offenses



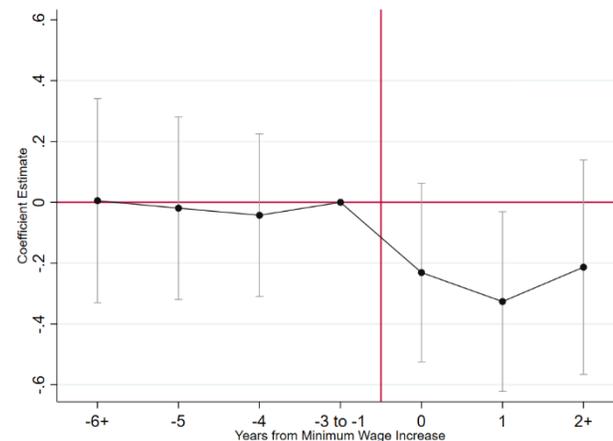
Panel (b): Arrests for Violent Offenses



Panel (c): Ln(Hourly Wages / Employment)



Panel (d): Usual Weekly Hours

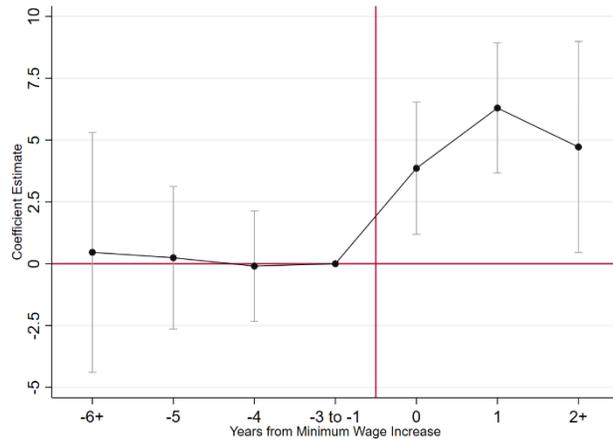


Notes: Event study coefficients plotted from the regression described in footnote 26. Error bars are 95 percent confidence intervals. Following Borusyak and Jaravel (2017), we use a multiperiod reference period of 1 to 3 years prior to enactment given that the model includes jurisdiction-specific time trends. Events are composed of dichotomous indicators for “large” minimum wage increases (in the top 25th percentile of the percentage increases of real minimum wages)

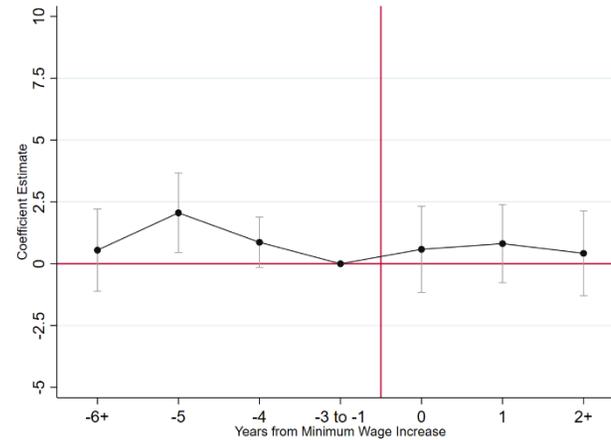
that occurred j years from the date of the minimum wage increase. Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports for panels (a) and (b) and the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG) for panels (c) and (d). Regressions in panels (a) and (b) control for county fixed effects, year fixed effects, state-specific linear and quadratic time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Regressions in panels (c) and (d) include controls for age fixed effects, state fixed effects, year fixed effects, month fixed effects, state-specific linear and quadratic time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. For panels (a) and (b), samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Additionally, estimates from panels (a) and (b) are weighted by the county population ages 16-to-24, with standard errors clustered at the state level. Estimates from panels (c) and (d) are weighted using the sample weights provided by the CPS, with standard errors are clustered at the state level.

Appendix Figure 2. Robustness of Event Study Analysis to Use of State-Specific Linear Time Trends

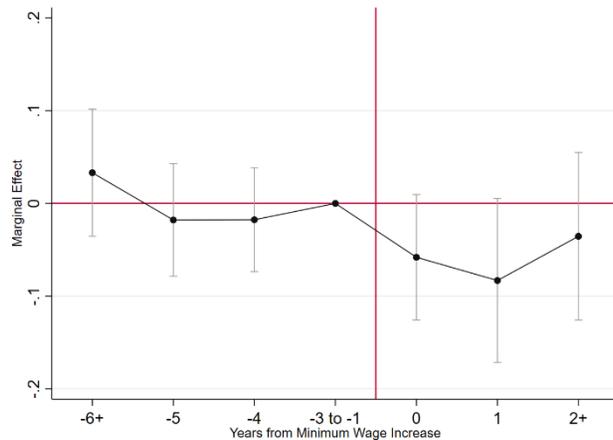
Panel (a): Arrests for Property Offenses



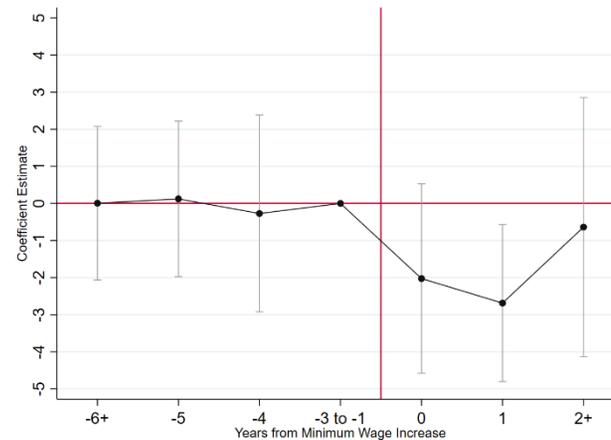
Panel (b): Arrests for Violent Offenses



Panel (c): Employment



Panel (d): Usual Weekly Hours / Employment

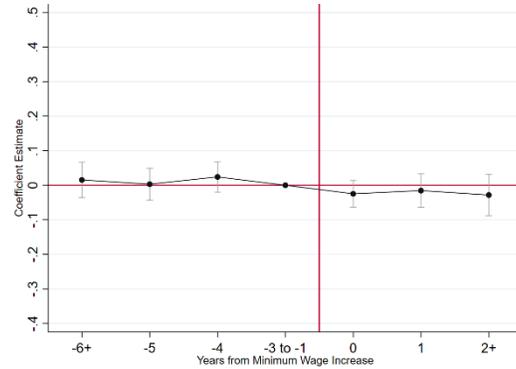


Notes: Event study coefficients plotted from the regression outlined in equation (2). Error bars are 95 percent confidence intervals. Following Borusyak and Jaravel (2017), we use a multiperiod reference period of 1 to 3 years prior to enactment given that the model includes jurisdiction-specific time trends. Estimates are generated using data from the 1998-2016 Uniform Crime Reports for panels (a) and (b) and the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG) for panels (c) and (d). Regressions in panels (a) and (b) control for county fixed effects, year fixed effects, state-specific linear time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1).

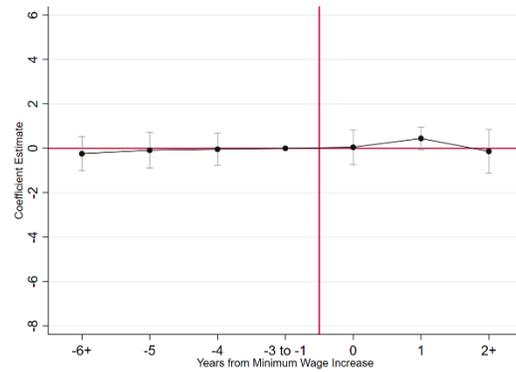
Regressions in panels (c) and (d) include controls for age fixed effects, state fixed effects, year fixed effects, month fixed effects, state-specific linear time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. For panels (a) and (b), samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Additionally, estimates from panels (a) and (b) are estimated via OLS and weighted by the county population ages 16-to-24, with standard errors clustered at the state level. Estimates from panels (c) and (d) are weighted using the sample weights provided by the CPS, with standard errors are clustered at the state level. Panel (c) is estimated via probit and panel (d) is estimated via OLS.

Appendix Figure 3. Event Study Analysis of Estimated Relationship between Minimum Wages and Labor Market Outcomes for those ages 25-to-64 with at least a College Degree, CPS, 1998-2016

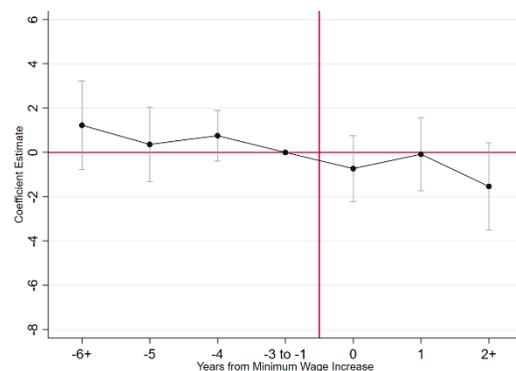
Panel (a): Ln(Hourly Wages | Employment)



Panel (b): Usual Weekly Hours | Employment



Panel (c): Usual Weekly Hours



Notes: Event study coefficients plotted from the regression outlined in equation (2) (modified for CPS analysis). Following Borusyak and Jaravel (2017), we use a multiperiod reference period of 1 to 3 years prior to enactment given that the model includes jurisdiction-specific time trends. Weighted OLS estimates are generated using data drawn from the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG). All regressions include age fixed effects, state fixed effects, year fixed effects, month fixed effects, state-specific linear and quadratic time trends, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). For the economic controls, the prime-age male unemployment rate and prime-age wage rate are replaced with the state-level housing price index. Estimates are weighted using the sample weights provided by the CPS, and standard errors are clustered at the state level.

**Appendix Table 1A. Descriptive Statistics of Crime Arrest Rates by Age and Gender,
UCR, 1998-2016**

	(1)	(2)
	<i>Property Crime Arrest Rates</i>	<i>Violent Crime Arrest Rates</i>
Ages 16-19	20.15 (13.06) [44,203]	5.11 (7.29) [44,157]
Ages 20-24	12.25 (7.86) [44,325]	4.97 (5.97) [44,117]
Ages 25-34	7.87 (6.47) [44,391]	3.53 (4.78) [44,187]
Ages 35-49	4.72 (4.26) [44,290]	2.04 (2.69) [44,194]
Ages 50+	0.96 (0.79) [44,240]	0.42 (0.51) [44,001]
Ages 16-64	6.69 (4.52) [44,526]	2.56 (3.35) [44,357]
<u>Ages 16-24</u>		
Males	20.49 (13.35) [44,200]	8.18 (10.65) [44,231]
Females	10.70 (7.10) [44,201]	1.74 (2.30) [43,932]

Notes: Weighted means and standard deviations are generated using data from the 1998-2016 Uniform Crime Reports. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Standard deviations are in parentheses and number of observations are in brackets. Means and standard deviations are weighted using the county population of specified age (and gender) group.

Appendix Table 1B. Descriptive Statistics of Crime Arrest Rates for those ages 16-to-24 by Type of Crime, UCR, 1998-2016

<i>Panel I: Specific Property Crimes</i>				
(1)	(2)	(3)	(4)	
<i>Burglary</i>	<i>Larceny</i>	<i>Motor Vehicle Theft</i>	<i>Arson</i>	
3.047 (2.20)	11.485 (7.45)	1.124 (2.01)	0.081 (0.145)	
[44,023]	[44,272]	[43,826]	[43,708]	
<i>Panel II: Specific Violent Crimes</i>				
(5)	(6)	(7)	(8)	
<i>Aggravated Assault</i>	<i>Robbery</i>	<i>Homicide</i>	<i>Rape</i>	
3.266 (3.59)	1.401 (2.69)	0.141 (0.26)	0.189 (0.26)	
[44,161]	[44,002]	[43,839]	[43,803]	
<i>Panel III: Minor Crimes</i>				
(9)	(10)	(11)	(12)	(13)
<i>Vandalism</i>	<i>Liquor Law Violations</i>	<i>Drunkenness</i>	<i>Disorderly Conduct</i>	<i>Drug</i>
2.60 (2.20)	9.12 (13.69)	3.43 (5.40)	5.48 (7.53)	16.40 (16.12)
[44,019]	[44,105]	[44,495]	[44,183]	[44,397]

Notes: Weighted means and standard deviations are generated using data from the 1998-2016 Uniform Crime Reports. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Standard deviations are in parentheses and number of observation in brackets. Means and standard deviations are weighted by the county population ages 16-to-24.

Appendix Table 2. Descriptive Statistics of Selected Variables, NLSY, 1998-2016

	(1)	(2)	(3)
	All	Ages 16- to-24	Ages 25+
<i><u>Crime and Arrest</u></i>			
Any Crime	0.10 (0.31)	0.14 (0.35)	0.04 (0.20)
Property Crime	0.06 (0.24)	0.08 (0.28)	0.02 (0.14)
Sold Drugs	0.04 (0.20)	0.05 (0.22)	0.02 (0.15)
Assault	0.04 (0.20)	0.05 (0.22)	0.02 (0.13)
Arrest	0.04 (0.20)	0.05 (0.22)	0.03 (0.17)
<i><u>Labor Market Outcomes</u></i>			
Hours Per Week	33.79 (13.27)	30.68 (13.22)	37.30 (12.42)
Total Weeks Worked	45.02 (27.22)	36.66 (18.83)	54.65 (31.81)
Total Hours Worked	1,670.28 (1,234.72)	1,213.82 (773.97)	2,186.00 (1,438.35)
Hourly Wage	17.76 (412.15)	13.45 (236.36)	22.65 (546.86)
Employed	0.94 (0.25)	0.93 (0.25)	0.94 (0.24)
<i><u>Individual Characteristics</u></i>			
Minimum Wage Bound	0.029 (0.165)	0.036 (0.183)	0.018 (0.135)
Non-Black, non-Hispanic	0.52 (0.50)	0.54 (0.50)	0.51 (0.50)
Maternal Education	12.50 (2.92)	12.54 (2.90)	12.46 (2.94)
Math PIAT Score	97.64 (19.27)	98.33 (19.08)	96.72 (19.47)
Household Income 1997	46,836.23 (41,169.69)	47,427.43 (41,333.50)	46,142.21 (40,966.52)
N	71,702	38,376	33,326

Notes: Weighted means and standard deviations for selected variables from the 1998-2016 National Longitudinal Study of Youth 1997 (NLSY97). Reported observations correspond to maximum sample size. Because of missing observations, the sample size is smaller for some variables.

Appendix Table 3. Coefficient Estimates for Controls Variables from Table 2, Column 4

	(1)	(2)
	<i>Property Crime Arrest Rates</i>	<i>Violent Crime Arrest Rates</i>
Number of reporting agencies	0.057 (0.125)	-0.108 (0.079)
Shares of males	20.946 (39.216)	13.054 (24.602)
Shares of African American	17.459* (9.985)	14.371* (7.332)
Shares of Hispanic	-12.761 (7.999)	5.624 (7.405)
Shares of individuals ages 25+ with a BA degree	11.249 (9.885)	-6.010 (3.640)
Shall issue laws	-0.341 (0.772)	0.528** (0.213)
Ln(police expenditures per capita)	1.181 (2.488)	1.155 (0.893)
Ln(police employment per capita)	1.074 (1.632)	-1.095** (0.480)
Ln(prime-age hourly wages)	-6.083 (5.972)	2.811 (2.516)
Ln(prime-age male unemployment rates)	0.763 (0.588)	-0.259 (0.187)
State refundable EITC	0.754 (0.612)	-0.348* (0.187)
Presence of Medicaid for childless adults	-0.288 (0.473)	-0.187 (0.413)
SNAP all-vehicles exemption	-0.046 (0.366)	-0.168 (0.151)
Minimum dropout age of 18+	0.096 (0.795)	0.478** (0.208)
E-verify	0.516 (0.545)	0.038 (0.148)
Ban the box laws	0.393 (0.546)	-0.430* (0.229)
Marijuana decriminalization	-1.860*** (0.630)	-0.521 (0.333)
Medical marijuana laws	-0.022 (0.660)	0.273 (0.301)
Ln(beer taxes)	0.768 (0.515)	0.375*** (0.124)
N	44,259	44,203

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Appendix Table 4. Sensitivity of Estimates to the Inclusion of Controls for State/County-Specific Time Trends and Census Region-Specific Year Fixed Effects, UCR, 1998-2016

	(1)	(2)	(3)	(4)
<i>Panel I: Property Crime Arrest Rates</i>				
Ln(MW)	3.300** (1.589)	2.906* (1.477)	4.899*** (1.423)	2.612** (1.088)
<i>Elasticity</i>	0.208	0.184	0.310	0.165
N	44,259	44,259	44,259	44,259
<i>Panel II: Violent Crime Arrest Rates</i>				
Ln(MW)	1.721 (1.179)	0.498 (0.566)	-0.330 (0.241)	0.019 (0.272)
<i>Elasticity</i>	0.342	0.099	-0.066	0.004
N	44,203	44,203	44,203	44,203
State-Specific Linear Trends	Yes	No	No	No
State-Specific Quadratic Trends	Yes	No	No	No
State-Specific 3rd-order Trends	Yes	No	No	No
State-Specific 4th-order Trends	Yes	No	No	No
Census Region-Specific Year FE	No	Yes	No	Yes
County-Specific Linear Trends	No	No	Yes	Yes

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Appendix Table 5. Heterogeneity in Relationship between Minimum Wages and Property Crime Arrest Rates by Age Group, UCR, 1998-2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ages							
	16-to-19	20-to-24	25-to-29	30-to-34	35-to-44	45-to-54	55-to-64	16-to-64
<i>Panel I: Baseline Model</i>								
Ln(MW)	3.261*	3.310**	0.678	0.215	-0.943	-0.683*	-0.090	0.564
	(1.904)	(1.619)	(1.142)	(1.068)	(0.898)	(0.365)	(0.106)	(0.772)
Elasticity	0.162	0.270	0.078	0.031	-0.177	-0.235	-0.089	0.084
N	44,203	44,325	44,310	44,172	44,277	44,248	44,052	44,526
<i>Panel II: With State-Specific Quadratic Time Trends</i>								
Ln(MW)	5.541***	4.766***	1.252*	0.824	0.077	0.077	0.135	1.048**
	(1.182)	(1.275)	(0.640)	(0.660)	(0.532)	(0.225)	(0.082)	(0.467)
Elasticity	0.275	0.389	0.145	0.117	0.014	0.026	0.134	0.157
N	44,203	44,325	44,310	44,172	44,277	44,248	44,052	44,526
<i>Panel III: With State-Specific 4th-order Time Trends</i>								
Ln(MW)	3.708	3.147**	1.328	0.837	0.117	-0.044	0.074	1.111
	(2.243)	(1.314)	(1.286)	(1.219)	(0.909)	(0.328)	(0.101)	(0.803)
Elasticity	0.184	0.257	0.154	0.119	0.022	-0.015	0.074	0.166
N	44,203	44,325	44,310	44,172	44,277	44,248	44,052	44,526

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people of the specified age group. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. Regressions in columns (3)-(8) do not control for prime-age male unemployment rates, prime-age wage rates, or the share of individuals with a college degree to avoid controlling for mechanisms; instead, they control for the state-level housing price index. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population of the specified age group. Standard errors are clustered at the state level.

Appendix Table 6. Separating State-Specific Legislative Minimum Wage Changes from State Changes Due to Federal Minimum Wage Increases (Bound), 1998-2016

	<i>Property Crime Arrest Rates</i>	<i>Violent Crime Arrest Rates</i>
Ln(MW)	4.752** (1.798)	0.666 (0.931)
<i>Elasticity</i>	0.300	0.132
Ln(MW)*Bound	1.454 (3.741)	-0.842 (1.130)
<i>Elasticity</i>	0.092	-0.167
Bound	-1.424 (7.266)	1.665 (2.290)
N	44,259	44,203

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Appendix Table 7. Estimated Relationship between Living Wages and Crime Arrest Rates for those ages 16-to-24, UCR, 1998-2016

	(1)	(2)
	<i>Property Crime Arrest Rates</i>	<i>Violent Crime Arrest Rates</i>
Ln(MW)	3.307** (1.633)	0.481 (0.754)
<i>Elasticity</i>	<i>0.209</i>	<i>0.096</i>
Living Wage	-0.930 (0.727)	0.502** (0.244)
<i>%Δ associated with law</i>	<i>-0.059</i>	<i>0.100</i>
Living Wage*Financial Assistance	1.441* (0.777)	0.197 (0.422)
<i>%Δ associated with law</i>	<i>0.091</i>	<i>0.039</i>
N	44,259	44,203

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 1998-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people ages 16-to-24. All regressions include controls for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population ages 16-to-24. Standard errors are clustered at the state level.

Appendix Table 8. Descriptive Statistics of Labor Market Outcomes and School Enrollment for those ages 16-to-24 without a High School Diploma, CPS, 1998-2016

<i>Panel I: Labor Market Outcomes</i>	
<i>Hourly Wages (2016\$) Employment</i>	9.252 (3.432) [78,071]
<i>Employment</i>	0.272 (0.445) [293,216]
<i>Usual Weekly Hours Employment</i>	25.423 (13.217) [83,731]
<i>Usual Weekly Hours</i>	6.905 (13.241) [293,216]
<i>Usual Weekly Earnings (2016\$)</i>	69.149 (153.96) [293,216]
<i>Panel II: School Enrollment</i>	
<i>School Enrollment</i>	0.763 (0.425) [95,674]

Notes: Weighted means and standard deviations in Panel I are generated using data from the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG). Weighted means and standard deviations in Panel II are generated using data from the 1998-2016 CPS October Supplement. Standard deviations are in parentheses and number of observations in brackets. Means and standard deviations are weighted using sample weights provided by the CPS.

Appendix Table 9. Robustness of Estimates to Controlling for State Housing Price Index for those ages 16-to-24, UCR and CPS, 1998-2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Property Crime Arrest Rates</i>	<i>Violent Crime Arrest Rates</i>	<i>Ln(Hourly Wages / Employment)</i>	<i>Employment</i>	<i>Usual Weekly Hours</i>	<i>Usual Weekly Hours / Employment</i>	<i>Usual Weekly Earnings</i>	<i>School Enrollment</i>
Ln(MW)	3.505** (1.702)	0.517 (0.855)	0.157*** (0.025)	-0.092*** (0.031)	-2.796*** (0.865)	-2.654** (1.129)	-17.404* (9.995)	-0.017 (0.035)
<i>Elasticity</i>	<i>0.221</i>	<i>0.103</i>	<i>0.157</i>	<i>-0.341</i>	<i>-0.405</i>	<i>-0.104</i>	<i>-0.252</i>	<i>-0.023</i>
N	44,259	44,203	78,071	293,216	293,216	83,731	293,216	95,674

Notes: Weighted estimates are generated using data from the 1998-2016 Uniform Crime Reports for columns (1) and (2), the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG) for columns (3) through (7), and the 1998-2016 CPS October Supplement for column (8). Regressions in columns (1) and (2) control for county fixed effects, year fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. Regressions in columns (3) through (8) include controls for age fixed effects, state fixed effects, year fixed effects, month fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls. For the economic controls, the prime-age male unemployment rate and prime-age wage rate are replaced with the state-level housing price index. Columns (1)-(3) and (5)-(7) are estimated via OLS and columns (4) and (8) are estimates via probit. For columns (1) and (2), samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Additionally, estimates from columns (1) and (2) are weighted by the county population ages 16-to-24, with standard errors clustered at the state level. Estimates from columns (3) through (8) are weighted using the sample weights provided by the CPS, with standard errors are clustered at the state level.

Appendix Table 10. Event Study Analysis of Estimated Relationship between Minimum Wages and Labor Market Outcomes and School Enrollment for those ages 16-to-24 without a High School Diploma, CPS, 1998-2016

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Ln(Hourly Wages / Employment)</i>	<i>Employment</i>	<i>Usual Weekly Hours / Employment</i>	<i>Usual Weekly Hours</i>	<i>Usual Weekly Earnings</i>	<i>School Enrollment</i>
6+ Years Prior	0.031 (0.036)	0.014 (0.037)	-0.505 (0.931)	-0.047 (1.121)	-4.156 (12.087)	0.107** (0.054)
5 Years Prior	0.030 (0.041)	-0.022 (0.034)	0.050 (1.102)	-0.654 (0.976)	-10.076 (11.949)	0.026 (0.057)
4 Years Prior	0.050 (0.032)	-0.023 (0.030)	-0.361 (1.221)	-0.319 (0.861)	1.775 (10.446)	-0.042 (0.042)
1 to 3 Years Prior	-	-	-	-	-	-
Year of	0.156*** (0.027)	-0.057 (0.035)	-1.805 (1.161)	-2.074** (0.884)	-9.035 (10.484)	-0.010 (0.055)
1 Year After	0.225*** (0.038)	-0.082** (0.040)	-2.405** (1.019)	-2.626*** (0.891)	-6.189 (10.622)	-0.049 (0.064)
2+ Years After	0.211*** (0.042)	-0.059* (0.035)	-1.245 (1.417)	-1.281 (1.093)	0.231 (13.923)	-0.120** (0.049)
N	78,071	293,216	83,731	293,216	293,216	95,674

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Data are drawn from the 1998-2016 Current Population Survey (CPS) Outgoing Rotation Groups (ORG) for columns (1)-(5) and the CPS October Supplement for column (6). All regressions include age fixed effects, state fixed effects, year fixed effects, month fixed effects, socio-demographic controls, economic controls, crime policy controls, and social welfare & health controls (available in Table 1). See discussion of equation (2) for construction of the minimum wage events. Columns (1), (3), (4), and (5) are estimated via OLS; columns (2) and (6) are estimated via probit. Estimates are weighted using the sample weights provided by the CPS, and standard errors are clustered at the state level.

Appendix Table 11. Sensitivity of Estimated Relationship between Minimum Wages and Crime Arrest Rates to Years included in the Sample, 16-to-24 year-olds, UCR, 2000-2016

	(1)	(2)
<i>Panel I: Property Crime Arrest Rates</i>		
MW	0.412	0.546***
	(0.252)	(0.201)
<i>Elasticity</i>	0.194	0.269
N	35,296	40,347
<i>Panel II: Violent Crime Arrest Rates</i>		
MW	0.125	0.112
	(0.081)	(0.072)
<i>Elasticity</i>	0.189	0.176
N	35,250	40,295
Years	2000-2014	2000-2016
States	UCR States	UCR States

***Significant at 1% level **Significant at 5% level *Significant at 10% level

Notes: Weighted OLS estimates are generated using data from the 2000-2016 Uniform Crime Reports. Arrest rates are arrests per 1,000 people of the specified age group. All regressions include controls for county fixed effects, year fixed effects, the number of agencies reporting to a county, county level percent male, African American, white, ages 0-19, 20-29, 30-39, 40-49, and 50-64, as well as state level police per capita, state level housing price index, the share of individuals with a college degree, an indicator for whether the governor is democrat, whether drug convicts are eligible for TANF benefits, whether parolees are eligible to vote, and the presence of a state EITC top-up. Samples are restricted to counties with crime arrest rates within two standard deviations of the county's average crime arrest rate, and counties with a coverage indicator of at least 65%. Estimates are weighted by the county population of the specified age group. Standard errors are clustered at the state level.