When the Economy Slows, So Does Crime?

The Great Recession's Lasting Effects on Crime*

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March 24, 2026

Abstract

We analyze the Great Recession's lasting impact on U.S. crime rates. Exploiting spatial variation in recession severity across commuting zones, we find that a one percentage point (pp) increase in the local unemployment rate during 2007–2009 led to a decline of 0.60 reported offenses per 1,000 individuals, with effects persisting through 2016. At the mean unemployment shock of 4.6 pp, this implies a 5.1 percent reduction in reported offense rates. Evidence on mechanisms points to a wealth-based channel: the collapse in household wealth reduced returns to property crime, as indicated by offense-type heterogeneity and mediation analysis using bankruptcy rates and housing prices. The effects are heterogeneous by socioeconomic status: areas with larger shares of high-SES residents experienced crime reductions, while low-SES areas experienced crime increases, reinforcing the regressivity of the recession's welfare consequences. Using a border-county-pair design, we estimate that a 10-week increase in maximum UI benefit duration raised reported offense rates by approximately 3 percent, consistent with the wealth-based mechanism. Incorporating crime into a sufficient statistics welfare framework reduces the estimated welfare cost of the Great Recession by 6 percent.

Keywords: Great Recession, crime, unemployment insurance
JEL Codes: E24, J63, K42

*We thank Ryan Kim, Ming-Jen Lin, Bruce Weinberg, and Ning Zhang for helpful comments. We additionally thank seminar participants at Korea University and Singapore Management University. All errors are our own.

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1 Introduction

The Great Recession produced the sharpest rise in unemployment since the Great Depression. The national unemployment rate doubled from 5 percent to 10 percent by October 2009, housing prices fell by approximately 30 percent, and the stock market declined by nearly 60 percent. Standard models of criminal behavior predict that such a severe labor market deterioration should increase crime by lowering the opportunity cost of criminal activity (Becker, 1968; Ehrlich, 1973). Yet during this period, national crime rates continued to fall rather than reversing course (Figure A1), a pattern that the literature has left largely unexplained (Rosenfeld, 2014).

Understanding how recessions affect crime has direct implications for the welfare costs of economic downturns and the design of stabilization policies. Lucas (1987, 2003) argued that consumption fluctuations yield trivial welfare losses in representative-agent models, while Krebs (2007) demonstrates that persistent earnings declines generate large welfare costs. Yagan (2019) empirically corroborates the latter by documenting persistent effects of the Great Recession on employment, while Finkelstein et al. (2025) show persistent mortality-reducing effects of the same episode. Crime, however, has received little attention in this literature despite imposing an estimated annual social cost of $2.6 trillion, approximately 13 percent of U.S. GDP, in 2017 (Miller et al., 2021).

The Great Recession was not a typical recession. Whereas conventional downturns are often driven by supply-side shocks or tightened credit (e.g., the early 1980s and 1990s recessions; Reinhart and Rogoff (2009)), the Great Recession was fundamentally a balance-sheet recession driven by the collapse of housing wealth (Mian and Sufi, 2014b,a). This distinction is critical for understanding crime impacts of economic downturns. Canonical economic models emphasize earnings losses as the key mechanism: higher unemployment lowers earnings, lowering the opportunity cost of crime. The wealth-based channel operates differently. When household wealth collapses, the value of criminal targets falls alongside it, reducing returns to property crime (Cohen and Felson, 1979; Draca et al., 2019; Wright et al., 2017). In a balance-sheet recession, these two channels work in opposing directions, and their relative magnitudes are ultimately empirical questions. The Great Recession, precisely because of its character as a housing wealth shock, provides the right setting for identifying which channel dominates.

In our first set of empirical analyses, we estimate the lasting effects of the Great Recession on crime, leveraging spatial variation in recession severity during 2007–2009 across commuting zones (CZs). We employ a dynamic difference-in-differences (DID) design following Yagan (2019) and Finkelstein et al. (2025). We measure crime rates as the number of reported offenses per 1,000 individuals using data from the Federal Bureau of Investigation (FBI) Uniform Crime Reporting (UCR) Program.

We document that CZs more severely affected by the Great Recession experienced persistently larger crime reductions than less affected areas. A one percentage point increase in the CZ-level unemployment rate between 2007 and 2009 led to a decline in reported offense rates of 0.604 per 1,000 individuals, representing a 1.1 percent decline from the 2006 mean. These effects persisted through 2016, well beyond the official end of the recession. At the average unemployment shock

of 4.6 percentage points, the recession reduced reported offenses by 2.77 per 1,000 individuals per year during 2007–2016, a 5.1 percent decline relative to the 2006 mean. The pattern is consistent across the criminal justice pipeline: the Great Recession also reduced arrest rates and, less precisely, incarceration rates.

These findings are inconsistent with the opportunity-cost channel and with a broad prior literature documenting that adverse economic conditions raise criminal activity (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008; Britto et al., 2022). The wealth-based channel provides a more coherent explanation. If the Great Recession reduced crime primarily by contracting household wealth and lowering the value of criminal targets, the effect should be concentrated in offenses whose returns depend directly on target values. Consistent with this prediction, we find that the baseline results are driven almost entirely by burglary, motor vehicle theft, and robbery, which together account for 81 percent of the overall crime reduction. Mediation analyses using bankruptcy rates and housing prices as proxies for household wealth suggest these channels explain most of the estimated crime effect. By contrast, we find no evidence that the recession increased time spent at home or reduced mobility, inconsistent with guardianship-based explanations. We also show, using demographic-specific unemployment shocks, that crime reductions are larger when the shock falls on groups with greater wealth exposure (White and older individuals) rather than groups with higher baseline criminal activity, further discriminating between the two channels. We further show that our results remain robust when employing the county-level housing net worth shock constructed by Mian and Sufi (2014b), which directly proxies for the deterioration of local household balance sheets associated with the housing bust, as an alternative measure of recession severity.

We then examine heterogeneous effects by socioeconomic status. The adverse impacts of recessions fall disproportionately on low-SES households (Hoynes et al., 2012; Krueger et al., 2016), and the crime response reflects this asymmetry. In CZs with larger shares of high-education or high-income residents, the Great Recession led to significant and persistent crime declines. In CZs with larger shares of low-SES residents, the recession increased crime. This pattern is consistent with the interplay of two channels: the wealth-based mechanism dominates in high-SES areas where homeownership and asset exposure are greater, while the opportunity-cost channel dominates in low-SES areas where labor market deterioration is more severe and wealth shocks are smaller. Once crime responses are incorporated, the welfare consequences of the Great Recession are more regressive than consumption-only analyses suggest.

In our second set of analyses, we estimate the effects of UI benefit extensions on crime. In response to the recession's severity, the federal government expanded maximum UI benefit duration from the standard 26 weeks to as many as 99 weeks. Because UI benefits partially preserve household income and consumption, they also partially preserve the value of criminal targets that the recession would otherwise have diminished (e.g., motor vehicle theft). The wealth-based mechanism therefore implies that UI extensions would increase crime. Identifying this effect is complicated by the fact that benefit extensions correlate with state-level recession severity. We address this by employing a border-county-pair (BCP) design following Boone et al. (2021), which compares ad-

jacent counties straddling state borders that share local economic conditions but face different UI policies due to state-level jurisdiction. A 10-week increase in maximum UI benefit duration raised reported offense rates by 3.1 percent, concentrated in property crimes consistent with the target-value mechanism.

In our third set of analyses, we quantify the welfare implications of these crime effects using sufficient statistics approaches. We augment Krebs's (2007) framework by incorporating crime as a nonmarket welfare channel alongside consumption losses. The crime-reducing effects of the Great Recession decrease the estimated welfare cost by 6 percent relative to consumption-only estimates. The distributional analysis confirms that incorporating crime reinforces the regressivity of the recession's welfare consequences, contrasting with Finkelstein et al. (2025), who find that incorporating mortality reductions mitigates regressivity. For UI policy, we benchmark the crime externality against the consumption-smoothing gains estimated by Ganong and Noel (2019) and find that the crime cost is quantitatively non-trivial, suggesting that conventional welfare evaluations of UI extensions may overstate net benefits by omitting this channel.

This study makes three contributions. First, we contribute to the literature on economic conditions and crime. Existing studies find that adverse economic conditions generally increase criminal activity, particularly property crimes (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008; Fougère et al., 2009; Britto et al., 2022), focusing primarily on contemporaneous relationships and individual-level labor market shocks. Our study documents a novel exception: a balance-sheet recession can reduce crime through the target-value channel. Combined with the findings of our heterogeneity analysis by SES, this study provides novel insights into how opportunity cost or wealth contraction determines the direction of the crime response.

Second, we contribute to the literature on UI benefits. Prior work evaluates UI primarily through consumption smoothing and employment effects (Gruber, 1997; Lalive, 2007; Chetty, 2008; Ganong and Noel, 2019; Boone et al., 2021; Acosta et al., 2023; Jessen et al., 2025). Using the BCP design, we provide the first evidence that UI benefit extensions during the Great Recession increased crime, identifying a previously undocumented externality whose direction and mechanism follow directly from the wealth-based channel documented in our first contribution.

Third, we contribute to the literature on the welfare costs of recessions. This study extends the previous literature by considering crime as another important nonmarket outcome for the welfare evaluation of economic downturns. On the distributional implications, incorporating crime effects reinforces the regressivity of the recession's welfare consequences, while Finkelstein et al. (2025) find that incorporating mortality mitigates it. Our results on UI further imply that the crime externality may substantially offset the policy's conventional welfare gains.

The remainder of the paper is organized as follows. Section 2 describes our data sources and presents descriptive features of crime patterns. Section 3 outlines our empirical strategy. Section 4 presents main results and mechanism analysis. Section 5 presents the effects of UI benefit extensions on crime. Section 6 discusses welfare implications. Section 7 concludes.

2 Data

2.1 Data Sources

Our empirical analysis uses multiple datasets aggregated to the CZ level. We analyze 741 CZs representing distinct local labor markets in the United States. We construct CZ-level panel data spanning 2003 to 2016 to avoid potential contamination from the early 2000s recession, following the literature (Yagan, 2019; Finkelstein et al., 2025).

To capture local labor market conditions, we follow Yagan (2019). We obtain unemployment data from the Bureau of Labor Statistics (BLS) and construct CZ-level measures as population-weighted averages of county-level unemployment rates. We measure recession severity as the change in unemployment rates between 2007 and 2009.[1]

Our crime data come from the FBI's UCR Program, which provides comprehensive nationwide information on criminal offenses, arrests, and police staffing. We use reported offenses as our primary outcome, following the economics of crime literature (Donohue and Levitt, 2001; Gould et al., 2002; Lin, 2008). Property crimes include burglary, larceny-theft, motor vehicle theft, and arson. Violent crimes include murder and nonnegligent manslaughter, rape, robbery, and aggravated assault. The UCR data allow analysis by offense subcategory. We construct crime rates as the number of reported offenses per 1,000 individuals aged 15–65 in each CZ, using population data from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program.

To examine the criminal justice pipeline beyond reported offenses, we use the UCR data on arrests. According to the UCR Program, an arrest is counted for each separate instance in which a person is arrested, cited, or summoned for an offense. We construct arrest rates per 1,000 individuals aged 15–65. The arrest data provide information on offenders' age, race, and gender, which is not available in the reported offense data, enabling heterogeneity analysis by these characteristics.[2] We also incorporate incarceration data from the Bureau of Justice Statistics, available only at the state level, measuring the number of inmates in prison or jail per 1,000 individuals aged 15–65 (Lochner and Moretti, 2004).[3]

A potential concern with UCR data is that they rely on voluntary reporting of offenses and arrests by law enforcement agencies within counties (Maltz, 2006). Figure A3 shows that the Great Recession did not systematically affect the probability of agency non-reporting, suggesting that differential non-reporting does not bias our estimates. As a robustness check, we re-estimate the main specification restricting the sample to offenses from law enforcement agencies that reported

[1] As a robustness check, we repeat the analysis at the state level. The results reported in Figure A2 remain robust.

[2] As the UCR provides offenders' granular demographic information only for arrests, some studies use arrest rates as their primary crime measure (Bell et al., 2018, 2022). However, arrests reflect both criminal activity and law enforcement response, potentially influenced by police practices and resources unrelated to actual crime incidence. We therefore use reported offenses as our main outcome and consider arrests as a supplementary measure.

[3] We use the jurisdiction count including all state and federal inmates held in public or private prisons, those in jail facilities, and those temporarily out to court or in transit between jurisdictions. 87 percent of all inmates who violate state law are sent to that state's department of corrections as of 2022 (Carson and Kluckow, 2023).

for all 12 months in every year and find similar results (Figure A4).[4]

2.2 Descriptive Statistics

Figure A1 presents longer-term trends of crime rates since 1970. Total and property crime rates rose steadily throughout the 1970s and 1980s, peaking in the early 1990s. Beginning in the mid-1990s, crime rates declined sharply and persistently, a period known as the long-term "Great American Crime Decline" (Zimring, 2007; Tcherni-Buzzeo, 2019). While this downward trend slowed in the early 2000s, the decline resumed in the mid-2000s and continued through the 2010s. Violent crime followed a similar pattern, albeit at a lower level. Figure A5 shows that victimization rates from the National Crime Victimization Survey also declined during 2003–2016, suggesting that the observed drop in reported offenses reflects genuine crime reductions rather than changes in reporting or policing practices.[5]

Table A1 presents summary statistics for key variables at the CZ level for the year 2006, the pre-recession baseline. The mean total crime rate was 54.2 per 1,000 individuals. By category, property crimes (47.4) were almost seven times more common than violent crimes (6.84). Arrest rates varied significantly across demographic groups. The mean arrest rate for Black individuals (78.9) was more than twice that for White individuals (32.8), and the mean arrest rate was over three times higher for males (81.6) than for females (24.6). By age, arrest rates were highest among those aged 16 to 19 (112.4), followed by those aged 20 to 29 (91.9).

3 Empirical Strategy

To examine the dynamic effects of the Great Recession on crime, we exploit spatial variation in recession severity across CZs following Yagan (2019) and Finkelstein et al. (2025). The primary dynamic DID specification is:

$$Crime_{c,t} = \sum_{t \neq 2006} [\beta_t \cdot Shock_c \cdot \mathbf{1}(Year = t)] + \alpha_c + \gamma_t + \varepsilon_{c,t} \tag{1}$$

where $Crime_{c,t}$ denotes the crime outcome of interest, such as the number of reported offenses per 1,000 individuals aged 15–65 in CZ c and year t. $Shock_c$ measures the percentage point change in the CZ's unemployment rate between 2007 and 2009, capturing the CZ-level severity of the Great Recession. $\mathbf{1}(Year = t)$ is an indicator for calendar year t. α_c and γ_t are CZ and year fixed effects, respectively. $\varepsilon_{c,t}$ is an error term.

The parameters of interest are the coefficients β_t. We omit 2006 as the reference year. The estimated β_t's thus represent the relationship between a one percentage point increase in the local unemployment rate during the Great Recession and crime rates in year t relative to the pre-recession

[4] A stricter alternative would exclude any CZ whose agencies do not report in every year. This restriction is infeasible because it would reduce the sample to only 25 CZs.

[5] We do not have access to micro-level NCVS data and thus cannot analyze victimization effects using CZ or state level variation in the severity of the Great Recession.

baseline year. Interpreting the estimated coefficients as causal effects of the Great Recession requires that crime rate trends are parallel across CZs in the absence of the Great Recession. Since this counterfactual scenario is unobservable, we indirectly examine this identification assumption by testing whether estimated β_t (for $t < 2007$) are statistically indistinguishable from zero. As we use CZ-year cell-level data, we use the 2006 CZ population aged 15–65 as weights. For statistical inference, we calculate standard errors clustered at the CZ level and corrected for heteroskedasticity.

To summarize the dynamic DID estimates, we also estimate two simplified specifications. The first specification estimates the average effect over the entire post-treatment period:

$$Crime_{c,t} = \beta_1 \cdot Shock_c \cdot Post_t + \alpha_c + \gamma_t + \varepsilon_{c,t}, \tag{2}$$

where all notation follows equation (1), except that $Shock_c$ is interacted with $Post_t$, an indicator equal to 1 in and after 2007. The parameter of interest, β_1, captures the overall impact of the Great Recession.

The second specification distinguishes between the recession period (2007–2009) and the post-recession period (2010–2016):

$$Crime_{c,t} = \beta_1 Shock_c \cdot Rec_t + \beta_2 Shock_c \cdot After_t + \alpha_c + \gamma_t + \varepsilon_{c,t}, \tag{3}$$

where all notation follows equation (2), except that $Post_t$ is split into two indicators: Rec_t and $After_t$. Rec_t is an indicator for the recession period (2007–2009), and $After_t$ is an indicator for the post-recession period (2010–2016). The parameters of interest, β_1 and β_2, capture the effects during and after the recession, respectively.

To quantify the importance of potential mechanisms, we conduct mediation analysis following Finkelstein et al. (2025) and estimate the following equation:

$$Crime_{c,t} = \sum_{t \neq 2006} [\beta_t^s \cdot Shock_c \cdot \mathbf{1}(Year = t)] + \sum_{t \neq 2006} [\beta_t^m \cdot MED_c \cdot \mathbf{1}(Year = t)] + \alpha_c + \gamma_t + \varepsilon_{c,t}, \tag{4}$$

where MED_c represents the change in the mediating variable between 2006 and 2010, capturing persistent effects of the Great Recession through that channel. All other notation follows equation (1). Under two additional identifying assumptions, comparing β_t^s across specifications with and without the mediator provides an estimate of the share of the crime effect attributable to each channel. The first assumption is sequential ignorability: conditional on the treatment and pre-treatment covariates, the mediator is as good as randomly assigned. In our setting, this requires that two CZs experiencing the same unemployment shock but differing in, say, bankruptcy rates do so for reasons unrelated to unobserved determinants of crime. The second is that no unmeasured confounders jointly affect the mediator and the outcome conditional on the treatment. Because both assumptions are strong (the mediators are themselves endogenous responses to the recession and may share unobserved determinants with crime), we interpret the mediation shares as suggestive decompositions rather than as precise causal estimates.

4 Lasting Effects of the Great Recession on Crime

4.1 Main Results

4.1.1 Effects on Reported Offense Rates

Figure 1 presents estimates from equation (1). Panel A demonstrates that CZs experiencing more severe unemployment shocks during the recession (2007–2009) exhibited substantially larger declines in reported offenses (violent and property crimes combined) than less affected areas. These crime reductions persisted through 2016, well beyond the recession's end. The pre-recession coefficient estimates are small and statistically insignificant.[6] During the recession period (2007–2009), total reported offense rates declined by approximately 0.17 per 1,000 individuals for each percentage point increase in the unemployment rate, although the estimates are statistically insignificant. Crime-reducing effects intensified in the post-recession period (2010–2016), averaging 0.79 per 1,000 individuals, approximately 4.7 times as large as during the recession itself. This persistent pattern suggests the Great Recession induced lasting behavioral changes in criminal activity rather than temporary displacement, consistent with Yagan's (2019) findings on employment hysteresis.

The aggregate magnitude of these effects is economically substantial. The average increase in unemployment during the Great Recession was 4.6 percentage points. Multiplying this by our estimated coefficient of -0.604 (2007–2016) implies that the recession reduced crime rates by 2.77 per 1,000 individuals, representing a 5.1 percent decline from the 2006 mean of 54.2. To contextualize this magnitude, the total crime rate in the United States fell by approximately 12 per 1,000 individuals between 2007 and 2016, implying that the Great Recession accounts for roughly 23 percent of the aggregate crime decline during this period.[7] In Figure A7, we show that the average effect on property crime over 2007–2016 is -0.456 per 1,000 individuals, roughly three times the violent crime effect of -0.148.[8]

Previous studies have examined potential causes of the crime reductions during the 1990s (Donohue and Levitt, 2001; Gould et al., 2002). For example, Donohue and Levitt (2001) estimate that legalized abortion explained approximately 50 percent of that decade's crime decline. Although crime rates continued falling during the 2000s and 2010s even amid the Great Recession, the literature has provided limited explanations for these subsequent declines. Our findings suggest that temporary macroeconomic shocks can generate durable shifts in criminal activity.

We also estimate the effects of three earlier recessions: the early 1980s recession (1980–1982), early 1990s recession (1990–1991), and early 2000s recession (2001).[9] Figure A8 shows little evidence

[6] Figure A6 plots the 2006 residualized crime rate against the 2007–2009 change in the unemployment rate across CZs, where residuals are obtained by regressing crime rates on CZ fixed effects over 2003–2006. The small and statistically insignificant slope indicates that CZs experiencing larger unemployment shocks did not exhibit systematically different pre-recession crime trends.

[7] This calculation applies the estimated cross-CZ coefficient to the national average unemployment shock of 4.6 percentage points, assuming a constant marginal effect across the shock distribution. Hence, this estimate should accordingly be interpreted as a rough order of magnitude.

[8] Property offenses include burglary, larceny-theft, motor vehicle theft, and arson. Violent offenses include murder, rape, robbery, and aggravated assault.

[9] The early 1980s recession was triggered primarily by tight monetary policy aimed at curbing inflation. The early

of systematic crime reductions during these recessions, in stark contrast to the patterns documented for the Great Recession. This comparative analysis suggests that the persistent crime decline was specific to the Great Recession. Similarly, Finkelstein et al. (2025) find that the mortality-reducing effects are specific to the Great Recession and do not appear for earlier recessions.

We next examine heterogeneous arrest effects by offenders' demographic characteristics such as gender, race, and age in Figure A9.[10] By gender, the estimated impact is larger for males than for females in magnitude. By race, the recession's impact on arrest rates is most pronounced for Black individuals. By age, the strongest effects appear among individuals in their 30s and 40s. In each case, the demographic groups exhibiting larger arrest rate declines are those with higher baseline criminal involvement (Federal Bureau of Investigation, 2007).

We conduct several robustness checks. First, we examine whether our results are driven by offender migration, encompassing changes in both where offenders commit crimes and where they reside. The Great Recession could have induced offenders to relocate toward CZs with less severe economic shocks, where expected returns to crime were higher. Such migration would overestimate the crime-reducing effects by generating larger crime declines in harder-hit CZs. Since we lack individual-level longitudinal data on offenders, we cannot directly track residential or offending locations. However, offender migration is unlikely to bias our baseline estimates for two reasons. First, the literature emphasizes that offending is highly localized around routine activity spaces such as offenders' homes, implying that long-distance relocation of offending is rare (Wheeler, 2012). Second, we re-estimate our baseline specification using state-year cells, which absorb any within-state migration across CZs given that cross-state migration is far more limited.[11] If our baseline results were driven by offender migration, the estimated crime-reducing effects should attenuate at this higher level of aggregation. Figure A2 shows that the estimated effects remain robust at the state level.[12]

Second, we assess whether our baseline results depend on the linear specification of the unemployment shock. Figure A10 presents binned scatterplots of crime rate changes against recession severity, grouping CZs into twenty equal-sized bins based on the 2007–2009 change in unemployment rates. The plots show a generally monotonic negative relationship between recession severity and crime changes across all periods, with no evidence of nonlinearities that would invalidate our linear specification.

Third, we re-estimate the effects of the Great Recession on reported offense rates using offenses only from agencies that reported for all 12 months in every year. Consistent with our finding

1990s recession was characterized by sharp oil price increases and a mild financial crisis. The early 2000s recession followed the dot-com bubble collapse and the September 11 attacks.

[10]The UCR data do not provide offender demographic characteristics for reported offenses. Since changes in arrest rates primarily reflect changes in reported offense rates, we address this limitation by examining heterogeneous effects of the Great Recession on arrest rates by offenders' demographic characteristics.

[11]We also estimate our baseline specification at the county level to verify that the results are not sensitive to the level of geographic aggregation. The results remain similar.

[12]While assessing different outcomes but using similar identification strategies, Yagan (2019) and Finkelstein et al. (2025) both show that migration did not bias their estimation of the Great Recession's long-term impact on employment and mortality, respectively.

that the non-reporting probability is orthogonal to recession severity documented in Section 2.1, Figure A4 shows that the results remain similar.

4.1.2 Effects Across the Criminal Justice Pipeline

To assess whether the observed reductions in reported offenses reflect genuine declines in criminal activity or changes in law enforcement practices, we examine the effects of the Great Recession on arrests and incarceration. Panel A of Figure 2 shows that arrest rates exhibit patterns similar to reported offense rates in Figure 1. The pre-recession coefficients are small in magnitude and statistically insignificant, supporting the parallel trends assumption. Effects during the recession (-0.045) are modest and imprecise, but intensify substantially in the post-recession period (2010–2016), averaging -0.274 per 1,000 individuals (a 3.2 percent decline from the 2006 mean of 8.45).

To rule out reduced law enforcement capacity as an explanation, Panel A of Figure A11 documents that a one percentage point increase in recession severity is associated with a reduction in sworn police officers of 0.011 per 1,000 individuals over 2007–2016, a statistically and economically insignificant 0.3 percent decline from the 2006 mean. Panel B confirms through mediation analysis that changes in police staffing cannot explain the crime rate declines. These results indicate that declines in reported offenses drove the corresponding declines in arrest rates, rather than changes in enforcement capacity.

Panel B of Figure 2 presents the effects of the Great Recession on incarceration using state-year level data. The point estimates suggest that incarceration rates declined in states more severely affected by the recession, consistent with the patterns observed for reported offenses and arrests. However, the estimates are statistically insignificant, warranting cautious interpretation.

4.2 Mechanisms

Having established that the Great Recession persistently reduced crime, we now investigate the mechanisms underlying this pattern. Our primary analysis focuses on whether the Great Recession reduced crime through declining household wealth, which lowered the value of potential criminal targets, or through increased guardianship as unemployed individuals spent more time at home. We also consider a range of alternative explanations, including community stabilization programs, secular crime trends, changes in incarceration and policing, and UI benefit extensions.

4.2.1 Wealth-Based Mechanisms

To examine the wealth-based mechanism, we first estimate the effects of the Great Recession on crime rates by specific offense type. Consistent with our conjecture that declining target values drive crime reductions, Figure 3 shows that the effects were concentrated in motor vehicle theft, burglary, and robbery. These three offenses account for 81 percent of the total crime reduction documented in Figure 1.

We further investigate this mechanism using proxies for household wealth. The Great Recession was fundamentally a balance-sheet recession driven by collapsing housing wealth rather than traditional supply-side shocks or tightened credit (Mian and Sufi, 2014b,a). We examine whether rising bankruptcy rates and declining housing prices reduced economic returns to property crime.

Panels A and B of Figure 4 show the effects of the Great Recession on bankruptcy rates and the log-transformed Housing Price Index (HPI) using equation (1). Both panels demonstrate that the Great Recession persistently reduced household wealth as measured by these indicators. Panels A and B of Figure A12 present mediation analysis using equation (4). In both cases, controlling for the mediator substantially attenuates the estimated crime effects. Rising bankruptcy rates account for 27.0 percent of the overall crime reduction, while declining housing prices explain more than 100 percent (156 percent), implying that the Great Recession would have increased crime rates in the absence of the wealth-based mechanism. These results indicate that declining housing wealth is likely the primary mechanism underlying the crime-reducing effects of the Great Recession.

As an additional test of the wealth-based mechanism, we re-estimate our baseline specification using demographic group-specific recession severity as the treatment measure, defining the shock as the change in unemployment rates between 2007 and 2009 for each demographic subgroup. We conjecture that, under the opportunity-cost mechanism, labor market shocks among groups with higher baseline criminal involvement, such as younger individuals and minorities, should be most strongly associated with aggregate crime changes. In contrast, under the wealth-based mechanism, unemployment shocks among groups holding greater wealth should matter more because such shocks signal larger contractions in target values. Figure A13 shows that unemployment shocks among White and older individuals are associated with larger crime reductions than shocks among their respective counterparts. These patterns are consistent with the wealth-based mechanism. White households have homeownership rates of 73 percent versus 46 percent for Black households and hold roughly six times greater median net worth (Aladangady et al., 2023). Similarly, median net worth increases substantially with age, as older households accumulate more housing equity and financial assets. Given that White and older individuals have lower baseline crime rates, our results support the interpretation that crime reductions during the Great Recession reflected a decline in potential criminal targets, which lowered the expected returns to property crime.

Lastly, we use an alternative measure of the severity of the Great Recession. If the crime-reducing effect is indeed driven by declining household balance sheets, we should observe a similar pattern when measuring recession severity using housing net worth shocks. We employ the county-level housing net worth shock constructed by Mian and Sufi (2014b), which directly proxies for the deterioration of local household balance sheets associated with the housing bust.[13] This measure combines movements in local house prices with pre-recession leverage and housing exposure, providing a direct test of the wealth-based channel. Panel A of Figure A14 shows that the results remain similar to those in Figure 1: counties experiencing larger negative housing net worth shocks exhibit

[13] We follow Mian and Sufi (2014b) in adopting the county as our primary unit of analysis. Figure A14 shows a robust result when we use the house net worth shock as an alternative treatment.

larger declines in crime. This finding provides further evidence that recession-induced declines in household wealth drove the observed crime reductions.[14]

4.2.2 Guardianship and Routine Activity Mechanisms

The routine activity framework suggests that unemployed individuals may spend more time at home during economic downturns (Aguiar et al., 2013), deterring potential offenders by increasing the presence of capable guardians in residential areas (Cohen and Felson, 1979; Matheson et al., 2024). We examine this alternative mechanism using the ATUS data and road traffic data from the U.S. Department of Transportation.[15]

Panel C of Figure 4 provides little evidence that the Great Recession increased time allocated to home production. The estimated effects are imprecise and generally statistically insignificant, suggesting that increased guardianship is unlikely to explain our baseline findings. The mediation analysis in Panel C of Figure A12 supports this interpretation: the estimated crime effects remain unchanged when controlling for time allocated to home production as a mediator. Panel D of Figure 4 corroborates this finding using vehicle miles traveled as an alternative measure of mobility.[16] Panel D of Figure A12 also shows that the estimated crime effects are unaffected by controlling for road traffic as a mediator. Together, these results indicate that guardianship-based mechanisms do not explain the crime-reducing effects of the Great Recession.

4.2.3 Other Alternative Mechanisms

In addition to wealth-based and guardianship-based mechanisms, we assess several other alternative explanations.

Community Programs. During the Great Recession, several place-based programs were implemented that could have reduced crime. We examine the Neighborhood Stabilization Program (NSP), which allocated roughly $7 billion across three funding rounds (2008–2010) to acquire and rehabilitate foreclosed or abandoned properties. Under the broken-windows hypothesis (Kelling

[14] Panel B of Figure A14 presents the effects of the Great Recession with the alternative measure of severity on detailed types of crime. It shows that for property offenses, the results are largely driven by motor vehicle theft. In contrast, burglary and larceny theft show no sustained post-recession pattern, with point estimates remaining imprecise and fluctuating around zero. For violent offenses, murder and aggravated assault show persistent negative estimates in the post-recession period, suggesting sustained decline in harder hit areas. Robbery also exhibits some decline, though the estimates are imprecise and there is some pre-existing differences. Manslaughter and rape show no discernible pattern.

[15] The ATUS collects complete 24-hour time-use diaries for a nationally representative sample of individuals aged 15 and older drawn from CPS households. We convert diary minutes into weekly hours and construct state-by-year averages using sampling weights adjusted for day-of-week composition, since the sample is too sparse for reliable CZ-level measures. Following Aguiar et al. (2013), our main dependent variable captures non-market activity plus leisure: unpaid home production (household tasks, maintenance, homeownership activities, gardening, shopping, errands, and caregiving, including related travel) and leisure broadly defined (socializing, sports, hobbies, entertainment, eating, sleeping, and personal care). Robustness checks using non-market work and leisure separately yield qualitatively similar results.

[16] Due to data limitations, we construct traffic measures at the state level rather than the CZ level.

and Wilson, 1982), reducing visible disorder could lower crime. Because the NSP allocations were targeted toward harder-hit states, simple state-level comparisons are likely confounded by both policy impacts and recession severity. We therefore implement a BCP design (see Section 5.2.1), comparing adjacent counties across state borders that share local economic conditions but face discontinuous variation in NSP intensity due to state-level jurisdiction. Figure A15 shows estimated effects that are close to zero and statistically insignificant, providing little support for NSP-driven crime reductions.

Secular Crime Declines. U.S. crime has trended downward since the 1990s, raising the concern that our baseline results could partly reflect a continuation of this secular decline. However, our identification relies on differential changes across areas with varying recession severity, not on aggregate time-series variation. The event-study estimates in Figure 1 show no differential pre-trends prior to the Great Recession, confirming that long-run national declines do not drive our results.

Incarceration. Rising incarceration could mechanically reduce the pool of potential offenders and thus lower observed crime. However, Panel B of Figure 2 shows little evidence of differential pre-trends in incarceration rates by recession severity, making it unlikely that differential changes in incarceration explain our baseline estimates.

Policing. If policing had intensified more in harder-hit areas, increased enforcement could have contributed to the observed crime reductions. Panel A of Figure A11 shows that the number of sworn police officers actually fell in harder-hit areas during the Great Recession, which is inconsistent with this hypothesis.

UI Benefit Extensions. The federal government expanded maximum UI benefit duration substantially during the Great Recession. As extended benefits help displaced workers maintain consumption, one might expect this expansion to reduce financially motivated crime. However, we document the opposite: UI benefit extensions increased crime (Section 5.2.2). This channel therefore works against our baseline findings, implying that the underlying recession-induced crime reduction is, if anything, larger than our estimates suggest.

4.3 Heterogeneous Effects by SES

We examine heterogeneous effects of the Great Recession on crime by SES. The adverse impacts of recessions are often more severe for low-SES households (Hoynes et al., 2012; Krueger et al., 2016). Previous studies have examined how employment and consumption expenditure varied across SES groups during the Great Recession, but the distributional consequences for crime remain underexplored. To address this gap, we estimate heterogeneous crime effects of the Great Recession using CZ-level shares of education attainment levels and household income levels from the 2000

Census as proxies for local SES composition. Specifically, we interact recession severity with each SES proxy and year indicators, controlling for the main effects of both:

$$Crime_{c,t} = \sum_{t \neq 2006} \left[\theta_t \cdot Shock_c \cdot E_c^j \cdot \mathbf{1}(Year = t)\right] + \sum_{t \neq 2006} \left[\beta_t \cdot Shock_c \cdot \mathbf{1}(Year = t)\right] + \sum_{t \neq 2006} \left[\delta_t \cdot E_c^j \cdot \mathbf{1}(Year = t)\right] + \alpha_c + \gamma_t + \varepsilon_{c,t}, \tag{5}$$

where all notation follows equation (2), except that E_c^j denotes the CZ-level share of SES group j, measured in 2000. The coefficients of interest, θ_t, capture how the effect of the Great Recession on crime varies with local SES composition.

Figure 5 reveals that the crime effects of the Great Recession differ sharply by local SES composition, with a non-monotonic pattern across groups. Panel A shows that CZs with a larger share of adults without a high school diploma exhibit an increase in crime following the Great Recession. The pattern reverses as education levels rise. Panel B presents consistent results for household income. CZs with a higher concentration of households earning under $15,000 exhibit positive and persistent coefficients, while coefficients for the higher-income brackets are negative or indistinguishable from zero. These results reinforce the conclusion that incorporating crime makes the welfare consequences of the Great Recession more regressive.

The non-monotonic pattern in Figure 5 is informative about mechanisms. Crime increased in CZs with larger shares of the lowest-SES residents (those without a high school diploma or with household income below $15,000), but it reduced in areas with larger shares of higher-SES residents. This pattern is consistent with the interplay of two channels. The first is the opportunity-cost channel: higher unemployment reduces the returns to legal work, lowering the opportunity cost of time spent on crime and thus increasing criminal activity. The second is the wealth-based mechanism associated with the housing crisis (Mian and Sufi, 2014b). The collapse in housing prices sharply reduced household wealth and may have induced financially strained homeowners to liquidate durable assets such as vehicles, thereby reducing the stock of attractive targets for property crime. Low-SES households had lower homeownership rates (Aladangady et al., 2023) and had experienced relatively small housing-related wealth shocks during the Great Recession. For these groups, the opportunity-cost channel likely dominated, leading to increases in crime.

5 Effects of UI Benefit Extensions on Crime during the Great Recession

The crime-reducing effects of the Great Recession imply that macroeconomic stabilization policies may generate smaller welfare gains than conventional analyses suggest. As UI benefits buffer income losses, they are likely to affect both consumption and crime. We exploit the large expansion in the maximum duration of UI benefits during the Great Recession to estimate the effects of this policy change on crime.

5.1 UI Benefit Extensions during the Great Recession

In response to the severity of the Great Recession, the federal government substantially expanded UI benefit duration, raising the maximum from the standard 26 weeks to as many as 99 weeks. Three sets of policies generated the cross-state variation in maximum benefit duration that we exploit. The primary driver was the Emergency Unemployment Compensation (EUC) program, a temporary federal measure enacted in June 2008. Initially providing a uniform 13-week extension across all states, the EUC subsequently evolved into a complex, multi-tier system (up to four tiers) that allocated additional weeks based on state unemployment rates, and expired at the end of 2013. This federal expansion was supplemented by the permanent Extended Benefits (EB) program, which has provided additional 13 or 20 weeks of benefits since 1970 when a state's unemployment rate exceeded specific thresholds.[17] These expansions generated considerable geographic heterogeneity in benefit generosity, as shown in Figures A16 and A17.

5.2 Effects of UI Benefit Extensions on Crime

5.2.1 Empirical Strategy

Following Boone et al. (2021), we use UI benefit duration data from the Department of Labor and state government websites, as compiled by Boone et al. (2021). We merge these data with county-year-level reported offense rates from the FBI UCR data for 2003–2016. To identify the effects of UI benefit extensions while controlling for recession severity, we employ the BCP design following Boone et al. (2021). This design compares adjacent counties straddling state borders that share local economic conditions but face different UI policies due to state-level jurisdiction. Because the UI extensions began with the EUC's enactment in June 2008, we use 2008 as the reference year for this analysis so that the pre-period captures variation in crime trends before any treatment. Specifically, we estimate the following equation:

$$y_{c,p,t} = \sum_{s \neq 2008} \beta_s \cdot Benefits_c \cdot \mathbf{1}[t = s] + \alpha_{c,p} + \delta_{p,t} + u_{c,p,t}, \tag{6}$$

where $y_{c,p,t}$ represents the number of reported offenses per 1,000 individuals aged 15–65 years in county c belonging to pair p in year t; $Benefits_c$ denotes the change in average UI benefit duration from the pre-extension period (November 2007–October 2008) to the extension period (November 2008–December 2013), divided by 10 so that coefficients represent the effects of a 10-week extension. $\alpha_{c,p}$ and $\delta_{p,t}$ represent county-by-county-pair and pair-year fixed effects, respectively. $u_{c,p,t}$ is an error term. For statistical inference, we calculate standard errors two-way clustered at the state and state-pair levels, accounting for both shared state-level shocks and the mechanical correlation in multiple pairs. Following Boone et al. (2021), we use the pre-trend-trimmed (PT-trimmed) sample,

[17] Although the federal government fully funded the EB program during the recession, variation arose because states retained discretion over optional triggers for activating these benefits. Concurrent with these expansions, some states reduced their regular UI benefit durations starting in 2011 due to trust fund solvency concerns (e.g., Missouri, North Carolina, and Florida).

excluding the quartile of county pairs with the highest mean squared error in each outcome variable between 2003 and 2008 after removing level differences.

The parameters of interest, β_s, capture how crime rates in counties with longer UI benefit durations evolved relative to their cross-border neighbors, with 2008 as the reference year. Interpreting these estimates as causal effects of the UI benefit extensions requires that trends of crime rates across border county pairs are parallel in the absence of the extensions. Since this counterfactual is unobservable, we indirectly examine this identification assumption by testing whether pre-recession β_s estimates are small in magnitude and statistically insignificant.

Another identification concern is offender migration. If offenders respond to differential UI benefit durations across state borders, they may relocate or shift their offending to counties with longer benefit durations. Such cross-border substitution would bias our estimates upward. To address this concern, we conduct two robustness checks. First, we interact the distance between each pair of border counties' centroids with $Benefits_c$ and year fixed effects, under the assumption that migration-induced bias is larger for geographically proximate county pairs. Second, we construct hinterland-county-pairs (HCP) by pairing interior counties that are adjacent to each border county in a given BCP but located farther from the state line. These hinterland counties face the same state-level UI policy variation as their border counterparts but are more geographically removed from the border. Thus, cross-border spillovers should be attenuated.

5.2.2 Empirical Results

Figure 6 shows that longer UI benefit durations increased reported offense rates. Before the extension, the coefficient estimates are small in magnitude and statistically insignificant, supporting the parallel trend assumption. Subsequently, crime rates increased as UI benefit durations extended. After 2013, when the temporary benefit extensions ended, the crime effects diminished but did not fully revert to pre-extension levels. The average crime impact of a 10-week increase in UI duration during 2009–2016 is estimated to be 1.67 offenses per 1,000 individuals. This magnitude represents a 3.1 percent increase from the 2006 mean.

To further examine this pattern, we estimate the effects of UI benefit extensions on specific crime types. Figure A18 presents evidence that property crime increases are primarily concentrated in burglary and motor vehicle theft, the same offense categories that drove the baseline crime reductions in Section 4.2, and consistent with the wealth-based mechanism. UI benefits partially sustained the household income and asset values that the recession would otherwise have eroded, preserving the value of criminal targets (e.g., motor vehicles). Among violent crimes, the estimates suggest increases in rape and aggravated assault, though the effects are less precisely estimated.[18]

We next examine the robustness of our baseline results to offender migration. If offenders relocated to counties with longer UI benefit durations or shifted their criminal activity across

[18] We also estimate the effects of UI benefit extensions on arrest rates to track effects through the criminal justice pipeline. Figure A20 shows little evidence of increased arrest rates, which may reflect the weaker statistical power of arrest data at the county-pair level. We cannot examine effects on incarceration rates because these data are available only at the state level, which is incompatible with the BCP design.

state borders, our BCP estimates would overstate the true crime effect by attributing cross-border displacement to the policy itself. We address this concern with two checks. Figure A19 shows that our results remain robust when controlling for inter-county distance (Panel A) and when using the HCP sample instead of the BCP sample (Panel B).[19] The dynamic patterns closely resemble those in Figure 6, indicating that offender migration does not drive our findings.

6 Welfare Costs of the Great Recession

This section quantifies the welfare implications of the crime effects documented in Sections 4 and 5 using sufficient statistics approaches.

6.1 Conceptual Framework

We quantify the welfare costs of the Great Recession using a sufficient statistics framework. Our approach extends Krebs's (2007) model, which characterized the welfare cost of recessions through consumption losses, by incorporating crime as an additional nonmarket channel.[20]

We consider a representative agent who maximizes expected lifetime utility. The lifetime utility function is:

$$U = E\left[\sum_{t=0}^{T} \beta^t \, u(c_t, \pi_t)\right], \tag{7}$$

where β is the discount factor. The per-period utility function, $u(c_t, \pi_t)$, depends on consumption, c_t, and the crime rate, π_t. We assume additive separability between consumption and crime:

$$u(c_t, \pi_t) = v(c_t) - h(\pi_t), \tag{8}$$

where $v(\cdot)$ represents utility from consumption and $h(\cdot)$ captures disutility from crime exposure.[21] For simplicity, we distinguish two period types: normal and recession. We denote consumption and crime rate during the normal (recession) period as c_N (c_R) and π_N (π_R), respectively.

We define the welfare cost of the Great Recession as the compensating variation in consumption, denoted Δ^*, that would make an individual indifferent between the recession and normal periods. Formally, Δ^* solves:

$$v((1+\Delta^*)c_R) - h(\pi_R) = v(c_N) - h(\pi_N).$$

[19] The heterogeneous effects of UI benefit extensions by distance are small in magnitude and statistically insignificant (Figure A21), further confirming that proximity to the state border does not amplify the estimated crime effects.

[20] Except for the crime risk and unless stated otherwise, other components of the model such as stochastic income processes follow Krebs (2007). We do not present a multi-period version of the model for simplicity.

[21] $v(\cdot)$ and $h(\cdot)$ are assumed to be concave and convex, respectively, and are both increasing and twice differentiable. One can extend this framework to incorporate mortality effects by introducing endogenous survival probability, as in Finkelstein et al. (2025). Appendix B provides the full derivation incorporating mortality, crime, and consumption channels. Because we do not incorporate mortality in our baseline calibration, the life expectancy terms cancel and we present the simplified two-channel version here.

To characterize Δ^*, we define the welfare cost function:

$$W(\Delta) = [v((1+\Delta)c_R) - h(\pi_R)] - [v(c_N) - h(\pi_N)]\,, \tag{9}$$

and solve for Δ such that $W(\Delta) = 0$. Employing a first-order Taylor expansion around $\Delta = 0$ (details in Appendix B), we obtain:

$$\Delta^* = \delta - \varphi \cdot \epsilon_c^{\pi}, \tag{10}$$

where δ denotes the welfare cost attributable to the consumption loss from the Great Recession, φ denotes the percentage decline in crime during the recession, and ϵ_c^{π} denotes the consumption elasticity with regard to crime.

Equation (10) provides an intuitive decomposition of the welfare cost of a recession. The first term captures the direct consumption loss. The second term captures the welfare gain from reduced crime, scaled by the consumption elasticity with respect to crime. The welfare cost is larger when (i) the consumption loss is larger (higher δ), (ii) the crime reduction is smaller (lower φ), or (iii) the marginal willingness to pay to avoid crime is smaller (lower ϵ_c^{π}).

6.2 Calibration

We calibrate each parameter in equation (10) using estimates from the literature and our empirical analysis. This approach measures the per-period welfare cost during the recession decade (2007–2016), abstracting from lifecycle considerations such as discounting and age-specific effects. This keeps the number of required assumptions to a minimum and allows readers to directly trace how each empirical input maps to the welfare estimate. This differs from Finkelstein et al. (2025), who amortize recession impacts over the remaining lifetime. Because consumption and crime effects enter equation (10) symmetrically, the relative magnitude of the crime offset is robust to this modeling choice.

For the consumption effect, we set $\delta = 5.5$ percent, the estimated average annual decline in consumption during 2007–2016 attributable to the Great Recession. This estimate comes from Finkelstein et al. (2025), who find that a one percentage point increase in the unemployment rate reduced consumption by 1.2 percent using the same spatial variation in unemployment rate changes that we exploit. Multiplying by the national average unemployment shock of 4.6 percentage points yields a 5.5 percent decline.

For the crime effect, we set $\varphi = 5.1$ percent based on our baseline results in Section 4.1. For the consumption elasticity with respect to crime, we draw on Mejía and Restrepo (2016), who find that a one percent increase in property crime reduces total consumption expenditure by 0.065 percent, using the 1986–2003 Consumer Expenditure Survey. Assuming that this estimate generalizes to all crime types, we set $\epsilon_c^{\pi} = 0.065$.[22]

Substituting into equation (10), the welfare cost of the Great Recession accounting for crime

[22] We note that the consumption elasticity, ϵ_c^{π}, is estimated from the pre-Great Recession period (1986–2003). To the extent that this relationship shifted during the recession, our welfare estimates would be affected proportionally.

reductions is $\Delta^* = 5.17$ percent of annual consumption ($= 5.5 - 5.1 \times 0.065$). This is 0.33 percentage points below the 5.5 percent cost based on consumption effects alone. The crime-reducing effects decrease the welfare cost by 6 percent relative to consumption-only estimates.

6.3 Welfare Implications of UI Benefit Extensions

We next assess whether the crime externality from UI benefit extensions is large enough to materially affect the welfare evaluation of this policy. The welfare framework in Section 6.2 evaluates the aggregate recession as an exogenous shock to consumption and crime. Evaluating a policy response to the recession requires a different approach: the Baily–Chetty sufficient-statistics framework (Baily, 1978; Chetty, 2008), which characterizes optimal UI generosity by balancing the marginal insurance value against the fiscal externality. We augment this framework with the crime externality identified by our BCP estimates. The marginal welfare effect of extending potential benefit duration P is:

$$\frac{dW}{dP} = \underbrace{I(P)}_{\text{insurance value}} - \underbrace{M(P)}_{\text{fiscal externality}} - \underbrace{\kappa \frac{d\pi}{dP}}_{\text{crime externality}}, \tag{11}$$

where $I(P)$ is the consumption-smoothing value of the marginal week of benefits, $M(P)$ is the fiscal externality from UI-induced changes in unemployment duration, κ is the marginal social cost of crime, and $d\pi/dP$ is the causal effect of benefit duration on crime incidence. The crime term enters the social welfare function additively because it represents a cost imposed on potential victims that is external to the private insurance–incentive trade-off between the unemployed worker and the government; in standard public economics, such third-party externalities augment the planner's objective as separate terms.[23] Because this framework evaluates the marginal welfare effect of a policy change rather than the aggregate welfare cost of the recession, it answers a different question from Section 6.2. Both assessments are expressed in consumption-equivalent terms to facilitate comparison, while noting important differences in time horizons and policy margins.

Combined with the consumption elasticity $\epsilon_c^\pi = 0.065$ (Mejía and Restrepo, 2016), our BCP estimate that a 10-week extension raises reported offenses by 3.1 percent implies a per-period consumption-equivalent welfare cost of approximately 0.2 percent of annual consumption (0.031×0.065). To gauge whether this magnitude is economically meaningful, we compare it to the consumption-smoothing gains estimated by Ganong and Noel (2019), who find that a one-month duration extension generates a net welfare gain of 0.016 percent of lifetime income after accounting for the fiscal externality from behavioral distortions. For a 10-week extension, this scales to approximately 0.037 percent. Because their welfare metric is a permanent lifetime-income equivalent

[23] Because our BCP estimates capture the average effect of a large, discrete extension (from 26 to as many as 99 weeks) rather than an infinitesimal change, equation (11) serves as a linear approximation to the welfare effect of the observed policy change. Appendix C provides the full derivation connecting κ to the consumption elasticity ϵ_c^π from the framework in Section 6.1. In recessions, Landais et al. (2018) argue that part of the behavioral response represents a search-congestion externality that would attenuate $M(P)$, increasing the net welfare gain from extensions. Our baseline calculation abstracts from this channel, which works against our finding that the crime externality offsets part of the consumption-smoothing gain.

while our crime cost is a per-period flow during the extension years, a direct numerical comparison requires placing both on a common footing. Amortizing the cumulative per-period crime cost over a representative remaining lifetime yields an annualized equivalent of approximately 0.04 percent of annual consumption, a magnitude of the same order as the Ganong and Noel estimate (Appendix C provides the detailed calculation). The crime externality is thus quantitatively nontrivial, suggesting that the crime channel may substantially offset the consumption-smoothing value of UI extensions.[24]

7 Concluding Remarks

This study documents that the Great Recession persistently reduced crime in the United States, challenging conventional expectations about the relationship between economic downturns and crime. Exploiting spatial variation in recession severity across commuting zones, we find that areas more severely affected by the recession experienced lasting reductions in criminal activity, particularly property offenses. These effects persisted through 2016, well beyond the recovery in local unemployment rates.

Our findings reflect the Great Recession's distinctive character as a balance-sheet recession. The collapse in housing prices and the widespread destruction of household wealth reduced the economic returns to crime, with this wealth-based channel dominating the countervailing pressure from higher unemployment. Mediation analysis suggests that bankruptcy rates and housing prices account for a substantial share of the crime reduction. We also document that UI benefit extensions that cushioned household income losses partially offset these crime reductions, providing consistent evidence of this wealth-based interpretation.

These findings carry three sets of welfare implications. First, our welfare analysis demonstrates that incorporating crime as a crucial nonmarket outcome alters the welfare evaluation of both recessions and stabilization policies. Incorporating crime reductions decreases the estimated welfare cost of the Great Recession by approximately 6 percent relative to consumption-only estimates. Second, our distributional analysis reveals that the welfare consequences of the Great Recession are more regressive once crime is incorporated. Third, the crime externality from UI benefit extensions suggests that conventional welfare evaluations of UI that omit crime may overstate net benefits.

Beyond these welfare implications, this study is also related to understanding the long-run decline in U.S. crime rates. Based on our estimates, the persistent crime-reducing effects of the Great Recession account for approximately 23 percent of the aggregate crime rate decline during 2007–2016, suggesting that temporary macroeconomic shocks can generate persistent shifts in criminal activity.

[24] Ganong and Noel (2019)'s estimates come from post-recession data (2014–2016), when UI duration had returned to the standard 26 weeks and the labor market had substantially recovered. The consumption-smoothing value of extensions was likely higher during the Great Recession. As job-finding rates were substantially lower, a larger share of the unemployed reached benefit exhaustion, increasing the insurance value of longer benefits. The implied offset from the crime externality should accordingly be interpreted as an upper bound.

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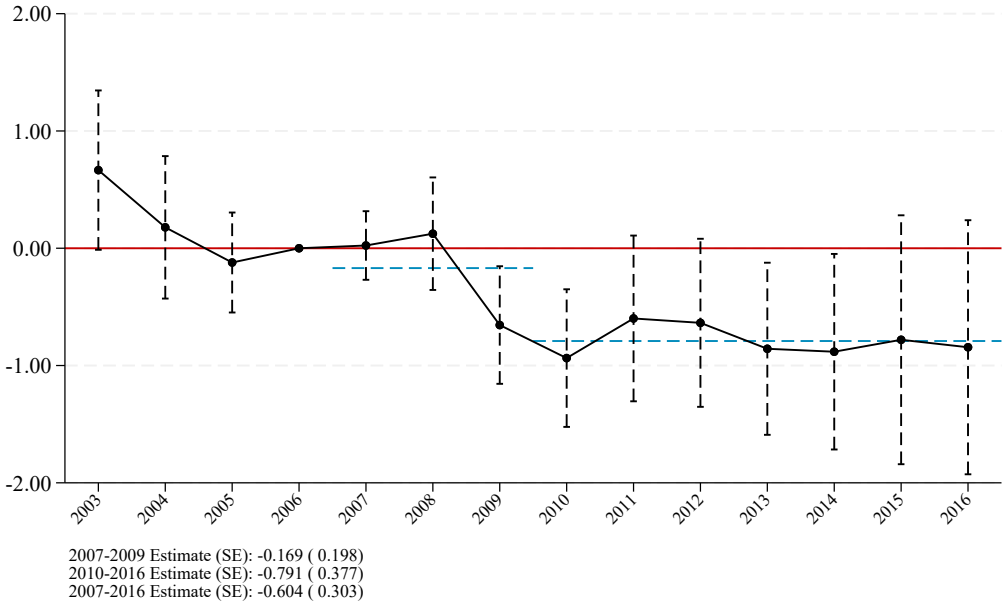
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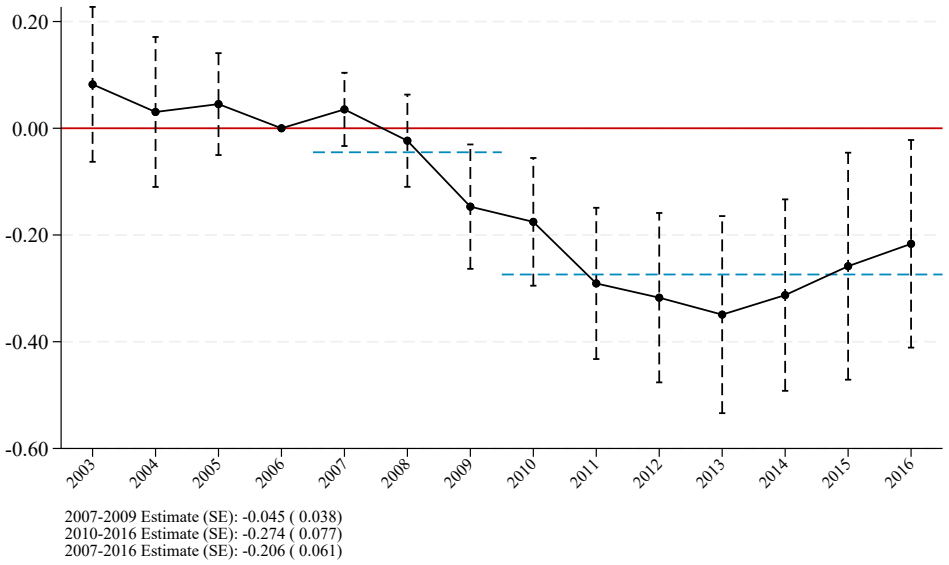
Figure 1: Effects of the Great Recession on Reported Offense Rates



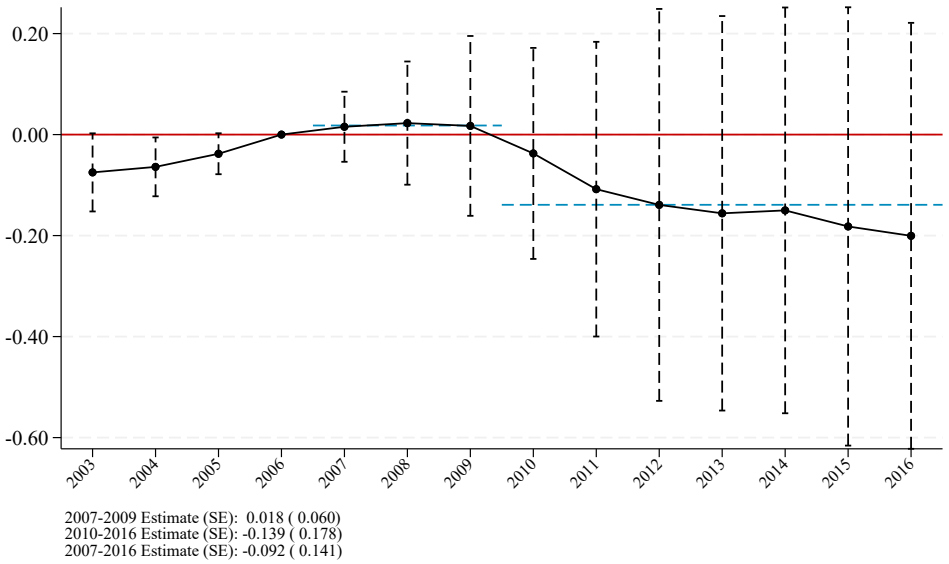
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure presents coefficient estimates from equation (1), where the dependent variable is the number of reported offenses per 1,000 individuals aged 15–65 in each commuting zone (CZ). Figure 1 shows total offenses (violent and property combined). Black dots represent point estimates; vertical lines with caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, appear in the lower left corner of each panel. Standard errors are clustered at the CZ level. All regressions are weighted by 2006 CZ population.

Figure 2: Effects of the Great Recession Across the Criminal Justice Pipelines

(a) Arrest Rates

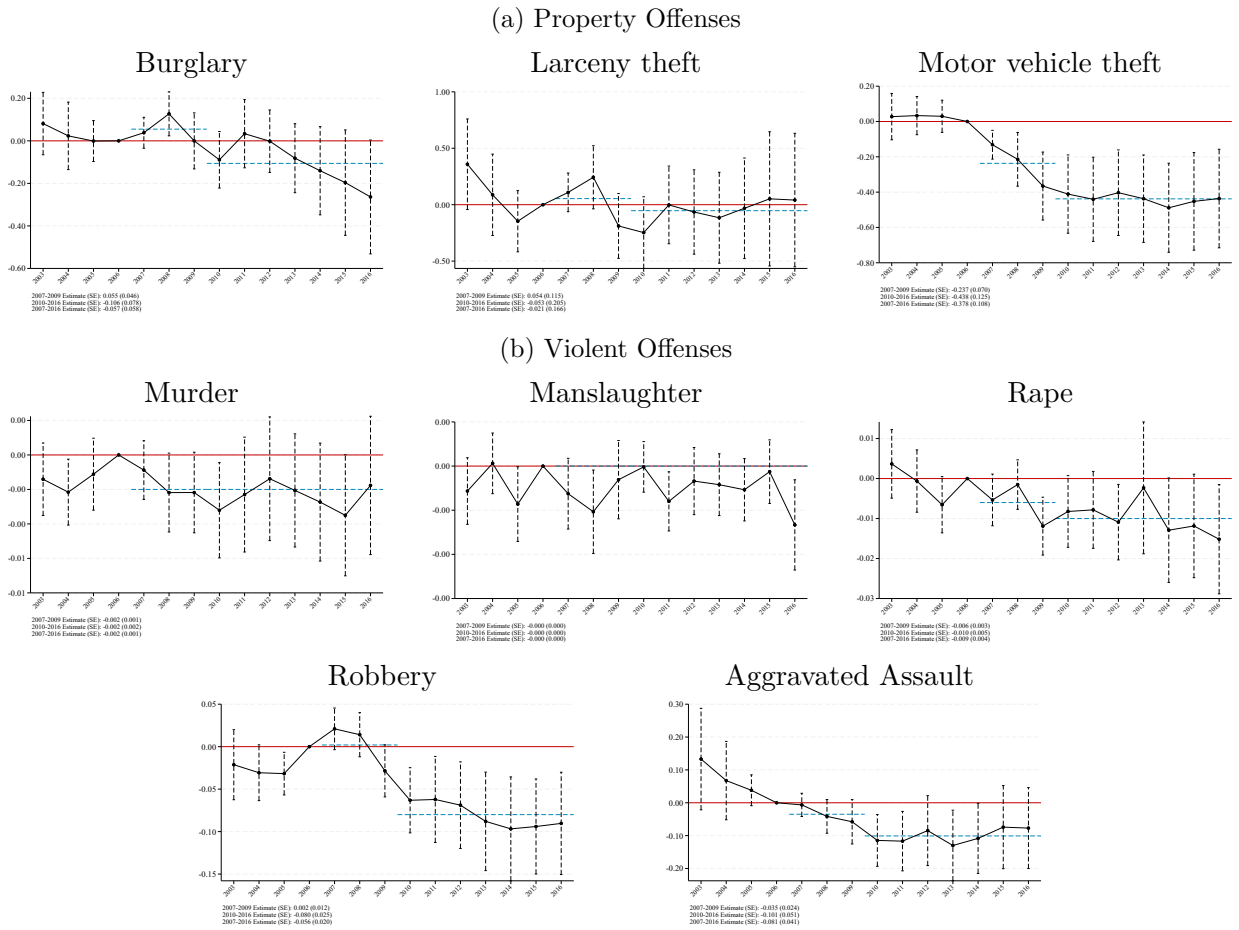


(b) Incarceration Rates



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure presents the effects of the Great Recession on arrests rates (Figure 2a) and incarceration rates (Figure 2b), each measured per 1,000 individuals aged 15–65. Black dots represent coefficient estimates from equation (1). Caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the CZ level, and all regressions are weighted by 2006 CZ population.

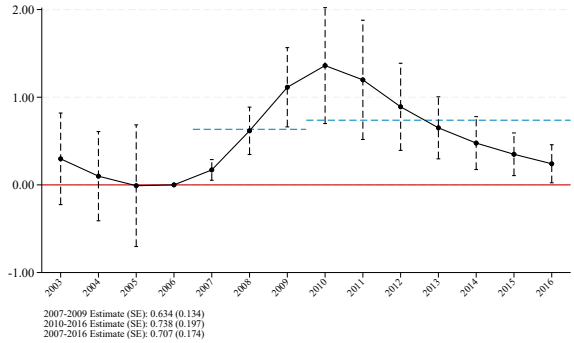
Figure 3: Effects of the Great Recession on Reported Offense Rates By Offense Type



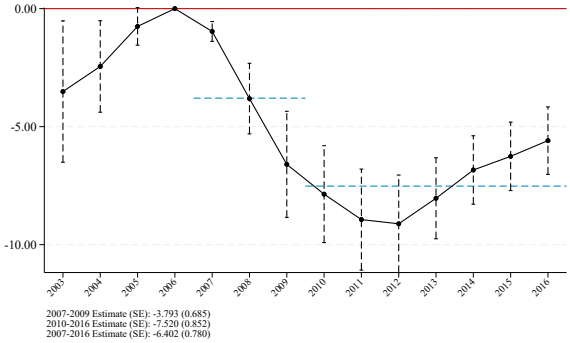
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure shows the effects of the Great Recession on all specific crime offenses available in the UCR data. In Figure 3a, the outcome variable is either the burglary rate, larceny theft or the motor vehicle theft rate (offenses per 1,000 individuals aged 15-65), specific categories of property crimes. In Figure 3b, the outcome variable is, in turn, the rate of murder, manslaughter, rape, robbery, or aggravated assault (offenses per 1,000 individuals between 15-65). Black dots plot the coefficient estimates from equation (1) when each offense-specific rate is used as the dependent variable. Caps denote 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the CZ level, and all regressions are weighted by 2006 CZ population.

Figure 4: Mechanism Analysis

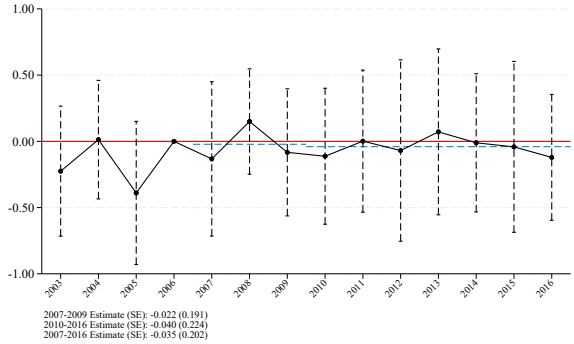
(a) Bankruptcy Rate



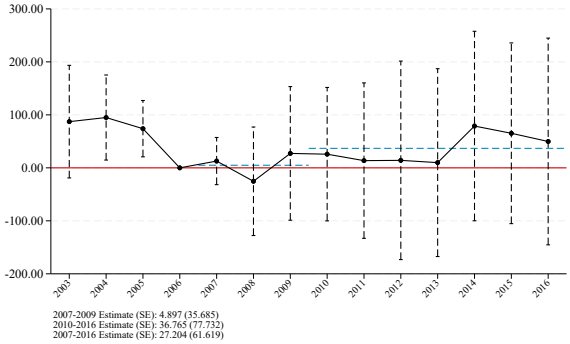
(b) Housing Price Index



(c) Time Use: Home production



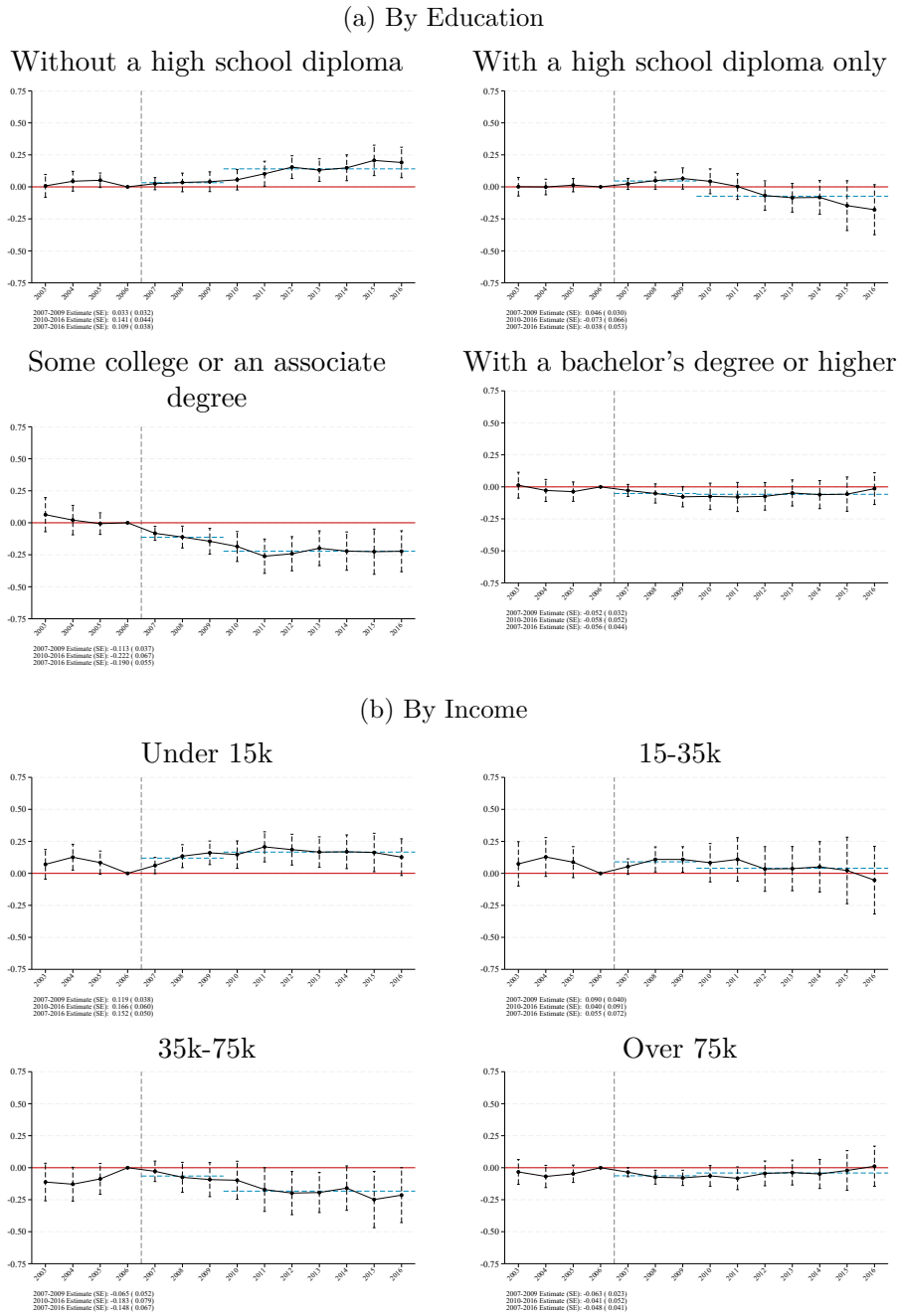
(d) Vehicle Miles Traveled



Data source: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS, US Courts, US Department of Transportation, The Federal Housing Finance Agency, ATUS.

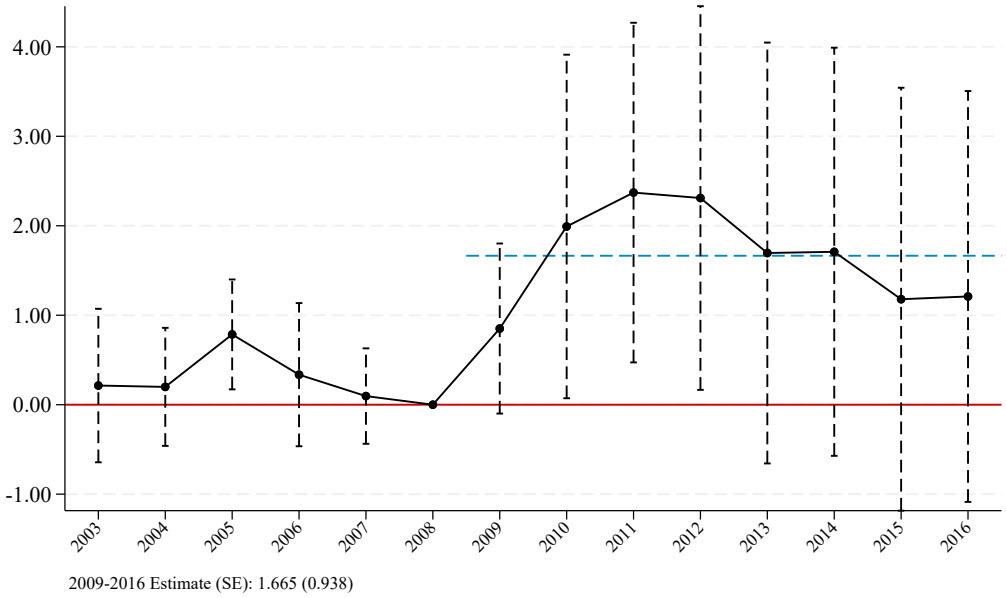
Notes: Black dots represent the coefficient estimates from equation (1) for each mechanism at the CZ level for HPI and at the state level for the bankruptcy, time use, and traffic. Caps indicate 95% confidence intervals. Blue dashed lines indicate the mean of coefficient estimates representing 2007-2009 and 2010-2016, respectively. These estimates are reported in the lower left-hand corner alongside the entire 2007-2016 period. For HPI, standard errors are clustered at the CZ level. CZ-level 2006 population is used as weights to account for population size heterogeneity, and for the bankruptcy, time use and traffic, the standard errors are clustered at the state level.

Figure 5: Effects of the Great Recession on Reported Offense Rates by Education and Income



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS, and US Census.
Notes: This figure plots the estimated coefficients from equation (5), where the dependent variable is total reported offenses per 1,000 individuals aged 15–65. Each panel estimates the interaction between the change in the CZ-level unemployment rate from 2007 to 2009 and a CZ-level socioeconomic characteristic measured in 2000—either the percentage of households in a given income bracket (under $15K, $15K–$35K, $35K–$75K, $75K+) or the share of adults at a given education level (no high school diploma, high school diploma only, some college, bachelor's degree or higher). The base year is 2006. Black dots plot the coefficient estimates; capped bars denote 95 percent confidence intervals. Blue dashed lines show the mean coefficient for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower-left corner. All specifications include CZ and year fixed effects. Standard errors are clustered at the CZ level, and all regressions are weighted by 2006 CZ population aged 15–65.

Figure 6: Effects of UI Benefit Extensions on Crime Rates

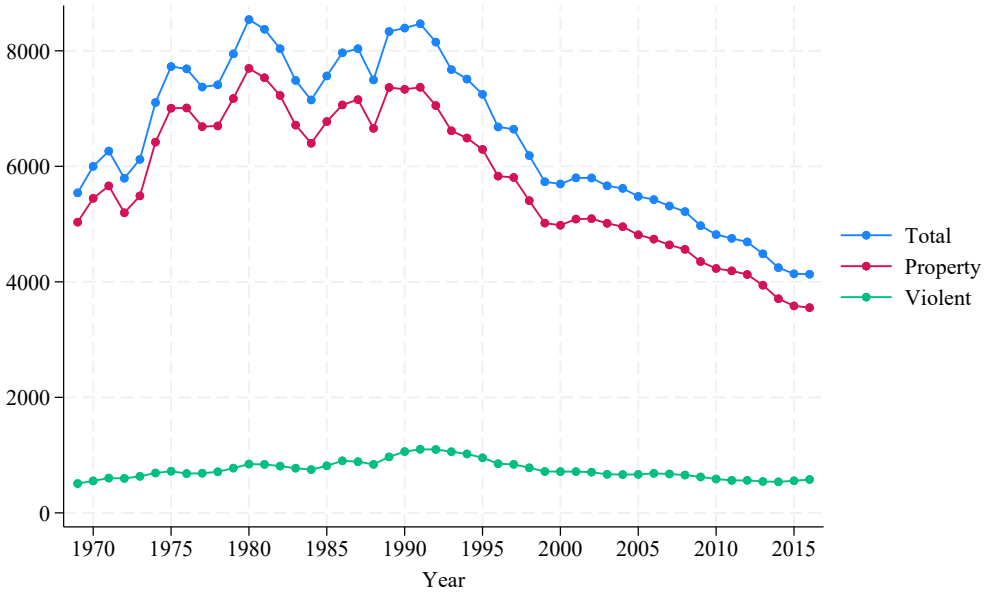


Data sources: FBI Uniform Crime Reporting and SEER, CDC.
Notes: The figure plots the set of βs coefficients from the equation (6) estimated over the set of border county pairs in the PT-trimmed sample following Boone et al (2021). The outcome is either offense reported per 1,000 individuals aged 15-65. The average treatment intensity, $treat_c$ continuous measure defined as the average unemployment insurance duration during the treatment period (2008:11–2013:12) minus average duration from the 12 months prior (2007:11–2008:10), divided by 10. The caps indicate the 95 percent confidence interval, standard error clustered at the state and state-pair level. PT-trimming removes the quartile of county pairs with the highest mean squared error in crime rate between 2003 and 2008 (after partialling out a fixed level difference). Boone et al (2021), using the PT-trimmed sample, leveraged the variation in unemployment insurance duration in the Great Recession between adjacent counties located in neighboring states to examine the effect of unemployment insurance expansion on the aggregate employment.

Appendix

A Appendix Figures and Tables

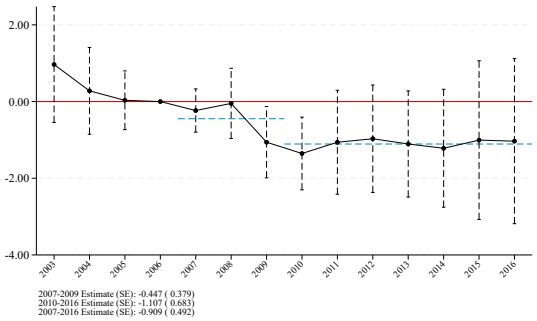
Figure A1: Trends of Reported Offense Rates per 100,000



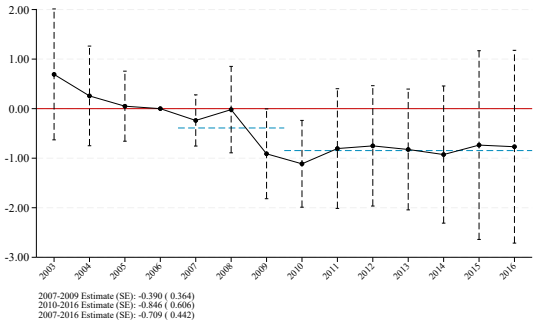
Data sources: FBI Uniform Crime Reporting and SEER.
Note: Red, green, and blue dots plot the time series of reported violent, property, and total (violent + property) offenses per 100,000 individuals, respectively.

Figure A2: Effects of the Great Recession on Reported Offense Rates at the State Level

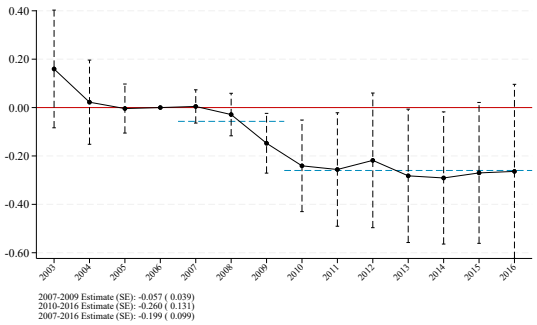
(a) Total Offenses



(b) Property Offenses

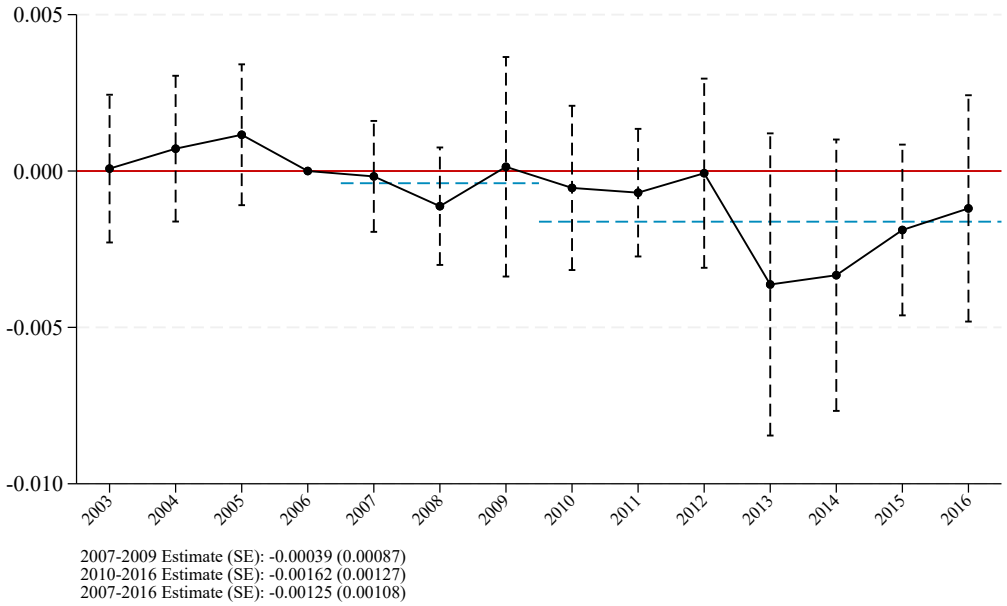


(c) Violence Offenses



Data sources: FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure replicates the results of Figure 1 using state-year cells. The outcomes are offenses per 1,000 individuals aged 15–65: total violent and property offenses in Figure A2a, property offenses in Figure A2b, and violent offenses in Figure A2c. Black dots represent coefficient estimates from equation (1) for each outcome. Caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the state level, and all regressions are weighted by 2006 state population aged 15-65.

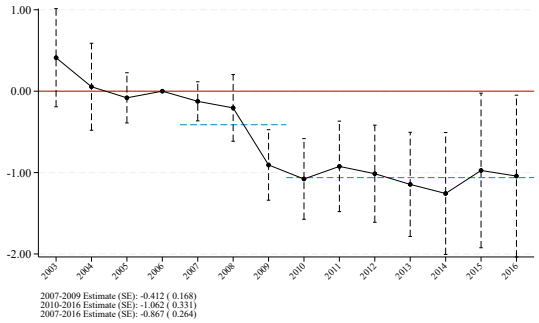
Figure A3: Effects of the Great Recession on the Probability of Not Reporting Known Offense



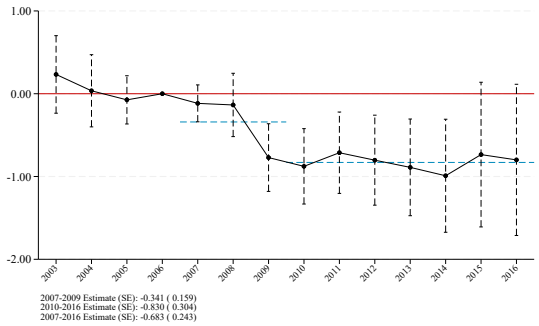
Data sources: FBI Uniform Crime Reporting.
Notes: The outcome variable is a dummy equal to one if, in a given CZ-year, at least one agency has at least one month of missing data in that year in the offense sample. Black dots plot the coefficient estimates from equation (1) when the dependent variable is replaced by each dummy variable. Caps denote 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the CZ level. Regressions are weighted by 2006 CZ population to account for differences in population size.

Figure A4: Effects of the Great Recession Restricting to Full-Reporting Agencies

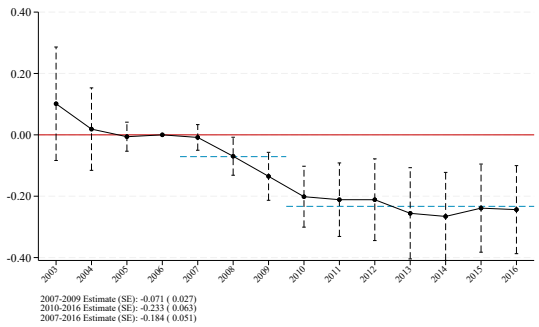
(a) Total



(b) Property



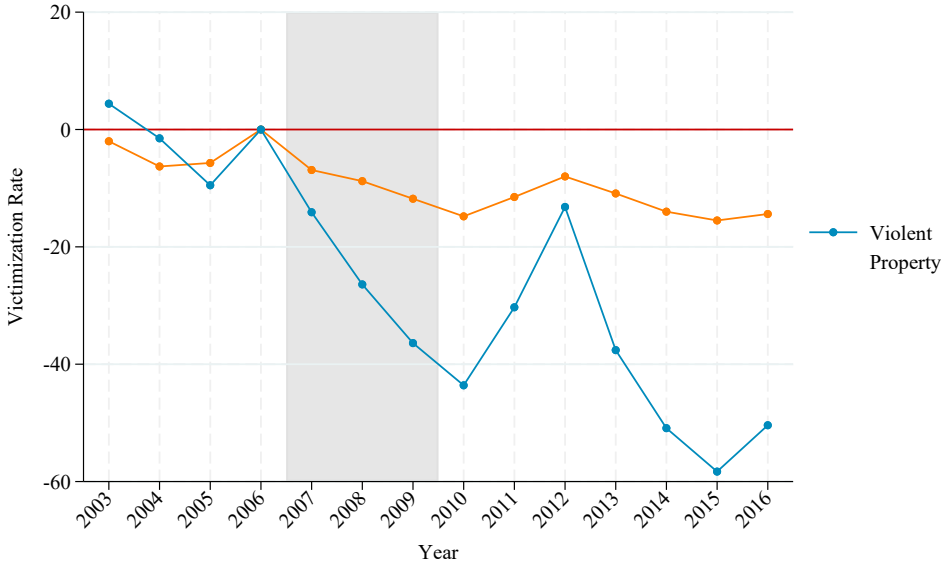
(c) Violence



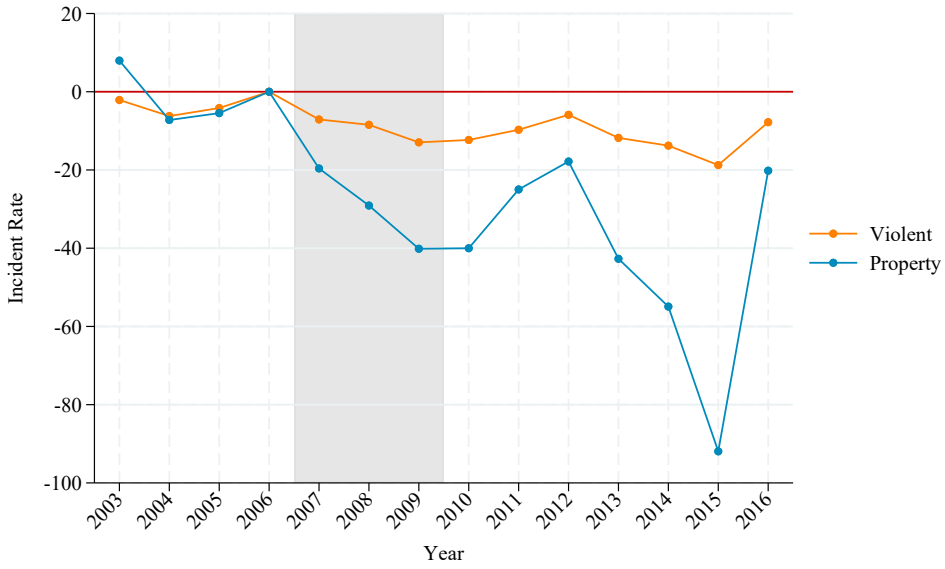
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS.
Notes: This figure replicates Figure 1, restricting the sample to offenses from law enforcement agencies that reported for all 12 months in every year from 2003 to 2016. The outcomes are offenses per 1,000 individuals aged 15–65: total violent and property offenses (Figure A4a), property offenses (Figure A4b), and violent offenses (Figure A4c). Black dots represent coefficient estimates from equation (1), and caps indicate 95 percent confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, along with the mean for the entire 2007–2016 period, are reported in the lower-left corner. Standard errors are clustered at the CZ level. All regressions are weighted by 2006 CZ population.

Figure A5: Trends of Victimization Rates

(a) Victimization Rate



(b) Incident Rate



Data source: NCVS.
Notes: This figure presents yearly national victimization and incident rates for violent crimes (per 1,000 persons age 12 or older) and property crimes (per 1,000 households) from 2003 to 2016. Figure A5a shows yearly victimization rates, and Figure A5b shows yearly incident rates. Following the Bureau of Justice Statistics, victimization rates are assigned to the survey year, whereas incident rates are assigned to the year in which the incident occurred.

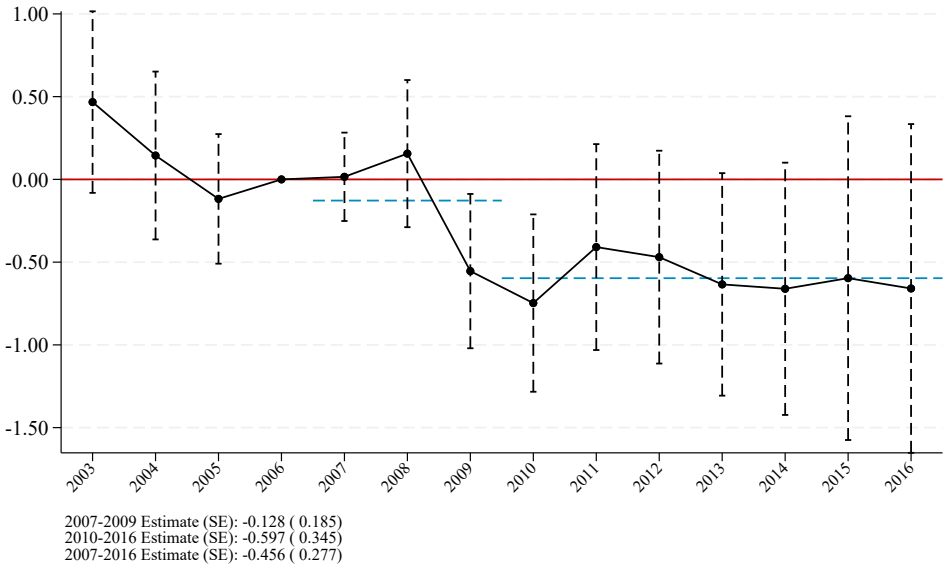
Figure A6: Recession Severity and Residualized Crime Rate



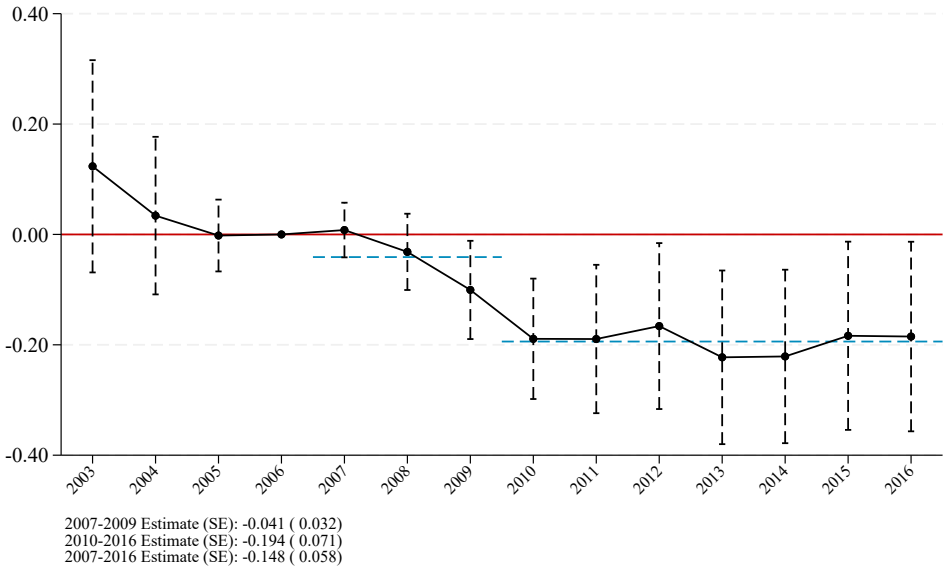
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS.
Notes: The figure plots the relationship between local recession severity and pre-recession crime rates across commuting zones (CZs) in 2006. The vertical axis shows the CZ-level crime rate (per 1,000 population aged 15-65) residualized by CZ fixed effects estimated over 2003–2006. The horizontal axis shows the 2007–2009 change in the CZ unemployment rate. The dashed line represents a population-weighted OLS fit. Robust standard errors in parentheses. The figure supports the identifying assumption that pre-recession crime trends are uncorrelated with subsequent recession severity.

Figure A7: Effects of the Great Recession on Reported Offense Rates

(a) Property

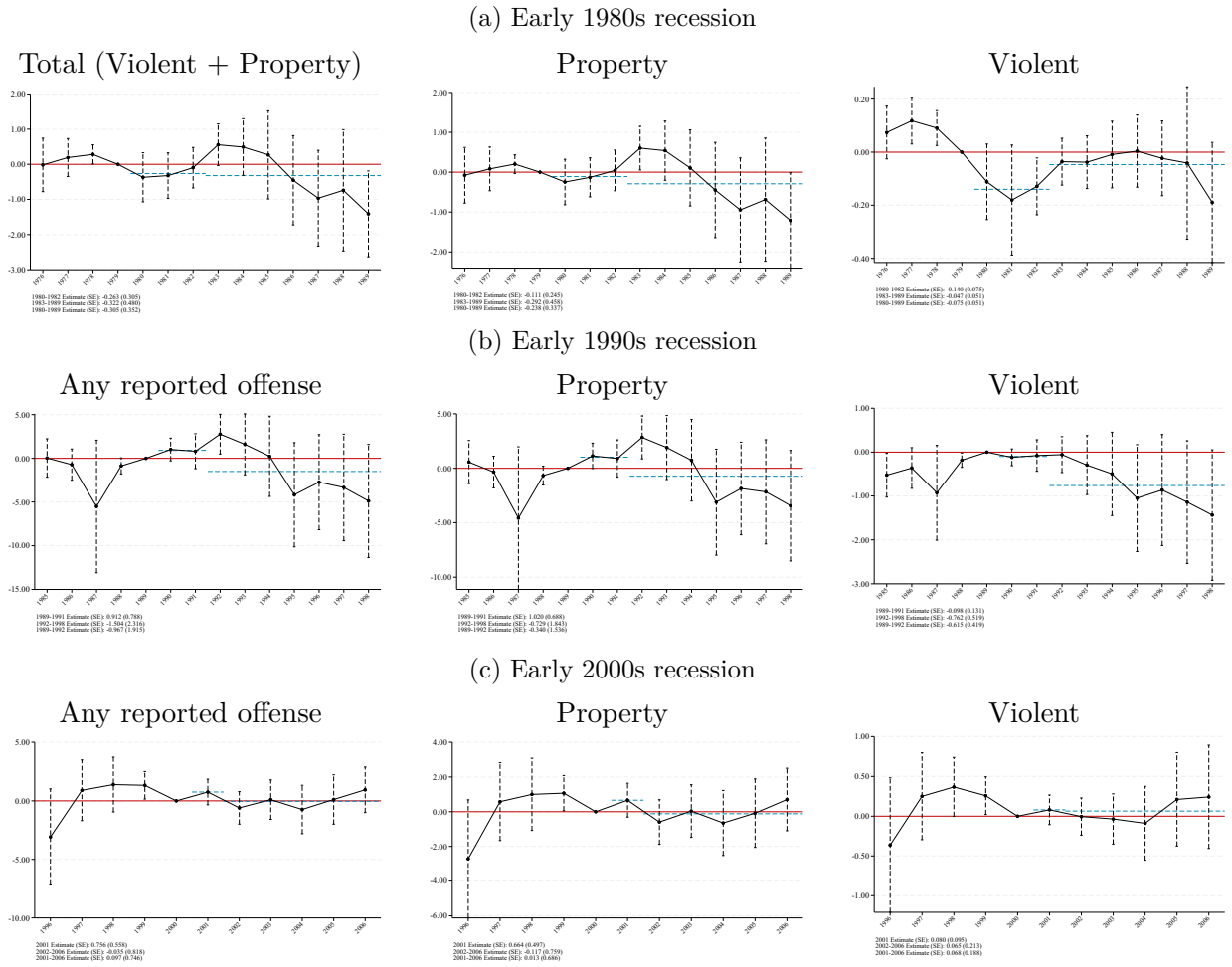


(b) Violent



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure presents coefficient estimates from equation (1), where the dependent variable is the number of reported offenses per 1,000 individuals aged 15–65 in each commuting zone (CZ). Figure A7a shows property offenses, and Figure A7b displays violent offenses. Black dots represent point estimates; vertical lines with caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, appear in the lower left corner of each panel. Standard errors are clustered at the CZ level. All regressions are weighted by 2006 CZ population aged 15-65.

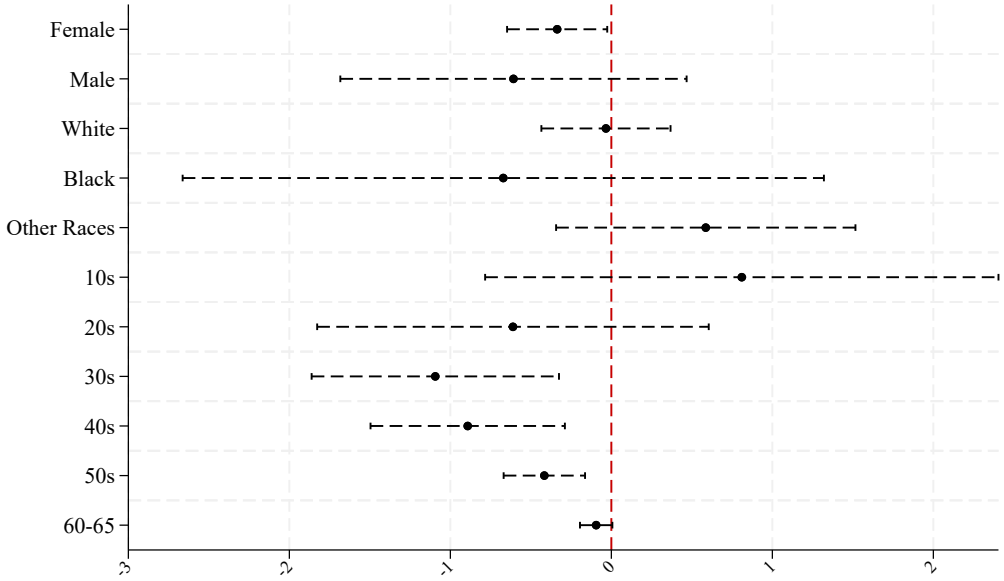
Figure A8: Effects of Previous Recessions on Reported Offense Rates



Data sources: FBI Uniform Crime Reporting, SEER, and BLS.

Notes: The outcomes include violent or property offenses reported per 1,000 individuals aged 15 and 65. Black dots represent the coefficient estimates from equation (1). Caps indicate 95% confidence intervals. Blue dashed lines indicate the mean of coefficient estimates representing during recession and post-recession, respectively. These estimates are reported in the lower left-hand corner alongside the entire period. Standard errors are clustered at the CZ level. CZ-level population in the reference year is used as weights to account for population size heterogeneity. Figure A8a, Figure A8b, and Figure A8c represent the results from the periods in 1980s, 1990s, and early 2000s. We leverage the changes in unemployment rate to estimate recession shock between 1980 and 1982, and 1990 and 1991, and 2000 and 2001.

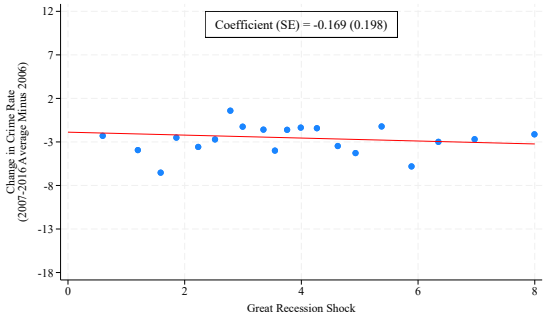
Figure A9: Heterogeneous Effects of the Great Recession on Arrest Rates By Offenders' Demographic Characteristics



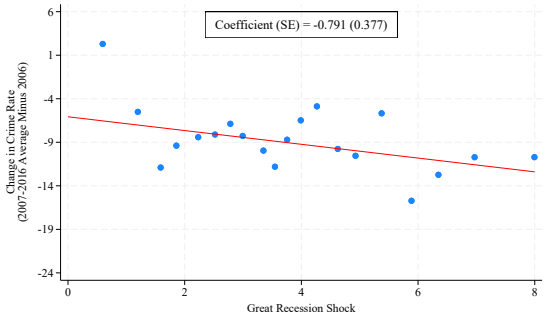
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure reports heterogeneous effects of the Great Recession on arrest rates by demographic characteristics. The dependent variable is number of crime arrests per 1,000 individuals aged 15–65 within each demographic group. Point estimates from equation (2) are shown with 95% confidence intervals. Standard errors are clustered at the CZ level. Regressions are weighted by the 2006 CZ population of each demographic group.

Figure A10: Binned Scatter Plots: Recession Severity and Crime Changes

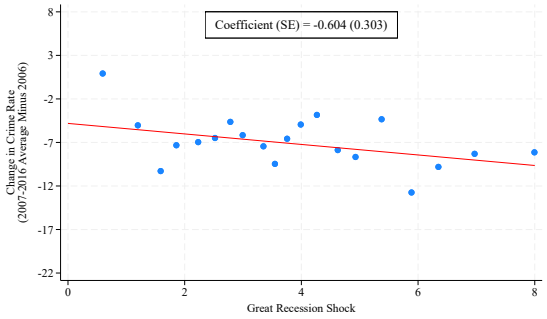
(a) Change in Crime Rate, 2006 vs 2007-2009 Average Recession Shock



(b) Change in Crime Rate, 2006 vs 2010-2016 Average Recession Shock



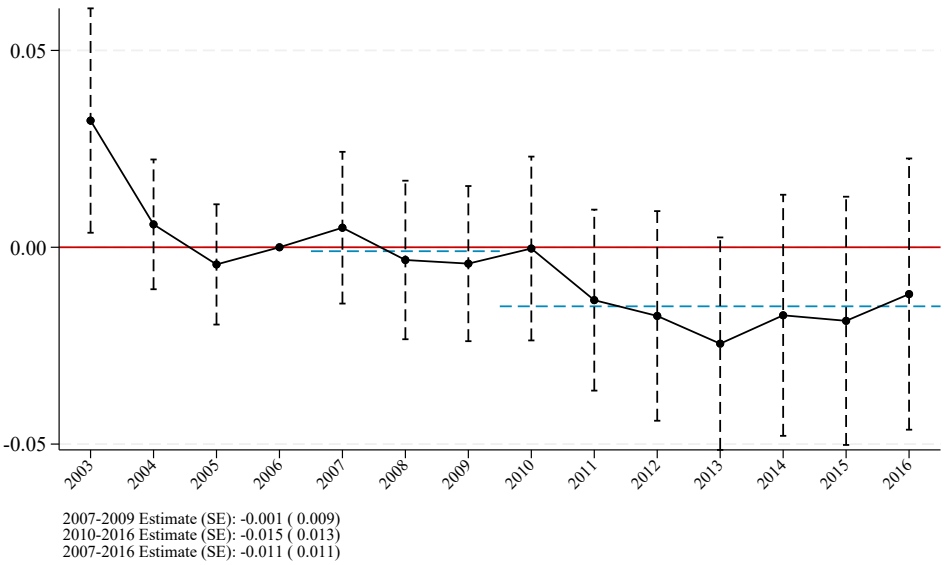
(c) Change in Crime Rate, 2006 vs 2007-2016 Average Recession Shock



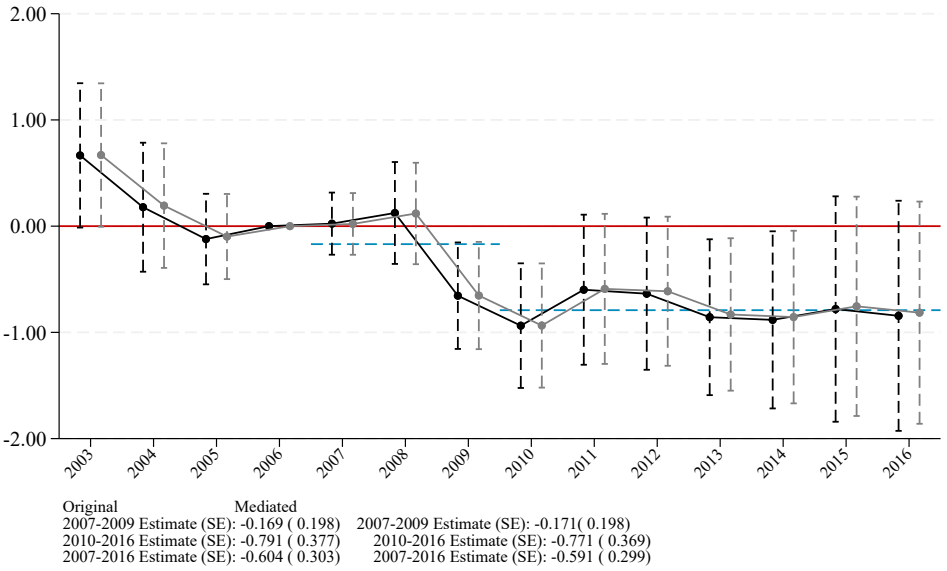
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure shows the cross-sectional relationship between the Great Recession shock and changes in crime at the CZ level. Crime is measured using total offense rate per 1,000 residents aged 15-65. The outcome is the difference between the average crime rate in the post period and the 2006 level: 2007–2009 in Figure A10a, 2010–2016 in Figure A10b, and 2007–2016 in Figure A10c. Each dot represents a ventile (20 equal-sized bins) of the shock distribution, plotting the 2006-population-weighted mean shock and the corresponding 2006-population-weighted mean change in crime within that ventile. The solid line reports fitted values from a weighted OLS regression of the CZ-level change in crime on the shock using the full CZ sample (weights: 2006 CZ population).

Figure A11: Effects of the Great Recession on Arrest Rates Controlling for Police Force

(a) Effects of the Great Recession on the Number of Sworn Police Officers per 1000 population



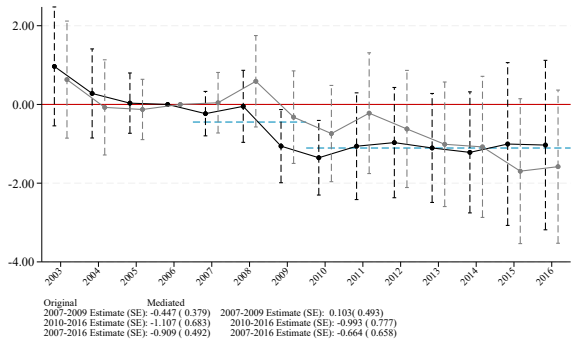
(b) Effects of the Great Recession on Crime Arrest Rates while Controlling for the Effects of the Police Force



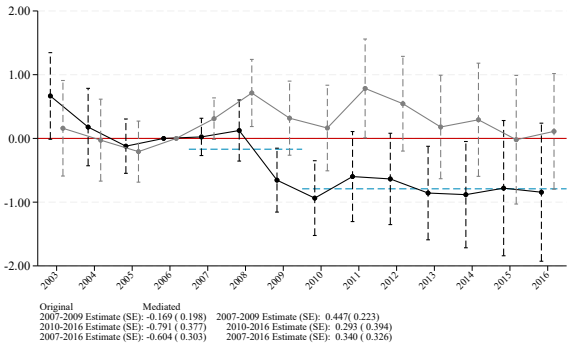
Notes: Figure A11a plots the yearly coefficients from equation (1) where the outcome variable is the number of sworn police officers per 1,000 individuals aged 15–65. Figure A11b plots the yearly coefficients from equation (1) (black solid line) and the corresponding coefficients obtained when controlling for changes in the police force (gray solid line). For the latter, we estimate $Crime_{c,t} = \gamma_t[Shock_c \cdot \mathbf{1}_{Year=t}] + \phi_t[Mechanism_Shock_c \cdot \mathbf{1}_{Year=t}] + \mu_c + \lambda_t + \varepsilon_{c,t}$, where $Mechanism_Shock_c$ denotes the 2006–2010 change in log of the number of sworn police officers per 1,000 individuals aged 15–65 in CZ. Officer measures are constructed at the CZ level and measured as the number of officers per 1,000 individuals aged 15–65. Caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the CZ level, and all regressions are weighted by 2006 CZ population to account for differences in population size.

Figure A12: Mediation Analysis

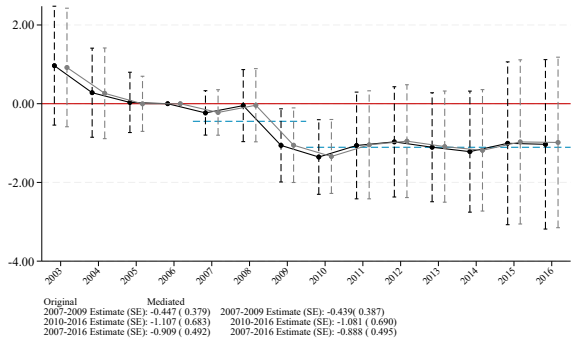
(a) Bankruptcy



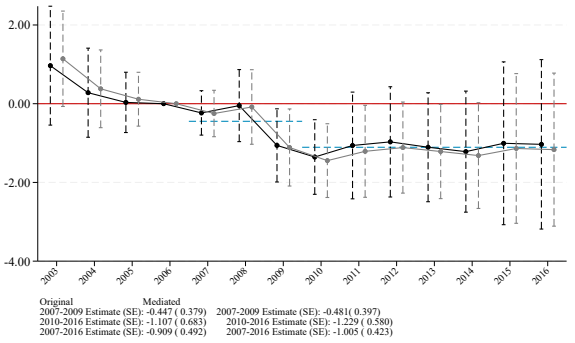
(b) HPI



(c) Home activity



(d) Traffic

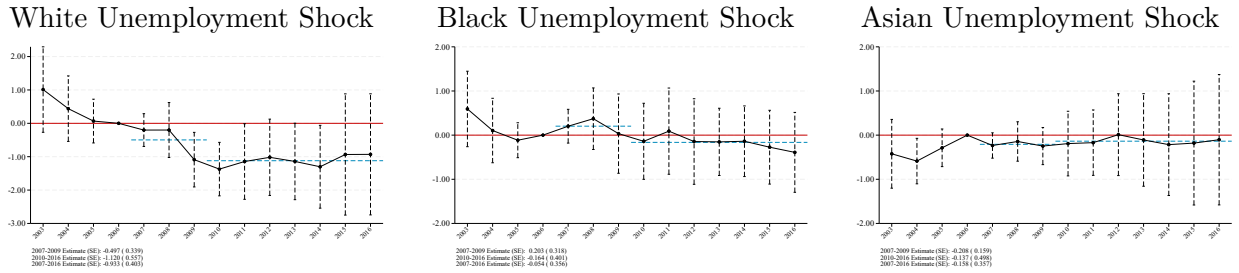


Data source: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS, Administrative Office of the US Courts, US Department of Transportation, The Federal Housing Finance Agency, ATUS.

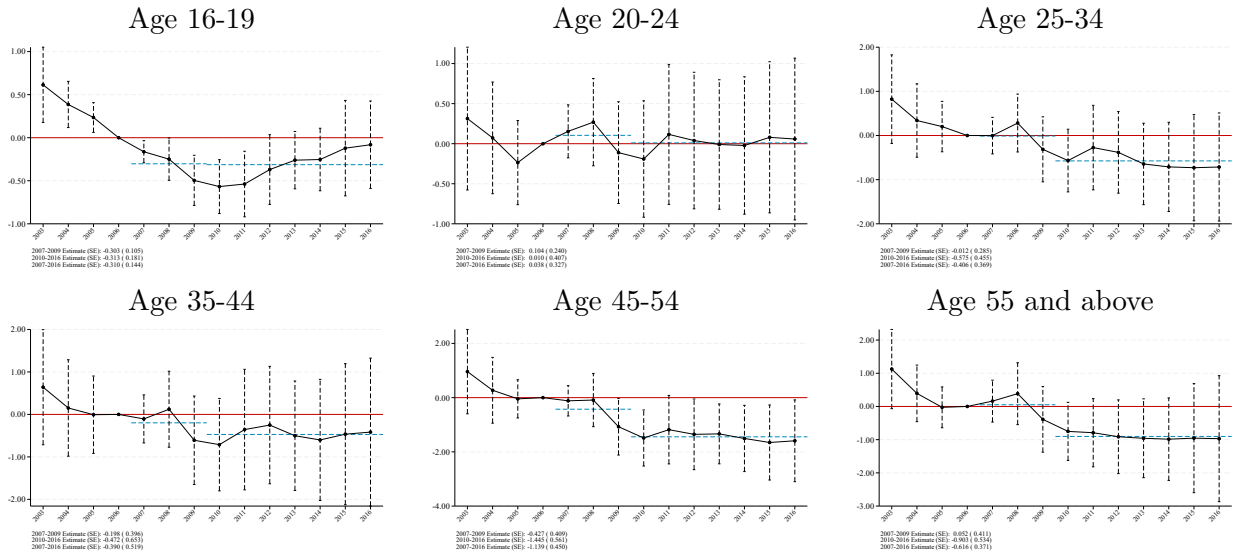
Notes: This figure plots the yearly coefficients β_t from equation (1) (black solid line) and the corresponding coefficients obtained when controlling for changes in each mechanism variable (gray solid line). For the latter, we estimate $Crime_{c,t} = \gamma_t[Shock_c \cdot \mathbf{1}_{Year=t}] + \phi_t[Mechanism_Shock_c \cdot \mathbf{1}_{Year=t}] + \mu_c + \lambda_t + \varepsilon_{c,t}$ where $Mechanism_Shock_c$ denotes the 2006–2010 change in the relevant mechanism variable in CZ/state c. For HPI and traffic, we use the negative of the 2006–2010 change; for bankruptcy, we use the raw change. Officer and HPI measures are constructed at the CZ level, while traffic and bankruptcy measures are constructed at the state level due to data limitations. Traffic, and bankruptcy variables are measured as the log of the number of each variable per 1,000 individuals aged 15–65. HPI is measured as 100 times the log of the HPI index for ease of interpretation. Caps indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016, respectively; these means, as well as the mean for the entire 2007–2016 period, are reported in the lower left-hand corner. Standard errors are clustered at the CZ level, and all regressions are weighted by 2006 CZ population from 15 to 65 to account for differences in population size.

Figure A13: Effects of the Great Recession by Demographic-Specific Recession Severity

(a) Measuring Recession Severity by Race



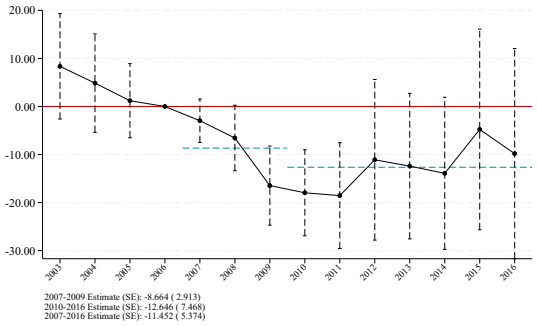
(b) Measuring Recession Severity by Age



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure reports coefficient estimates from equation (1). The dependent variable is the number of reported offenses per 1,000 individuals ages 15–65 in each commuting zone (CZ). We replace $SHOCK_c$ with the change in the demographic-specific unemployment rate. Figure A13a plots estimates using race-specific unemployment-rate changes, and Figure A13b plots estimates using age-specific unemployment-rate changes. Black dots denote point estimates and capped vertical lines indicate 95% confidence intervals. Blue dashed lines show the mean coefficient estimates for 2007–2009 and 2010–2016; these means, along with the mean for the full 2007–2016 period, are reported in the lower-left corner of each panel. Standard errors are clustered at the CZ level. All regressions are weighted by 2006 CZ population.

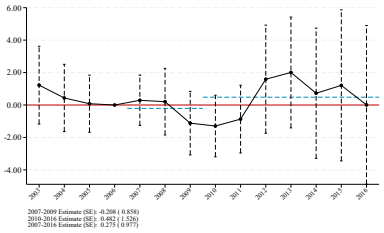
Figure A14: Effects of House Net Worth Shock on Crime Rate

(a) Total

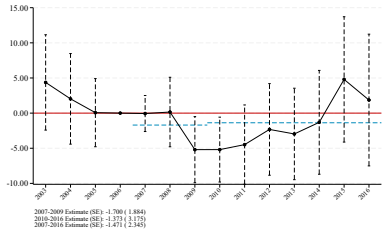


(b) Property Offenses

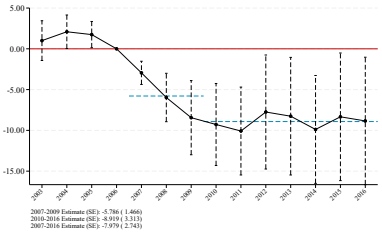
Burglary



Larceny theft

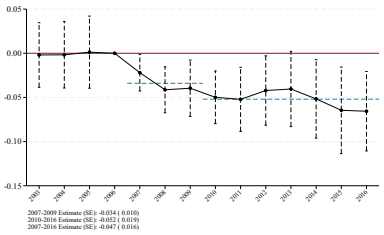


Motor vehicle theft

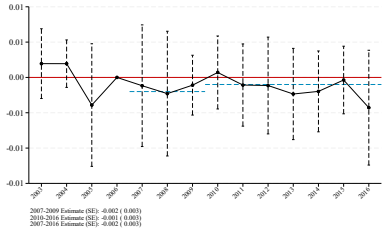


(c) Violent Offenses

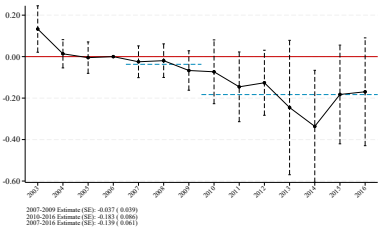
Murder



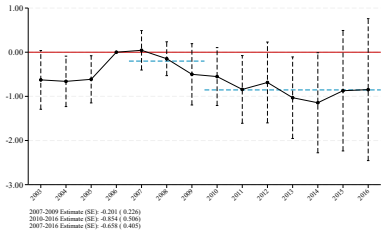
Manslaughter



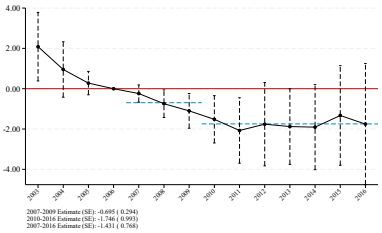
Rape



Robbery

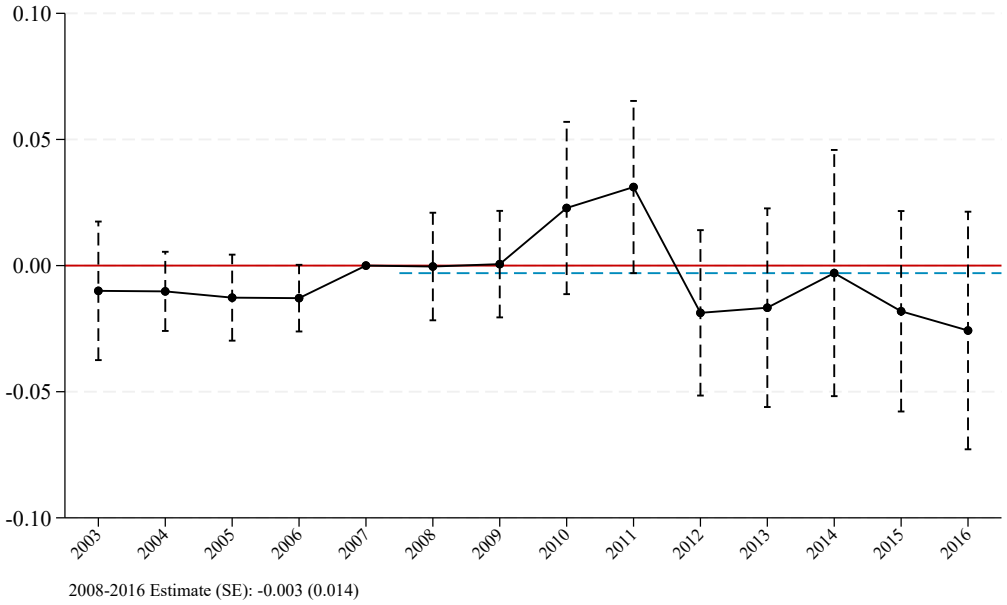


Aggravated Assault



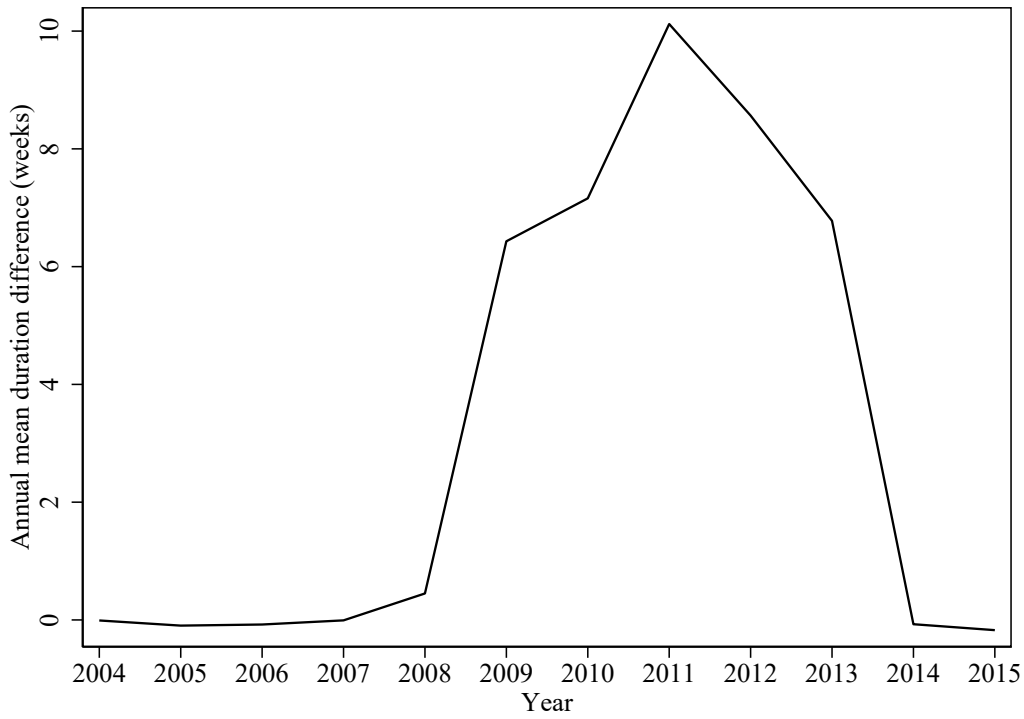
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, BLS, and CBP.
Notes: This figure plots the estimated coefficients β_t from the regression $y_{it} = \sum_{t \neq 2006} \beta_t \cdot (-\Delta HNW_i) \times \mathbf{1}[\text{Year} = t] + \alpha_i + \gamma_t + \varepsilon_{it}$, where y_{it} is the total crime rate per 1,000 population from 15 to 65 in county i and year t, and ΔHNW_i is the housing net worth shock from Mian and Sufi (2014). The treatment variable is multiplied by -1 so that a negative coefficient indicates that counties with a larger housing net worth decline experience a greater decrease in crime. The omitted year is 2006. All regressions include county and year fixed effects, and are weighted by 2006 county population from 15 to 65. The vertical bars represent 95 percent confidence intervals based on standard errors clustered at the state level. The horizontal dashed lines indicate the average estimated effect over the 2007–2009 period and the 2010–2016 period, respectively.

Figure A15: Effects of Neighborhood Stabilization Program on Crime Rates



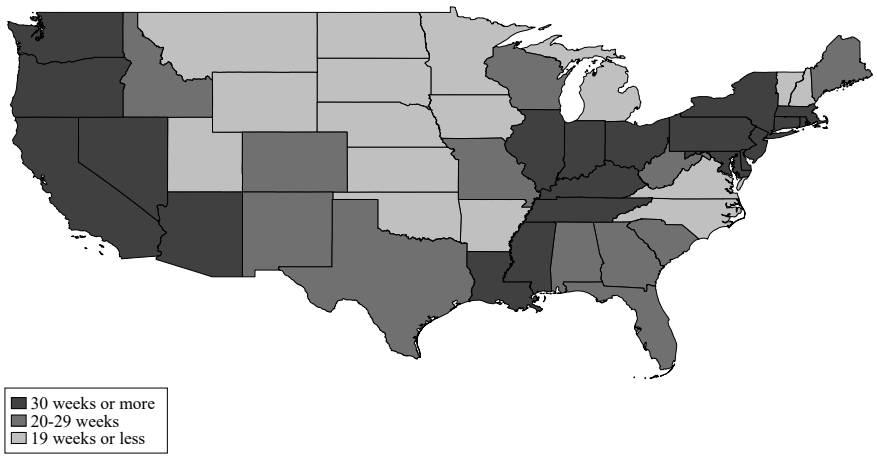
Data sources: FBI Uniform Crime Reporting and SEER, HUD Exchange Grantee Awards.
Notes: This figure presents the effects of the Neighborhood Stabilization Program (NSP) on total reported offense rates per 1,000 individuals aged 15–65 using a border-county-pair (BCP) design. The treatment variable measures NSP funding in millions of dollars. Each point represents the estimated coefficient from equation (6), with 2007 as the omitted reference year. Vertical bars indicate 95 percent confidence intervals. The red horizontal line marks zero; the blue dashed line indicates the average estimated effect over 2008–2016. The sample excludes the quartile of county pairs with the highest mean squared error in the pre-period (2003–2007). Regressions include county and pair-by-year fixed effects. Standard errors are two-way clustered at the state and state-pair levels.

Figure A16: Trend of the Within-Border County Pair Difference in UI Benefit Duration between High-Treatment and Low-Treatment Counties across State Borders



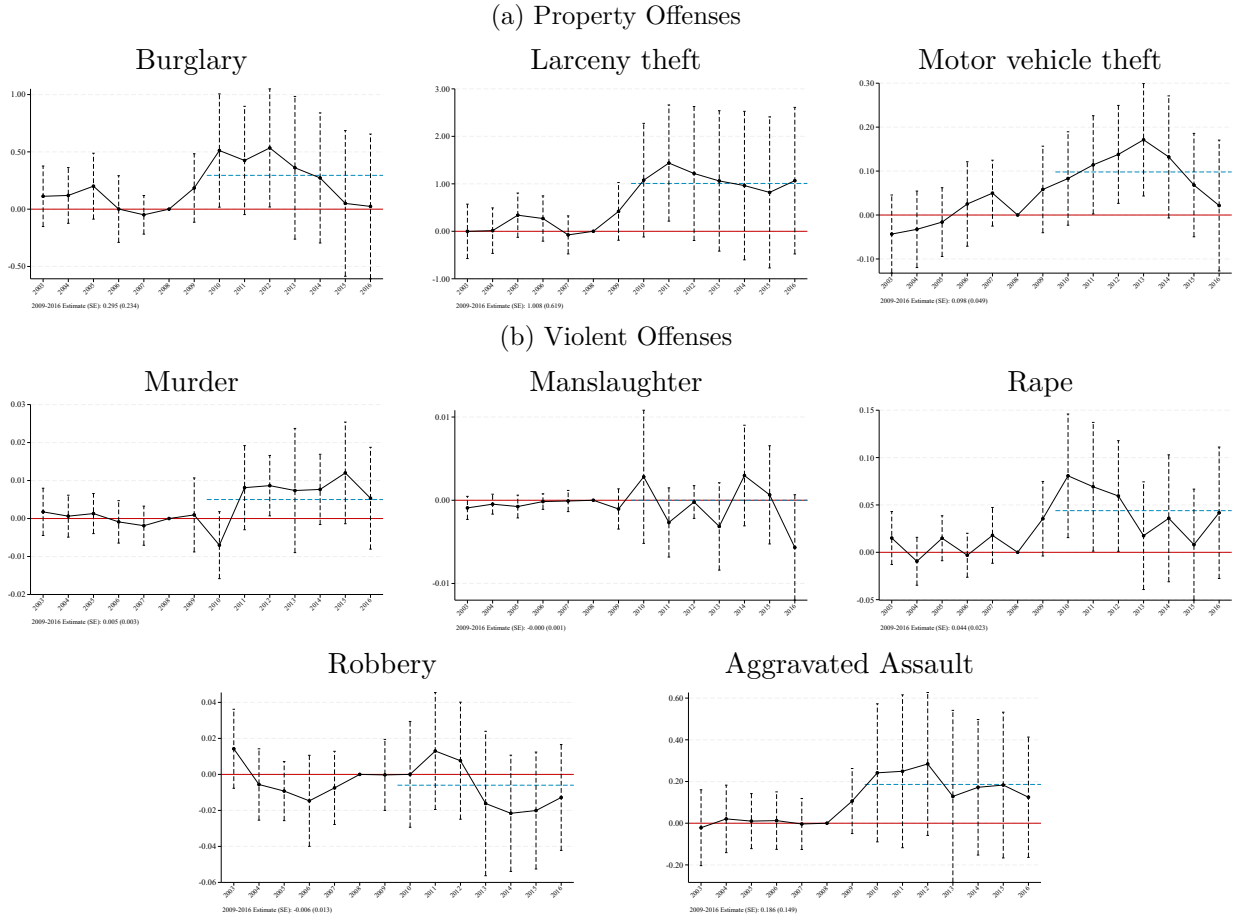
Notes: For each pair, we compute the difference in maximum UI benefit duration between the high-duration county and the low-duration county. We first average this difference across all county pairs within each month, and then take the annual mean of these monthly averages. "High" and "low" status is determined by comparing the average duration from 2008:11–2013:12 to the average duration from 2007:11–2008:10 and 2014:1–2014:12. County pairs with no differential treatment in this comparison are assigned arbitrarily to the "high" and "low" sets.

Figure A17: Reduction in UI Benefit Duration from the December 2013 Expiration of EUC



Source: US Department of Labor; Boone, Dube, Goodman, and Kaplan (2021).
Note: This figure displays the geographic variation in the reduction of maximum Unemployment Insurance (UI) benefit duration triggered by the expiration of the Emergency Unemployment Compensation (EUC) program in December 2013. The map categorizes states based on the number of benefit weeks lost: 19 weeks or fewer (light gray), 20–29 weeks (medium gray), and 30 weeks or more (dark gray). Data are reproduced from Boone, Dube, Goodman, and Kaplan (2021).

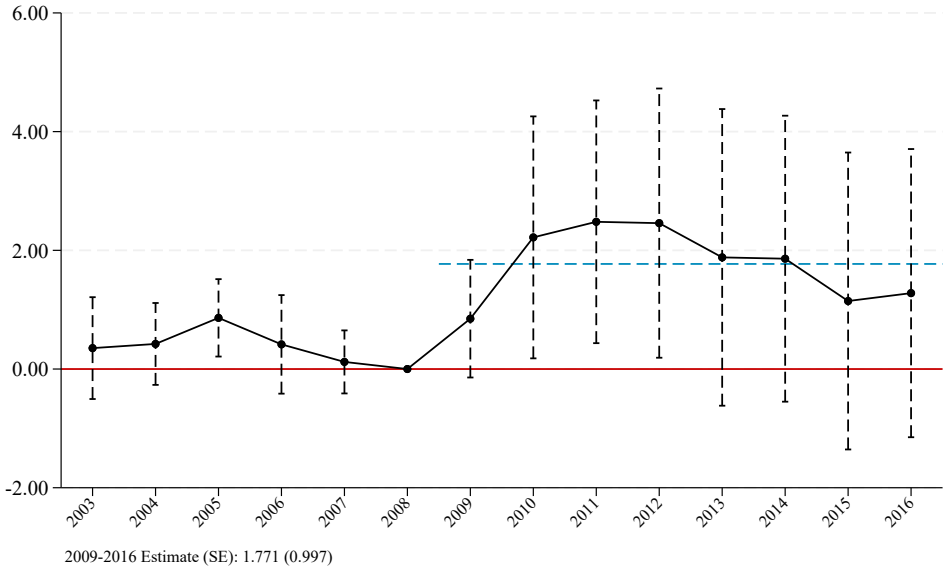
Figure A18: Effects of the UI Extension on Reported Offense Rates By Offense Type



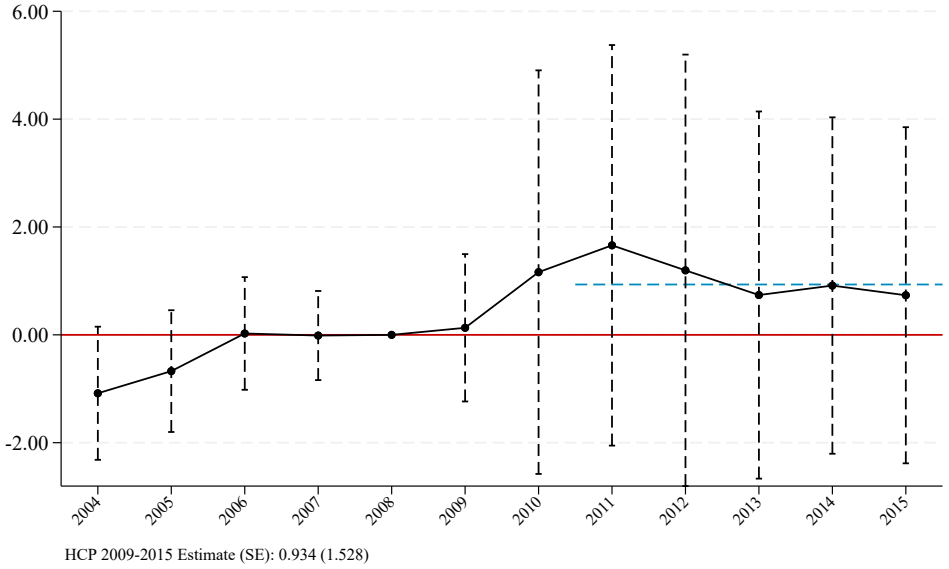
Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure shows the effects of the UI extension on all specific crime offenses available in the UCR data. In Figure A18a, the outcome variable is either the burglary rate, larceny theft or the motor vehicle theft rate (offenses per 1,000 individuals aged 15-65), specific categories of property crimes. In Figure A18b, the outcome variable is, in turn, the rate of murder, manslaughter, rape, robbery, or aggravated assault (offenses per 1,000 individuals between 15-65). Each outcome is regressed on interactions between year indicators and a continuous measure of UI generosity (scaled by 1/10). The omitted year is 2008. All specifications include border-county-pair fixed effects and pair-by-year fixed effects, with standard errors clustered two-way by state and state pair. The sample is restricted to county pairs with pre-trend mean squared error below the 75th percentile. The blue dashed horizontal line represents the average of the post-period coefficients (2009–2016), with the estimate and standard error reported at the bottom. Capped vertical lines denote 95% confidence intervals.

Figure A19: Effects of UI Benefit Extensions on Crime Rates Controlling for Offenders' Migration

(a) Controlling the distance between a pair of counties

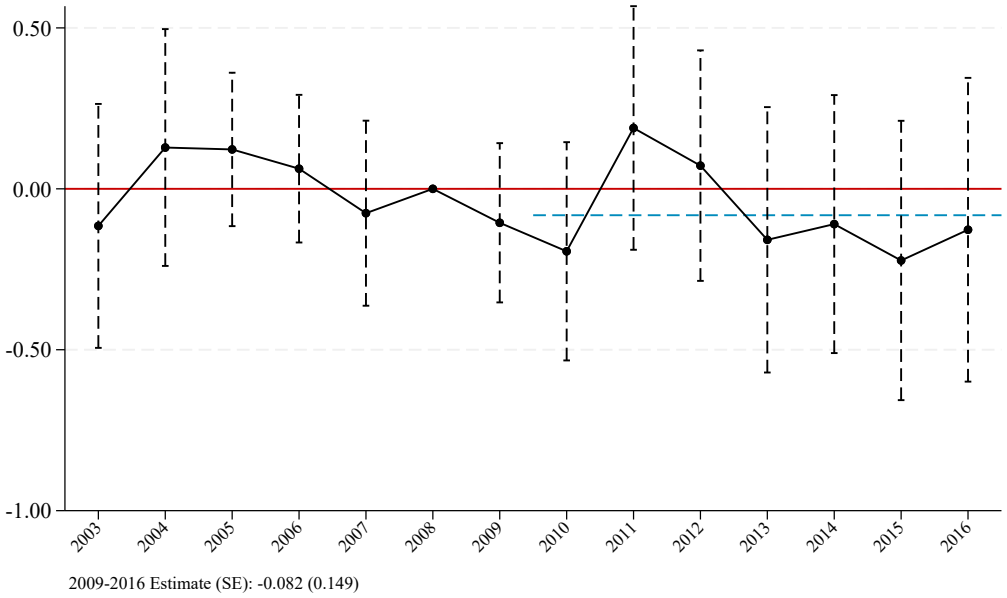


(b) Using HCP sample



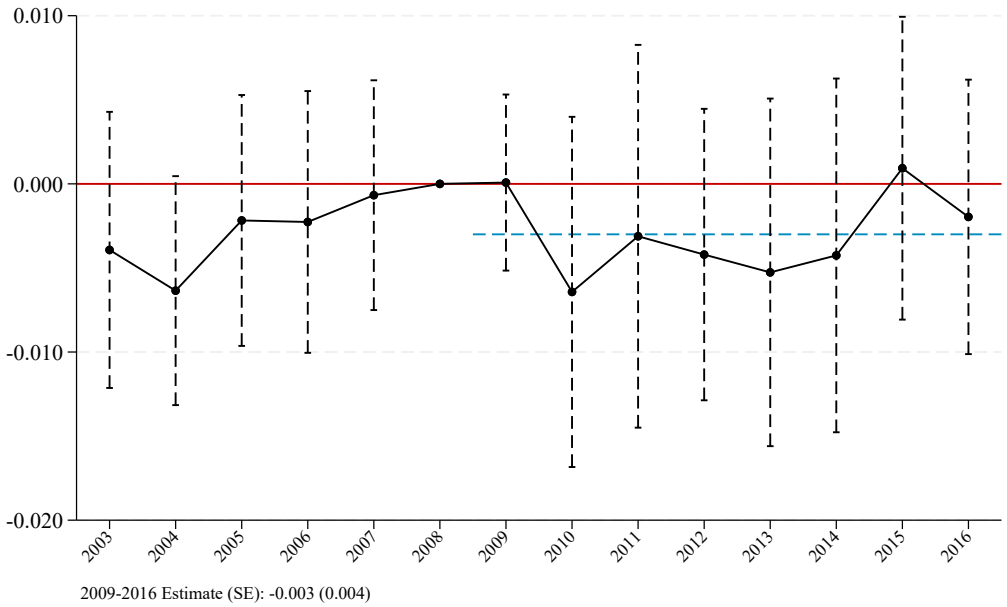
Sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure plots event study estimates of the effect of UI benefit extensions on total crime rates per 1,000 population between 15-65, relative to 2008. Figure A19a reports results from the PT-trimmed border county sample, controlling for the interaction between UI benefit extension, year, and distance to the border. Figure A19b uses the Hinterland County Pair (HCP) sample to address potential spillovers from offenders' migration. The blue dashed line indicates the average post-2009 effect. Vertical bars represent 95 percent confidence intervals, clustered two-way at the state and state-pair level.

Figure A20: Effects of the UI Extension on Arrest Rates



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure plots the set of βs coefficients from equation (6) estimated over the set of border county pairs in the PT-trimmed sample following Boone et al (2021). The outcome is either arrests reported per 1,000 individuals aged 15-65. The average treatment intensity is a continuous measure defined as the average unemployment insurance duration during the treatment period (2008:11–2013:12) minus average duration from the 12 months prior (2007:11–2008:10), divided by 10. The caps indicate the 95 percent confidence interval, standard error clustered at the state and state-pair level. PT-trimming removes the quartile of county pairs with the highest mean squared error in crime rate between 2003 and 2008 (after partialling out a fixed level difference). Boone et al (2021), using the PT-trimmed sample, leveraged the variation in unemployment insurance duration in the Great Recession between adjacent counties located in neighboring states to examine the effect of unemployment insurance expansion on the aggregate employment.

Figure A21: Effects of UI Benefit Extensions on Crime Rates by Distance between Two Border Counties' Centroids



Data sources: 2003–2016 FBI Uniform Crime Reporting, SEER, and BLS.
Notes: This figure reports the coefficients on the interaction between the UI benefit extension and the distance between the centroids of the paired border counties. The specification controls for the main effect of the UI extensions, as well as county-pair and pair-by-year fixed effects. The dependent variable is the total crime rate per 1,000 population. The sample uses the PT-trimmed border county pairs, excluding the quartile with the highest pretreatment mean squared error. The blue dashed line indicates the average interaction effect from 2009 to 2016. Vertical bars represent 95 percent confidence intervals, robust to two-way clustering at the state and state-pair level.

Table A1: Summary Statistics for 2006

	Mean	(SD)
Any Crime Rate	54.23	(16.41)
Property Crime Rate	47.39	(14.57)
Violent Crime Rate	6.84	(2.87)
Arrest Rate	8.45	(4.07)
Arrest Rates by demographic groups		
By race		
White	32.84	(17.02)
Black	78.89	(48.64)
Others	14.26	(25.38)
By gender		
Male	81.57	(39.31)
Female	24.60	(13.25)
By age		
16-19	112.39	(61.10)
20-29	91.90	(45.96)
30-39	54.40	(28.33)
40-49	39.20	(20.39)
50-59	15.89	(8.54)
60-64	6.25	(3.67)

Notes: The unit of observation is the commuting zone (CZ) in 2006. "Crime Rate" reports total, property, and violent offense rates per 1,000 residents between 15 and 65. "Arrest Rate" reports total, property, and violent arrest rates per 1,000 residents between 15 and 65. All entries are population-weighted means across CZs with standard deviations in parentheses. Arrest rates by demographic group are computed as arrests of the corresponding group per 1,000 group population (weights = group population in CZ). Age-specific arrest rates aggregate single-year ages into 16–19, 20–29, 30–39, 40–49, 50–59, and 60–64. "Other" race combines Asian and American Indian.

B Derivation of the Welfare Cost Formula

This appendix provides the detailed algebraic derivation of equation (10) in Section 6.1, which characterizes the welfare cost of the Great Recession incorporating both consumption and crime effects. We also present an extended version of the framework that incorporates mortality effects, following Finkelstein et al. (2025). The key identifying assumption is additive separability between consumption utility and crime disutility, which allows the welfare contributions of each channel to be computed as distinct, additive components.

Setup

The per-period utility function is additively separable:

$$u(c_t, \pi_t) = v(c_t) - h(\pi_t), \tag{B.1}$$

where $v(\cdot)$ represents utility from consumption (increasing and concave) and $h(\cdot)$ captures disutility from crime exposure (increasing and convex). Both functions are twice differentiable. We compare two steady states: a "normal" period and a "recession" period. We denote consumption and crime rates during the normal (recession) period as c_N (c_R) and π_N (π_R), respectively.

Baseline Derivation (Without Mortality)

In the baseline framework used in Section 6, we abstract from mortality and define the welfare cost of the Great Recession as the compensating variation in consumption, Δ^*, satisfying the indifference condition:

$$v((1 + \Delta^*)c_R) - h(\pi_R) = v(c_N) - h(\pi_N). \tag{B.2}$$

We define the welfare cost function $W(\Delta)$ as the difference in per-period utility between the compensated recession state and the normal state:

$$W(\Delta) = [v((1 + \Delta)c_R) - h(\pi_R)] - [v(c_N) - h(\pi_N)]\,. \tag{B.3}$$

The optimal compensating variation Δ^* satisfies $W(\Delta^*) = 0$. Using a first-order Taylor expansion of $W(\Delta)$ around $\Delta = 0$:

$$W(\Delta) \approx W(0) + W'(0) \cdot \Delta. \tag{B.4}$$

Setting $W(\Delta^*) = 0$ and rearranging yields:

$$\Delta^* = -\frac{W(0)}{W'(0)}. \tag{B.5}$$

Computing $W(0)$. Evaluating equation (B.3) at $\Delta = 0$:

$$W(0) = [v(c_R) - h(\pi_R)] - [v(c_N) - h(\pi_N)]\,.$$

We parameterize recession effects as percentage deviations from normal-period values:

$$c_R = c_N(1-\delta), \qquad \pi_R = \pi_N(1-\varphi),$$

where $\delta > 0$ is the percentage decline in consumption and $\varphi > 0$ is the percentage decline in crime during the recession. Using first-order Taylor expansions of $v(c_R)$ around c_N and $h(\pi_R)$ around π_N:

$$v(c_R) - v(c_N) \approx v'(c_N) \cdot (c_R - c_N) = -\delta \cdot c_N \cdot v'(c_N), \tag{B.6}$$

$$h(\pi_R) - h(\pi_N) \approx h'(\pi_N) \cdot (\pi_R - \pi_N) = -\varphi \cdot \pi_N \cdot h'(\pi_N). \tag{B.7}$$

Substituting into the expression for $W(0)$:

$$W(0) \approx -\delta \cdot c_N \cdot v'(c_N) + \varphi \cdot \pi_N \cdot h'(\pi_N). \tag{B.8}$$

The first term is negative (the consumption loss reduces utility) and the second term is positive (the crime reduction increases utility).

Computing $W'(0)$. Differentiating equation (B.3) with respect to Δ:

$$W'(\Delta) = v'((1+\Delta)c_R) \cdot c_R.$$

Evaluating at $\Delta = 0$ and using the approximation $c_R \approx c_N$ (valid for small δ):

$$W'(0) \approx v'(c_N) \cdot c_N. \tag{B.9}$$

Solving for Δ^*. Substituting (B.8) and (B.9) into (B.5):

$$\Delta^* \approx \delta - \varphi \cdot \frac{\pi_N \cdot h'(\pi_N)}{c_N \cdot v'(c_N)}. \tag{B.10}$$

We define the consumption elasticity with respect to crime as:

$$\epsilon_c^\pi \equiv \frac{\pi_N \cdot h'(\pi_N)}{c_N \cdot v'(c_N)},$$

which measures how much consumption an individual would forgo to avoid a one percent increase in crime. This yields:

$$\Delta^* \approx \delta - \varphi \cdot \epsilon_c^\pi. \tag{B.11}$$

This is equation (10) in the main text. The welfare cost of the Great Recession equals the consumption loss (δ) minus the welfare gain from reduced crime ($\varphi \cdot \epsilon_c^\pi$). Substituting the calibrated values from Section 6.2 ($\delta = 5.5$ percent, $\varphi = 5.1$ percent, $\epsilon_c^\pi = 0.065$), the welfare cost accounting for crime reductions is $\Delta^* = 5.17$ percent of annual consumption ($= 5.5 - 5.1 \times 0.065$), which is 0.33

percentage points (approximately 6 percent) below the consumption-only estimate of 5.5 percent.

Extended Derivation (Incorporating Exogenous Mortality)

One can extend this framework to incorporate mortality by introducing endogenous survival probability, as in Finkelstein et al. (2025). The agent's lifetime utility, weighted by survival probability, is given by equation (B.12). For tractability, the derivation below abstracts from time discounting by setting $\beta = 1$, so that lifetime utility reduces to per-period utility multiplied by remaining life expectancy. As β appears symmetrically in both the numerator and denominator of the compensating variation formula, it cancels to a first-order approximation and does not affect equation (B.16).

$$U = E\left[\sum_t \beta^t \, S(m(t)) \cdot u(c_t, \pi_t)\right], \tag{B.12}$$

where β is the discount factor, $S(m(t)) = \prod_{\tau \le t}(1 - m(\tau))$ is the cumulative survival probability, and $m(t)$ is the mortality rate at time t. We denote remaining life expectancy during the normal (recession) period as T_N (T_R), with $T_R = T_N(1 + d_T)$, where $d_T > 0$ represents the percentage increase in life expectancy during the recession.

The indifference condition becomes:

$$[v((1 + \Delta^*)c_R) - h(\pi_R)] \cdot T_R = [v(c_N) - h(\pi_N)] \cdot T_N. \tag{B.13}$$

Following the same Taylor expansion approach as above, we define:

$$W(\Delta) = [v((1 + \Delta)c_R) - h(\pi_R)] \cdot T_R - [v(c_N) - h(\pi_N)] \cdot T_N, \tag{B.14}$$

and solve $W(\Delta^*) = 0$. The derivation proceeds as in the baseline case, with the additional step that T_R appears in both $W(0)$ and $W'(0)$. Denoting per-period utility in the normal state as $u_N = v(c_N) - h(\pi_N)$ and using the approximation $W'(0) \approx v'(c_N) \cdot c_N \cdot T_N$, we obtain:

$$\Delta^* \approx \delta - d_T \cdot \frac{u_N}{c_N \cdot v'(c_N)} - \varphi \cdot \epsilon_c^{\pi}. \tag{B.15}$$

Following Finkelstein et al. (2025), the ratio $u_N/(c_N \cdot v'(c_N))$ can be expressed as $VSLY/c_N$, where $VSLY$ denotes the value of a statistical life year. This yields:

$$\Delta^* \approx \delta - d_T \cdot \frac{VSLY}{c_N} - \varphi \cdot \epsilon_c^{\pi}. \tag{B.16}$$

The welfare cost of the Great Recession equals the consumption loss (δ) minus the welfare gain from reduced mortality ($d_T \cdot VSLY/c_N$) minus the welfare gain from reduced crime ($\varphi \cdot \epsilon_c^{\pi}$). When mortality is exogenous ($d_T = 0$), the mortality term drops out and equation (B.16) reduces to equation (B.11), which is equation (10) in the main text. Because we do not incorporate mortality in our baseline calibration, we use the simplified two-channel version in Section 6.

C Welfare Calculations of UI Benefit Extensions

This appendix derives the welfare formula underlying Section 6.3. Because this analysis employs the Baily–Chetty sufficient-statistics framework (Baily, 1978; Chetty, 2008; Schmieder et al., 2012) rather than the Krebs (2007) model used in Section 6.2, we present the two assessments separately and compare their magnitudes in consumption-equivalent units.

Augmented Welfare Formula

The crime externality enters the social welfare function as an additively separable term. This structure reflects the economic nature of crime as a third-party externality: UI benefits affect the consumption and labor supply of the unemployed worker, but the induced changes in crime impose costs on potential victims who are not party to the insurance–incentive trade-off. In standard public economics, externalities augment the planner's objective as separate terms because they represent social costs not internalized by the agent whose behavior is being evaluated (see the treatment of fiscal externalities in Chetty, 2008). The additive separability assumption in equation (B.1) provides the microfoundation: because the representative agent's disutility from crime exposure, $h(\pi_t)$, does not interact with consumption utility, $v(c_t)$, the marginal social cost of crime can be expressed independently of the consumption margin.

The standard sufficient-statistics formula for UI duration extensions characterizes optimal generosity by balancing the consumption-smoothing value of an additional week of benefits against the fiscal externality from longer unemployment spells (Chetty, 2008; Schmieder et al., 2012). Our estimates identify an additional channel: UI benefit extensions raise crime, generating costs not captured by the standard insurance–incentive trade-off. Augmenting the Schmieder et al. (2012) formula with this externality yields equation (11) in the main text, reproduced here for convenience:

$$\frac{dW}{dP} = \underbrace{I(P)}_{\text{insurance value}} - \underbrace{M(P)}_{\text{fiscal externality}} - \underbrace{\kappa \frac{d\pi}{dP}}_{\text{crime externality}}, \tag{C.1}$$

where $I(P) = s(P) \cdot b \cdot [u'(c_u^e) - u'(c_e)]$ is the consumption-smoothing value of the marginal week of benefits, with $s(P)$ denoting the probability of remaining unemployed until week P, b the weekly benefit, c_u^e consumption at benefit exhaustion, and c_e consumption when employed; $M(P)$ is the fiscal externality arising from UI-induced changes in unemployment duration scaled by the marginal cost of public funds; κ is the marginal social cost of crime; and $d\pi/dP$ is the causal effect of benefit duration on crime incidence.[25]

Because our BCP estimates capture the average effect of a large, discrete extension (from 26 to as many as 99 weeks) rather than an infinitesimal change, equation (C.1) serves as a linear approximation to the welfare effect of the observed policy change.[26]

[25] We use π for crime incidence, consistent with Appendix B.

[26] In recessions, Landais et al. (2018) argue that part of the behavioral response represents a search-congestion externality: when jobs are scarce, UI-induced reductions in search effort are less socially costly because individual

Consumption-Equivalent Crime Cost

To express the crime externality in consumption-equivalent units, we connect κ in equation (C.1) to the utility primitives of Appendix B. Under the additive separability assumption in equation (B.1), the marginal disutility of crime relative to the marginal utility of consumption is $\kappa = h'(\pi_N)/v'(c_N)$. Using the definition of the consumption elasticity with respect to crime, $\epsilon_c^\pi \equiv \pi_N\, h'(\pi_N)/[c_N\, v'(c_N)]$, from equation (B.11), we have $\kappa/c_N = \epsilon_c^\pi/\pi_N$. The consumption-equivalent crime cost of a benefit extension, expressed as a share of baseline consumption, is therefore:

$$\frac{\kappa}{c_N} \cdot \frac{d\pi}{dP} \;=\; \epsilon_c^\pi \cdot \frac{d\ln \pi}{dP}. \tag{C.2}$$

Our BCP estimates indicate that a 10-week extension in maximum UI benefit duration increases total reported offenses by approximately 3 percent (Section 5.2.2).[27] Using $\epsilon_c^\pi = 0.065$ from Mejía and Restrepo (2016), as calibrated in Section 6.2, the per-period consumption-equivalent crime cost is approximately 0.2 percent of annual consumption:[28]

$$0.031 \times 0.065 \;\approx\; 0.002. \tag{C.3}$$

Comparison with Consumption-Smoothing Gains

To gauge whether this magnitude is economically meaningful, we compare it to the consumption-smoothing gains estimated by Ganong and Noel (2019), who express welfare changes as a percentage of permanent lifetime income. To put the two quantities on a common footing, we amortize the per-period crime cost over a representative remaining lifetime. Let $r = 0.03$ denote the annual discount rate and $T_{\text{ext}} = 5$ the number of years during which benefits were actively extended (2009–2013). The present value of the cumulative crime cost, evaluated at the start of the extension period, is:

$$\text{PV}_{\text{crime}} = 0.002 \times \sum_{t=1}^{5} (1+r)^{-t} = 0.002 \times 4.580 \approx 0.0092. \tag{C.4}$$

Annualizing over a remaining lifetime of $T = 40$ years:

$$\text{Annualized crime cost} = \frac{0.0092}{\sum_{t=1}^{40}(1+r)^{-t}} = \frac{0.0092}{23.115} \approx 0.0004, \tag{C.5}$$

or approximately 0.04 percent of annual consumption.

search partly reallocates a fixed stock of vacancies. This "rat-race" channel would attenuate $M(P)$, increasing the net welfare gain from extensions. Our baseline calculation abstracts from this channel, which works against our finding that the crime externality offsets part of the consumption-smoothing gain.

[27] This figure is the average BCP coefficient over 2009–2016. During the active extension period (2009–2013), the crime effect was likely larger, as Figure 6 shows diminishing effects after the extensions expired. Using the full-period average provides a conservative estimate.

[28] As noted in Section 6.2, Mejía and Restrepo (2016) estimate this elasticity for property crime. To the extent that the non-monetary costs of crime (fear, avoidance behavior) exceed the consumption response, $\epsilon_c^\pi = 0.065$ likely understates the true social cost.

Ganong and Noel (2019) estimate that a one-month UI duration extension generates a net welfare gain of 0.016 percent of lifetime income after accounting for the fiscal externality. Scaling to a 10-week extension yields approximately 0.037 percent.[29] The annualized crime externality of 0.04 percent is of the same order of magnitude, suggesting that the crime channel may substantially offset the consumption-smoothing value of UI extensions.

This comparison is necessarily approximate given the differences in welfare metrics. Two additional considerations affect its interpretation, both working against our finding. First, Ganong and Noel's (2019) estimates come from post-recession data (2014–2016), when the labor market had recovered; the consumption-smoothing value was likely higher during the recession itself. Second, Landais et al. (2018) and Acosta et al. (2023) provide evidence that the fiscal externality is attenuated in recessions due to search congestion, further raising the net welfare gain from extensions. Both considerations imply that the Ganong and Noel (2019) estimate provides a conservative lower bound on the consumption-smoothing value during the Great Recession, so the proportional offset from the crime externality should be interpreted as an upper bound.

[29] This linear scaling assumes constant marginal returns to duration extensions. Because the marginal consumption-smoothing value likely declines at longer durations, this assumption probably overstates the gain.