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Abstract. Leveraging county-level variation in exposure to industry-specific foreign-based robotics shocks, this study is the first to explore the relationship between U.S. robotics expansions and crime. Instrumental variables estimates show that a 10 percent increase in robotics exposure led to a 0.2 to 0.3 percent increase in property crime arrests. In contrast, we find little evidence of a relationship between robotics expansions and violent crime. Our estimates are consistent with robotics-induced declines in employment and earnings among low-skilled manufacturing workers. A back-of-the-envelope calculation suggests that during the period over which robotics exposure induced adverse employment effects, such exposure generated approximately $322 million (2024$) in additional crime costs nationally.

JEL Codes: D24, K42

Keywords: robots; crime; automation; labor supply; employment

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The march of technology is both a blessing and a curse. It has led to job loss and increased crime, but it also has the power to create new opportunities and drive economic growth.

– U.S. President William J. Clinton

1. Introduction

The current robotics-based technological revolution is characterized by the development of artificial intelligence (AI) and greater automation in the production process (Acemoglu and Restrepo 2020; Autor et al. 2015).[1] According to the United States Government Accountability Office (2022), up to 47 percent of jobs are at risk of automation in the near future. One projection estimates that, by 2030, roughly 375 million jobs worldwide could be automated (McKinsey Global Institute 2017). Economists and policymakers have expressed concerns that these innovations may lead to the permanent displacement of workers, from those employed in low-skilled jobs to laborers in the higher-skilled science and technology sectors (Muro et al. 2019).[2]

Recent research by Acemoglu and Restrepo (2020) provides strong evidence that the expansion of robotics in the United States has led to declines in employment and wages, particularly in manufacturing. They find that one additional U.S. robot per 1,000 workers leads to a 0.5-1.5 percentage-point decline in the local employment-to-population ratio of adults aged 18-64 and a 0.4 to 0.8 percent decline in their wages. Given these findings, and a large literature that has established a link between criminal behavior and the labor market,[3] exploring the relationship between robotics

[1] The effect of skill-biased technological change on labor market outcomes has long been a topic of interest among economists (see, e.g., Berman et al. 1993; Card and DiNardo 2002; Acemoglu 2003; Autor et al. 2015; Goos and Manning 2009). In fact, the issue of technology-driven unemployment was prominently raised by both Smith (1776) and Keynes (1930).

[2] This displacement also extends to higher education, where work-related tasks by faculty can be replaced by robotics, AI, and employer ownership of educational-related public goods (i.e., electronic teaching materials).

[3] See, for instance, Raphael and Winter-Ebmer 2001; Gould et al. 2002; Machin and Meghir 2004; Levitt 2004; Oster and Agell 2007; Lin 2008; Mustard 2010; Schnepel 2018; Fone et al. 2023). Other important external social costs that flow from diminished attachment to the labor market include reduced civic engagement (Alesina and La Ferrara 2000; Putnam 2000), greater government dependency (Blank 1989; Moffitt 2002), and increases in racial animus (Anderson et al. 2020).

expansion and crime is important with respect to social welfare implications.[4] That is, crime-related spillovers capture important external costs or benefits of robotics expansion. Because crime is estimated to generate $4.71 to $5.76 trillion in social costs each year in the United States (Anderson 2021), even small robotics-induced changes in crime could have important social welfare implications.

This study is among the first to study the relationship between local robotics expansions and crime and the first to do so in the context of the U.S. Our analysis spans the period from the early 1990s through 2010, a period that coincides with large automation-driven declines in manufacturing employment (Acemoglu and Restrepo 2020). We capture plausibly exogenous variation in robotics exposure driven by shifts in the global technology frontier by leveraging (1) pre-treatment differences in county-level industrial composition and (2) temporal variation in industry-specific adoption of robotics in the European Union (EU), which is used as an instrument for U.S. robotics expansion.

Reduced-form estimates indicate that potential exposure to one additional robot per 1,000 U.S. workers is associated with a 0.059 increase in property crime arrests per 1,000 adults, or about 1.2 percent. We find no evidence that robotics expansion is associated with arrest rates for violent crimes. These findings are consistent with an income-generating motive for property crime, which could be used to replace income from job loss (Raphael and Winter-Ebmer 2001; Levitt 2004; Machin and Meghir 2004; Oster and Agell 2007; Lin 2008; Mustard 2010). Importantly, event-study

[4] According to the International Federation of Robotics (IFR), an industrial robot constitutes a "multipurpose, automatically controlled, and reprogrammable machine" (IFR 2014). These machines are fully autonomous, obviating the need for human operators, and are programmable to perform an array of manual tasks, ranging from welding and painting to assembly, material handling, and packaging. In contrast to specialized equipment like textile looms, elevators, and cranes, which are designed for a unique function and frequently necessitate human operation, industrial robots offer the flexibility of reprogramming for various tasks. Industrial robots have disproportionately affected manufacturing, including the automotive sector, machinery, electronics, and food processing.

analyses show no evidence of systematic pre-treatment trends, consistent with the notion that the parallel trends assumption holds.

Instrumental variable (IV) estimates, where we use EU robotics expansion to instrument for robotics expansion across similar industries in the U.S., suggest that actual exposure to one additional U.S. robot per 1,000 workers led to a 4 to 5 percent increase in property crime arrests. This translates to a property crime arrest elasticity with respect to robotics exposure of approximately 0.02 to 0.03. Moreover, the estimated effect sizes are consistent with the robotics-induced employment and wage declines found by Acemoglu and Restrepo (2020) and previously documented arrest elasticities with respect to employment and wages. Intriguingly, our crime estimates differ sharply from those obtained by Fang and Miao (2025) who studied the Chinese experience with robotics expansions in the mid-late 2010s and found that robots and low-skilled labor were *complements* in production.

Finally, auxiliary analyses using detailed policing data from the Law Enforcement Management and Administrative Statistics (LEMAS), the Census of State and Local Law Enforcement Agencies (CSLLEA), and data on criminal incidents from the National Incident-Based Reporting System (NIBRS) suggest that our results cannot be explained by systematic changes in policing practices. A back-of-the-envelope calculation suggests that during the period over which expansions in robotics exposure induced adverse employment effects, such exposure generated approximately $322 million (2024$) in additional crime costs nationally.

2. Background

2.1. Employment and Wage Effects of U.S. Robotics Expansion

While skill-biased technological change has been studied by economists for decades (Berman et al. 1998; Card and DiNardo 2002; Acemoglu 2003; Autor et al. 2015; Goos and Manning 2009), the rise of automation and artificial intelligence[5] has received special attention, in part due to concerns that without substantial investments in new skills that are complementary to emerging technologies, worker displacement may be permanent (Goldsmith and Casey 2022).

There is strong evidence that the U.S. robotics expansion reduced employment and wages among adults in the period prior to 2010. In their seminal paper, Acemoglu and Restrepo (2020) use Census data from 2004-2010 and a shift-share IV strategy and find that one additional U.S. robot per 1,000 U.S. workers diminishes the employment-to-population ratio among 18-64-year-olds by 0.5 to 1.5 percentage points and reduces average wages by about 1 percent. This result is consistent with job displacement, suggesting that many affected workers (particularly in the manufacturing sector) serve as substitutes for robots.[6] In the post-2010 period, there is much less evidence of a robotics-induced adverse employment effect (Chung and Lee 2023), which may be explained by structural shifts toward service-oriented industries (Autor 2019), a growing complementarity between automation and human labor (Acemoglu and Restrepo 2023), and increased reskilling and workforce adaptation (Bessen 2019).[7]

Researchers have also studied the relationship between robots and labor market outcomes in non-U.S. countries. For instance, Dauth et al. (2019) find that for each robot per 1,000 workers adopted in Germany two manufacturing jobs are lost. Using data for 16 countries in the European Union, Bachmann et al. (2022) find that robotics expansion modestly reduces the rate of job

[5] Robots used before 2010 were generally not powered by artificial intelligence (AI) in the way we conceptualize AI today (Nature 2024)

[6] Relatedly, Anelli et al. (2024) finds that U.S. robotics expansion reduced the male-female employment and wage gaps. However, this finding is explained, at least in part, by a heavier concentration of males employed in industries that see larger employment displacement effects (e.g., the automobile sector). Moreover, men who are employed in manual-labor intensive industries may find that their skills do not transfer readily to female-dominated industries, such as certain jobs in the service sector.

[7] Chung and Lee (2023) investigate the effects of industrial robots on U.S. labor markets during the period 2005-2016. Similar to Acemoglu and Restrepo (2020), they find that initial exposure to robots leads to a reduction in employment. However, this effect diminishes over time, eventually transitioning to a slight increase in employment.

separation and increase the rate of job finding. Furthermore, they find that positive (or non-negative) employment effects are more likely when the relative price of labor to robots is low. Lastly, using longitudinal data on households, Giuntella et al. (2025) study the expansion of robots in China. They find that increased exposure to robots is associated with declines in labor force participation, employment, and hourly wages, and these estimated effects are concentrated among less-educated individuals.[8]

2.2. Spillover Effects of Robotics Expansions

Two recent studies have considered potential spillover effects of U.S. robotics expansion on health-related outcomes. Using data from the United States for the period 1993-2007, O'Brien et al. (2022) find that robotics expansion increased all-cause mortality among persons aged 45-to-54, and that these effects were driven by increases in deaths involving drugs, suicides, and cardiovascular disease. They attribute their findings to an increase in automation-induced job loss.

Also using data from the United States, Gihleb et al. (2022) estimate the effects of industrial robot adoption on workplace injuries, substance use-related mortality, and mental health issues. They find that U.S. robotics expansions reduce the rate of workplace injuries, and attribute this finding to the automation of dangerous jobs. Gihleb et al. (2022) also find that robotics penetration increases both drug- and alcohol-related mortality, and adversely affects psychological wellbeing.[9]

[8] The U.S.-China Relations Act of 2000, which granted Permanent Normal Trade Relations (PNTR) to China, has also been found to reduce manufacturing employment (Autor et al. 2013; Pierce and Schott 2016; Acemoglu et al. 2016) and led to an increase in arrests (Che et al. 2018). We control for this shock in our regression specifications below. There is less evidence that the 1994 North American Free Trade Agreement (NAFTA) had large effects on employment (Burfisher et al., 2001; Hufbauer & Schott, 2005).

[9] In a supplementary analysis, Gihleb et al. (2022) explore individual-level data from Germany and find that robotics expansion reduces the likelihood of "job intensity" and disability, results consistent with improved workplace safety. However, they find no evidence of an effect on psychological wellbeing. In related research, Anelli et al. (2024) explore how U.S. robotics expansions affected marriage and fertility decisions. They find that robotics expansions increase rates of divorce and cohabitation, each of which could be downstream effects of job displacement. Finally, the authors find that robotics expansions are associated with a decline in marital fertility, but an increase in nonmarital fertility.

These findings are consistent with the hypothesis that automation-induced job loss may adversely affect the mental health of displaced workers and lead to risky health behaviors as well as "deaths of despair" (Case and Deaton 2020).

2.3. Channels through which Robotics Could Affect Crime

Consistent with Becker (1968), there is strong evidence that criminal behavior responds to economic conditions. High rates of local unemployment (Raphael and Winter-Ebmer 2001; Gould et al. 2002; Machin and Meghir 2004; Levitt 2004; Oster and Agell 2007; Lin 2008; Mustard 2010), business cycle contractions (Arvanites and Defina 2006; Rosenfeld and Fornango 2007; Mocan and Bali 2010), and depressed wages (Gould et al. 2002) all predict increases in criminal offending. Moreover, changes in local labor markets appear to matter most for those on the margin of criminal activity (e.g., lower-skilled, less-educated males). For instance, Gould et al. (2002) find that a 10 percent increase in the wages of non-college-educated men is associated with a 5.4 and 10.8 percent decrease in property and violent crime, respectively.[10] In addition, robotics-induced substance use may also play a role in criminal behavior. There is evidence that crime rises with increased alcohol consumption (Carpenter and Dobkin 2010) and illicit drug use (Markowitz 2005; Dobkin and Nicosia 2009; Dave et al. 2021; Doleac and Mukherjee 2022), each of which have been linked to robotics expansions (O' Brien et al. 2022). Property and violent crime offenses could be affected through the psychotropic effects of job loss-induced substance use as well as the need to generate income to support addictive behaviors.

[10] Gould et al. (2002) also find that a one percentage-point increase in the unemployment rate for non-college-educated adult men is associated with a 2.3 and 1.3 percent increase in property and violent crime, respectively. There is also evidence that rates of recidivism are lower when labor market conditions are more favorable (Schnepel 2018). Schnepel finds that ex-offenders released in counties with higher low-skilled wages are less likely to recidivate, especially in sectors more apt to hire ex-offenders.

A final pathway through which U.S. robotics expansions may affect criminal behavior is through psychological wellbeing. Those with mental health disorders are more likely to be arrested (Choe, Teplin, and Abram 2008; Donnellan et al. 2005; Elbogen and Johnson 2009; Teplin et al. 2005; Trzesniewski et al. 2006; White et al. 2006) and rates of mental illness are higher among the incarcerated population (Marcotte and Markowitz 2011). Using individual panel data and a variety of fixed effects identification strategies, Anderson et al. (2015) find that adolescent depressive symptomatology is associated with an increase in the likelihood of committing property crimes as an adult.[11]

Only one recent published study of which we are aware studies the relationship between robotics expansion and crime. Fang and Miao (2025) examine the impact of industrial robot adoption on crime in China. They find that increased exposure to robotics is associated with *lower* crime rates, driven largely by robot-induced improvements in employment opportunities for low-educated workers in the mid-late 2010s.[12]

2.4 Contributions

This study contributes to literature in four important ways. Most importantly, it is the first to explore the relationship between U.S. robotics expansions and crime, a vital consideration when calculating changes in social welfare due to automation. Second, we build on prior work on the Chinese labor market (Fang and Miao 2025), which differs fundamentally from the U.S. labor market in terms of government regulation of employment, wage rigidity, industry concentration,

[11] In theory, robotics expansions could also affect crime through marriage. While studies in the criminology literature generally find a negative association between marriage and crime, the research designs employed should be considered descriptive in nature (Skardhamar et al. 2015).
[12] In contrast, a working paper by Zhang and Zhang (2023), which also studies the Chinese context using similar data over the same time period, finds the opposite, that is increased exposure to robots resulted in higher violent, property, and fraud crimes. It is not clear why these findings of these two studies differ.

educational attainment, skill accumulation, and informal labor market work, all of which may result in differential impacts of robotics expansions on labor market outcomes and, thus, crime.

Third, we introduce dynamic difference-in-differences event-study models to this literature. In doing so, we are able to investigate whether pre-treatment trends in treated versus control counties appear similar prior to the robotics expansion. This is critical in assessing the validity of the instrument used by Acemoglu and Restrepo (2020). Finally, we assess the likely mechanisms at work in explaining the observed relationship between robotics expansions and arrests that we find.

3. Data and Methods

3.1. Robotics Measures

We obtain annual country- and industry-specific data on robots from the International Federation of Robotics (IFR). To date, the IFR is the best available data source for industrial robots and have been used frequently to estimate the effects of robotics penetration (Graetz and Michaels 2018; Acemoglu and Restrepo 2020; Gihleb et al. 2022). We begin by focusing on robotics data for the years 1993-2010, the period studied by Acemoglu and Restrepo (2020) where the strongest evidence for negative employment effects from robotics shocks have been detected in prior studies (Acemoglu and Restrepo 2020; Chung and Lee 2023). As noted above, following 2010, adverse employment impacts of robotics appear to diminish because of several factors, including a growing complementarity between automation and human labor (Acemoglu & Restrepo, 2023), an increased emphasis on reskilling and workforce adaptation (Bessen, 2019) and broader structural shifts toward service-oriented industries (Autor, 2019). While much of our analysis focuses on the 1993-2010 period, we also study the period including years after 2010, in part to establish smaller crime effects over a period including smaller employment effects

The IFR data set contains robotics information for key European countries from 1993 through 2016; for the United States robotics information is available beginning in 2004.[13] The data include information on robot stocks for 19 industries, 13 of which are within the manufacturing sector: food and beverages, textiles, wood and furniture, paper and printing, plastic and chemicals, glass and ceramics, basic metals, metal products, metal machinery, electronics, automotive, "other" vehicles, and "other" manufacturing industries. In addition, the IFR data set also contains information on robotics in non-manufacturing sectors, including agriculture, service, and utilities.

We construct a measure of localized exposure to robotics across U.S. markets as follows:

$$\text{Actual Exposure}_{ct} = \sum_j S_{cj1990} \left(\frac{\text{US Robotics}_{jt}}{\text{Worker}_{j1990}}\right), \tag{1}$$

where S_{cj1990} represents the baseline share of employment in county *c* and industry *j* in 1990. The numerator, US Robotics_{jt}, measures the number of robots utilized within industry *j* during year *t* in the U.S.; and the denominator, Worker_{j1990}, measures the number of workers (in thousands) in industry *j* in 1990. To calculate the number of workers in each county-industry in 1990, we use data from the U.S. Census Bureau's County Business Patterns (CBPs), which includes county identifiers and North American Industry Classification System (NAICS) codes.[14] To calculate robot exposure in county *c* and year *t*, we sum the number of robots per 1,000 workers in each county-year across industries *j*.

The penetration of robotics in the U.S. may be endogenous to industry-specific trends within the U.S. (for instance, shifts in product demand or input prices) and/or trends across local labor markets that vary in the concentration of these industries with varying adoption rates of

[13] The dataset also covers selective years for other industrialized nations such as China, Brazil, and Japan, with detailed industry-level data available through 2016. However, these countries were not included in Acemoglu and Restrepo's main IV. Moreover, because their focus was on Census-based employment outcomes, Acemoglu and Restrepo (2020) only used robotics data from 1993, 2000, and 2010.
[14] The data spans from 1975 to 2016 and was prepared by Eckert et al. (2020).

robotics.[15] To the extent that these trends also correlate with employment and related outcomes such as poverty rates, mental health or criminal activity, our estimates of the impact of robotics expansion on crime will be biased. To address this concern, we follow the identification strategy developed by Acemoglu and Restrepo (2020) and utilize a measure of exogenous exposure to robotics based on the penetration of robotics across industries in other developed economies.

Specifically, we construct an analogous measure to (1) which proxies industry exposure to robotics in the U.S. with the penetration of robotics in the same industries in the European Union (EU):

$$\text{Potential Exposure}_{ct} = \sum_j S_{cj1975} \left(\frac{\text{EU Robotics}_{jt}}{\text{Worker}_{j1990}}\right), \tag{2}$$

The numerator, EU Robotics_{jt}, now measures the number of robots utilized in the EU within industry *j* during year *t*. Following Acemoglu and Restrepo (2020) and Gihleb et al. (2022), we use data from the nine European countries that have consistently reported to the IFR over the sample period.[16] As before, S_{cj} represents the share of employment for county *c* and industry *j* in the U.S. We lag this baseline share further to 1975 in order to isolate historical and long-standing differences across local markets in industry specialization and to bypass mechanical correlation that may arise due to mean reversion.[17]

We use this *Potential Exposure* measure in (2) to instrument for direct exposure to robotics in the U.S. (as defined in 1). This measure is in the spirit of a Bartik "shift-share" instrument, which interacts local industry shares with broader industry-specific shifts (in this case, shift in the penetration of robotics) (Goldsmith-Pinkham et al. 2020). By leveraging the diffusion of robotics

[15] One concern, for instance, may be that unobserved shocks to local labor demand would affect firms that operate in the area, and affect their propensity to adopt newer technology such as robotics. To the extent that these shocks would also drive other outcomes, estimates of the effects of robotics adoption on these outcomes would be biased.
[16] The nine European countries are France, Denmark, Finland, Italy, Germany, Norway, Spain, Sweden, and the United Kingdom.
[17] Results are not materially different if we alternately use the share in 1980 or 1990.

across industries over time in other high-income economies, this measure plausibly isolates variation in exposure to robotics in the U.S. that is occurring solely due to advancements in the world technology frontier for robotics and which is arguably orthogonal to U.S.-specific industry shocks or shocks to local labor markets. The baseline share parameter, S_{cj1975}, further capitalizes on variation across local areas with respect to industry specialization; thus, counties which historically had a higher concentration of employment in industries, that were more prone to later adopt robotics globally, are more intensely treated relative to counties that specialized in industries with little to no penetration of robotics.

3.2. Arrest Measures

To measure arrests among adults ages 18 and older, we draw on data from the Federal Bureau of Investigation's Uniform Crime Reports (UCR). Arrest data are collected for Part I offenses, which include property crimes (larceny, burglary, motor vehicle theft, and arson) and violent crimes (homicide, rape, robbery, and aggravated assault). We construct the variable, $Arrest_{ct}$, which is the county-year offense-specific arrest rate per 1,000 adults.

There are several measurement issues of note with regard to the UCR. First, the arrest rate understates the true level of crime because not all crimes committed are reported to the police (Gould et al. 2002) and not all reported offenders are arrested. Despite this, prior research indicates that arrest data serve as an accurate representation of underlying criminal activity (Hindelang 1978, 1981).[18] Moreover, one would not expect measurement error in arrests to be systematically correlated with robotics expansion.

[18] Based on the UCR data, Lochner and Moretti (2004) show that correlations between arrests and crimes committed tend to be quite high. For instance, they report correlations of 0.96 for rapes and robberies, 0.94 for murders, assaults, and burglaries, and 0.93 for motor vehicle thefts.

Nonetheless, to address this concern, we consider two approaches. Following Fone et al. (2023), we use data from the Law Enforcement Management and Administrative Statistics (LEMAS) and the Census of State and Local Law Enforcement Agencies (CSLLEA) to directly control for local policing resources in our regressions. In addition, we supplement our arrest analysis with an analysis of incident-level data from the National Incident-Based Reporting System (NIBRS), which allows us to estimate the relationship between robotics penetration and reported criminal behavior irrespective of whether the report resulted in an arrest.[19]

Second, the number of agencies reporting arrests to the UCR can change over time. To ensure that our measure of arrests is not capturing changes in reporting practices, we control for the number of agencies reporting within a county for any given year. Following Anderson (2014), we also drop county-year arrest rates that are further than two standard deviations from the mean and limit our sample to counties with more than ten years of available arrest data. Relaxing these assumptions to generate our analysis sample yields a qualitatively similar pattern of arrest results as reported below. Table 1 provides descriptive statistics while Figure 1 maps the geographic distribution of robotics expansion between 1993 and 2010 versus changes in arrests over the same period.

3.3. Reduced-form Estimates

We begin by estimating the reduced-form relationship between exogenous potential exposure to robotics in the U.S. – drawing on variation in the diffusion of robotics across similar

[19] This approach also allows us to rule out the possibility that robotics expansion is correlated with advances in policing technologies that directly affect the likelihood of criminals being apprehended. The expansion of industrial automation may contribute to broader technological diffusion, where advances in robotics — such as machine learning, automation hardware, and sensor technologies — become more widely adopted in law enforcement for surveillance, digital forensics, and automated policing tools (Thomson Reuters, 2023).

industries in advanced EU economies – and adult arrest rates for the period 1993-2010, based on the following regression model:

$$Arrest_{ct} = \beta_0 + \beta_1 Potential\ Exposure_{ct} + \mathbf{X}_{ct}'\boldsymbol{\beta_2} + \theta_c + \tau_t + \varepsilon_{ct}, \quad (3)$$

where $Arrest_{ct}$ and $Potential\ Exposure_{ct}$ are defined as described above, θ_c is a time-invariant county effect, and τ_t is a county-invariant year effect.[20] The vector $\mathbf{X}_{ct}$ includes controls for demographics, policing resources, social welfare policies, and the housing price index. See Table 1 for a list of the county-level covariates along with variable definitions.[21] The coefficient of interest, β_1, captures the impact of exogenous potential exposure to robotics in U.S. markets on crime.

This reduced-form model as it offers several advantages. First, because data on robotics penetration in industries in the EU are available for a longer-time span (since 1993) in comparison to the U.S. (since 2004), the reduced-form specification allows us to estimate the relationship over a longer period that experienced almost a five-fold increase in the use of industrial robots. Second, given that this measure of EU robotics penetration provides the key identifying variation, the reduced-form model – which is essentially a continuous treatment difference-in-differences (DiD) specification – permits standard tests of the identification assumption underlying these and our subsequent IV analyses. Third, within this DiD reduced-form framework, we are also able to assess dynamics in the relationship between robotics exposure and crime, as well as apply newer estimators that are robust to potential bias from temporal and spatial heterogeneity.

Specifically, to assess the credibility of our identification strategy, we estimate the following event-study model:

$$Arrest_{ct} = \gamma_0 + \textstyle\sum_{e\neq -1} \gamma_e D_{ct}^e + \mathbf{X}_{ct}'\boldsymbol{\alpha} + \mu_c + \tau_t + \varphi_{ct}, \quad (4)$$

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[20] Note that θ_c subsumes the main effect of S_{cj1975}, that is the baseline historical variation in industry specializations across counties in the U.S.

[21] With regard to the housing price index, our goal is to capture business cycle fluctuations without "over controlling" for mechanisms through which robotics expansion could affect crime (e.g., employment or income effects). In the appendix, we explore the robustness of our estimates to controlling for additional macroeconomic indicators.

where e denotes event time and D^{e}_{ct} is a set of variables that captures the "intensity" of EU robotics diffusion (i.e., the difference between the number of robots per thousand workers) that occurred e periods from period t. Each γ_e is the estimated treatment effect over time relative to the reference period -1. For the event-study figures, we depict the cumulative annual estimated effects of potential exposure to robotics in the U.S. across various time horizons.[22]

In addition to estimating an event-study specification, we consider the following extensions to our baseline empirical strategy. First, we control for county-specific linear time trends to capture smoothly evolving unobservables that could affect robotics penetration and crime. Second, we employ the estimator developed by de Chaisemartin and D'Haultfœuille (2020; 2024), which expunges bias due to heterogeneous and dynamic treatment effects by restricting the counterfactual units to counties where potential exposure to robotics penetration remained constant over the sample period ("stayers").[23] Third, we explore falsification tests where we randomize (1) the *shift* (robots per worker) in each industry in 1990, and (2) the *share* (the initial employment share across industries), and re-estimate equation (3) 1,000 times. We then show the distribution of placebo effects as well as the share of placebo tests that yield an estimate as large as the treatment effect obtained from a regression using the actual measure of *EU Robotics*$_{ct}$.

3.4. Instrumental Variables (IV) Estimates

The reduced-form analyses yield an intention-to-treat effect, that is how *potential* exposure to robotics across counties in the U.S. (based on local industry concentration and diffusion of robotics across these same industries outside the U.S.) impacts crime. In order to derive the "treatment-on-

[22] Following the approach of Schmidheiny and Siegloch's (2023) and Cengiz et al. (2019), event-study coefficients at each j show the average of the estimates of γ_e for all j < -1 for j <-1 and for all j > -1 for j >-1.
[23] While our research design does not mirror the typical "staggered adoption" difference-in-differences model, our estimate of β_1 in equation (3) is identified off of county-specific changes in treatment over time. The dCDH approach allows us to use a closer-to-continuous treatment variable (via six bins in ascending values of *EU Robotics*$_{ct}$).

the-treated" effects of actual robotics penetration across local markets and industries in the U.S., we next turn to an instrumental variables (IV) strategy. Following Acemoglu and Restrepo (2020), Gihleb et al. (2022), and Anelli et al. (2024), we instrument for U.S. robotics penetration (defined in 1) using EU robotics penetration (defined in 2). This analysis is restricted to the period for which data on both EU and U.S. robotics are each available (i.e., 2004-2010).[24] The first stage estimating equation is as follows:

$$\textit{Actual Exposure}_{ct} = \eta_0 + \eta_1 \textit{Potential Exposure}_{ct} + \mathbf{X}_{ct}'\eta_2 + \theta_c + \tau_t + \sigma_{ct}.^{25} \quad (5)$$

In the second stage, we regress arrests on predicted actual U.S. robotics penetration. The critical underlying assumption of this approach is that EU robotics expansion affects crime only through robotics expansion in the United States. This approach identifies the causal effect by relying only on the common correlation between the adoption of robotics across similar industries in the EU and in the US, which is arguably driven by shifts in the world technology frontier rather than country-specific industry-wide shocks or local labor market shocks within the U.S. While we cannot directly test the exogeneity of the instrument, the event study analysis of the reduced form (detailed above) allows us to assess if the instrument is orthogonal to differential trends across local areas and industry concentrations in the U.S.

We take several additional approaches to explore the potential validity of the instrument. Recent studies highlight the importance of testing the assumptions underpinning the Bartik instrument featured in equation (5) (see, for example, Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2018). These studies argue that the exogeneity of initial industry shares within local manufacturing is crucial for generating consistent estimates. Moreover, they posit that

[24] Auxiliary analyses, which expand the sample to include data prior to 2004 — either by assuming no U.S. robots were present during this period or by projecting pre-2004 data (Acemoglu and Restrepo, 2020) — yield a similar pattern of results.
[25] Lee et al. (2021) recommend using instruments where the first-stage F-statistic exceeds approximately 105 to ensure reliable inference.

having a diverse set of industries (within, and perhaps outside, the manufacturing sector) contributing to the identification is also crucial.[26] For instance, there is concern that if all of the identifying variation comes from a single industry, specifically, the automotive sector, then the estimates may be inconsistent if the sector experienced a unique trend. While we note that the Bartik instrument employed in this paper is based on data from nineteen distinct industries, minimizing the risk that a single dominant sector could non-randomly skew the estimates, to address this concern directly, we decompose our EU robotics adoption measure into two components: one measuring penetration of robots in the automotive industry, and the other capturing their penetration in all other industries.

4. Results

Our main findings appear in Tables 2 through 7 and Figures 2 through 4. Supplemental analyses are found in the appendix. All regressions are weighted by county population and standard errors are clustered at the county level (Bertrand et al. 2004).

4.1. Reduced-form Estimates

Estimates of β_1 from equation (3) for the period 1993-2010 are shown in Table 2. In the first column of panel I, we consider a parsimonious model that controls for county fixed effects, year fixed effects, and the number of reporting agencies. Based on this specification, potential exposure to one additional robot per 1,000 U.S. workers is associated with a 0.085 increase in property crime arrests per 1,000 adult population (≈ 1.7 percent increase). The inclusion of controls for sociodemographic characteristics (column 2), policing investments (column 3), and import

[26] For example, if a single large manufacturing industry (such as automobiles, in our case) disproportionately influences the variation in the instrument, the IV estimates may be inconsistent, especially if that dominant industry is non-randomly distributed in the initial period.

competition from China (column 4), reduces the estimate of β_1 only slightly. Based on the estimate reported in column (4) of panel I, potential exposure to one additional robot per 1,000 U.S. workers is associated with 0.076 more property crime arrests per 1,000 adult population (≈ 1.5 percent increase). In the fully specified model (column (5)), where we control for the economic and social welfare policies listed in Table 1, the effect magnitude declines to 1.2 percent increase in the arrest rate for property crimes.

The pattern of estimates for violent crime arrests is quite different (Table 2, panel II). Across all specifications, there is little evidence of a relationship between potential exposure to robotics and the arrest rate for violent crimes in the United States. The estimated effects are uniformly small and statistically indistinguishable from zero. Taken together, the findings in Table 2 are consistent with an income-generating motive for crime and highlight an important external cost of local market exposure to robotics penetration.

Event-study estimates of the relationship between our plausibly exogenous measure of potential exposure to robotics and U.S. crime are shown in Figure 2. These estimates underscore two points. First, they provide a strong degree of support for our identifying assumption. In panels I (property crime) and II (violent crime), the estimated coefficients on the leads are small and statistically insignificant, consistent with the assumption of parallel trends. This indicates that the adoption of robots across industries in the EU is not systematically correlated with pre-treatment exposure crime trends across local areas in the U.S. or across areas with varying industry specializations. Second, for property crimes, we observe an immediate increase in the post-treatment period, and the estimated effect slightly attenuates over time. Compared to the pre-treatment period, one year after treatment, the adjusted effect is 0.127; two years after treatment, it reduces to 0.104; and by four or more years after treatment, it declines further to 0.038.[27] Consistent with the results shown in Table 2, there is little evidence of a relationship between potential exposure to robotics expansion and violent crime in the United States.

Developments in the difference-in-differences literature raise concerns that, in the presence of heterogeneous treatment effects, estimates from two-way fixed effects models may be biased. While our "shift-share" treatment is not equivalent to a policy based on "staggered adoption," identification does come from county-specific changes in potential robotics penetration over time. Thus, in Figure 3, we report results from the dynamic estimator developed by de Chaisemartin and D'Haultfoeuille (2020; 2024). This approach does not require creating an ad hoc "all-absorbing" treatment cutoff from a continuous variable but instead allows the value of the treatment variable to fall into "bins' that represent one-unit changes in the value of treatment. To operationalize this estimator, we group EU robotics expansion into the following six (6) bins based on evenly spaced cutoffs: [0-1), [1-2), [2-3), [3-4), [4-5), and 5+. The counterfactual is restricted to "stayer" counties, where potential exposure to robotics expansion remained constant over time. In Figure 3, the event-study estimates for property crime are similar to those reported above and generally measured with increased precision. Compared to the pre-treatment period, one year after treatment, the estimated event-study coefficient is 0.161; two years after treatment, the estimated effect decreases to 0.084; by four or more years after treatment, the estimated treatment effect is 0.062. Again, we find little evidence of a relationship between exposure to robotics and the violent crime arrest rate.

4.2. Robustness Checks for Reduced-form Estimates

[27] The adjusted effect is calculated by subtracting the average of the pre-treatment estimates from the post-treatment estimates.

In Figure 4, we display the results of two falsification tests. First, we randomize the industry-specific "shift" in EU robotics adoption and conduct a permutation test 1,000 times. The distribution of these estimates is displayed alongside the actual treatment effect, marked by a vertical red line. The results indicate that the distribution of coefficients is largely centered around zero, and the true estimated treatment effect never intersects with the confidence intervals generated by the falsification tests.

Subsequently, we shuffle the initial employment share across industries (essentially shuffling "treated" and "control" counties), while retaining the same EU robotics penetration measure, and perform another set of permutation tests. The results echo those found in panels I and II. Taken together, the findings in Figure 4 provide strong evidence that our estimated treatment effects are unlikely to have occurred by chance.

Another concern is that our findings could be predominantly influenced by the extreme ends of the distribution or by a single industry (particularly the automotive industry), which, from 1993 to 2010, saw the highest adoption of robots compared to other sectors, is a tradable sector, and may also be subject to distinct economic trends. In Appendix Table 1, we first exclude highly exposed counties identified as those where robot penetration exceeds the 95th percentile (in column 1). The reduced-form findings are still robust and indicate that potential exposure to one additional robot per 1,000 workers is associated with a 3.34 percent rise in property crime arrests, while having no noticeable impact on violent crime rates. Next, we separate our potential exposure measure (EU Robotics) into two parts: one exploiting the penetration of robots in the automotive industry (column 2) and the other exploiting the penetration of robots in all other industries (column 3). We also include both components simultaneously as shown in column 4. The analyses reveal that the impact of potential exposure to robotics penetration on arrests is larger for non-automotive than the automotive sector. These outcomes are reassuring for two reasons: first, they confirm that our findings are not exclusively attributed to the automotive industry; and secondly, they reveal the influence of robotics integration across multiple industries.

Additionally, in Appendix Table 2, we explore the sensitivity of our estimates to an unweighted specification (column 1), use of a log-linear model (column 2), restricting the sample to counties with population larger than 10,000 persons, where arrest data are perhaps more reliable (column 3), and the use of a balanced panel of county-years (column 4). Across these different samples and specifications, our findings consistently demonstrate robustness.

Finally, in Appendix Table 3, we turn to an additional data source, the National Incident-Based Reporting System (NIBRS). These data provide incident-based counts rather than arrest-based counts, allowing us to measure criminal *incidents* that are reported to law enforcement agencies, but do not necessarily lead to an arrest. This will allow us, in part, to further address unmeasured changes in policing practices from changes in criminal behavior. Because the data are incomplete, we follow best practices and do not aggregate to the county- or state-levels, but rather estimate regressions based on count data at the law enforcement agency level. We employ both the OLS model and a Poisson regression, with the city-level population serving as the exposure variable. The findings from the Poisson regression are presented in column (1), while those from the OLS model appear in column (2). All results are suggestive of a robotics expansion-driven increases in property crime but not violent crime. These results add to our confidence that the arrest effects we detect above are likely driven by changes in criminal behavior.[28]

[28] In Appendix Table 3, we further assess whether more detailed controls for the quality and type of policing strategies could affect our estimated treatment effects. In column (3), we add a wide vector of additional controls to account for county-level police presence, local law enforcement resources, and local policing practices. We follow Fone et al. (2023) and collect data from the Law Enforcement Management and Administrative Statistics (LEMAS) and the Census of State and Local Law Enforcement Agencies (CSLLEA) to measure (1) local policing resources (per capita number of community police offers, school resource officers, and patrol officers; as well as police operating budget per capita), and (2) local policing policies (presence of special units devoted to hate crime/bias, local policing policies on racial profiling, and policies related to diverse cultural populations). The inclusion of these controls has very little impact on our estimated treatment effect.

4.3 Reduced Form Crime and Employment Effects Over Time

In Table 3, we explore whether the arrest effects we observe in Table 2 differ over time. In panels I and II of column (1), we first replicate our estimates from column (5) in Table 2. We find that during this period, the robotics shock was associated with a decline in employment (column 1, panel III), consistent with Acemoglu and Restrepo (2020). Specifically, we find that potential exposure to one additional robot per 1,000 U.S. worker is associated with a 1.13 percentage-point decline in the adult employment-to-population ratio.

In sharp contrast, when we examine the later period 2011-2016, (column 3)[29], we find little evidence that the robotics expansion increased arrests for part I offenses. This result is consistent with the hypothesis that the employment effects of robotics expansion dissipated following 2010 (Chung and Lee 2023), a finding we confirm in panel III. The pattern of findings is also reassuring in that it suggests that positive property crime effects of robotics expansion only exist during windows where we observe negative employment effects.

4.4. Instrumental Variables (IV) Estimates

[29] Our analysis extends through 2016 due to the availability of County Business Patterns data, which is consistently accessible from the same source only up to that year, as noted by Eckert (2020).

Next, we turn to our IV estimates, which are shown in Table 4.[30,31] Across specifications, we find that exposure to one additional U.S. robot per 1,000 workers is associated with 0.22 to 0.26 more property crime arrests per 1,000 population (≈ 4.6 to 5.6 percent increase). How plausible are estimated effects of this magnitude? Over the same time frame, Acemoglu and Restrepo (2020) observe that an increase of one robot per 1,000 U.S. workers is associated with a 0.5 to 1.0 percentage point decline in the employment-to-population ratio (Acemoglu and Restrepo 2020; p. 32; Table 3) .[32] Lin (2008) finds that a one-percentage-point reduction in manufacturing employment leads to a 4 to 6 percent rise in property crime arrests. This would imply that if the effects we estimate were driven entirely by reductions in employment, we would expect a 2 to 6 percent increase in the property crime arrest rate.[33]

In addition, it is important to reemphasize that employment effects are only one channel through which robotics expansion could affect crime. Acemoglu and Restrepo (2020) also find that

[30] As noted above, our IV estimates focus on the period from 2004-2010 because data on robotics adoption in the U.S. are only available beginning in 2004. For comparability to our reduced form estimates, in Appendix Table 4 we restrict our analysis period to 2004-2010 and re-estimate equation (3). We find that potential exposure to robotics expansion (based on EU robotics adoption across industries) is associated with a notably larger increase in the property crime arrest rate than reported in Table 2. Specifically, in the fully saturated specification, we find that potential exposure to one additional robot per 1,000 U.S. workers is associated with 0.28 more property crime arrests per 1,000 population (≈5.8 percent increase). This result is also consistent with descriptive evidence observed in Appendix Figure 1, which tracks the evolution of robotics expansion in the EU across sectors from 1993 to 2010. Notably, the period from 2000 to 2010 witnesses the swiftest surge in robotics utilization, especially in automation-intensive industries such as automotive, electronics, plastics and chemicals, metal products, and food and beverages. An examination of industry-specific employment trends in panel (III) shows a steep decline in employment in industries most affected by robotics expansion over the same period. This is consistent with a much larger employment-driven property crime effect over the 2004-2010 period. We consistently find no evidence that EU robotics penetration affects adult violent crime arrests during the 2004-2010 period.

[31] First-stage estimates are shown in Appendix Table 5. They suggest almost a one-to-one relationship in that potential exposure to one additional robot per 1,000 U.S. workers (based on extra-U.S. industry diffusion) is associated with 1.0 to 1.1 additional U.S. robots per 1,000 U.S. workers. Importantly, the F-statistic ranges from 170 to 180, which satisfies the standards proposed by Lee et al. (2021). The results of the naive OLS are presented in Appendix Table 6.

[32] Acemoglu and Restrepo (2020) also found that the estimated effects were similar across most education groups, with the exception being individuals holding a master's degree or higher. The employment prospects of the most highly educated were unaffected by increased robotics expansion.

[33] An alternate way to frame our effect magnitude is to impute the structural marginal effect on property crime arrests of the decrease in employment induced by the adoption of robotics. To do so, we can take a Wald-type ratio of the reduced-form impacts on crime and on employment (reported in Table 3), after ensuring that the effects are in the same metrics (e.g. per 1,000 adults). Doing so implies that penetration of robotics resulted in one additional property crime arrest per approximately 60 workers displaced from employment (based on the extended period estimates in column 1).

an additional U.S. robot per 1,000 workers is associated with a 0.4 to 0.8 percent decline in wages. Based on these estimates and those produced by Gould et al. (2002), we might expect an additional reduction in property crimes of 0.2 to 0.4 percent due to wage effects. Thus, the magnitudes of the estimated effects shown in panel I of Table 4 are plausible given the employment and wage elasticities reported in the literature.[34]

In panel II of Table 4, we report IV estimates of the relationship between robots and the violent crime arrest rate. Similar to our reduced-form estimates, we find no evidence that violent crime arrests are related to U.S. robotics expansion. Again, this is consistent with the hypothesis of an income-generating motive for property crime.

In Table 5, we turn to a long-differenced model in the spirit of Acemoglu and Restrepo (2020) using data from 2004-2010. This approach allows to reduce biases from transitory shocks or measurement errors in annual panel data while addressing potential serial correlation over longer periods (Griliches and Hausman 1986). Using this long-differencing approach, we find that an additional U.S. robot per 1,000 workers leads to 0.084 more property crime arrests per 1,000 population (panel I, column (1)), which is equivalent to a 1.8 percent increase.

Columns (2) through (4) of Table 5 re-estimate long-differenced models, but instead property crime arrest rates are measured in years prior to robotics expansion. We estimate the model based on the following pairs of years: 1984 and 1990 (column (2)), 1974 and 1980 (column (3)), and 1974 and 1990 (column (4)). Reassuringly, our estimates show no evidence that U.S. robotics expansion during the 2000s affected arrest rates in prior decades.[35]

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[34] As noted above, substance use and mental health could be additional channels through which robotics expansion affects crime (O'Brien et al. 2022).

[35] In Appendix Table 7, we conduct an analysis similar to that in Appendix Table 2. While estimates using the non-automotive industry are somewhat less precise, the general pattern of results indicates that our estimates are not purely driven by the automotive industry.

4.4. Heterogeneity by Offense Type and Demographic Characteristics

We conclude with a discussion of heterogeneity by type of offense (Table 6) and demographic characteristics (Table 7). The results shown in panel I of Table 6 illustrate that the estimated effects for property crimes are driven by burglaries, larcenies, and motor vehicle thefts. Specifically, an additional U.S. robot per 1,000 workers is associated with a 3.7 percent increase in burglary arrests, a 3.6 percent increase in larceny arrests, and a 14.8 percent increase in arrests for motor vehicle theft. For arson arrests and for all of the individual violent crimes shown in panel II, the estimated effects are small and statistically indistinguishable from zero.

Turning to demographic characteristics in Table 7, our results show increases in property crime arrests for both males (column (1)) and females (column (2)).[36] Examining race-specific effects, we find evidence of a positive relationship between robots and crime for both Blacks and Whites. Specifically, for Blacks, we find that an additional U.S. robot per 1,000 workers is associated with a (statistically insignificant) increase in property crime arrests of 0.57 per 1,000 population (≈ 5.7 percent increase). For Whites, an additional robot per 1,000 workers is associated with 0.22 more property crimes per 1,000 population (≈ 5.6 percent increase).

In the last three columns of Table 7, we observe that the estimated effects for property crime are driven by individuals under the age of 55. An additional robot per 1,000 workers is associated with an increase in the property crime arrest rate of 6.6 percent for both young adults (ages 18-24) and the prime age working population (ages 25-54). These estimates are consistent with those reported in Acemoglu and Restrepo (2020), who find that younger and prime-age workers faced the largest employment and wage reductions compared to their older counterparts.

5. Conclusion

[36] Acemoglu and Restrepo (2020) found similar adverse employment effects for both men and women.

This study is among the first to explore the relationship between robotics expansion and crime, and the first to do so for a developed country context. Using data from the U.S., and a shift-share IV approach, we find that an additional robot per 1,000 workers led to a 4 to 5 percent increase in the arrest rate for property crimes. This translates to a property crime arrest elasticity with respect to robotics expansion of approximately 0.2 to 0.3. The magnitude of this effect is consistent with what would be implied from (1) elasticities of employment and wages with respect to robotics (Acemoglu and Restrepo 2020), and (2) elasticities of arrests with respect to employment and wages (Lin 2008; Gould et al. 2002). Together with null effects on violent crimes, our results are consistent with an income-based motive for committing property crimes.

A back-of-the-envelope calculation (using our estimated IV effects over the 2004-2010 period) suggests that the observed expansion in robotics exposure, when inflated to the national level, generated a total of approximately 47,319 property crime arrests over this period. Using the cost-of-crime estimates provided by McCollister et al. (2010), this translates to an added cumulative social burden of $322 million (2024$).[37] We note that that this finding does not necessarily imply that robotics expansion worsens social welfare, as there are likely efficiency gains for producers and price reductions for consumers. Moreover, this external cost appears to have been mostly temporary, tapering off after 2010. However, our findings do underscore that there were likely important short-run distributional consequences associated with robotics expansion, particularly

[37] To estimate the implied crime costs due to the observed increase in actual robotics exposure from 2004 to 2010, we begin by using the IV-based coefficients on property crime from Table 4 (column 6). Applying this estimated effect for each year-to-year observed increase in exposure to robotics yields the predicted impact on the property crime arrest rate for the average exposed county. Using the average U.S. adult population, we inflate this county-level estimate to the national level (essentially simulating the effect of the observed robotics expansion if felt nationally across all areas), and then sum these year-to-year effects over 2004-2010 to obtain a combined estimate of the total number of property crime arrests generated nationally from this exposure. In order to monetize this increase in property crime, we apply estimates of the cost per property crime from the literature. Specifically, McCollister et al. (2010) present crime-specific estimates, combining the tangible and intangible costs, for Part I and some Part 2 crimes. Aggregating their property crime estimates, based on the specific shares of each offense in total property crimes over 2004-2010, and converting to 2024 dollars yields the total cost of a property offense as $6,815. Thus, monetizing the additional 47,319 property crime arrests based on this estimate, the total estimated crime cost attributable to increased robotics penetration from 2004 to 2010 is approximately $322 million, or around $50 million per year.

within industries that were disproportionately affected by this technological change. The economic dislocation of workers in these industries generated not only shorter-run private costs — in the form of diminished economic wellbeing and poorer health (Gihleb et al. 2022) — but also imposed costs on third parties due to increased property crime. This suggests that interventions to aid dislocated workers in the short run may not only generate benefits to them, but also to victims of crime.

Finally, the longer-run effects of robotics expansion on criminal behavior are uncertain. Low-skilled workers who are adversely affected by robotics expansions may upskill and the employment of workers with skills that are complementary to robots may rise (Mann and Püttmann, 2023). Such growth in employment may blunt, or even reverse, the short-run effects we estimate above.

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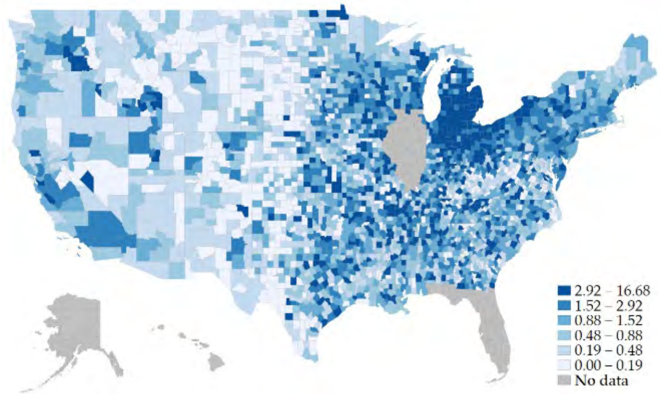
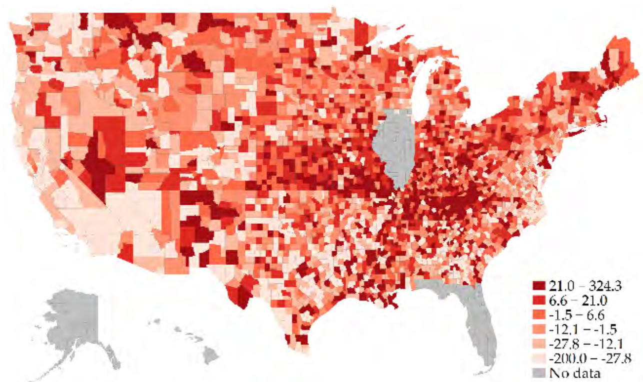
Figure 1. County Variation in Robotics Expansion and Adult Property and Violent Crime Arrests, 1993 vs 2010

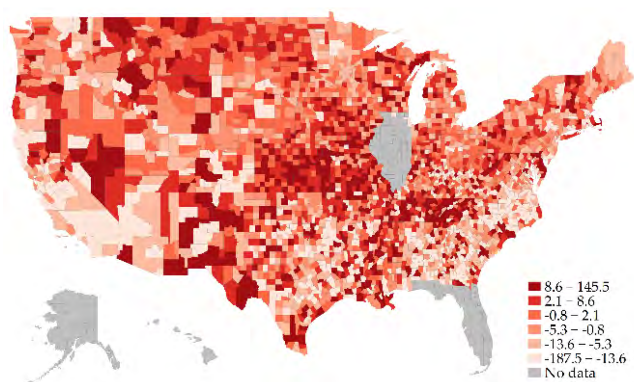
Panel (a): EU Robotics Expansion



Panel (b): Property Crime Arrests



Panel (c): Violent Crime Arrests

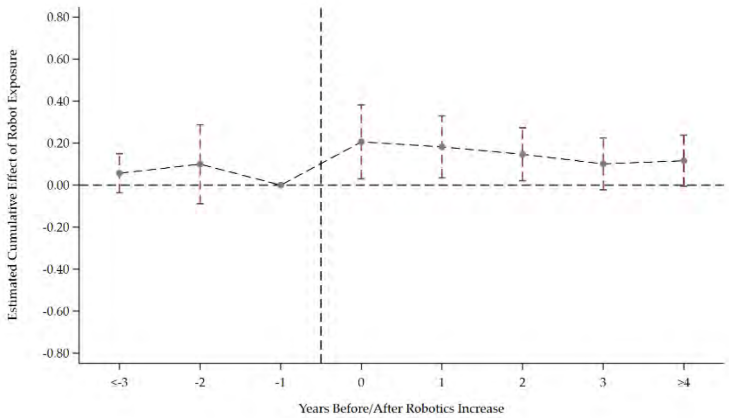


Note: Arrest rates are measured as the number of arrests for 18-year-olds per 1,000 adult population, adjusted for reporting by law enforcement agencies. The robotics expansion metric represents the change in robotics from 2004 to 2010, calculated as the change in one robot per 1,000 workers. Due to data limitation and UCR reporting issues, the map does not include data for Washington, D.C., Hawaii, Alaska, and Florida, as well as all counties in Illinois except for Cook County and Winnebago County.

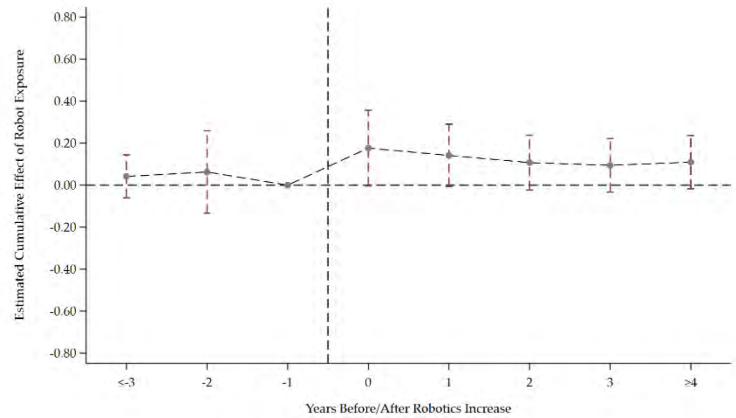
Figure 2. Event-Study Analysis of Potential Exposure to Robotics and Adult Arrests, Using Two-Way Fixed Effects Estimates, 1993-2010

Panel (a): Property Crime Arrests

(i) County and Year Fixed Effects, and Number of Agencies

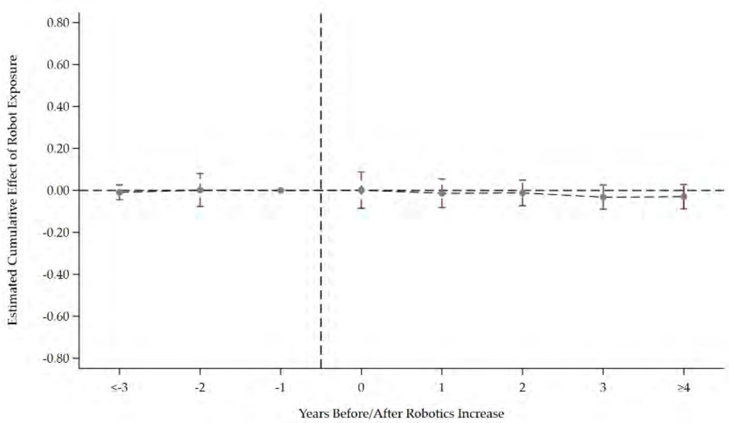


(ii) Controls in (i) Plus Time-Varying Covariates

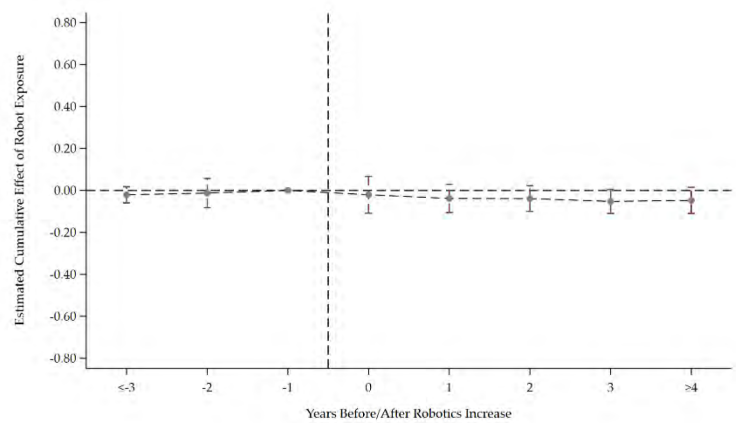


Panel (b): Violent Crime Arrests

(i) County and Year Fixed Effects, and Number of Agencies



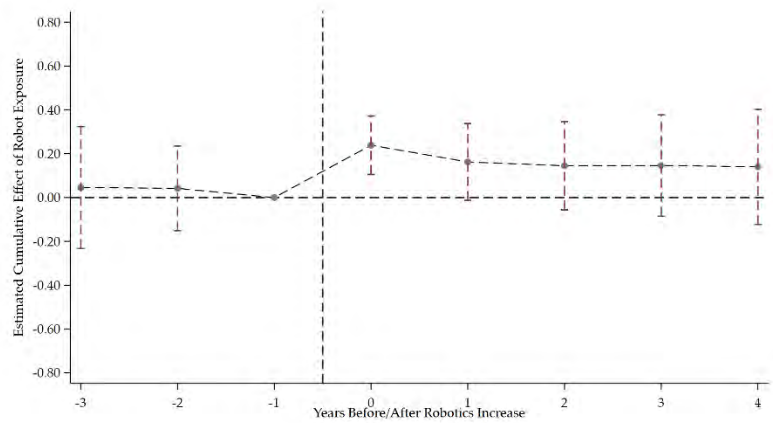
(ii) Controls in (i) Plus Time-Varying Covariates



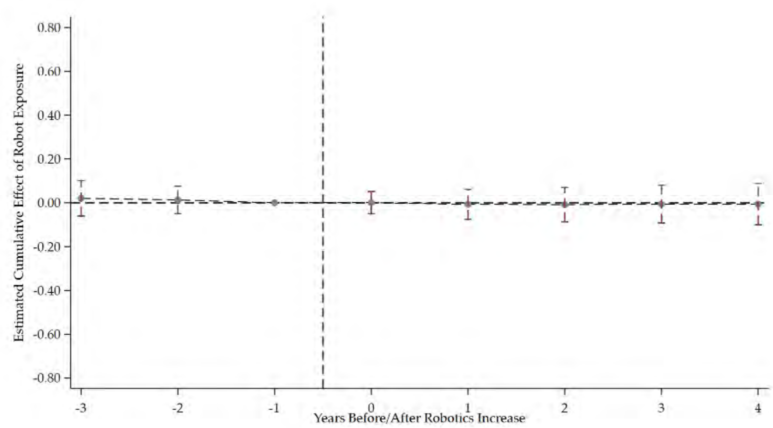
Note: Population weighted OLS estimates (and their 95% CIs) from an event study regression model are shown. All regressions include county fixed effects, year fixed effects, and number of agencies. Saturated model (b) includes additional controls for the percentage of the population that is female, Black or Hispanic; nominal log per capita police expenditures and log per capita police employment (per 1,000 population); state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of 4, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index; import penetration from China, follows the approach outlined in Autor et al. (2013);. The vertical bars represent 90% confidence intervals around the estimated treatment effect over event time. Standard errors are corrected for arbitrary clustering at the county level.

Figure 3. Event-Study Analysis of Potential Exposure to Robotics and Arrests, Using de Chaisemartin and D'Haultfœuille Estimates

Panel (a): Property Crime Arrests



Panel (b): Violent Crime Arrests

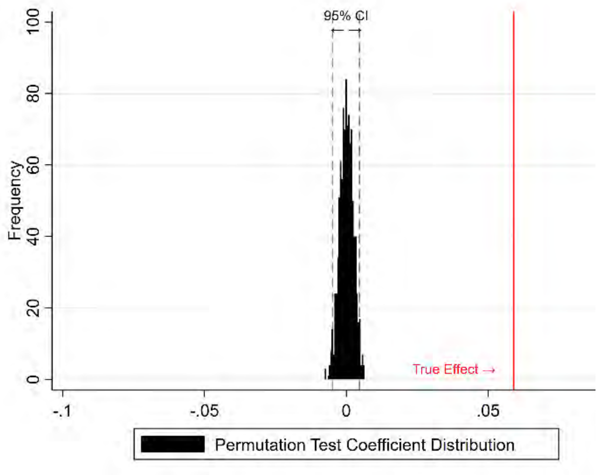


<u>Note</u>: Each regression is estimated using the de Chaisemartin and D'Haultfœuille (2020) estimator and includes controls for county fixed effects, year fixed effects, and the number of agencies. The vertical bars represent 95% confidence intervals around the estimated treatment effects over event time. Standard errors are corrected for arbitrary clustering at the county level. Cutoff values are set at intervals of [0-1), [1-2), [2-3), [3-4), [4-5), and 5 and above.

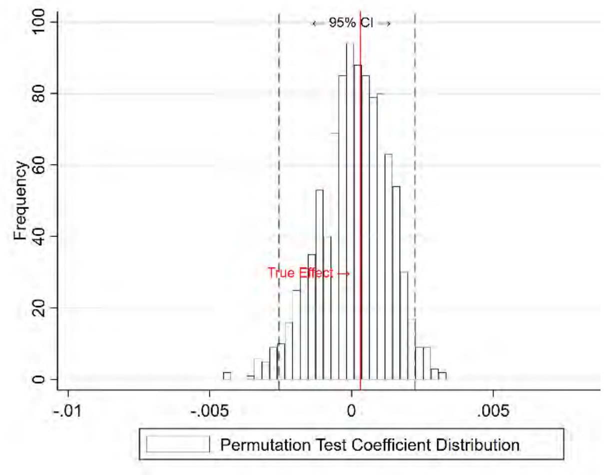
Figure 4. Distribution of Placebo Treatments, Using Randomized Shift and Share, 1993-2010

Panel (a): Placebo Treatments Using Randomized Shift

(i) Property Crime Arrests

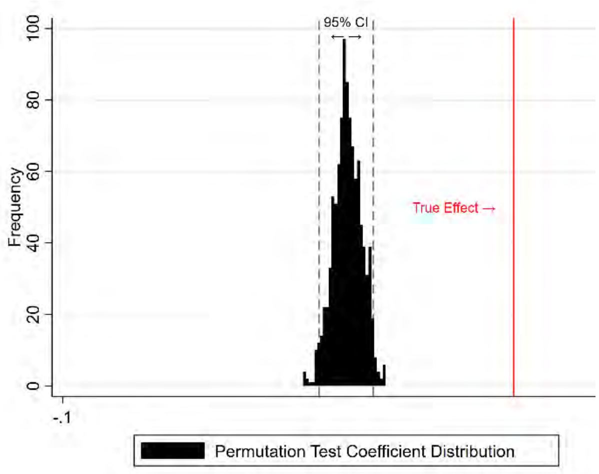


(ii) Violent Crime Arrests

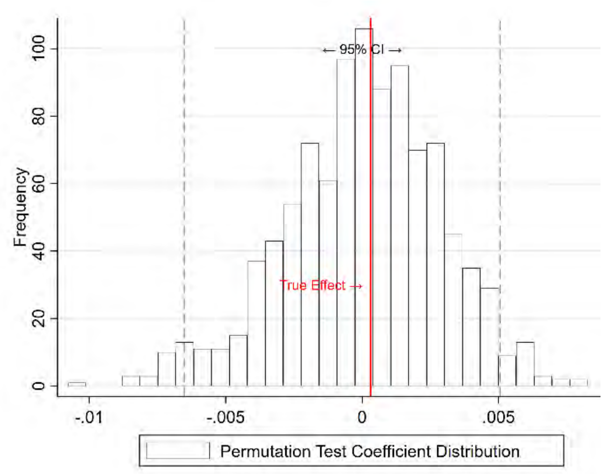


Panel (b): Placebo Treatments Using Randomized Share

(i): Property Crime Arrests



(ii) Violent Crime Arrests



Note: In this analysis, we randomize both the shift in industry-specific robotics (in panel a) and the initial county employment share (in panel b). A 95% confidence interval for the permuted coefficients is plotted alongside the actual treatment effect for reference. Notably, in 1,000 iterations, the true effect for property crime arrests consistently remains outside the confidence intervals yielded by the falsification permutation tests. Conversely, the true effect for violent crime arrests consistently falls within these intervals.

Table 1. Descriptive Statistics, Uniform Crime Reports, 1993 – 2010

	1993-2010	2004-2010	Description
Dependent Variables			
Property Crime			
Adult Property Crime Arrest Rate	4.913 (2.499)	4.719 (2.129)	Number of total adult property crime arrests per 1,000 adults
Male Property Crime Arrest Rate	7.745 (5.032)	6.586 (3.462)	Number of total male property crime arrests per 1,000 male adults
Female Property Crime Arrest Rate	3.281 (2.093)	3.279 (1.969)	Number of total female property crime arrests per 1,000 female adults
White Property Crime Arrest Rate	4.396 (2.686)	4.089 (2.170)	Number of total White adult property crime arrests per 1,000 White adults
Black Property Crime Arrest Rate	15.669 (23.449)	12.504 (9.446)	Number of total Black adult property crime arrests per 1,000 Black adults
Adult Property Arrest Rate (18-24)	14.472 (8.400)	14.676 (8.329)	Number of total property crime arrests per 1,000 adults aged 18 to 24
Adult Property Arrest Rate (25-54)	4.808 (3.054)	4.955 (2.973)	Number of total property crime arrests per 1,000 adults aged 25 to 54
Adult Property Arrest Rate (55-64)	0.837 (0.601)	0.898 (0.611)	Number of total property crime arrests per 1,000 adults aged 55 to 64
Violent Crime			
Adult Violent Crime Arrest Rate	2.052 (1.528)	1.818 (1.154)	Number of total adult violent crime arrests per 1,000 adults
Male Violent Crime Arrest Rate	3.457 (3.838)	2.913 (2.129)	Number of total male violent crime arrests per 1,000 male adults
Female Violent Crime Arrest Rate	0.667 (0.707)	0.632 (0.534)	Number of total female violent crime arrests per 1,000 female adults
White Violent Crime Arrest Rate	1.448 (1.500)	1.297 (1.070)	Number of total White adult violent crime arrests per 1,000 White adults
Black Violent Crime Arrest Rate	6.868 (10.499)	5.664 (4.570)	Number of total Black adult violent crime arrests per 1,000 Black adults
Adult Violent Arrest Rate (18-24)	5.294 (4.327)	5.102 (3.589)	Number of total violent crime arrests per 1,000 adults aged 18 to 24
Adult Violent Arrest Rate (25-54)	2.211 (1.938)	2.149 (1.734)	Number of total violent crime arrests per 1,000 adults aged 25 to 54
Adult Violent Arrest Rate (55-64)	0.401 (0.400)	0.399 (0.375)	Number of total violent crime arrests per 1,000 adults aged 55 to 64
Independent Variables			
Actual Exposure	.	0.572 (0.981)	Number of US robots per 1,000 US workers
Potential Exposure	2.539 (3.690)	3.277 (4.666)	Number of EU robots per 1,000 US workers
Controls			
Number of Agencies	25.288 (31.408)	25.657 (31.020)	Number of law enforcement agencies reporting data to the Uniform Crime Reporting (UCR)

Demographic Controls			
Percent Female	50.952 (1.245)	50.859 (1.231)	Percentage of the Female population
Percent Black or Hispanic	25.930 (19.242)	28.087 (19.517)	Percentage of the Black or Hispanic population
Policing Controls			
Police Expenditure per capita	219.249 (79.522)	272.898 (76.512)	Police expenditure per capita
Police Employment per capita	3.412 (0.783)	3.523 (0.763)	Capita police employment per capita (per 1,000 population)
China Shock			
Chinese Import Penetration	1.726 (1.834)	2.826 (2.230)	Imports from China per thousand workers
Economic and Welfare Controls			
State EITC Rate	0.045 (0.093)	0.061 (0.102)	State-level Earned Income Tax Credit (EITC) rate
TANF Benefits for Family of Four	499.451 (191.654)	524.347 (209.464)	Maximum Temporary Assistance for Needy Families (TANF) benefits for a family of four (in USD)
SNAP Benefits for Family of Four	466.246 (78.490)	542.768 (62.154)	Maximum Supplemental Nutrition Assistance Program (SNAP) benefits for a family of four (in USD)
State Minimum Wage	5.536 (1.079)	6.385 (1.022)	State Minimum Wage
State Housing Price Index	288.524 (123.128)	373.385 (133.851)	Average State Housing Price Index
N	48,335	18,251	

Notes: Weighted means are generated using data from the 1993–2010 Uniform Crime Reports (UCR). Arrest rates are calculated per 1,000 population.

Table 2. Estimates of Relationship Between Potential Exposure to Robotics and Adult Arrests, 1993-2010

	(1)	(2)	(3)	(4)	(5)
	Panel I: Property Crime Arrests				
Potential Exposure	0.0853***	0.0728***	0.0756***	0.0755***	0.0590**
	(0.0261)	(0.0265)	(0.0258)	(0.0256)	(0.0247)
Mean of dependent variable	4.911	4.911	4.911	4.911	4.911
N	46789	46789	46789	46789	46789
Semi-elasticity (%)[a]	1.74%	1.48%	1.54%	1.54%	1.20%
	Panel II: Violent Crime Arrests				
Potential Exposure	0.0003	-0.0001	0.0023	0.0023	-0.0082
	(0.0251)	(0.0255)	(0.0243)	(0.0244)	(0.0232)
Mean of dependent variable	2.050	2.050	2.050	2.050	2.050
N	47117	47117	47117	47117	47117
Semi-elasticity (%)[a]	0.02%	-0.004%	0.11%	0.11%	-0.40%
Controls:					
County and Year FE?	Yes	Yes	Yes	Yes	Yes
Number of Agencies?	Yes	Yes	Yes	Yes	Yes
Demographic Characteristics?	No	Yes	Yes	Yes	Yes
Policing Investments?	No	No	Yes	Yes	Yes
China Shock?	No	No	No	Yes	Yes
Economic & Social Welfare Policies?	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of 4, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

[a]The semi-elasticity is calculated as the percent change in the dependent variable (in percentage terms) from a one robot per 1,000 US workers increase.

Table 3. Estimates of Effects of Potential Exposure to Robotics on Employment and Arrests, by Sample Period

	(1)	(2)	(3)
	1993-2010	**2004-2010**	**2011-2016**
	Panel I: Property Crime Arrests		
Potential Exposure	0.0590**	0.275***	0.102
	(0.0247)	(0.0866)	(0.129)
Mean of dependent variable	4.911	4.715	4.772
N	46789	18095	17993
Semi-elasticity[a]	1.20%	5.95%	3.16%
	Panel II: Violent Crime Arrests		
Potential Exposure	-0.0082	0.0170	-0.0389
	(0.0232)	(0.0399)	(0.0418)
Mean of dependent variable	2.050	1.814	1.614
N	47117	18251	18305
Semi-elasticity[a]	-0.40%	0.94%	-2.41%
	Panel III: Employment to Population Ratio		
	1993-2010	2004-2010	2011-2016
Potential Exposure	-0.321***	-1.127***	0.0517
	(0.0350)	(0.184)	(0.280)
Mean of dependent variable	53.68	52.99	51.23
N	50921	19817	16986
Semi-elasticity[a]	-0.60%	-2.13%	0.10%

***Statistically significant at 1% level **at 5% level *at 10% level.

Notes: Estimates are generated using weighted least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index. For easy interpretation, we multiply the employment-to-population ratio by 100.

[a]The semi-elasticity is calculated as the percent change in the dependent variable (in percentage terms) from a one robot per 1,000 US workers increase.

Table 4. IV Estimates of Relationship Between Actual Exposure to Robotics and Adult Arrests, 2004-2010

	(1)	(2)	(3)	(4)	(5)
	Panel I: Property Crime Arrests				
Actual Exposure	0.239***	0.242***	0.216***	0.216***	0.264***
	(0.0835)	(0.0832)	(0.0809)	(0.0807)	(0.0885)
Mean of dependent variable	4.715	4.715	4.715	4.715	4.715
First-stage F-statistic	170.9	170.5	175.3	174.3	177.6
N	18095	18095	18095	18095	18095
Semi-elasticity[a]	5.06%	5.13%	4.58%	4.58%	5.59%
	Panel II: Violent Crime Arrests				
Actual Exposure	0.0165	0.0218	0.0172	0.0177	0.0163
	(0.0358)	(0.0344)	(0.0336)	(0.0337)	(0.0386)
Mean of dependent variable	1.814	1.814	1.814	1.814	1.814
First-stage F-statistic	168.6	168.0	172.7	171.9	174.6
N	18251	18251	18251	18251	18251
Semi-elasticity[a]	0.92%	1.21%	0.95%	0.98%	0.90%
Controls:					
County and Year FE?	Yes	Yes	Yes	Yes	Yes
Number of Agencies?	Yes	Yes	Yes	Yes	Yes
Demographic Characteristics?	No	Yes	Yes	Yes	Yes
Policing Investments?	No	No	Yes	Yes	Yes
China Shock?	No	No	No	Yes	Yes
Economic & Social Welfare Policies?	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. IV estimates (using EU Robotics as the instrument) are generated using weighted two-stage least squares regression with each county's population as the weight. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

[a]The semi-elasticity is calculated as the percent change in the dependent variable (in percentage terms) from a one robot per 1,000 US workers increase.

Table 5. Long-Differenced IV Estimates of the Relationship Between Actual Exposure to Robotics and Adult Arrests, Main Sample Window and Placebo Lead Arrest Windows

	(1)	(2)	(3)	(4)
Analysis Period:	2004 and 2010	1984 and 1990	1974 and 1980	1974 and 1990
	Panel I: Property Crime Arrests			
ΔActual Exposure	0.0834*	-0.125	-0.0364	-0.0853
	(0.0469)	(0.116)	(0.0668)	(0.115)
Mean of dependent variable	4.715	6.766	5.442	6.223
First-stage F-statistic	110.1	110.3	111.1	111.3
N	2779	2773	2755	2752
	Panel II: Violent Crime Arrests			
ΔActual Exposure	0.0146	0.0209	-0.00756	0.0427
	(0.0215)	(0.0441)	(0.0302)	(0.0530)
Mean of dependent variable	1.814	2.351	1.927	2.148
First-stage F-statistic	108.2	108.4	95.73	95.87
N	2801	2795	2775	2772

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the difference in county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population in the two years listed in the column headings. IV estimates (using change in Potential Exposure as the instrument) are generated using weighted two-stage least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Table 6. IV Estimates of Relationship Between Actual Exposure to Robotics and Adult Arrests, By Offense Type

	(1)	(2)	(3)	(4)
	Panel I: Property Crime Arrests			
	Burglary	Larceny	MTV Theft	Arson
Actual Exposure	0.0305**	0.124**	0.0454***	0.00134
	(0.0144)	(0.0568)	(0.0164)	(0.00110)
Mean of dependent variable	0.818	3.475	0.307	0.0271
First-stage F-statistic	176.6	176.8	177.5	178.7
N	17670	18115	18343	17944
Semi-elasticity[a]	3.73%	3.57%	14.81%	4.94%
	Panel II: Violent Crime Arrests			
	Murder	Rape	Robbery	Agg. Assault
Actual Exposure	0.0008	-0.0015	0.0007	0.0070
	(0.0011)	(0.0021)	(0.0062)	(0.0314)
Mean of dependent variable	0.0404	0.0692	0.332	1.340
First-stage F-statistic	170.5	176.8	177.1	172.5
N	17714	17092	18538	18111
Semi-elasticity[a]	1.99%	-2.21%	0.20%	0.53%

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. IV estimates (using EU Robotics as the instrument) are generated using weighted two-stage least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

[a]The semi-elasticity is calculated as the percent change in the dependent variable (in percentage terms) from a one robot per 1,000 US workers increase.

Table 7. Heterogeneity in IV Estimates, by Gender, Race, and Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Male	Female	Black	White	Ages 18-24	Ages 25-54	Ages 55-64
				Panel I: Property Crime Arrests			
Actual Exposure	0.299***	0.195***	0.568	0.223***	0.906***	0.313***	0.0259
	(0.115)	(0.0557)	(0.363)	(0.0579)	(0.268)	(0.104)	(0.0216)
Mean of dependent variable	6.240	3.059	10.05	3.973	13.73	4.719	0.905
F-statistic	183.0	178.7	114.7	182.7	212.1	175.1	184.7
N	18198	18151	17340	18208	16802	17117	17906
Semi-elasticity[a]	4.79%	6.38%	5.65%	5.61%	6.60%	6.63%	2.86%
				Panel II: Violent Crime Arrests			
Actual Exposure	0.0516	-0.0092	0.0356	-0.0003	0.0176	0.0276	-0.0094
	(0.0559)	(0.0165)	(0.159)	(0.0217)	(0.117)	(0.0479)	(0.0120)
Mean of dependent variable	2.660	0.565	4.675	1.120	4.772	1.958	0.400
F-statistic	182.3	180.3	106.3	184.4	210.9	175.8	184.7
N	18360	18165	17561	18320	17149	17265	17906
Sem-elasticity[a]	1.94%	-1.62%	0.76%	-0.03%	0.37%	1.41%	-2.35%

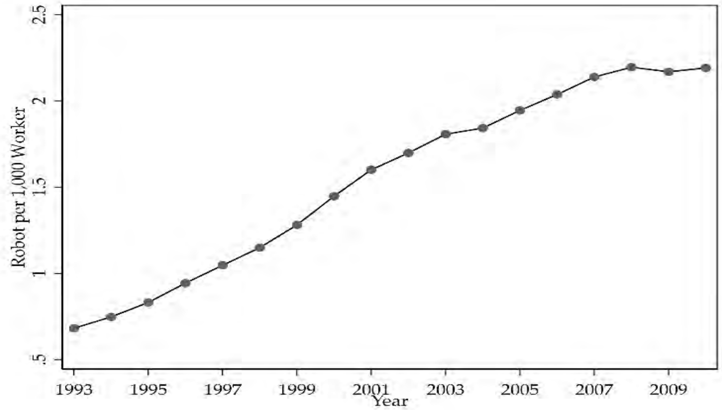
***Statistically significant at 1% level **at 5% level *at 10% level.

Notes: IV estimates (using EU Robotics as the instrument) are generated using weighted two-stage least squares regression with each county's relevant group population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

[a]The semi-elasticity is calculated as the percent change in the dependent variable (in percentage terms) from a one robot per 1,000 US workers increase.

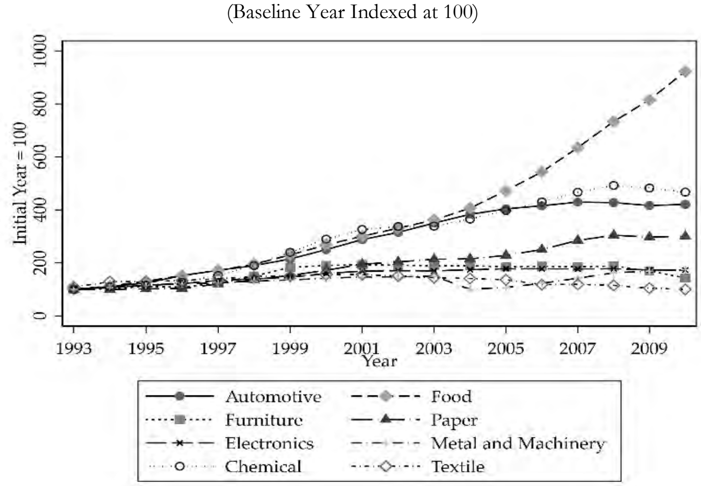
Appendix Figure 1. Robotics Expansion and Manufacturing Employment

Panel (a) Total Robotics Expansion



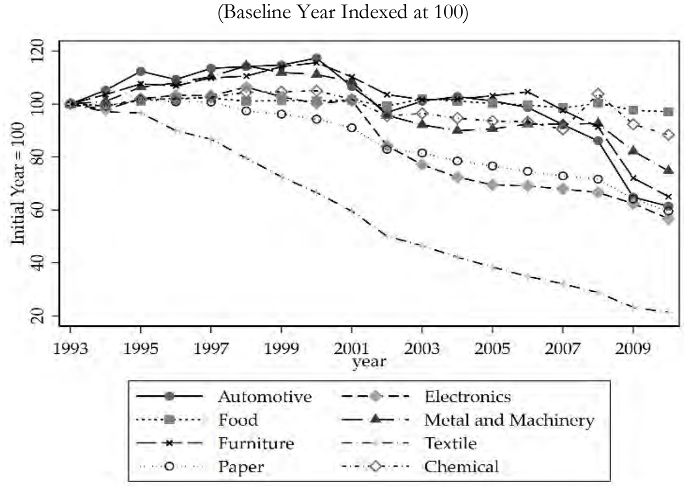
Panel (b): Robotics Decomposition in Manufacturing Sector

(Baseline Year Indexed at 100)



Panel (c): Manufacturing Employment Decomposition

(Baseline Year Indexed at 100)



Appendix Table 1. Robustness of Reduced Form Effect of Potential Exposure to Restrictions on Industries, 1993-2010

	(1)	(2)	(3)	(4)
	Drop High Exposure Area	Automotive Industry Only	Non-Auto Industries	Both Auto and non-Auto
	Panel I: Property Crime Arrests			
Potential Exposure	0.166** (0.0832)			
Potential Exposure in Automotive Industry		0.0509** (0.0238)		0.0527** (0.0235)
Potential Exposure in Non-Automotive Industry			0.284** (0.114)	0.294** (0.114)
Mean of dependent variable	4.967	4.911	4.911	4.911
N	44512	46789	46789	46789
Semi-Elasticity (%)	3.34%	1.04%	5.78%	7.05%
	Panel II: Violent Crime Arrests			
Potential Exposure	0.0080 (0.0591)			
Potential Exposure in Automotive Industry		-0.0110 (0.0234)		-0.0105 (0.0234)
Potential Exposure in Non-Automotive Industry			0.0819 (0.0844)	0.0799 (0.0846)
Mean of dependent variable	2.072	2.050	2.050	2.050
N	44830	47117	47117	47117
Semi-elasticity (%)	0.38%	-0.55%	4.00%	1.39

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Appendix Table 2. Robustness of Reduced Form Effect of Potential Exposure to Robotics on Arrests to Weighing, Functional Form, and Sample Composition, 1993-2010

	(1)	(2)	(3)	(4)
	Unweighted	Log (arrest rate) is dependent variable	Large Counties (>10,000 pop)	Balanced Panel
	Panel I: Property Crime Arrests			
Potential Exposure	0.0414***	0.0241**	0.0509***	0.0561***
	(0.0122)	(0.0106)	(0.0143)	(0.0139)
Mean of dependent variable	3.517	1.431	3.961	3.919
N	46789	43117	36836	36937
Semi-elasticity (%)	1.18%	2.41%	1.29%	1.43%
	Panel II: Violent Crime Arrests			
Potential Exposure	0.0043	0.0104	0.0055	0.0110*
	(0.0052)	(0.0134)	(0.0061)	(0.0059)
Mean of dependent variable	1.385	0.422	1.514	1.530
N	47117	42190	37163	37281
Semi-elasticity (%)	1.57%	1.04%	0.36%	0.53%

***Statistically significant at 1% level **at 5% level *at 10% level.

<u>*Note*</u>: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Appendix Table 3. Sensitivity of Reduced Form Estimates to Policing Controls and Use of NIBRS Dataset to Measure Criminal Incidents, 1993-2010

	(1)	(2)	(3)
Data Source:	NIBRS	NIBRS	UCR
	Panel I: Property Crime Arrests		
Potential Exposure	0.0100*	0.0634*	0.0636**
	(0.00525)	(0.0364)	(0.0257)
Mean of dependent variable	8.406	8.406	4.936
N	28478	28508	40528
Semi-elasticity (%)	1.00%	0.75%	1.29%
	Panel II: Violent Crime Arrests		
Potential Exposure	0.00157	0.102	-0.0063
	(0.00525)	(0.114)	(0.0238)
Mean of dependent variable	27.54	27.54	2.067
N	28469	28508	40833
Sem-elasticity (%)	0.16%	0.37%	-0.30%
Estimation Strategy:	Poisson	OLS	OLS
Full Controls	Yes	Yes	Yes
Additional Policy Controls	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

__Note__: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Additional policy controls include a comprehensive set of variables for police presence, law enforcement resources, and policing practices. Following Fone et al. (2023), we use LEMAS and CSLLEA data to capture (1) local policing resources (per capita community officers, school resource officers, patrol officers, and police budgets) and (2) local policing policies (hate crime units, racial profiling policies, and policies for diverse cultural populations).

Appendix Table 4. Estimates of Relationship Between Potential Exposure to Robotics and Adult Arrests, 2004-2010

	(1)	(2)	(3)	(4)	(5)
			Panel I: Property Crime Arrests		
Potential Exposure	0.262***	0.264***	0.231***	0.231***	0.275***
	(0.0861)	(0.0850)	(0.0817)	(0.0814)	(0.0866)
Mean of dependent variable	4.715	4.715	4.715	4.715	4.715
N	18095	18095	18095	18095	18095
Semi-elasticity (%)	5.56%	5.59%	4.91%	4.90%	5.83%
			Panel II: Violent Crime Arrests		
Potential Exposure	0.0181	0.0238	0.0184	0.0189	0.0170
	(0.0388)	(0.0369)	(0.0356)	(0.0356)	(0.0399)
Mean of dependent variable	1.814	1.814	1.814	1.814	1.814
N	18251	18251	18251	18251	18251
Semi-elasticity (%)	1.00%	1.31%	1.01%	1.04%	0.94%
Controls:					
County and Year FE?	Yes	Yes	Yes	Yes	Yes
Number of Agencies?	Yes	Yes	Yes	Yes	Yes
Demographic Characteristics?	No	Yes	Yes	Yes	Yes
Policing Investments?	No	No	Yes	Yes	Yes
China Shock?	No	No	No	Yes	Yes
Economic & Social Welfare Policies?	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Appendix Table 5. First-Stage Effects of Potential Exposure to Robotics on Actual Exposure to Robotics, 2004-2010

	(1)	(2)	(3)	(4)	(5)
Potential Exposure	1.095***	1.089***	1.073***	1.072***	1.042***
	(0.0830)	(0.0824)	(0.0800)	(0.0802)	(0.0769)
Kleibergen-Paap F-statistic	170.9	170.5	175.3	174.3	177.6
N	18767	18767	18767	18767	18767
Controls:					
County and Year FE?	Yes	Yes	Yes	Yes	Yes
Number of Agencies?	Yes	Yes	Yes	Yes	Yes
Demographic Characteristics?	No	Yes	Yes	Yes	Yes
Policing Investments?	No	No	Yes	Yes	Yes
China Shock?	No	No	No	Yes	Yes
Economic & Social Welfare Policies?	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is Actual Exposure. Estimates are generated using weighted least squares regression with each county's population as the weight. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Appendix Table 6. OLS Estimates of Relationship Between Actual Exposure to Robotics and Adult Arrests, 2004-2010

	(1)	(2)	(3)	(4)	(5)
			Panel I: Property Crime Arrests		
Actual Exposure	0.112***	0.117***	0.0991***	0.0988***	0.108***
	(0.0365)	(0.0367)	(0.0357)	(0.0356)	(0.0388)
Mean of dependent variable	4.717	4.717	4.717	4.717	4.717
N	18141	18141	18141	18141	18141
Semi-elasticity (%)	2.38%	2.48%	2.10%	2.10%	2.30%
			Panel II: Violent Crime Arrests		
Actual Exposure	-0.0104	-0.0043	-0.0075	-0.0073	-0.0095
	(0.0157)	(0.0149)	(0.0147)	(0.0147)	(0.0166)
Mean of dependent variable	1.815	1.815	1.815	1.815	1.815
N	18297	18297	18297	18297	18297
Sem-elasticity (%)	-0.57%	-0.24%	-0.41%	-0.40%	-0.52%
Controls:					
County and Year FE?	Yes	Yes	Yes	Yes	Yes
Number of Agencies?	Yes	Yes	Yes	Yes	Yes
Demographic Controls?	No	Yes	Yes	Yes	Yes
Policing Control?	No	No	Yes	Yes	Yes
China Shock?	No	No	No	Yes	Yes
Econ & Social Welfare Controls?	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level.

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.

Appendix Table 7. Sensitivity of IV Estimates to Restrictions on Industries, 2004-2010

	(1)	(2)	(3)	(4)
	Drop High Exposure Area	Automotive Industry Only	Non-Auto Industries	Use both Auto &non-Auto
		Panel I: Property Crime Arrests		
Actual Exposure	0.766 (0.555)			0.239*** (0.0747)
Actual Exposure in Automotive Industry		0.224*** (0.0722)		
Actual Exposure in Non-Automotive Industry			0.857+ (0.537)	
Mean of dependent variable	4.761	4.761	4.761	4.761
First-stage F-Statistics	138.0	231.5	364.2	211.3
N	16682	18095	18095	18095
Semi-elasticity (%)	16.07%	4.75%	18.18%	5.07%
		Panel II: Violent Crime Arrests		
Actual Exposure	0.273 (0.230)			0.0022 (0.0327)
Actual Exposure in Automotive Industry		-0.00626 (0.0316)		
Actual Exposure in Non-Automotive Industry			0.207 (0.235)	
Mean of dependent variable	1.838	1.814	1.814	1.814
First-stage F-Statistics	123.8	228.3	350.7	206.1
N	16830	18251	18251	18251
Semi-elasticity (%)	14.84%	-0.34%	11.42%	0.12%

***Statistically significant at 1% level **at 5% level *at 10% level +at 11% level

Note: The dependent variable is the county-by-year number of arrests involving arrestees ages 18 and older per 1,000 population. Estimates are generated using weighted least squares regression with each county's population as the weight. All models include controls for county fixed effects, year fixed effects, number of reporting agencies, and the full set of observable controls. Demographic controls include the percentage of the population that is female, Black or Hispanic. Policing control includes nominal log per capita police expenditures and log per capita police employment (per 1,000 population). The measurement of the China shock follows the approach outlined in Autor et al. (2013). Economic and social welfare controls include state EITC credit rate, measured as a percentage of the Federal Credit, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the maximum AFDC/TANF benefit for a family of four, state minimum wage, and housing price index.