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ABSTRACT

Crime and the Labor Market*

The economics of crime has emerged as a critical field over the past 30 years, with economists increasingly exploring the causes and consequences of criminal behavior. This chapter surveys key contributions and developments from labor economists, who investigate the (often two-way) intersection of crime with labor market factors, such as education, wages, and unemployment. The chapter underscores the importance of understanding criminal decision-making in economic analysis through the lens of opportunity costs and labor market conditions. Methodological advancements, particularly those addressing causation, have propelled the field forward, enabling more accurate conclusions to be drawn for policy recommendations. The chapter also explores the role of social policies and international contexts, emphasizing the need for evidence-based reforms to effectively reduce crime. This comprehensive review underscores the transformative impact of economics on crime research and its potential to influence real-world policies.

JEL Classification: K42

Keywords: economics of crime, labor market, criminal record, education, research directions

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1. Introduction

High rates of crime are a major societal concern in countries worldwide, and there are many active debates on both the causes of criminal behavior and what policy reforms should be introduced to lower crime. Academic contributions to this debate have traditionally been from fields like criminology, sociology, and law. Over the last 30 years, however, a new tradition and field has emerged – *the economics of crime*. Economists are increasingly directing their attention to studying the causes and consequences of criminal behavior. The economics of crime can definitely be characterized as a ‘growth’ field in economics: as seen in Figure 1 below, the annual number of crime papers published in top general interest and field journals, including labor journals, has sky-rocketed from just three in 1990 to 93 in 2023. Moreover, the vast majority of this explosion happened after the publication of Richard Freeman’s first Handbook chapter on crime in 1999: 58 crime articles (in total) were published from 1990 to 1999, while 837 have been published since. A new handbook chapter on the economics of crime is long overdue.

Labor economists have played a significant role in the growth of this field for two central reasons. First, there is a substantive overlap between crime and topics that are more traditionally perceived as labor economics (e.g., education, wages, unemployment, discrimination). As will be seen throughout this chapter, criminal justice populations are negatively selected in many dimensions (e.g., worse family backgrounds, less education, higher unemployment rates, lower earnings) than the general population. Moreover, the theoretical framework put forward by Gary Becker in 1968 to study criminal behavior highlights that criminal decision making is not just determined by the perceived probability and severity of punishment but also by the opportunity costs of committing crime – namely legitimate labor market opportunities. We briefly review this framework below.

Second, labor economists have brought the “credibility revolution”, i.e., a methodological toolkit to disentangle correlation from causation, to the study of crime. Figure 1 below decomposes the annual number of crime publications into three categories: theoretical, empirical but non-causal, and empirical and causal. Before 1999, there were almost no causally identified empirical papers; but more than 70% of the 895 total papers included in this figure in fact fit into this category. In other words, the vast majority of the growth in the economics of crime research is driven by empirical research that we classify as causal. And much of it has occurred during the last ten years, which is later than when the credibility revolution diffused through many other sub-fields of applied economics. Why? As will be seen throughout this chapter, the study of crime poses many challenges to researchers. These range from traditional identification issues, like omitted variable bias and simultaneity, to less common issues like measurement error due, for instance, to the ‘non-random’ under-reporting of crime or discrimination by the police or judiciary. Moreover, research ethics make one of the economics discipline’s go-to tools for causal identification – randomized control

trials – a rarely viable alternative in the study of crime.¹ However, increased access to individual level crime and victimization data, a necessary ingredient for many causal quasi-experimental research designs, has helped contribute to the take-off of this revolution. From the perspective of helping policy makers implement evidence-based reforms, the credibility revolution in the economics of crime has the potential to have large real-world societal impacts.

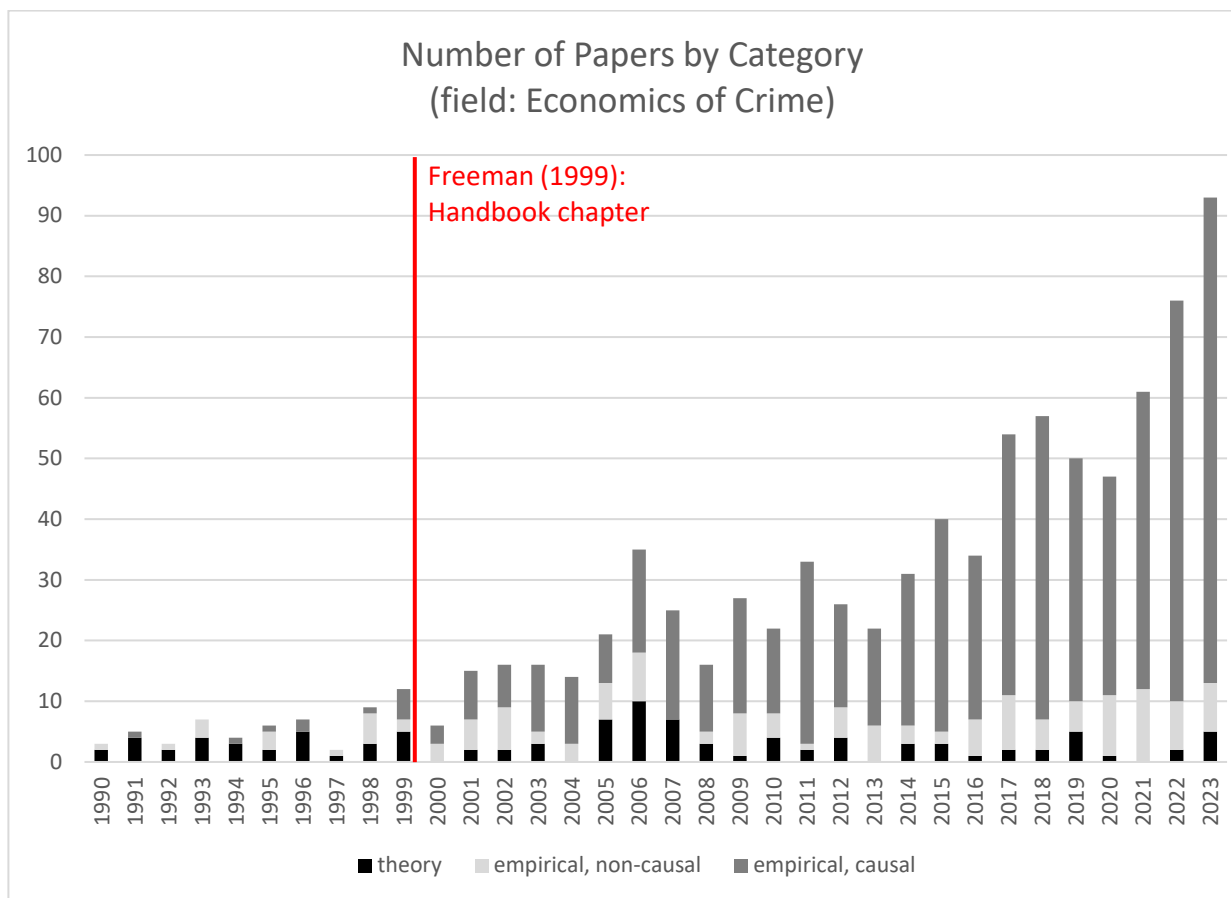


Figure 1. The Growth of the Economics of Crime

Note – This figure plots the annual number of publications in the Economics of Crime field, including a decomposition in the style of research: theoretical, non-causal empirical, and causal empirical. The publication data are sourced from the database maintained by Jennifer Doleac; see <https://jenniferdoleac.com/resources/> for a link to the database. The database includes crime and criminal justice system related papers in general interest and top field economics journals: AER, QJE, RESTUD, JPE, Econometrica, RESTAT, JEEA, AEJ: Applied, AEJ: Policy, AER: Insights, EJ, JOLE, JPUBE, JLE, JURBANE, JPAM, JHR, JDE, JEH, EEH, JHE. Classification of the type of research is done by the authors of this handbook chapter.

The economics of crime literature is quite broad in scope, and has been greatly influenced by the Becker (1968) framework (and that of Ehrlich, 1973) where individuals decide whether or not to engage in

¹ Stevenson (2023) provides a recent review of randomized controlled trials conducted to study crime.

crime by carrying out a cost-benefit calculation under uncertainty. A stylized version of this framework can be depicted in the following equation:

$$(1 - \pi)U(W_C) - \pi S > U(W_L),$$

where π is the probability of getting caught, S is the sanction, and W_C and W_L are the economic returns to criminal and legitimate work, respectively. In this framework, an individual decides to commit a crime by rationally comparing the expected costs and benefits from criminal and legal activities. The left side of the expression displays the expected benefits of crime, which is the expected returns from illegal work offset by the expected costs or punishment. The right side of the expression includes the expected returns from legal work, which we often think of as the opportunity cost of committing crime. This framework makes clear that the criminal justice system can play a significant role in deterring crime by impacting the probability and severity of punishment. The earliest economics of crime research, and a still growing empirical literature, indeed focuses on testing these relationships. Though beyond the scope of this handbook chapter, we point the interested reader to some recent surveys of this literature (Nagin, 2013; Chalfin and McCrary, 2017; Owens and Ba, 2021; Apel, 2022).

Rather, this handbook chapter focuses on that empirical research inspired by the other terms in the above equation: the returns from legal and illegal ‘work’. The returns from legal work are of course a function of one’s education and the labor market. Much of the empirical literature thus evaluates the implications of the economic model of crime that improved educational attainment (e.g., quantity and quality) and better labor market conditions (e.g., wages, income, employment) decrease crime. This contrasts to date a much smaller literature on the impact of the economic returns to criminal work, including, for instance, the value of the loot. Sections 3 and 4 of this chapter review the literature on the impact of labor markets on crime and crime on labor market outcomes, respectively. Section 5 reviews the literature linking education and crime.

This chapter highlights not only that there is more economics of crime research, but that it has changed in a multitude of ways since the Freeman (1999) handbook review of crime and the labor market. There are a number of recurring themes throughout the chapter, each of which has very much altered the takeaways from what the research says, at least relative to when Freeman (1999) was drawing his general conclusions. First, there has been a significant shift from using aggregated data (e.g., at the US state level) to highly disaggregated data – with an increasing prominence for individual level micro data and individual decisions. Second, the literature is increasingly non-US centric, in part because of the availability of micro-level crime data in a number of international contexts. Third, there has been an increasing shift from only focusing on the role for criminal justice policy (e.g., police and prisons) to affect crime to also the role for

social policy (e.g., schools and labor market barriers). Fourth, the literature is increasingly considering that social interactions and spillovers may be important in explaining crime behavior and measuring the costs and benefits of policies impacting crime. Fifth, the new economics of crime literature pays careful attention to the causal identification challenges and quasi-experimental solutions. Sixth, new doors are being opened in the field to study questions beyond the Becker framework, like the costs of crime victimization (including labor market costs) and the role of criminal organizations; we offer a discussion of these especially new literatures in the conclusion.

The remainder of this chapter proceeds as follows. Given the international scope of the research surveyed, Section 2 begins by discussing some of the similarities and differences in, for instance, criminal justice systems around the world. We then survey the labor market-crime literature in Section 3, with sections on the roles of wages and income, unemployment, summer youth employment programs, and the returns to illegitimate labor. Section 4 presents the literature concerned with how criminal records impact labor market outcomes, the willingness of firms to hire workers with records, and the effect of policies aimed at improving labor market outcomes. Section 5 reviews the education – crime literature, including that on causal impacts, on incapacitation, the quantity and quality of schooling, productivity and on education policy. Section 6 concludes by highlighting some of the new directions in which the labor and crime research is going, highlighting the economics of victimization and organized crime.

2. Descriptive Statistics and Stylized Facts: An International Perspective

This chapter surveys research on crime and the labor market in a wide range of international contexts, including, for instance, the United States, Brazil, United Kingdom, France, Italy, Sweden, Norway, Finland, Denmark, and the Netherlands. The international breadth of this literature speaks to (i) societal concerns about crime being a worldwide phenomenon, even if the nature of crime varies across countries, and (ii) the implications of Becker's (1968) economic model of crime, e.g., that increased legitimate labor market opportunities should decrease crime, not being country specific.

One fundamental challenge in studying the causes and consequences of criminal behavior is accurately observing and measuring crime. There are many potential sources of measurement error in official crime statistics. For instance, aggregated arrest data do not only reflect crime incidents but also reporting rates and police clearance rates. Moreover, these sources of measurement error can vary systematically across crime categories and international contexts. This issue is highlighted in Figure 2. Homicide rates, which are the most accurately recorded both within and across countries, are negatively related to GDP per capita. Burglary rates, which are more sensitive to under-reporting, legal system differences, and police effort, are positively related to GDP. Similar patterns are seen for other crime categories.

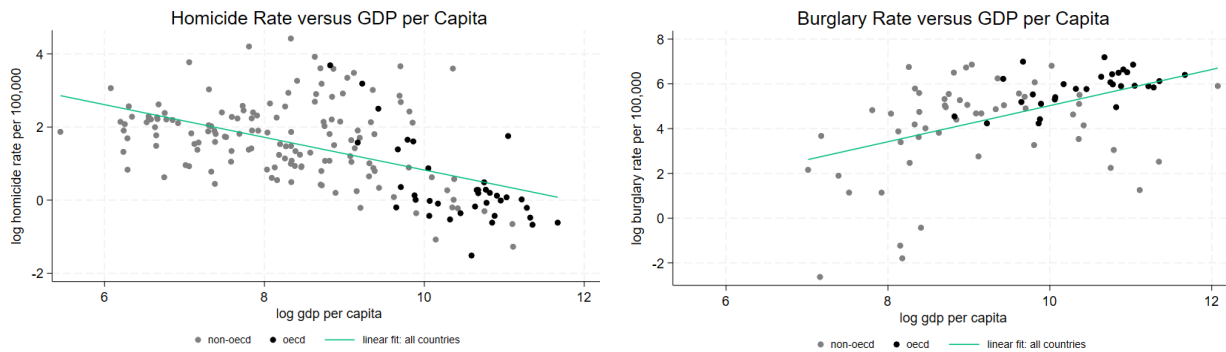


Figure 2. GDP per Capita and Crime Rates Across Countries

Note – These statistics are based on the following sources. Burglary rates per 100,000 across countries in 2018 were obtained from <https://dataunodc.un.org/dp-crime-corruption-offences>. GDP per capita in 2018 and the homicide rate per 100,000 were obtained from the World Bank and World Health Organizations, respectively. See: <https://databank.worldbank.org/reports.aspx?source=2&series=NY.GDP.PCAP.CD&country=#> and <https://www.who.int/data/gho/data/indicators/indicator-details/GHO/estimates-of-rates-of-homicides-per-100-000-population>.

A significant advancement of the new crime and labor market literature has been the shift away from many of the problems associated with aggregate statistics to micro-level data. This is in fact one practical reason for the non-US focus of this research, and the over-representation of research in a Scandinavian context; in selected countries, population-wide crime and victimization registers can be matched to other national education, labor market, and health registers.

At the same time, studying the link between crime and the labor market in an international context is motivated by the fact that the US is an outlier in many dimensions, three of which are depicted in Figure 3, related to crime and the criminal justice system. The 2018 US homicide rate per 100,000 persons is 5.78, which is more than three times larger than neighboring Canada and nearly ten times that of countries like Spain and Norway. Though cross-country comparisons of crime rates are typically complicated by differential crime definitions and reporting rates, such concerns are minimal for the case of homicides. Second, incarceration rates (depicted by the black squares in Figure 3) are also markedly higher in the US than Canada or Western Europe. In fact, the 2021 US incarceration rate of 531 inmates per 100,000 persons is even higher than that of Brazil, which had a rate of 390 in 2022. Though these international differences in incarceration rates in part reflect differential crime rates, this is not the only explanation; after all, the Brazilian homicide rate was more than five times larger than that in the US. Rather, there are also international differences in punishment severity. At the extreme, this is seen in Figure 3 with respect to the death penalty. All depicted states but the US have abolished capital punishment for all crimes in both peacetime and wartime (the complete abolition date is listed on the x-axis).

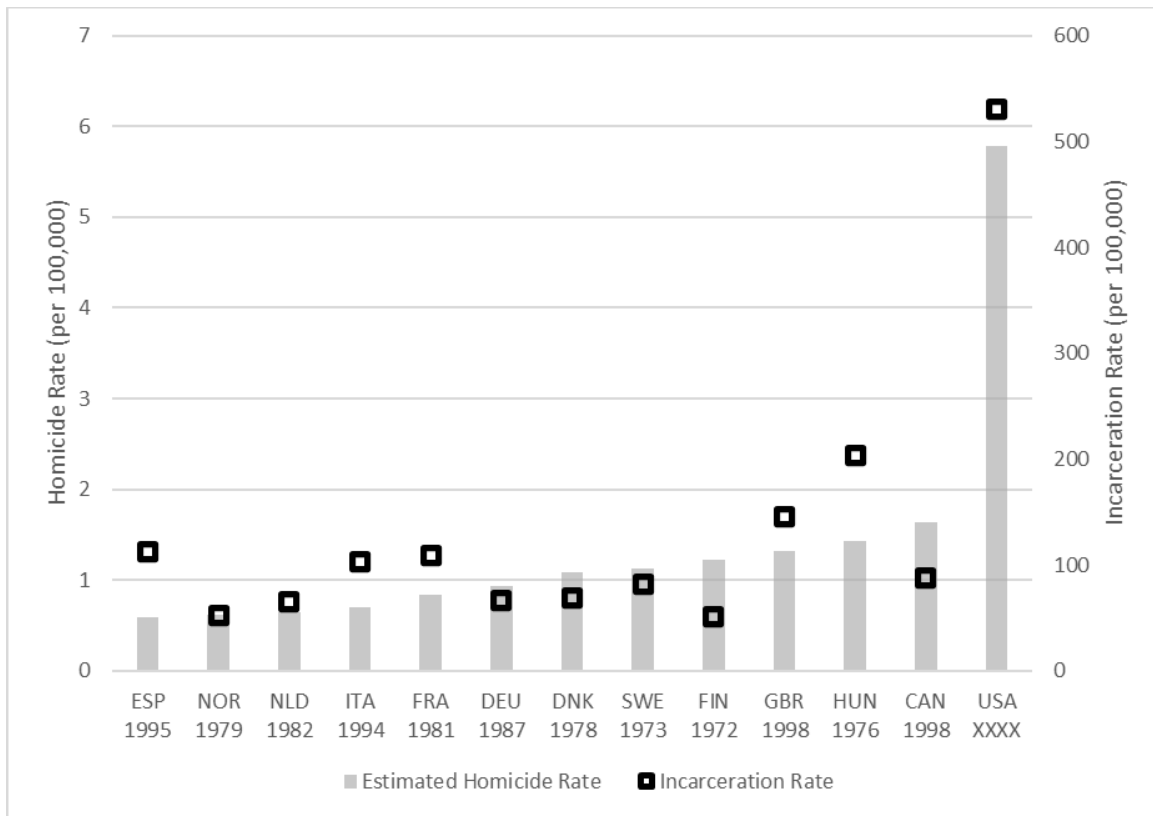


Figure 3. International Homicide and Incarceration Rates.

Note – For each country, we present the year the death penalty was completely abolished, including for war related crimes under the country name. This information was sourced for most countries from <https://deathpenaltyinfo.org/policy-issues/international/countries-that-have-abolished-the-death-penalty-since-1976>. For other countries (Sweden, Finland, Spain, and Canada), we conducted web searches. The death penalty was abolished for peacetime crimes even earlier in many countries. Homicide rate statistics (gray bars) are estimates provided by the World Health Organization for 2018. <https://www.who.int/data/gho/data/indicators/indicator-details/GHO/estimates-of-rates-of-homicides-per-100-000-population> Incarceration rate statistics (black squares) were sourced from the most recent available year (2021-2024) from <https://www.prisonstudies.org/world-prison-brief-data>.

But there are also many less extreme dimensions in which ‘incarceration’ differs across countries, including both the rate at which offenders are sentenced to prison (especially for more minor drug and property offenses) as well as the sentence length. For instance, Hjalmarsson and Lindquist (2022) note that the average time spent in prison for inmates convicted in Sweden from 1991 to 2001 was 4.7 months, which compared to about 8 months in Western Europe on average in 2001 and more than 30 months in US state and federal prisons. Moreover, the prison conditions or experience vary substantially across countries, and even across regions or states within countries. Prison conditions are known to be especially good in Scandinavian countries. In 2015, Sweden and Norway spent more per prisoner than any other country (nearly 150,000 US dollars) and about four times that of the US (Hjalmarsson and Lindquist, 2022). This translates into significant cross-country differences in the inmate to staff ratio, in-prison treatment programs and rehabilitation conditions, healthcare, overcrowding, and job training and education opportunities.

The impact of the labor market on crime or that of a criminal record on employment outcomes may depend on more than the nature of the criminal justice system and sanctions. In particular, international differences in the labor market and social welfare systems may play just as significant a role. Public spending on social expenditures was about 20% of GDP in the OECD in 2015. But this masks significant heterogeneity across member countries, ranging from 30% or more of GDP in France, Finland, and Denmark to less than 20% of GDP in the US, Canada, and Netherlands.²

Other international policies and norms may also be especially salient to the crime and labor market question. These include: (i) whether having a criminal record actually disqualifies one from welfare eligibility or particular jobs, (ii) the availability of criminal records online, (iii) the extent to which criminal background checks are mandated and/or used during hiring, and (iv) the culture of whether offenders should be given a second chance.

This section has thus far highlighted the many criminal justice and societal differences across countries faced by potential or former offenders. These differences are of two-fold importance. First, they motivate the need for the replication of research across different countries and contexts. Second, understanding these institutional differences may play a fundamental role in explaining heterogenous results.

In light of the discussion, it is natural to ask whether one can at all generalize or apply the results of one study or context to another. We believe the answer to this question is yes, given that, despite these many differences, there are also common stylized facts that characterize criminal justice populations worldwide. For instance, in terms of demographics, offenders are disproportionately young and male. Moreover, the age-crime profile, in which crime tends to increase until young adulthood and then decrease, is a pattern seen across countries and throughout history (Bindler and Hjalmarsson, 2017). Criminals also tend to be negatively selected in many dimensions. With regards to education, for instance, 41% of US prisoners in 1997 had not completed high school (compared to 18% of the general population) while more than 75% of Italian convicts in 2001 had not completed high school (Harlow, 2003; Buonanno and Leonida, 2006). Similar findings are documented in Sweden (Hjalmarsson et al, 2015) and the UK (Machin, Marie and Vujic, 2011). Finally, offenders often have mental health and/or substance abuse problems themselves and disproportionately come from disadvantaged family backgrounds, including single parent households, having criminal fathers, or parents with alcohol or mental health problems (Hjalmarsson, 2022).

² See https://stats.oecd.org/Index.aspx?datasetcode=SOCX_AGG.

3. Labor Market Impacts on Crime

The prime research focus over the years has been to study whether, and how, the labor market impacts crime. Over time, the research has been framed in different, but mutually compatible ways where empirical connections between crime and different labor market measures have been studied. These have evolved considerably since the previous handbook chapter and on this aspect, reflecting the very recent work in this area of the past ten years or so, and since some other more recent reviews of work on crime and the labor market (e.g., Draca and Machin, 2015).

That said, through time, the crime-unemployment relation is the one that has received most attention. Reaching strong conclusions in the early work was hampered both by data limitations and by biases from the use of aggregated cross-sectional data and reverse causation. At the time of Freeman's (1999) economics of crime Handbook chapter, it was becoming clear that the mostly aggregate studies he reviewed did not find much evidence of unemployment being important. Indeed, in his review, Freeman (1999) summarized the evidence then as follows: "unemployment is related to crime, but if your prior was that the relation was overwhelming, you were wrong. Joblessness is not the overwhelming determinant of crime that many analysts and the public a priori expected it to be".

Around that time, some new studies emerged that placed a directed focus on wages, earnings or income rather than unemployment, and it turns out they had more empirical success. In this section, we therefore begin with a discussion of these studies, and then turn to the newer literature that revisits a lack of work as a determinant of criminality. Interestingly, and for a variety of reasons to be discussed, this work reaches a quite different view in comparison to the take-away from the Freeman (1999) chapter. The new, more voluminous literature on crime and the labor market – both the new studies of unemployment and that on wage effects – is much more supportive of the economics of crime model predictions that labor market outcomes matter for crime. This is especially so for the impact of joblessness, but also for wages and income; moreover, the methodological advances in some studies show such effects to be causal.

3.1. Wages and Income

In the basic economics of crime framework, legitimate income opportunities represent the "opportunity cost" of crime and, as such, are one of the main deterrents to criminal behavior. From a theoretical perspective, differences in actual and potential earnings may explain, for instance, why the rich typically commit less crime than the poor, or why more educated individuals commit fewer crimes than the less educated. At the same time, individuals with different income opportunities may differ along several other dimensions, such as family background, the areas where they live, the peers they are exposed to, and so on. Moreover, involvement in crime may itself affect income opportunities in legitimate markets, as previous offenders typically face implicit or explicit barriers to accessing legitimate economic activities, as discussed

at length in Section 4 below. For all these reasons, empirically identifying the effect of income opportunities on crime requires plausibly exogenous variation in legitimate income, particularly labor earnings which is the main source of income for poorer individuals at the margin between committing or not committing crimes.

As discussed already, the first round of crime and labor market research primarily focused on the issue of whether crime rates, in particular property crime rates, were related to unemployment rates in a variety of settings. It is on the basis of these studies that Freeman (1999) reported to find any relationship to be “fragile, at best”. Around the same time, or just after, the publication of the Freeman (1999) review, a first set of studies looking at earnings effects on crime emerged. Typically, though not always, these studies also looked at unemployment effects and concurred with the earlier work that these effects were hard to detect. But they did find that wages, earnings or income mattered for crime.

Grogger (1998) is the first of these studies. Using the National Longitudinal Survey of Youth (NLSY) cohort data, he shows that many people who self-report some criminal activity are also working in the labor market. This both makes them sensitive to wage changes along an extensive margin between legal and illegal work, and also gives a rationale for weak effects from lack of work. Indeed, in estimates of a probit model of crime incidence there is a strong negative association of crime with wages and less of an effect from unemployment.

Gould et al. (2002) provide evidence based on a US panel of counties, using the wages for non-college educated males as their earnings measure. They include wage and unemployment measures contemporaneously, which allows for some benchmarking of effects. For example, over the 1979-1993 period the recorded 23.3 percent fall in unskilled wages predicted 43 percent of the total increase in property crime while the 3.05 percentage point increase in unemployed predicted 24 percent of the change. Wages also dominated the results for violent crime (predicting 53 percent of the increase versus 8 percent for unemployment). They address potential problems related to the endogeneity of crime and economic conditions using an instrumental variables strategy that interacts fixed state-level characteristics with aggregate economic shocks (following the logic of Bartik, 1991). They find that the instrumented estimates are larger than those estimated by least squares for the wage measure, but are lower for unemployment.

Machin and Meghir (2004) analyze a 20-year panel of police force areas of England and Wales. They use a wage measure based on the 25th percentile of the distribution and empirically find that the marginal effect of a 10 percent increase in the wage measure corresponds to 0.7 percentage point fall in the crime rate. Similarly, Entorf and Spengler’s (2000) analysis of data on German regions over time uncovers significant associations between crime and income, again in line with the notion that changing economic incentives in the labor market matter for crime.

These panel studies all find that wages matter for crime, and more so than unemployment. Another way to consider the relationship between crime and wages is the longitudinal analysis of arrests and wages of Grogger (1995). In a study that is arguably ahead of its time in utilizing administrative data (in this case, for California on criminal histories and labor market earnings), his empirical strategy includes fixed effects in a longitudinal earnings model to enable tracking out the wage effects of arrest over a number of quarters. Whilst the effects are moderate – equal to around 4 percent of earnings in the quarter contemporaneous with arrest and falling to an average of around 2-3 percent over the next 5 quarters before fading out to a zero statistical effect – they are also suggestive of a wage-crime empirical connection.

A small body of work has taken a rather different approach, by looking at what happens to crime when minimum wage floors are increased. For police force areas of England and Wales, Hansen and Machin (2002) report that crime fell in relative terms in places where the UK introduction of a national minimum wage in 1999 boosted low wages by more. In the US, there is evidence that reductions in recidivism result from minimum wage increases in Agan and Makowsky (2023); Corman and Mocan (2005) also report a negative time series relation in New York City between crime and the real minimum wage. However, other US work does not find evidence of crime reduction (Beauchamp and Chan, 2014; Fernandez et al., 2014; Fone et al. 2023). Thus, it seems reasonable to conclude that the evidence from this small number of studies proves mixed.

Finally on wage and income effects, a study that attempts to move to causal effects is the interesting economic history paper of Bignon et al. (2018). They track the progressive influx of the phylloxera virus that diffused across areas in France in the 19th century. In doing so it hugely damaged wine production, and both reduced the incomes and livelihoods of wine producers and impacted crime. In an instrumental variable setting, they show the phylloxera crisis caused a substantial increase in property crime rates and a significant decrease in violent crimes, thereby generating causal effects of income on crime. Their interpretation of the findings offers interesting insight, as they conclude the property crime increase arises from what the economic model of crime suggests as income opportunities degenerated, but that the violent crime increase in this setting comes about from reduced alcohol consumption as wine was both less available and more expensive.

3.2. Unemployment

Returning to the question of how unemployment and lack of work affect crime, it is evident that job loss is possibly the biggest (negative) shock to labor earnings. Workers losing their jobs experience an immediate drop in legitimate income and, in case they struggle to find a new job, they may experience even greater income losses in the medium to long run. Though the severity and timing of these income losses depend on the specific institutional context, particularly the generosity and duration of unemployment insurance, job

loss always brings significant risks of prolonged periods of unemployment with long-lasting consequences for human capital accumulation, health, and earning potential (see, e.g., Pissarides, 1992, and von Wachter, 2020). These insights underpin the direction to which some of the more recent work on crime and unemployment has moved.

Any economic model of crime would predict that job loss increases the probability of committing crimes. However, as already noted, the earlier empirical studies found only mixed evidence of an effect of job loss on crime. For instance, Freeman and Rodgers III (1999) estimate that, across US states, (youth) crime increases by as little as 1.5% for a one-point increase in unemployment, and Cullen and Levitt (1999) even estimate a null effect across US cities. One big limitation of these earlier studies is that they rely mostly (or exclusively) on correlational evidence. In most cases, they regress crime rates on unemployment rates across geographical areas – states, counties, or cities – without a clear strategy for identifying causal effects. More recent papers exploiting plausibly exogenous variation in economic shocks across areas, as predicted by shift-share instruments interacting national-level sectoral shocks with local sectoral shares, tend to find stronger evidence of a causal effect of unemployment on crime.

Raphael and Winter-Ebmer (2001) estimate the effect of unemployment on crime across US states over the period 1971-1996 using state exposure to oil shocks and military spending directed to each state as an instrument for state-level unemployment rates. The results reveal a statistically significant, positive effect of unemployment on property crime, while the relationship with violent crime is much weaker. Findings that unemployment matters for crime have also been obtained for European countries characterized by quite different labor market institutions than the US – notably, more generous welfare support to displaced workers. Oster and Agell (2007) and Fougere et al. (2009) use a shift-share approach to identify the effect of unemployment on crime across, respectively, Swedish municipalities and French departments during the 1990s. Both papers find that unemployment increases property crime. Bell et al. (2018) estimate the same relationship in the UK and the US using a different, previous unexplored empirical strategy for crime, namely comparing cohorts entering the labor market in different periods. They show that individuals entering the labor market during recessions in the UK and the US are, respectively, 4 and 10 percent more likely to be arrested.

These studies begin to make it clear that the question of whether a lack of work matters for crime requires more consideration of the way in which unemployment can affect crime than the available data and methodologies could address in the early, first round work on crime and unemployment. Two important insights emerge. First, studying unemployment rates of the appropriate population who may commit crime – mostly young men – does find evidence of an empirical relation between youth crime and youth unemployment. Second, formulating the question in terms of entry unemployment rates (i.e., when individuals enter the labor market) produces strong evidence of a crime-unemployment relation. People

who enter the labor market in bad times (local and national recessions) are strongly and persistently more likely to engage in crime. Bell et al. (2018) argue therefore that criminal careers can indeed be ‘made’ according to initial labor market conditions and so recessions can act as a turning point for the onset of criminal careers.

Outside Europe and the US, Dix-Carneiro et al. (2018) exploit the asymmetric impact of the 1990s trade liberalization across Brazilian regions to identify the impact of unemployment on homicides which, they argue, is the only crime that can be consistently measured across regions during their sample period (1980-2010). Regions that experienced a negative trade shock (in the form of higher foreign competition) saw a temporary increase in local homicide rates compared to the national average. Therefore, in contexts with high levels of violence (as is certainly the case for Brazil), violent crimes also seem to respond to unemployment. This is also the case in Mexico where, according to Dell et al. (2019), municipalities experiencing higher increases in unemployment induced by greater competition from China also witness an increase in drug-related homicide rates.

Overall, the evidence emerging from more recent papers leveraging plausibly exogenous economic shocks across local areas is consistent with the theoretical prediction that unemployment increases crime. However, it may be hard to defend the exclusion restriction that economic shocks impact crime only through unemployment rates. For instance, economic crises that hit severely not only the private sector but, also, local public finances, may reduce expenditures on police and other public services, in which case the shift-share approach (or other approaches leveraging exogenous variation from economic shocks) would overestimate the impact of unemployment on crime. Conversely, local authorities anticipating negative effects on crime may decide to allocate larger budgets to law enforcement, or they could invest in other policy tools aimed at counteracting the expected crime increase, such as assistance to poor families; in this case, the instrumental variable approach would underestimate the impact of unemployment on crime. In sum, estimates obtained using shift-share instruments and aggregate data at the local level may conflate the effect of unemployment with that of other local factors. Therefore, the plausibility of the exclusion restriction would depend on the specific context.

A second, related problem with local-level data is that they may prevent us from assessing the effectiveness of some policy remedies such as unemployment benefits or conditional cash transfers, as the latter are typically uniformly defined at the national level.

Finally, in all countries (including the most violent ones), crimes remain relatively rare events that are committed by a low number of serial offenders, so averaging data on crime and unemployment within local areas may hide the relationship between the two variables. Put differently, aggregate data may be ill-powered to detect the effect of unemployment on crime, even in cases in which the effect size is large. This

problem is particularly severe for violent crimes, particularly murder and other most serious offenses, because they are much rarer than property crimes.

For all the reasons just discussed, there are some inherent limitations to what we can learn from aggregate data. Individual-level data may overcome some of these limitations, and they have been used to study the impact on unemployment on crime since the 1980s. Witte (1980) and Schmidt and Witte (1989) used individual-level data on former prison inmates in North Carolina to study several determinants of recidivism, including employment. Witte and Tauchen (2000) focus on a 10 percent random sample of the 1945 birth cohort in Philadelphia surveyed over multiple periods of time, which allows them to use panel data methods. These papers find stronger support for the relationship between unemployment and crime compared to contemporaneous papers using only aggregate data. At the same time, the empirical analysis remains correlational, so the estimated coefficient of unemployment cannot be given a causal interpretation. Moreover, in all these studies, the sample remains quite small and, in the case of Witte (1980) and Schmidt and Witte (2013), it includes only former offenders and, thus, is not representative of the national population.

Several recent papers address these limitations by leveraging large registry data and plausibly exogenous variation in job loss. In particular, Bennett and Ouazad (2019) focus on Danish (high-tenure) workers who lost their job in mass layoff events during the period 1990-1994. They show that the probability of committing crime increases by 0.57 percentage points in the year of displacement (+32 percent over the baseline probability of committing crime), and that such an effect is primarily driven by property crimes. The dynamics of the effect is non-monotonic, spiking at 0-1, 3-4, and 6 years after job loss. The authors attribute this pattern to transitions out of unemployment benefits, determined by the design of unemployment insurance schemes. Using a similar research design, Rege et al. (2019) detect a 20 percent increase in crime among Norwegian workers displaced in mass layoffs during the period 1992-2008, but the effect is transitory and vanishes before the fourth year after job loss.

Both of these previous papers look at developed countries with relatively low crime rates and generous welfare systems, but the implications of job loss for crime are likely more severe in developing countries in which crime and poverty are much more widespread, and safety nets are less generous. Focusing on Colombian workers in the city of Medellin during the period 2006-2015, Khanna et al. (2021) find that job loss increases the probability of committing a crime by 46% in the first year after layoff, and by 35% the second year after layoff. The effect is concentrated on property crimes, and is attenuated by greater access to credit, driven by a financial reform that expanded the number of bank branches. Finally, Britto et al. (2022) estimate the impact of job loss on crime using employer-employee data on the universe of workers in Brazil matched with judicial records over the period 2009-2017. The probability of committing a crime increases by over 20 percent, an effect that persists for at least four years after job

displacement. The large sample size allows the authors to precisely identify significant increases for almost all types of offenses, including violent and non-economically motivated crimes; property crimes and other economically motivated crimes, however, increase the most, in line with prior evidence. Britto et al. (2022) then estimate the mitigating role of unemployment insurance by comparing displaced workers that obtained and did not obtain three-to-five months of unemployment benefits after the layoff. Since in Brazil eligibility for benefits varies discontinuously with the timing of previous layoffs, they compare displaced workers that are barely eligible and non-eligible for benefits. Receiving unemployment benefits causes a decrease in crime that completely offsets the increase due to job loss, but the effect is transitory and disappears immediately as benefits expire.

The results in Britto et al. (2022) refer to all types of crime except domestic violence because identifying the impact of job loss on the latter is particularly challenging. Differently from other types of crime, the dynamics of domestic violence and its reporting may depend on the bargaining power of both the offender and the victim. Specifically, a higher incidence of domestic violence after the (male) offender loses his job may reflect either an actual increase in violence, or a higher willingness of the (female) victim to report it, or both. To address this issue, Bhalotra et al. (forthcoming) combine the data used in Britto et al. (2022) with additional data on mandatory reports by health providers on suspect cases of domestic violence, which attenuate concerns about biased reporting. According to their results, both male and female job loss increase the risk of domestic violence – by 32 and 56 percent, respectively. These findings are in contrast with household bargaining models (Aizer, 2010), which predict that the risk of domestic violence increases with female job loss and decreases with male job loss. They are also in contrast with male backlash models (Macmillan and Gartner, 1999), which predict exactly the opposite, namely that domestic violence increases with male job loss and decreases with female job loss. Therefore, both these models predict opposite effects of male and female job loss, while the results of Bhalotra et al. (forthcoming) suggest that both male and female job loss increase domestic violence. They show that this result can be reconciled with a simple model in which domestic violence depends negatively on income and positively on time spent together in the couple, so both male and female job loss increase domestic violence through both mechanisms. Interestingly, unemployment benefits attenuate the income effect but increase time spent together, through the reduction in labor supply; the former effect prevails while benefits are paid, whereas the second effect prevails after they expire, so the overall effect of unemployment benefits is more ambiguous than for other types of crime.

Overall, the available evidence is largely consistent with the Becker-Ehrlich model, which predicts that job loss and unemployment increases crime by reducing its opportunity cost. At the same time, more recent evidence based on large registry data points at significant effects on non-economically motivated crimes such as substance abuse and violent crimes, including domestic violence. These findings suggest

that job loss and unemployment may affect criminal behavior through other (non-economic) mechanisms such as psychological stress. Reviewing the position of whether lack of work matters for crime now, as compared to 25 years ago in Freeman's (1999) chapter, paints a much clearer picture. More precise framing of the empirical hypotheses of interest, coupled with significant methodological and data advances, lead to the conclusions that being out of work is an important determinant of criminality, as are shifts in wages and income for individuals on the margins of committing crime.

3.3. Youth Labor Markets: Summer Youth Employment Programs

Crime is over-represented amongst youths and young adults. In fact, similar age-crime profiles – in which crime increases throughout the teenage years, peaks around age 19 or 20, and then gradually decreases – are seen throughout the world and history (Bindler and Hjalmarsson, 2017). This sub-section emphasizes the impact of youth labor markets on crime, and focuses in particular on one dimension that is specific to youths: summer youth employment programs (SYEP).

Heller and Kessler (forthcoming) highlight the prevalence of these programs in the United States. From 2014-2016, 27 of the 30 largest U.S. cities had a SYEP. These programs vary in many dimensions, including: (i) size, from just 70 participants in San Francisco to 54,000 in New York City per year, (ii) allocation mechanism (random, first-come first-serve, merit based, and criteria based), (iii) content (summer employment, supplementary job preparation training, and/or additional support like cognitive behavioral therapy), and (iv) target group (e.g., student versus non-students). One common aspect of almost every program is over-subscription: there are more applicants (often many times more) than positions. Such SYEP are also not specific to the United States – Swedish programs exist, for instance, and evaluations are underway.

The ultimate goals of SYEP are to: improve the short-term economic conditions of participants, provide work experience that can improve future education and employment outcomes, and keep participants out of trouble (Gelber, Isen, and Kessler, 2016). Crime can be impacted by SYEP participation through multiple channels, including: a contemporaneous incapacitation effect (in which youths are kept busy during the idle summer months), an improvement in legitimate labor market opportunities, an improvement in other behaviors (e.g., responsibility or societal attitudes), and a lower need for financially motivated crime due to higher earnings.

Given that youths who volunteer for SYEP are likely different than non-participants in observable and unobservable dimensions (e.g., risk preferences, motivation and family background), selection bias will act to confound estimates of the effects of SYEP on crime in the absence of an experimental design (LaLonde, 1986). In contrast to most of the literature surveyed in this chapter, recent studies of SYEP take advantage of a random assignment mechanism in select cities to overcome these challenges.

Heller (2014) conducted the first such evaluation in Chicago, where 1,634 disadvantaged high school youth were randomly assigned in 2012 to three groups: (i) control, (ii) jobs-only, with 25 hours per week of paid employment, and (iii) jobs plus therapy, with 15 hours of work and 10 hours of cognitive behavioral therapy per week. For both treatment arms, a 43% reduction in violent crime, but no effect on property crime, is seen over a 16-month period. What mechanisms underlie these effects? Given that the same effects are seen for groups with and without therapy, it appears to be something about the job and not just the therapy itself. Moreover, most of the effects accrued after the program was completed, suggesting they are not just a mechanical result of incapacitation (though this does not rule out a dynamic incapacitation story).

Davis and Heller (2020) further found that these effects replicated when expanding the Chicago program eligibility to include disadvantaged youths who were no longer enrolled in school. Moreover, these results are not specific to the Chicago program. Modestino (2019) evaluates the crime impacts of the 2015 Boston program, which reaches about 10,000 youth per summer and connects them to 900 local employers. Youths work for 25 hours per week for six weeks at minimum wage and receive 20 hours of job-readiness training, such as preparing for interviews, job search and job applications. Large effects on violent crime (35% reduction) were seen for Boston participants, while, in this program, property crime also decreased by 29%. Most of the effects were once again post-program. Modestino also finds in survey data short-term changes in attitudes and behaviors that could be related to crime, including attitudes towards the community and social skills and behavior (e.g., managing emotions and asking for help).

Gelber, et al. (2016) and Kessler et al. (2022) evaluate the impact of participating in the New York City program for youths aged 14 to 21. From 2005 to 2008, 294,100 applications were received, and 164,641 won a job through the computerized lottery. Gelber et al. (2016) find improvements in employment and earnings during the year of program participation, but by three years post program, there is a modest decrease in average earnings and no effect on college enrollment. Finally, the authors do find a reduction in incarceration in New York State prison (a proxy for serious crimes or criminal history) and the chance of death. Kessler et al. (2022) conduct a follow-up evaluation that digs deeper into the crime effects for SYEP participants aged 16 and older, for whom criminal records on adult arrests and convictions in New York state could be matched on. The authors find that the New York sample is less negatively selected than the Chicago sample: Just three percent had an arrest prior to the program. And it is just for this sub-sample that the authors find an effect: the chance of felony arrest is reduced by more than 20 percent during the program and remains large (though not statistically significant) five years later.

Taken together, this new body of work provides strong and convincing evidence that summer jobs programs for youth—and especially those for high-risk youth—reduce violent crime. Though the channels through which these effects occur are still unclear, the research suggests it is more than a pure incapacitation

effect and not driven by the therapy sometimes attached to the program. But will these programs continue to have such effects as they expand, given that expansion can change the marginal participating youths and/or employers? Heller (2022) provides evidence that SYEP may indeed successfully scale up by conducting evaluations in (i) Chicago with three times as many participants ($n = 5,405$) as her earlier evaluations and (ii) Philadelphia with more than 50 program providers. Despite variation in providers or expanded participant samples, both evaluations led to large reductions in criminal justice interactions in the first year of participation, with some preliminary evidence of a persistent decline in arrests in subsequent years.

3.4. Returns to Crime: Earnings and Prices

The basic economics of crime framework states that shifts in relative earnings from legal work compared to illegal work shape crime. However, there is far more empirical research on legal earnings as compared to illegal earnings from crime, the latter which can be thought of as the economic returns to crime. This much was true when Freeman (1999) wrote his Handbook chapter and, while discussing the paucity of research, he was able to point to studies on the attempted measurement of the earnings of criminals, conclusions from which are difficult to draw due to their small sample sizes, highly specific nature, and/or measurement difficulties. Subject to these caveats, these earlier survey-based findings suggest average illegal earnings to be close to the average legal earnings faced by criminals. But this is with variations across different crime types (e.g., drug dealing may be more lucrative), and also with only a subset benefitting from sizable crime returns, typically prolific offenders in a skewed distribution of returns.

Though things have moved on since then, there is still not much research in this area, which still to this day likely reflects the difficulty of obtaining good data (or any data) on criminal earnings. One notable exception is the field study of a drug selling gang by Levitt and Venkatesh (2000), which links the issue of illegal earnings to the economics of criminal enterprises, in this case a drug gang whose financial operations were documented over a four-year period. The focus provides some important context for understanding criminal earnings, namely the hierarchical structure of criminal work. Drug-selling is input intensive – the wage bill to revenue share is approximately one-third. Wages for street-level dealers are low – comparable to the minimum wage – and carry serious risks (the death rate for the sample was 7% annually). The incentive for gang participation therefore lies in the prospect of moving up the hierarchy within the gang, in line with a tournament model. Rewards at the top are high – with wages between 10 and 25 times higher than ‘foot soldier’ wages. The results do sit quite well with the economics of crime model, but at the same time, the message that emerges is one that crime does not pay much for most participants, but that a few criminals benefit significantly from a highly skewed structure of illegal rewards.

The second recent development on the returns to crime comes from studies that Draca and Machin (2015) refer to as the ‘internal rates of return’ to criminal opportunity. This means the cash flow or return generated by a criminal project – the “loot” from crime – holding the probability of detection or other costs fixed. This concept is most relevant for the case of property theft and there are by now a few empirical studies looking at the relationship between property theft rates and prices (i.e., measuring the value of loot).

Reilly and Witt (2008) examine the relationship between domestic burglaries and the real price of audio-visual goods (a major component of the ‘loot’ obtained in burglaries). They consider an annual time series of UK burglary and price data over the period from 1976-2005, when the retail price of audio-visual goods fell by an average 10 percent per annum. Their main specification is an error-correction model (ECM) that includes controls for unemployment and inequality (a Gini-based measure) together with their main price variable. The long-run estimates from this ECM indicate an elasticity of 0.286, such that a 10 percent fall in prices is associated with a long-run fall in the volume of domestic burglary of 2.9 percent.

The paper by Draca et al. (2019) pushes much further and looks at the relationship between goods prices and crime across a wide range of goods. They use records from the London Metropolitan Police’s (LMP) crime reporting system, which features a property type code that classifies stolen goods as part of theft, burglary and robbery incidents. These property types are then matched by label description to ONS data on retail prices. Results based on a panel of 44 matched goods – covering goods ranging from clothing, drink and foodstuffs, electronic equipment, household goods, and jewelry – indicate an average elasticity with respect to prices of 0.3-0.4. Furthermore, there is a short lag between price changes and crime, with the majority of adjustment occurring within three months of a given price change, limiting the scope for time-varying unobservables to explain the price effect.

Endogeneity concerns are addressed by focusing on a subset of goods – three metals (copper, lead and aluminum), as well as jewelry and fuel – where domestic prices can be plausibly linked to international prices. In the case of metals, they instrument local scrap metal dealer prices with global commodity prices, while fuel is instrumented with oil prices and jewelry with the price of gold. This approach has the advantage of isolating price changes that are a function of international demand (for example, commodity demand from China) rather than variations due to local demand, which could in turn change the local stock of goods available for theft. The results for this sub-set of goods show higher elasticities that mostly exceed unity, indicating that criminals are highly elastic with respect to prices and the implied value of criminal opportunities. The case of metal crime is also explored in Kirchmaier et al. (2020). Their emphasis is different, as they move on from a focus on returns for criminals by looking at price booms and busts, policing initiatives and regulation of second-hand buyers, specifically scrap metal dealers. But, in doing so, they too report a price elasticity greater than unity in the case of metal theft.

A final paper in this area, by Braakman et al. (forthcoming), moves in another direction on plausibly exogenous price movements by looking at burglaries when one recognizes that some groups in society keep valuables at home – in their case, UK households of South Asian origin having gold jewelry – and that criminals are aware of this. They very clearly show that as the gold price rises, burgling these households becomes a more lucrative business as the rising value of loot on offer generates increases in criminal earnings returns.

4. Criminal Record Impacts on the Labor Market

Bushway et al. (2022) studies the criminality of unemployed men in the 1997 National Longitudinal Survey of Youth (NLSY97): 64% had been arrested at least once by age 35 for non-traffic offenses and 46% had been convicted. Underlying these statistics are two potential causal pathways: (i) labor market experiences can impact criminal behavior and (ii) criminal behavior and records can impact employment. Of course, it may also be that there is no causal relationship at all but rather some common (un)observables that explain both criminal behavior and labor market outcomes. Besides being a source of endogeneity that researchers need to overcome to find causal evidence (of either pathway), these non-mutually exclusive simultaneous relationships are of fundamental concern for policy makers thinking about how to break the cycle of crime. This section surveys what we know about whether (and why) having a criminal record reduces labor market outcomes. We start by recognizing that observed employment is an equilibrium outcome of both the criminal worker's supply of labor and the firm's demand for labor.

On the supply side, low employment or earnings outcomes could arise because workers with criminal records are less likely to search for work or because their informal networks, which are generally known to play an important role in job finding, are weaker.³ Having a criminal record can also impact how workers search for jobs: for instance, they might be less likely to search if they know that potential employers will conduct background checks. Alternatively, a criminal record, especially one that places the offender in prison, can weaken social capital; this possibility is highlighted by Western et al. (2001) as one mechanism through which incarceration can causally impact employment. To our knowledge, there is virtually no empirical evidence in economics on how having a criminal record (or the nature of that record) impacts an individual's job search behavior. Outside of economics, Smith and Broege (2020) find using ten survey rounds of the NLSY97 that interacting with the justice system decreases both overall job search and shifts job search from more active to passive methods. One reason for this sparse evidence is the data demands required. There are few contexts in which one can observe job search behavior (as opposed to outcomes), method of search, networks, and of course criminal records.

³ See Topa (2011) and Ioannides and Loury (2004) for reviews of the theoretical and empirical evidence on the importance of informal networks in job finding.

We have more evidence on the demand-side of the story – namely that poor employment outcomes for individuals with criminal records arise because firms are less willing to hire such workers. A 2003 survey (conducted by the Institute for Research on Labor and Employment) of California employers found that just 2% would definitely hire a worker with a criminal record while 37% would definitely not (Raphael, 2014). A more recent survey by Cullen et al., (2022), discussed in more detail below, found that just 39% of US firms hiring temporary workers were willing to hire workers with a criminal record. Raphael (2014) highlights many reasons for being unwilling to hire a worker with a criminal record. (i) Most explicitly, state and federal laws ban individuals with records from certain jobs, such as those working with vulnerable populations or sensitive information.⁴ (ii) A firm may also exercise its own discretion in deciding not to hire an ex-offender due to risk aversion; firms may perceive the risks of loss due to employee theft or dishonesty or liability risks due to a harmed customer or employee to be higher for those with criminal records. Such risks again clearly should vary with the nature of the occupation. (iii) Alternatively, firms may be morally averse to hiring workers with criminal records. (iv) Finally, and in contrast to these examples of taste-based discrimination, firms may be statistically discriminating and using a criminal record as a signal of productivity.

We present three bodies of research. The first studies the impact of having a criminal record or interacting with the justice system on (equilibrium) employment outcomes. These studies largely use administrative records and quasi-experimental research designs. The second literature studies a firm’s willingness to hire workers with criminal records using experimental variation in surveys or audit and correspondence studies. A third literature considers how policies, such as ban the box or clean slate reforms, impact employment outcomes for workers with criminal records.

4.1. Effects of a Record on Labor Market Outcomes

The earliest work studying the effect of conviction and incarceration on employment and/or earnings was conducted prior to the first labor handbook chapter on crime (Freeman, 1999). Using data from the 1979 National Longitudinal Survey of Youth, which retrospectively asked about criminal justice involvement in the 1980 round, Freeman (1991) finds that the probability of employment is markedly lower after incarceration. Using administrative records for federally convicted offenders that contain an earnings measure from before and after conviction, Waldfogel (1994) finds that first time conviction significantly reduces employment and earnings. Though within individual comparisons of labor market outcomes go some way to controlling for correlated unobservables, these findings do not lend themselves to a causal interpretation due to the possibility of both simultaneity and/or remaining selection biases. Rather than

⁴ Bushway and Kalra (2021) note that there are more than 4,000 state licensing statutes that automatically disqualify people on the basis of their criminal history from obtaining a ‘license’.

having just one pre and post earnings measure, Grogger (1995) uses unemployment insurance records to construct a longitudinal data set of quarterly earnings data (from 1980 to 1984) that he merges with police records. With a flavor of contemporary event-study designs, Grogger estimates an individual fixed effects model of the effect of arrest on employment and earnings. He concludes that unobservable heterogeneity explains much of the labor market differences for arrested versus non-arrested individuals and there are only moderate, and short-lived, effects of arrest.

These results are in fairly sharp contrast to those of Agan et al. (2023) who conduct event study analyses of the impact of having a criminal record on taxpayer earnings and filing data using IRS records. Agan et al. use federal income tax data from 2000 to 2019, a period when online criminal record searches were common and feasible (in contrast to the 1980s period studied by Grogger). Agan and co-authors find a sharp drop in filing and reporting earnings after a criminal charge, and even after a charge that does not lead to a conviction.

The credibility revolution combined with increased access to administrative records and registers has led to a new body of work studying these old questions. Besides the modern-day event study design, three broad approaches have been used to overcome identification challenges. The first takes advantage of exogenous variation in record or sanction severity that is generated by randomly assigned judges (or other criminal justice agents). The second takes advantage of discontinuities in sentencing guidelines that lead to observably similar individuals – at least in terms of criminal histories and offenses – having different treatments. The third uses exogenous variation in treatment by the criminal justice system generated by the timing of reforms. Another advancement of the literature is to consider the labor market consequences of a wide range of justice system interactions: diversion, pre-trial detention, conviction, and incarceration.

Mueller-Smith and Schnepel (2021) study the effect of diversion away from the criminal justice system. Diversion is a tool used by justice agents to avoid a criminal record: defendants avoid a formal conviction by undergoing a period of community supervision (at least in the Texas context studied here). But who gets diverted? To deal with the non-random assignment of diversion, Mueller-Smith and Schnepel study two reforms that led to sharp changes in the use of diversion in Harris County, Texas. The first decreased diversion in 1994 for offenders charged with certain drug and property offenses while the second increased diversion in 2007 for low-risk defendants. Using a fuzzy regression-discontinuity framework that takes advantage of the timing of a charge relative to the reform dates, the authors find that diversion improves long-term outcomes for first time felony defendants: not only do future convictions decrease but employment rates increase.

Dobbie et al. (2018) study the impact of holding a defendant in pre-trial detention by matching Philadelphia and Miami-Dade court data to tax data from the Internal Revenue Service. Bail judges decide both whether to offer bail and the amount of bail. These decisions are again clearly not random but based

on factors like the degree of evidence, flight risks, and potential danger to society. Dobbie et al. (2018) capitalize on the quasi-random assignment of bail judges with different propensities to offer bail to isolate plausibly exogenous variation in pre-trial release. The authors find that pretrial release increases formal sector employment (by 25% four years after the bail hearing) as well as the receipt of employment and tax-related government benefits.

The remainder of the papers in this literature focus on incarceration at either the extensive margin (any incarceration) or intensive margin (length of incarceration). Over and above the effect of conviction, prison can impact labor market outcomes through multiple channels. Prisons often include education, training and treatment, which may improve an offender's chance of finding (and keeping) a job when released from prison. Time in prison can impact criminal and non-criminal networks. To the extent that criminal networks on the outside are weakened, one can have more incentive to participate in the legitimate labor market; on the flip side, if non-criminal networks are weakened, then one can have less access to the labor market.

Kling (2006) conducted one of the first studies of the labor market effects of incarceration following the credibility revolution: he links quarterly earnings data (predominantly from the 1990s) to federal court and state prison data in California and Florida, respectively, and applies a randomly assigned judge fixed effects design, in which sentence length is instrumented for with the average sentence of all other offenders facing the same judge. Kling (2006) finds no evidence that more time in prison harms earnings and employment. This is seen both in descriptive plots of the data but also instrumental variable specifications; the estimates, however, are fairly imprecise. Despite its imprecision and minimal formal testing of the identifying assumptions that are a prominent part of contemporary 'judge fixed effects' designs, Kling (2006) played a significant role in making the randomly assigned harsh judge or 'judge fixed effects' instrument a go-to tool to overcome the non-random nature of sanctions in the economics of crime.⁵ Another paper that finds an imprecise null effect of prison on employment using the harsh judge design is Loeffler (2013), which matches Cook County Illinois court records to unemployment insurance records.

The most recent work, however, by Garin et al. (2023) reaches the same conclusion: though employment and earnings are incapacitated while one is in prison, differences in labor market outcomes are not discernible by five years post sentence. These authors in fact reach these conclusions in two contexts – North Carolina and Ohio – using two identification strategies: sentencing guideline discontinuities and judge instruments, respectively. Both of these designs and contexts had been previously used to show crime

⁵ See Frandsen et al (2023) for a thorough discussion of identification issues, and proposed tests.

reducing effects of the prison systems in North Carolina on the offender themselves (Rose and Shem-Tov, 2021) and in Ohio on the offender's family (Norris et al., 2021).⁶

But, in some contexts, prison appears to have beneficial labor market effects. Thus far, the common theme to these findings is Scandinavia. In Denmark, Landerso (2015) finds that longer incarceration spells improve employment and earnings outcomes. Intuitively, Landerso estimates a difference-in-difference specification where he compares the change in labor market outcomes for those who offended after a 2002 reform that decreased incarceration lengths by about one month (the treated group) to the change in outcomes for those who offended prior to the reform (the control group). In Norway, Bhuller et al. (2020) also find, using a judge stringency instrument, that incarceration (at the extensive margin) increases employment and earnings while reducing crime. These results are not seen for everyone, however, but rather for those not working prior to incarceration: for these same individuals, the authors find a 34 percentage point increase in job training program participation. In contrast, for individuals employed prior to prison, incarceration results in the loss of their job, which is not quickly recovered.

What is the effect of prison on labor market outcomes? The answer is not straight-forward. A general take-away from the literature is one of mixed findings: depending on the context and/or research design, researchers find positive, null, or negative effects of incarceration on labor market outcomes. These differences could be driven by heterogeneities in the marginal offender off which the results are identified, heterogeneities in the conditions and characteristics of the prison system studied, and heterogeneous effects across offenders.

4.2. Firm Willingness to Hire Workers with Criminal Records

In addition to the firm surveys described above, there is also experimental evidence that at least some firms do not want to hire workers with criminal records. Pager's (2003) seminal paper conducts an audit study in Milwaukee, Wisconsin, in which there are two pairs of auditors (one Black pair and one white) that apply in person to jobs advertised by 350 employers. Both members of the pair apply to the same employer, but one member is assigned to have a criminal record. The results are striking: For white individuals, the chance of a callback for an interview decreased by 50% when the auditor had a criminal record while, for Black individuals, the chance of a callback decreased by 60%. The race-differential is even larger when accounting for the fact that Black auditors had a lower call-back rate overall. Another audit study conducted by Uggen et al (2014) finds that auditors in Minneapolis, Minnesota are less likely to have a call-back even

⁶ Mueller-Smith (2015) finds prison in Harris County Texas worsens labor outcomes and the analysis highlights potential violations of the exclusion restriction (i.e., the judge only affects outcomes via the prison sentence) and monotonicity assumption (if judges vary in their relative treatment of different types of defendants) in judge stringency designs.

when they are assigned a minor criminal record that did not result in conviction (albeit the effect is smaller than in the original study). Finally, in a correspondence study design, which allows for thousands of job applications and holds constant all else but the criminal record, Agan and Starr (2017) show that employers in New York and New Jersey, whose job ads included a box asking about criminal history, were more than 60% more likely to interview applicants without a felony conviction.

The best evidence to date on *why* firms are less willing to hire workers with criminal records comes from Cullen et al. (2022) who conduct a field experiment in partnership with a national staffing platform in the US: businesses submit job ads and the platform distributes them to qualified workers, who can accept the job on a first-come first-serve basis. Specifically, Cullen et al. ask hiring managers at nearly 1,000 businesses their willingness to hire a worker with a criminal record under various hypothesized treatments in which the treatment intensity is randomized. These treatments speak directly to the various reasons that may underlie a firm's preferences, as well as to potential policies that could mitigate the firm's concerns. In the baseline (with no treatments), 39% are willing to hire a worker with a criminal record. This statistic, however, varies with the nature of the job: it is 45% if there is no customer interaction, 51% if no high-value inventory, and 68% if the job is hard to fill. What works to increase demand? The firm's willingness to hire workers with criminal records would significantly increase when provided (potentially prohibitively) large wage subsidies, relatively small amounts (up to \$5,000) of insurance coverage, information on satisfactory previous job experience, and a clean record for at least one year.

4.3. Policies to Improve Labor Outcomes for Workers with Criminal Records

Ban the Box policies aim to make it illegal to ask about criminal records on the initial job application (i.e., the box) or interview. The hope is that getting workers with criminal records through the door will increase the chance of a job upon demonstrating their qualifications in an interview. Such policies were passed in at least 25 states and more than 150 jurisdictions (Agan and Starr, 2018). The theoretical effect of the ban the box policy on employment is not straight forward; moreover, it may depend on whether one actually has a criminal record. If it does get ex-offenders through the door and convince employers of a potential match, then employment could increase. But, if employers simply defer a background check until later in the process and are not willing to hire ex-offenders, then there may be no effect. Similarly, no effect could arise if workers with records sort across firms and/or industries that do and do not do background checks. Banning the box could even impact the employment outcomes of workers without criminal records. Depending on the group one belongs to (e.g., racial group), employment could increase or decrease via statistical discrimination. If firms do not want to hire workers with criminal records, they may use race, for instance, as a proxy via which to statistically discriminate.

The seminal paper on this question is by Agan and Starr (2018), which conducted an online correspondence study with employers in New Jersey and New York City before and after the adoption of ban the box policies in 2015. They submitted about 15,000 fictitious applications on behalf of young males. Applications were matched on race and randomly varied whether the applicant had a felony conviction. The main outcome studied, as is typical in correspondence studies, is callback rates. Call-back rates for workers with criminal records increases for both black and white applicants, suggesting that banning the box does get ex-offenders in the door. But, consistent with statistical discrimination on the basis of race, black and white call back rates decrease and increase, respectively, for workers without records.

A limitation of the Agan and Starr study, and correspondence studies in general, is that impacts on call backs may not translate into impacts on actual employment. This could happen if, for instance, background checks would have been done later in the process. Another potential limitation is that by construction the experiment has ex-offenders applying to the same jobs as non-offenders, which may not happen in reality. Subsequent papers have studied the impact of ban the box laws on employment in non-experimental contexts. Using variation in the timing of state and local ban the box laws and the Current Population Survey, Doleac and Hansen (2020) find that banning the box decreases employment for young, low-skill Black and Hispanic men. In contrast, using quarterly earnings data linked to statewide arrest and court records for 300,000 ex-offenders, Rose (2021) finds that Seattle ban the box laws did not improve labor market outcomes for ex-offenders. Other studies have found decreases (Jackson and Zhao, 2017) and increases in ex-offender employment (Craigie, 2020). The bottom line is that it is not clear what impact ban the box policies have on final labor market outcomes; these effects may depend on the context.

Another policy gaining traction is Clean Slate laws: ten US states have passed such legislation from 2018 to 2022.⁷ Common features of Clean Slate laws are the automation of record clearance, especially minor misdemeanor and/or arrest records. If employment gaps of workers with criminal records are attributable to background check failures, then clean slate laws could improve employment outcomes. On the other hand, if having a record and interacting with the justice system leads to gaps in labor market experiences and/or lower quality experiences, then there can permanent scarring effects on labor market outcomes. With the exception of one paper (Agan et al., forthcoming), there is minimal research on the labor market impacts of such policies. Agan et al. (forthcoming) study the impact of Proposition 47, which reduced certain felonies to misdemeanors. The authors focus on Joaquin County, where the nature of implementation of Proposition 47 created arguably exogenous variation in the timing of the automatic felony reduction. They also use an RCT in which a subset of individuals were notified about the record reduction to measure whether information plays a role in whether record reductions impact employment

⁷ See <https://www.cleanslateinitiative.org/states>. Last accessed May 5, 2023.

outcomes. Overall, the findings suggest little impact of reducing a felony to a misdemeanor on employment outcomes, even when notified about the reduction. There are a few exceptions, including an increase in platform gig work and employment increases for individuals with more recent convictions.⁸

5. Education and Crime

One central plank in the significant rise of the economics of crime as a research field over the past 20 years has been the large body of work studying the link between crime and education. In fact, in the upsurge of research described above in Figure 1, a significant portion of the published papers can be classified into the crime and education area. Figure 4 shows, for all years covered, the principal subject matter of just under 7 percent of the crime papers in the selected journals is about crime and education. Moreover, there were no papers about this when Freeman’s (1999) chapter was published. In the selected leading journals, all of the work in this burgeoning area of research is from 2001 onwards. In the later years in the Figure, there are even more crime publications classified in the crime and education area – comprising just under one in ten of the total 438 economics of crime field publications in the set of journals considered from 2017-2023.

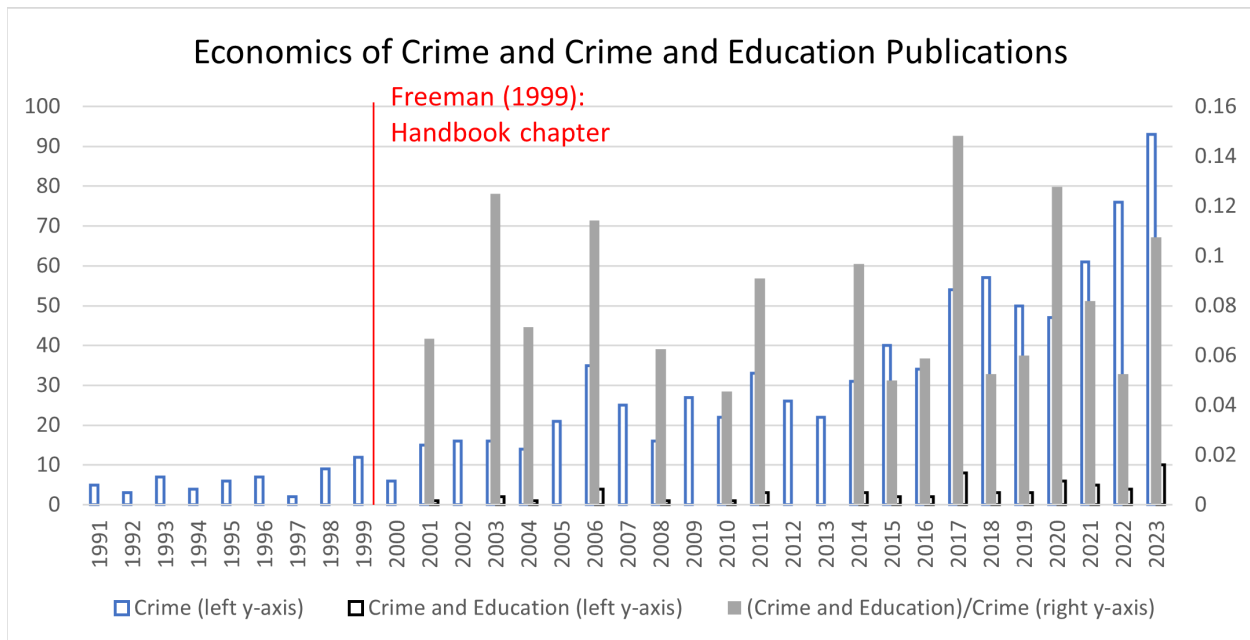


Figure 4. Economics of Crime and Crime and Education Publications (1991 to 2023)

Note – As for Figure 1. From Machin and Sandi (forthcoming).

This section reviews this now sizable crime and education research area – an area that is growing and expanding in various directions. In offering this review, we first highlight the research that (very much

⁸ Earlier work finds that record expungement leads to increased employment, but this is based on a sample of individuals who voluntarily select into expungement (Prescott and Starr 2020; Selbin et al. 2018).

in accordance with the “credibility revolution” running through applied economics) focused on identifying causal impacts. Various research designs have been used to study both the causal impact of education on crime and of crime on education.

These are important developments as in prior work in a range of social science disciplines (most notably criminology, sociology and psychology, and also in others, though rarely in economics), researchers have used observational data of different sorts to document a (non-causal) negative correlation between education and crime. This has been shown through time in many settings, and for most (but not all) crime measures. But the economics literature on which we focus tackles the causality issue head on, with significant research efforts and intention to move from correlational observation to establishing causality.

The key question on establishing causality is that both the following questions are plausible. Does education reduce crime? And/or does crime reduce education? A series of studies have by now quite convincingly established that there is a causal impact of education on crime by leveraging education policy changes to ensure there is a crime-reducing crime impact of education. Other research designs, to be discussed, have been used to corroborate this and reach a similar conclusion. So has the work showing causation running from crime to education, where juveniles who interact with the justice system, and especially prison, have causally worse education outcomes. The fact that both causal directions are plausible, clearly means that a solid identification strategy is needed, and we will discuss these carefully when reviewing the literature. Besides the simultaneous nature of this relationship, establishing causality is challenging given there are many potential unobservables correlated with both educational attainment and criminal behavior (e.g., ability and family background).

Why can education reduce crime? The first, immediately forthcoming response to this question comes from the economics of crime framework set out earlier in this chapter. It is straightforward to consider crime and education in this setup by making legal earnings W_L a function of education (e.g., through a Mincerian earnings function) so that, coupled with the large literature on positive wage returns to education (Card, 1999), there is a clear prediction that more education reduces crime.

This is not the only route by which a crime reducing impact of education can emerge. Nor need it be an income effect that generates the crime reduction. Other possibilities speak more closely to mechanisms by which an impact may emerge. In the income effect route, the crime reduction arises because extra time spent in school raises productivity and this is rewarded in the labor market by higher earnings, which reduce crime in the usual Becker/Ehrlich fashion. On the other hand, being in the classroom means that juveniles are in school being kept busy in a supervised environment and, thus, off the streets and not committing crime. This is an incapacitation effect, which need have no productivity implication, and thus offers a different crime reduction mechanism. And another possibility, with a more behavioral aspect to it, is that potential future criminals differ in their discount rates (rather like in the Card, 1999, decisions on

whether to invest in education or not), so that they value the present more than the future. Schooling therefore not only increases economic returns but may also increase a youth's patience, and lead them to put more weight on their potential future earnings (as in Becker and Mulligan, 1997).

Table 1 collates 64 crime and education publications from the past 20 years, ending in 2023, into four Panels that reflect the way the empirical literature has evolved over time and conveniently define the sub-sections below. Panel A alphabetically lists 12 studies that focus upon crime-education connections where crime is related to completed schooling levels. For the most part, these were the first round of studies on crime and education that aim to pin down a causal impact of education. Some of the more well-known studies implement research designs that exploit school dropout age reforms to ensure the direction of causation runs from education, specifically attendance at school, to crime. Panel B shows a further 25 studies that move beyond school attendance to look at the impact of other education measures. Some of the contributions in this Panel focus on the productivity and incapacitation aspects of the crime-schooling relation. The others, many of which have been published very recently and are an important feature of the big uptick of research in this area shown in Figure 4, study connections between criminality and a wider range of education measures, including both school quantity and quality. Panel C lists 15 studies that look at causality running the other way, by studying the impact of crime on education. Panel D contains 12 studies that look at the impact of policies and interventions connected to education on crime.

5.1. Causal Impacts of Education on Crime

The common feature of the 12 published studies shown in Panel A of Table 1 is their focus on the impact of completed education (usually, though not always, measured by years of education/secondary school dropout age) on crime. These include the pioneering causal studies that look at the impact of school attendance on crime by leveraging legislative changes in compulsory school leaving laws to measure a causal impact of completed education on crime.

The seminal US paper of Lochner and Moretti (2004), and the studies of England and Wales by Machin et al. (2011) and of Sweden by Hjalmarsson et al. (2015) are key highly cited papers in this area. The logic they adopt in the empirical research designs to ensure causality runs from education to crime is that raising the school leaving age was implemented as policy to improve education levels, with no obvious connection to crime. Thus, in a standard two-stage causal regression setup, the change in the dropout age measure is used only to predict education and acts as a legitimate instrumental variable for completed schooling in the crime equation. These studies carefully justify this use of education policy – the raising of the school leaving or dropout age – to generate crime reductions from education as an unintended consequence of the compulsory school leaving age legislation.

The US study by Lochner and Moretti (2004) relates prison, arrests and self-reported crime data to education. They leverage state level shifts in the US compulsory school leaving age matched to 1960, 1970 and 1980 Census data, to Uniform Crime Reports arrests data, plus self-reported NLSY crime. In their Census analysis, for example, they look at males aged 20-60. Their study shows a strong first stage, where being exposed to an increase in the school dropout age in the state where you were schooled significantly raises completed years of schooling. The reduced forms show a significant reduction in subsequent crime for treated birth cohorts. In their analysis of age-specific arrest data, their findings imply that an additional year of schooling reduces state level arrest rates by at least 11 percent, with similar effects for both violent and property crime. Causal crime reduction effects, in the same research design leveraging state-specific changes in compulsory school leaving ages, also feature in the Census imprisonment outcome and for the self-report individual-level NLSY data.

Machin et al. (2011) study causal connections via the raising of the compulsory school leaving age in England and Wales in 1973 from 15 to 16 in a regression discontinuity design. They (mostly) study men aged 18 to 40. By comparing cohorts who turn 15 immediately before and after the reform, they estimate that a one-year increase in average schooling reduces property crime conviction rates by up to 30 percent. Hjalmarsson et al. (2015) study the causal effect of educational attainment on conviction and incarceration using Sweden's compulsory schooling reform as an instrument for years of schooling. Their study of men and women aged 19 to 64 reports that one additional year of schooling in Sweden decreased the probabilities of conviction and incarceration for males by about 7 and 15 percent, respectively.

A crime-reducing impact of completed education is corroborated by the other studies in Panel A of Table 1. These either look at more up to date dropout age reforms in the US (Anderson, 2014; Bell et al., 2022; Cano-Urbina and Lochner, 2019; Gilpin and Pennig, 2015), or consider crime and education in other settings (Beatton et al., 2017 in Australia; Bennett, 2018, in Denmark; Brilli and Tonelli, 2018, Buonanno and Leonida, 2006, 2009 in Italy). The consensus reached in this research is of a beneficial crime-reducing effect of longer duration school attendance. The extended compulsory school period is particularly effective at reducing property crimes, while little evidence exists of a lasting violence-reducing effect of the extended compulsory school period. But the strong conclusion of a causal crime reducing impact, based on education policy reform, emerges. Interestingly, since the economic costs of crime are high, these crime reduction benefits contribute very strongly to the benefit-cost ratios of the dropout age policies being well above unity, indicating that these reforms generate crime reductions whose socio-economic benefits outweighs their costs (for example, see the calculations presented in the papers by Bell et al., 2022, Lochner and Moretti, 2004, and Machin et al., 2011, for more detail).

5.2 Incapacitation

Other crime and education research looks not at completed education and its impact on adult criminality, but rather studies (i) crime when individuals are still in the compulsory schooling system or (ii) the impact of aspects of schooling that occur prior to education completion. Panel B of Table 1 lists 25 such studies. The first, smaller group, covers the research that looks at the scope for incapacitation effects whilst in the schooling system to underlie the causal impact of education on crime. Six listed studies in the Table place a focus on this question (four listed in Panel B by Jacob and Lefgren, 2003, and Luallen, 2006, and Cook and Kang, 2016, for the US and by Berthelon and Kruger (2011) for Chile, together with the studies by Anderson, 2014, and Bell et al., 2022 that already featured in Panel A). The second group covers a larger number – 21 – of published studies that look at various aspects of education, including quantity and quality, and assess their impact on crime. These are reviewed in sub-section 5.3 which follows below.

Incapacitation studies focus on the short run effect of education on crime (Jacob and Lefgren, 2003; Luallen, 2006), whilst individuals' criminality is observed at ages while still in the schooling system. This contemporaneous impact of schooling contrasts with the focus on the long run crime-reducing effect of final educational attainment highlighted in Panel A of Table 1. Causal evidence on incapacitation emerges from work that considers sources of variation in school attendance coming about from idiosyncratic school closures for teacher training (Jacob and Lefgren, 2003), teacher strikes (Luallen, 2006), and school eligibility laws (Cook and Kang, 2016). These generate short run variation in the day-to-day propensity and possibility of young people committing a crime. A property crime-reducing effect of education arises in these studies, which reflects the short-run incapacitation effect of school on crime. In other words, the contemporaneous effect of compulsory schooling is to keep pupils busy and away from the street. In contrast, in the long run, the construction of human capital and enhanced labor market opportunities emerge as the lasting drivers of the reduction in property crime among more educated people; more educated individuals are able to find more and better jobs, and therefore have a lower crime propensity and a higher opportunity cost attached to criminal activity.

That both effects are relevant for the crime-reducing impact of education is further validated in recent evidence from the US by Bell et al. (2022), which refines theoretical arguments further by introducing the concept of dynamic incapacitation. This work studies how the dropout age reforms alter crime-age profiles of individuals. Increases in the minimum school dropout age incapacitates youths in school at a critical age, beyond which they become less likely to commit crime if they had not engaged in crime onset at an earlier age (see the crime persistence by age estimates before and after dropout age reform as described in Bell et al., 2022).

Another common theme of the short-run studies of the contemporaneous effect of schooling on crime (e.g. Jacob and Lefgren, 2003; Luallen, 2006) is that, in contrast to the reduction in property crime,

there is an increase in violent crime. This pattern is typically explained by considering the nature of violent crime – they require the in-person interaction of individuals. Incapacitating youths in schools also puts them in close contact with each other, generating a concentration effect and thus increasing the likelihood of violent social interactions.

5.3 Schooling Quantity and Quality

The studies in Panel B that look at connections between an array of education measures and crime taken overall show that education matters for crime, and that the education impact works through various aspects of education quantity and quality. The former quantity impact – more education reduces crime – is the main finding from the research reviewed in sections 5.1 and 5.2. The school quality studies featured in Panel B refine this further by showing also that *better schooling* matters for crime. The (mostly US) studies that look at the effects of attending or being admitted to better performing schools all show this. Cullen et al. (2006) for Chicago, Deming (2011) and McEachin (2020) for North Carolina, Dobbie and Fryer (2015) for New York City and Grey-Lobe et al. (2023) for Boston show that winning a lottery to be admitted to better achieving schools improves crime outcomes. Panel B of the Table also lists studies showing that a number of other aspects of school quality matter for crime, including the social disadvantage structure of the school population and peers (e.g., Gaviria and Raphael, 2001; Billings et al., 2014; Billings and Hoekstra, 2023), school age entry (Cook and Kang, 2016; Depew and Eren, 2016), school discipline (Barrett et al., 2021), grade retention (Eren et al., 2022) and testing regimes and school accountability (Figlio, 2006). Some of the research instead places a focus on cognitive and non-cognitive skills, including those acquired in early childhood education, offering evidence that they can help reduce crime (Garcia et al., 2023; Heckman et al., 2006; Jackson et al., 2020; Johnson and Jackson, 2019). At the time of writing, far fewer studies in this research area are based on evidence from outside the US, but with notable exceptions also showing crime reductions in Denmark by Landersø et al. (2017), in Finland by Huttunen et al. (2023) and in Trinidad and Tobago by Beuermann et al. (2023).

5.4 Crime Impacts on Education

A smaller, but also growing, area of crime and education research explores a causal relation in the opposite direction, from crime to education. Panel C of Table 1 shows 15 studies. This research looks at crime impacts on subsequent educational attainment. An initial set of US evidence demonstrates that juvenile interactions with the justice system, and especially incarceration, can causally harm educational attainment, as shown in Hjalmarsson (2008) and Aizer and Doyle (2015). Following these studies, various different research designs have further established the impact of crime on subsequent education. Variations in judge leniency in sentencing (“judge fixed effects”) have been used in Colombia (Arteaga, 2023) and in

the US (Eren and Mocan, 2021). Other US studies look at different dimensions of the crime impact on education, by studying inmate's education levels as a function of prior youth sentencing (Harlow, 2003), prior exposure to disruptive peers (Carrell and Hoekstra, 2010; Carrell et al., 2018), grade retention (Eren et al., 2017) and school suspensions (Pope and Zuo, 2023)

As discussed in more detail in the concluding section on future research directions, labor economists have in recent years shifted their focus from studying the human capital consequences of one's own criminality to that of exposure to criminal environments and incidents. We discuss the papers published to date here because of their relevance to the impact of crime on education, but also feature them below in the conclusion about emerging areas in the economics of crime research area with an increased focus on victimization. Among the set of very recently published papers (one in each of 2017 and 2019, and all others in the 2020s) about crime exposure, for instance, local violence incidents have also been shown to reduce education, with studies looking at proximity to police killings in Los Angeles (Ang, 2021), drug related crimes in Mexico (Brown and Velásquez, 2017; Michaelsen and Salardi, 2020; Padilla-Romo and Peluffo, 2023), conflict fatalities in the West Bank (Bruck et al., 2019) and homicides in Sao Paolo (Foureaux-Koppensteiner and Menezes, 2021).

5.5 Crime and Education Policies

The final group of crime and education papers, listed in Panel D of Table 1, are a more heterogeneous group that we have collated together owing to their focus on the impact on crime of education-related policies and interventions. They are somewhat more diverse than the core crime and education papers shown in Panels A to C, but are included to complete the picture on the current literature in economics on crime and education. The 12 papers listed in Panel D show that a range of education policies in various settings, including both developed and developing countries, have scope to reduce crime.

6. Future Directions

As can be seen from the survey thus far, the literature on crime and the labor market is vast, especially compared to its state at the time of Freeman's (1999) handbook chapter 25 years ago. Some areas, such as the education-crime link, are also clearly more developed than others. We conclude this chapter by highlighting two lines of research that are still in their infancy, but in our opinion, at the crime and labor market research frontier: the economics of victimization and gangs and organized crime.

6.1 Future Direction I: Victimization

Estimates of the cost of crime are regularly used to evaluate the relative costs and benefits of crime control policies, where the benefit is the cost of the crime prevented. Though direct costs (e.g., costs of policing, prison expenses, or the immediate health-related costs of a violent injury) are relatively easy to measure, it

is not trivial to observe and/or measure indirect and intangible victim costs.⁹ This is especially the case given that these costs may spill-over onto other individuals besides the victim and may vary with victim characteristics (e.g., youths versus adults). Anderson (2021) estimates a total annual cost of crime in the United States between 4.7 and 5.8 trillion dollars – more than 20% of GDP. Yet, despite this staggering statistic and its importance for policy makers, little attention has historically been given to improving the measurement of these costs.

Economists have only recently branched out beyond Becker’s workhorse economic model of *criminal* behavior to study the socioeconomic consequences of victimization and indirect crime exposure. This new literature overcomes two fundamental challenges – data and identification. Victimization registers (in selected countries) and geocoded crime incident data have only recently become available. In terms of identification, individuals select into neighborhoods, jobs, or schools with a non-random allocation of crime; consequently, many unobservable characteristics are related to both crime exposure or victimization and individual outcomes. Especially when studying the labor market effects, simultaneity can also be an issue. One’s employment status, type and location of employment, or (as we will see) even peers on the job can impact one’s risk of being victimized.

This section surveys the new economics of victimization literature, with an emphasis on the human capital and labor market effects. See Bindler et al. (2020) for a more comprehensive survey of the non-labor market effects of victimization.

Human Capital Costs of Youth Victimization. None of the many line-items in Anderson’s (2021) cost of crime estimates include the costs of school-aged juveniles exposed to crime. Because a disruption in human capital accumulation when young can yield a lifetime of employment and earnings losses, a recent international literature considers the human capital costs of youth crime exposure. Much of the recent work uses detailed information on the timing and geography of crime to measure the nature of crime exposure and develop quasi-experimental designs.¹⁰ For example, using temporal variation in exposure to gang violence across cohorts in the same favela or slum of Rio de Janeiro, as well variation in how far the neighborhood is from violence, Monteiro and Rocha (2017) find that students in exposed schools perform worse on math test. This effect increases with conflict intensity, length, and proximity to the exam and can potentially be driven by absent teachers, principal turnover, and temporary school closings. Foureaux Koppensteiner and Menezes (2021) also find that indirect exposure to violence harms educational

⁹ Examples of indirect victim costs include lost productivity, costs of precautionary behavior, long-term health consequences while intangible victim costs include, for instance, lower general well-being or the costs of fear. See Dominguez and Raphael (2015) for a discussion of the variety of approaches to estimate the costs of crime.

¹⁰ In earlier work, Grogger (1997) finds, using the High School and Beyond data, that moderate levels of ‘local’ violence reduce educational attainment, though school and neighborhood violence cannot be disentangled.

attainment by exploiting variation in time and space (i.e., whether a homicide is close to a child's school or school route) in Brazil.

The Brazilian context is perhaps extreme, with homicide rates more than 6 and 29 times that of the US and UK, respectively (Foureaux Koppensteiner and Menezes, 2021). Another extreme but less common event is mass shootings. Bharadwaj et al. (2021) study the 2011 killing of 69 people at a camp for about 600 Norwegian school-aged individuals. Using register data and a difference-in-difference design, in which survivors were matched to similar children not at the camp or from the same school, they show that survivors have significantly worse education outcomes and future labor market outcomes. Higher mental healthcare take-up suggests a role of psychological effects.

A handful of studies also consider the educational impacts of US school shootings using data on the universe of school shootings from the Center for Homeland Defense and Security. Cabral et. al., (2020) match 33 Texas school shootings in 1995 to 2016 to administrative data on public school students. Capitalizing on the arguably random timing of the shootings within schools, the authors compare within student education outcomes for the same students before and after a shooting to within student changes at matched control schools. They find that the shootings increased absenteeism and grade repetition and decreased high school graduation and college enrollment.¹¹

Ang (2021) highlights the impact on youth human capital outcomes of a significant but not typically discussed concern related to police use of force. To disentangle the impact of police use of force from selection effects (on where police presence and use of force is prevalent), Ang (2021) uses geocoded data to calculate the distance from each student home to each shooting. Comparing outcomes of those in the same neighborhood who lived close (within 0.5 miles) and slightly further (0.5-3 miles) from a killing, he finds spikes in absenteeism and reductions in both GPA and high school graduation. Moreover, the effects are driven by minority students and victims.

Though homicides, school shootings, mass killings, and police use of force are the most serious forms of violence (with large potential spill-over effects), they account for a relatively small share of crimes. Youths may be exposed to many other less extreme or less publicized crimes. One such category is domestic violence. Even in high income countries, lifetime rates of intimate partner violence are around 25% (Bhuller et al., 2024). But there is little knowledge on how children's exposure to household violence impacts their life outcomes. Bhuller et al (2024) study this question by matching 22 years of Norwegian domestic violence police reports to identifiers for the victim's children. Using a regression discontinuity design based

¹¹ Levine and McKnight (2020) find worse test scores and chronic absenteeism after the Sandy Hook shooting (28 fatalities), despite spending increases on instruction and support services. Deb and Gangaram (2024) use the 2003-2012 Behavioral Risk Factors Surveillance System surveys but cannot precisely measure shooting exposure.

on the timing of test dates relative to the domestic violence incident, the authors find that domestic violence exposure decreases both exam scores and the chance of completing the first year of high school.¹²

Victimization and the Labor Market. Early papers on the labor market effects of victimization use surveys that ask about both earnings and victimization. Given small samples and the rarity of victimization, these studies tended to result in imprecise estimates.¹³ Linked population-wide register data has rejuvenated this literature.

One way to measure victimization is via hospitalization data. Ornstein (2017) studies the effect of hospital-treated assaults using individual-level Swedish registers and a matching estimator that pairs a victim to an individual with comparable pre-assault characteristics. Ornstein finds a 25% (14%) decrease in earnings for female (male) assault victims. The use of hospital data to measure victimization has its limitations: Many crimes do not result in physical injury and, even for violent crimes, only assaults serious enough to require hospitalization can be studied.

Bindler and Ketel (2022) is the broadest study (in terms of offense types and outcomes) of the labor market effects of victimization. They link Dutch victimization registers of all offenses reported to the police to administrative records on employment, earnings, and unemployment insurance, disability, and welfare benefits. Using event study designs with individual fixed effects to control for time-invariant traits (which may be correlated with the risk of victimization), they find a significant reduction in earnings. The effects are immediate and large for violent offenses (robbery and assault) and smaller and more gradual for offenses like threat and burglary that do not result in injury. For most offenses, earnings do not return to pre-event levels within four years, even after health expenditures do. Finally, labor market effects tend to be worse for female victims.

Bhuller et al. (2024) consider the labor market consequences of domestic violence (in the same Norwegian study cited above about the human capital impacts). In a difference-in-difference framework that compares outcomes before and after the domestic violence report (using families who report domestic violence in the future as controls for those who report today), Bhuller et al. (2024) also find that victims' have higher disability insurance and lower earnings and employment.

Adams-Prassl et al. (2024) take advantage of matched Finnish victimization and employment registers to study the labor market consequences of violence against women at work. They identify more than 5,000 violent incidents in which both the victim and perpetrator were working at the same plant or

¹² Exposure to children from households with domestic violence has been shown to result in negative peer effects (Carrell and Hoekstra, 2010; Carrell et al., 2018).

¹³ Velamuri and Stillman (2008) conducted one of the first such studies using the 'Household, Income, and Labor Dynamics in Australia' (HILDA).

firm. A matched difference-in-differences design with individual fixed effects is again used to compare how employment outcomes of affected workers change before and after the workplace violence event relative to unaffected observationally identical workers. For both victims and perpetrators, employment drops immediately after an incident and does not completely recover in the next five years. An important piece of heterogeneity stands out. Employment effects are larger for perpetrators when both the victim and perpetrator are male, but about 60% larger for victims than perpetrators when the victim is female. Finally, there are firm level effects: female representation decreases (due to decreased hiring and retention) at male-managed firms with male-female violence.

Key Take-Aways from the New Economics of Victimization Literature. Three common themes emerge from the new literatures on the human capital and labor market effects of victimization and crime exposure.

- (i) Access to administrative victimization registers has given researchers power to study many dimensions of the human capital and labor market effects of victimization.
- (ii) There are significant human capital costs of indirect crime exposure (in a wide range of violent crime contexts), which are generally excluded from estimates of the cost of crime. Little is still known about the effects of property crime exposure.
- (iii) Victimization significantly reduces employment and earnings. Effects are persistent, increasing in offense severity, not limited to instances of physical injury, and often larger for females, even when not studying domestic violence.

6.2 Future Direction 2: Gangs and Organized Crime

Criminal organizations play in the underworld the same role that large companies play in legitimate markets: they allow their members to pursue complex (but more remunerative) enterprises that would not be feasible for individuals or for smaller groups of associates. For the case of criminal organizations, some of these enterprises include drug-trafficking, racketeering, extortions, tax frauds, infiltrations in procurement contracts, and so on. These crimes arguably have major economic effects, and yet until very recently, criminal organizations have been largely ignored by the economic literature (Pinotti, 2015a). An important reason for this apparent neglect is that measurement issues are even more challenging when studying organized crime than when studying other types of criminal activities. In most countries, “organized crime” is not even defined in the penal code, meaning that members of criminal organizations cannot be prosecuted unless they commit some other type of crime, such as trafficking or violence. In Italy, which is home to some of the oldest and most powerful criminal organizations in the World, it took until 1982 for the national Parliament to punish the *Associazioni di tipo mafioso* (“mafia-type associations”), defined *ex Art. 416-bis* of the Italian Penal Code as organizations “whose members use the power of

intimidation deriving from the bonds of membership, the state of subjugation and conspiracy of silence that it engenders to commit offences, to acquire direct or indirect control of economic activities, licenses, authorizations, public procurement contracts and services or to obtain unjust profits or advantages for themselves or others”. This definition highlights another important source of measurement error: under-reporting of criminal organizations may be more severe when and where such organizations are more powerful and intimidating. As a consequence, most papers use proxies or “intention-to-treat” approaches leveraging events that are likely to affect the presence and strength of criminal organizations, such as targeted policies and enforcement operations, historical events leading to the birth or the move of criminal organizations, and so on.

Pinotti (2015b) estimates that the expansion of organized crime in two Italian regions (Puglia and Basilicata) in the 1970s lowered their GDP per capita by 15-20 percent over the following decades relative to a “synthetic control” of similar regions. In a mirror natural experiment, Fenizia and Saggio (2024) show that dismissing Italian city councils infiltrated by organized crime increased employment and the number of firms by 17 and 9 percent, respectively. Sviatschi (2022) looks at the long-term consequences of criminal organizations on human capital accumulation and (criminal) career choice. In particular, she shows that the expansion of coca production in Peru (driven by US anti-drug operations in Colombia) led to a 30% increase in child labor in coca-suitable areas, along with a 26% increase in dropout rate at the beginning of secondary school; in the long run, children grown in coca suitable areas are 30% more likely to be incarcerated when adult (age 18-30). In contrast with these papers, Murphy and Rossi (2020) find that Mexican drug cartels bring an improvement of the socio-economic conditions in the municipalities in which they are present, as measured by average salaries, quality of public services, and (lower) illiteracy rates. To establish causality, they leverage the geographical distribution of Chinese immigrants at the beginning of the 20th century, who used opium as a recreational drug and carried with them poppy seeds and knowledge of production and consumption.

Besides large, structured criminal organizations, smaller criminal groups such as street gangs may also have significant economic consequences. Melnikov et al. (2022) estimate the effect of living in gang-controlled areas that were established in El Salvador after thousands of Salvadoran members of street gangs in the US were deported back to their home country. People living just within the border of gang-controlled areas experience lower income and worse employment conditions than people living just outside the border. The authors attribute these effects to the mobility restrictions imposed by gangs and to the higher dropout rates observed in gang-controlled areas. Brown et al. (forthcoming) document another mechanism through which gangs in El Salvador affect economic activity, namely extortion. Using administrative data on 50,000 extortion payments from a leading wholesale distributor over the period 2012-2019, they show that extortion increases with collusion between gangs and that there is a significant passthrough to retail prices.

Turning to other countries, Dustmann et al. (2023) show that refugees in Denmark that are randomly assigned to neighborhoods with significant gang presence are more likely to commit crime before age 19, which in turn affects their working career later in life.

Overall, this evidence suggests that organize crime imposes large economic costs on societies. At the same time, criminal organizations are often deeply intertwined with the official economy, as emphasized by the Art. 416-bis of the Italian Penal Code reported above (“to acquire direct or indirect control of economic activities, licenses, authorizations, public procurement contracts and services”). These connections with the official economy are essential for laundering and re-investing the proceedings from drug-trafficking and other illegal activities. In fact, the most common money laundering schemes, such as false invoicing and ‘smurfing’ (dividing a larger sum of money into multiple tiny transactions), require the collaboration of a large number of firms. In addition, criminal organizations may influence (or even create) firms for other purposes, such as acquiring social and political influence. Using classified data provided by the Financial Intelligence Unit of the Bank of Italy, Arellano-Bover et al. (2024) estimate that about 2 percent of all Italian firms have links with criminal organizations, and provide a taxonomy of such firms. Using similar data for the Italian region of Lombardy, Bianchi et al. (2022) show that connected firms report higher sales but lower profits than non-connected firms, and are more likely to file for bankruptcy. Miranda et al. (2022) find similar results relying on a different measure of connection that is based on the family name and area of origin of the firm directors and owners. Both these papers interpret the findings as evidence that connected firms mainly serve the purpose of laundering and re-investing profits from illicit activities. Consistent with this interpretation, Le Moglie and Sorrenti (2022) find that after the credit crunch of 2007-2008, Italian provinces with a greater presence of organized crime displayed more firm creation than provinces with lower organized crime presence, and attribute the differential to the stable supply of (illicit) funds from criminal organizations. Daniele et al. (2024) look instead at the demand side, particularly by financially distressed firms. Using the same data as Arellano-Bover et al. (2024) along with credit score ratings on the universe of Italian firms, they show that being downgraded to a substandard credit risk, thus losing access to bank credit, increases the probability of being infiltrated by criminal organizations. Finally, Calamunci and Drago (2020) show that anti-mafia operations targeting connected firms have positive spillovers on other firms in the market, while Slutzky and Zeume (forthcoming) document increases in innovation activity and competition following such operations.

7. Conclusions

The economics of crime field has substantially advanced our understanding of criminal behavior and its relationship with labor markets. This chapter has documented the profound influence of economic conditions, such as wages, employment opportunities, and educational attainment, on crime rates. It also

pays careful attention to the literature on the impact of criminal justice interactions and having a criminal record on human capital attainment and labor market outcomes. Not only does this latter literature highlight the need for researchers to acknowledge identification challenges, like simultaneity bias, but also the fact that there is an oftentimes reinforcing cycle between crime and inequality. By applying rigorous methodological approaches, economists have been able to disentangle correlation from causation, overcoming this and other identification challenges, thereby providing more reliable insights into the effectiveness of various policy interventions. The shift from aggregated data to individual-level analyses and the incorporation of international perspectives have further enriched what has begun a sizable literature, allowing for more nuanced and context-specific policy recommendations.

Looking ahead, the integration of new data sources and innovative research designs will continue to push the boundaries of what we know about the economics of crime. Future research should focus on exploring the long-term impacts of social, education and criminal justice policies, the role of social networks in criminal behavior, and the effectiveness of rehabilitative versus punitive measures. Additionally, understanding the socio-economic costs of crime victimization and the influence of criminal organizations on labor markets remains a vital area for further investigation. By continuing to bridge the gap between theory and practice, economists can play a pivotal role in shaping policies that not only reduce crime but also enhance overall societal well-being.

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Table 1. Literature in Economics on the Link between Crime and Education in the Last 20 Years (up to 2023)

Study	Setting	Data	Treatment	Research Design	Correlation	Causal Impact
Panel A. Crime and Education, Completed Education and Dropout Age Laws						
Anderson (2014)	US	FBI's Uniform Crime Reports (UCR) from 1980-2008; the National Center for Education Statistics' Digest of Education Statistics; U.S. Census Bureau; Bureau of Economic Analysis; Youth Risk Behavior Survey	Secondary school attendance	Minimum Dropout Age (MDA) reforms to specify a difference-in-difference-in-differences (DDD) approach	Compared with MDA 16-17 laws, MDA 18 laws correlated with ↑ arrest rates of youth aged 13-15 and 16-18	Compared with MDA 16-17 laws, MDA 18 laws lead to a ↓ 17.2% arrest rates, of which a ↓ 9.9% property and a ↓ 22.5% violent crime arrest rates of youth aged 16-18
Beatton et al. (2017)	Queensland, Australia	Queensland administrative data from the Department of Education and Training matched at the individual level with the Queensland Police Service from 2002-13	Secondary school attendance	Minimum Dropout Age (MDA)		Exposure to the MDA reform led to a ↓ 10.3% crime for all 15–21 year olds, to a ↓ 10.8% crime for males and to a ↓ 8.9% crime for females
Bennett (2018)	Denmark	Administrative Danish Register Data on twins born in 1965–82	Upper secondary school completion	OLS on sample of twins and with twin fixed effects		Upper secondary school completion leads to a ↓ 23 pp crime and to a ↓ 9.8 pp conviction for males, as well as to a ↓ 9 pp crime and to a ↓ 3.2 pp conviction for females
Bell et al. (2022)	US	FBI Uniform Crime Report (UCR) from 1974 onwards for males aged 15–24 years old	Secondary school attendance	Minimum Dropout Age (MDA) to define a Regression Discontinuity (RD) design	Cohorts exposed to the MDA reforms face a ↓ 4.6% arrest rate	Exposure to the MDA reforms leads to a ↓ 6.1% log arrest rates for young adults
Brilli and Tonello (2018)	Italy	Yearly aggregate data on school enrollments and youth crime from the administrative records of the Statistical Office of the Italian Ministry of Education (MIUR) and the Italian National Institute of Statistics (ISTAT) for the 1997/98 to 2001/02 school years; Labor Force Survey (ISTAT); Public Finance Database (Italian Ministry of the Interior), Italian Demographic Database (ISTAT); survey VS data on victimization from 1999-2008	Secondary school attendance	Minimum Dropout Age (MDA) to define an Instrumental Variable (IV) model in a difference in difference specification		A 1pp increase in enrollment rate leads to a ↓ 1.6% offending rate among 14 year olds

Buonanno and Leonida (2006)	Italy	Annual panel dataset for Italian regions (NUTS2) from 1980-95; Centre for North South Economic Research (CRENoS); ISTAT (Italian Statistics Bureau) population data	Secondary school attendance	Instrumental variable approach for panel data using the GMM-system estimator	Education, measured as the average years of schooling of the population, is linked with ↓ crime rate	
Buonanno and Leonida (2009)	Italy	Annual panel dataset for Italian regions (NUTS2) from 1980-95; Centre for North South Economic Research (CRENoS); ISTAT (Italian Statistics Bureau) population data and ISTAT Quarterly Labour Force Surveys	Secondary school attendance	Region and time fixed effects as well as region-specific time trends	Education, measured as the average years of schooling of the population, is linked with ↓ property crime rate	
Cano-Urbina and Lochner (2019)	US	US Census data from 1960-1980; 1960-90 FBI's Uniform Crime Reports	Educational attainment and school quality	Minimum Dropout Age (MDA) to define an Instrumental Variable (IV) model		One additional year of schooling leads to ↓ 0.04 pp incarceration rates for white women and ↓ 0.08 pp incarceration rates for black women
Gilpin and Pennig (2015)	US	School Survey on Crime and Safety (SSOCS, 2004, 2006, 2008, 2010)	Secondary school attendance	Reduced form analysis of Minimum Dropout Age (MDA) laws in a difference in difference specification		High schools in states that raise their MDA law to 18 experienced ↑ 21.4% school crimes
Hjalmarsson et al. (2015)	Sweden	25% random sample from Sweden's Multigenerational Register on all persons born from 1943-54 who have lived in Sweden at any time since 1961 matched with data on parents, siblings and children; Sweden's Education Register and 1970 Census of Sweden; Sweden's National Council for Crime Prevention records of all criminal convictions from 1973-2007 for each individual	Educational attainment	Minimum Dropout Age (MDA) to define an Instrumental Variable (IV) model		One additional year of schooling leads to ↓ 6.7% risk of conviction of men and to ↓ 15.5% incarceration of men, while estimates for women are similar in magnitude but not statistically significant
Lochner and Moretti (2004)	US	US Census data from 1960-1980 on males aged 20-60; 1960-90 FBI's Uniform Crime Reports; National Longitudinal Survey of Youth	Educational attainment and school quality	Minimum Dropout Age (MDA) to define an Instrumental Variable (IV) model in a difference in difference specification	One additional year of schooling leads to ↓ 0.1 pp incarceration for white men and ↓ 0.37 pp incarceration for black men	One additional year of schooling leads to ↓ 0.1 pp incarceration for white men and ↓ 0.3-0.5 pp incarceration for black men

Machin et al. (2011)	England and Wales	Offenders Index Database (OID) on criminal histories of offenders aged 18–40, born in 1946-70 and convicted of standard list offences from 1963 onwards; Office for National Statistics (ONS) population data by age cohort and year, separately for men and women; the UK General Household Survey (GHS) for the 1972-96 years; 2001-08 British Crime Survey	Secondary school attendance and school qualification	Minimum Dropout Age (MDA) to define a Regression Discontinuity (RD) design	A 10% increase in age left school is correlated with ↓ 2.1% for males	A 10% increase in age left school leads to ↓ 2.1% crime for males
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Panel B. Crime and Education, Quantity/Quality						
Barrett et al. (2021)	Louisiana, US	Student-level data provided by the Louisiana Department of Education (LDOE) from 2000-14	Student discipline disparities by race and family income		Black (poor) students are \uparrow 13 (9) pp more likely to be suspended in a given year than white (non-poor) students	
Berthelon and Kruger (2011)	Chile	Chile's National Socio-economic Characterization Survey, CASEN since 1990; administrative data from the Defensoría Penal Pública, i.e., the Chilean equivalent of a Public Defender's office	Length of the school day	School reform that lengthened the school day from half to full-day shifts		An increase of 20 pp in the municipal share of full-day high schools leads to a \downarrow 3.3% in the probability of motherhood in adolescence and to a \downarrow 11% to 24% in the juvenile crime rate
Beuermann et al. (2023)	Trinidad and Tobago	Official administrative Secondary Entrance Assessment (SEA) data for all applicants to a public secondary school in Trinidad and Tobago from 1995-2012; data on the NCSE exams (age 14), the CSEC exams (age 16) and the CAPE exams; official arrests records from the Trinidad and Tobago Police Service; official birth records from the Trinidad and Tobago Registrar General; official registry of active contributors to the national retirement fund by May 2017 of National Insurance Board	Whether schools' impact on test scores measure their overall impact on students	Exogenous school assignments		Schools' impacts on high-stakes tests are weakly related to impacts on arrests, dropout, teen motherhood, and formal labour market participation
Billings et al. (2014)	Charlotte-Mecklenburg, North Carolina, US	Administrative Charlotte-Mecklenburg school records (1995-2011); administrative records of adult arrests and incarcerations in Mecklenburg County (1998-2011); National Student Clearinghouse records of college attendance	School share minority	Discontinuous school boundary change		10 pp increase in assigned school share minority leads to \uparrow 8% arrest and incarceration among minority males
Billings and Hoekstra (2023)	Charlotte-Mecklenburg, North Carolina, US	Pupil records for Charlotte-Mecklenburg schools from 1998-2011; Mecklenburg County arrest records from 1998-2014; Mecklenburg County	School and neighbourhood peers whose	Cohort variation		A 5 pp increase in school peers linked to parental arrest leads to \downarrow 0.016 standard deviation in

		jail records from 1998-2014; North Carolina state prison records from 1998-2014	parents have been arrested			school achievement and to ↑ 5% in adult arrest rates
Cook and Kang (2016)	North Carolina, US	Individual-level administrative data from the NC public school system (1987-93 birth cohorts); official-juvenile-complaint data and adult-felony-conviction data in NC from the NC Department of Juvenile Justice and Delinquency Prevention and NC Department of Corrections	Delayed Entry Eligibility (DEE)			DEE leads to ↓ 31.8% criminality at age 13-15 and ↑ 14% serious criminality at age 17-19
Cullen et al. (2006)	Chicago, US	Chicago public schools (CPS) administrative data on applications in spring 2000/01; achievement and attainment of CPS students, student survey on degree of satisfaction with school, students treatment by teachers and peers, college expectations, arrest	High-achieving school attendance	Admission lottery into school		Lottery win to high-achieving school leads to nearly ↓ 60% self-reported arrest rates relative to lottery losers
Deming (2011)	Charlotte-Mecklenburg, North Carolina, US	Administrative Charlotte-Mecklenburg school records for students in grades 6-11 in 2002 and age 17-23 in 2009; administrative records of adult arrests and incarcerations in Mecklenburg County (2006-209); NC Department of Corrections from 2006 on	Better-achieving school attendance	Admission lottery into school		Lottery win to better-achieving school leads to roughly ↓ 50% criminality among high-risk youth relative to lottery losers
Depew and Eren (2016)	Louisiana, US	Administrative records from the Louisiana Department of Education from 1997-2012; administrative data from the Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice	School entry age	Parametric fuzzy Regression Discontinuity (RD) Design	Null correlation for black females between timing of school entry and likelihood to commit a juvenile crime	ITT estimates show that being born right after the school entry cutoff leads black females to a ↓ 3 pp in likelihood to commit a juvenile crime. IV estimates also show that late school entry by one year leads black females to a ↓ 3.5 pp in likelihood to commit a juvenile crime
Dobbie and Fryer (2015)	New York City, US	Survey data from youth entered in the 2005/06 Promise Academy sixth grade admissions lotteries in the Harlem Children's Zone; administrative data on high school test-taking from the New York City Department of	High-performing charter school attendance	Admission lottery into school		Lottery win to high-performing charter school leads to ↓ 10.1 pp teenage pregnancy and ↓ 4.4pp male incarceration

		Education and college enrolment data from the National Student Clearinghouse				
Eren et al. (2022)	Louisiana, US	Administrative records of the Louisiana Department of Education (LDOE); Louisiana Department of Public Safety and Corrections, Adult Services, from 1996-2012		Regression discontinuity design to study test-based promotion policy		Grade retention leads to \uparrow 1.05pp in the likelihood of conviction by age 25
Eriksson (2020)	US	Linked census data set of incarcerated and nonincarcerated men	Childhood access to primary schooling	School construction		Exposure to one new primary school built as part of the Rosenwald programme leads to \downarrow 1.9 pp risk of incarceration
Figlio (2006)	Florida, US	Administrative dataset on every disciplinary suspension, both in-school and out-of-school, during the four school years from 1996–97 through 1999–2000, i.e., following the introduction of the Florida Comprehensive Assessment Test (FCAT)	High stakes testing	Interaction between the testing calendar, the grade level of the student, and the expected performance level of the student		A one standard deviation increase in the test window manipulation measure is associated with a \uparrow 1.2 pp in the likelihood that a student will attain level 2 or better on the FCAT reading exam and a \uparrow 1.7 pp in the likelihood that a student will attain level 2 on the FCAT mathematics exam
Garcia et al. (2023)	US	Newly collected data on the original High Scope Perry Preschool Project participants through late / middle age and on their children into their mid-twenties	Long-term benefits of early childhood intervention on disadvantaged children	High Scope Perry Preschool Project		The intervention led to long-lasting \uparrow 0.2–0.4 standard deviation in the original participants' skills, \uparrow 10 pp more likely to be married at age 30, \uparrow \$10,000 average annual earnings, \downarrow 1 criminal offence, and \uparrow 15 pp healthier. Children of the original participants have \uparrow 16.9 pp in likelihood of never being suspended and \uparrow 25.8 pp in likelihood of employment, lower levels of criminal activity, and better health
Gaviria and Raphael (2001)	US	National Education Longitudinal Survey (NELS) 1988-94	Peer group influences	IV estimates using average background characteristics as IV	Drug use by parents correlated with \uparrow probabilities of drug, alcohol, and tobacco	Drug use by parents correlated with \uparrow probabilities of drug, alcohol, and tobacco consumption by their children

					consumption by their children by 19.4%, 13.2%, and 10.2%, respectively	by 19.4%, 13%, and 10.3%, respectively
Gray-Lobe et al. (2023)	Boston, US	All preschool applicants from fall 1997 to fall 2003 from the Boston Public Schools district; National Student Clearinghouse (NSC) data; administrative data from the Massachusetts Department of Elementary and Secondary Education (DESE)	Public preschool attendance			Preschool enrollment leads to ↑ 18% in college attendance, ↑ 9 pp in SAT test-taking and ↑ 6 pp in high school graduation. Preschool also leads to ↑ 0.17 standard deviation in disciplinary measures including juvenile incarceration, but with no detectable impact on state achievement test scores
Heckman et al. (2006)	US	National Longitudinal Survey of Youth 1979	Boosting cognitive and noncognitive skills	Simulations		Moving males in lowest decile of cognitive distribution from lowest to highest decile of noncognitive distribution substantially ↓ incarceration. Moving same males in lowest deciles of both distributions to highest decile of cognitive distribution only slightly ↓ incarceration
Huttunen et al. (2023)	Finland	Finnish joint application registry for cohorts who graduated from compulsory schooling from 1996-2003 and applied to further education immediately upon graduation; population-wide administrative registers from Statistics Finland from 1995–2013; the Finnish Longitudinal Employer-Employee Data (FLEED); Student Register and the Register of Completed Education and Degrees; Prosecutions, Sentences and Punishments based on the district court rulings	Secondary education	Admission cut-offs in over-subscribed programmes to generate Regression Discontinuity (RD) designs		Admission of men to secondary schools leads to ↓ 52% risk of conviction in a district court within 10 years after admission compared with men who are not admitted
Jacob and Lefgren (2003)	US	29 urban jurisdictions in NIBRS data, detailed school calendar	School attendance	Teacher in-service days		Youth property crime ↓ 14% on days when school is in session,

						while violent crime ↑ 28% on such days
Jackson et al. (2020)	Chicago, US	Administrative data from Chicago public schools on 133 public high schools for cohorts of ninth grade students who attended one of these schools in 2011-17	Attendance of schools with high socio – emotional development (SED) value added	Covariance of school value added across outcomes		Higher social value added leads to ↓ 0.728 pp risk of arrest; greater work hard value added leads to ↓ 0.766 pp risk of arrest; greater test score value added leads to ↓ 0.523 pp risk of arrest
Johnson and Jackson (2019)	US	Panel Study of Income Dynamics (PSID); National Archives Record Administration, Inter-university Consortium for Political and Social Research, and Surveillance, Epidemiology, and End Results population data	Early childhood exposure to investments designed to promote school readiness among disadvantaged children	Head Start and K–12 spending		For poor children exposed to a 10% increase in K–12 spending, exposure to Head Start led to ↑ 0.59 additional years of education, ↑ 14.8 pp likely to graduate high school, ↑ 17% higher wages, ↓ 4.7pp likely to be incarcerated, and ↓ 12pp less likely to be poor as an adult
Landersø et al. (2017)	Denmark	Danish register-based data for children born in mid-1981 to mid-1993	Delayed Entry Eligibility (DEE)	Discontinuous minimum school-entry age		DEE leads to ↓ criminality at (by) all ages until age 19 (22) for boys and at (by) age 15 (19) for girls
Lochner (2004)	US	National Longitudinal Survey of Youth 1979 and FBI’s Uniform Crime Report (UCR)	High school dropout	OLS	Roughly 30% (33%) of young men with <10 (≤11) years of schooling earn income from crime. Among high school graduates, 24% of men not staying on in school earned income from crime and 17% of men pursuing college did. Additional estimates show that high school graduates face ↓ 81% risk of incarceration	
Luallen (2006)	Washington State, US	Administrative Washington Juvenile Court Case Records 1980-2001,	School attendance	Teacher strikes		Youth crime ↑ 21.4% on days when strikes occur. In

		Census 2000 Summary File 3 and 1990 Summary Tape File 3, Public Employee Strikes in Washington 1967-2003, news articles from the Associated Press and the Seattle Times				particular on such days, mischievous crime ↑ 48%, property crime ↑ 28.8% and violent crime ↓ 31.5%
McEachin (2020)	North Carolina, US	Administrative data of the North Carolina Department of Public Instruction for all students in North Carolina public schools in 2004-16; administrative data from the North Carolina Department of Public Safety and population-level records from the North Carolina Board of Elections	Charter school attendance	Doubly-robust inverse probability weighted approach		Compared with students who attended a traditional public school in both 8th and 9th grade, charter school entrants face ↓ 0.9 pp risk to commit any crime, and ↓ 0.7 and ↓ 0.4 pp risk to be convicted for a misdemeanor and felony off bases of 3, 0.2, 1.3 pp

Panel C. Crime Effects on Education						
Aizer and Doyle (2015)	Chicago, US	Chicago Public Schools Student Database (1990–2006); Juvenile Court of Cook County Delinquency Database (1990–2006); Illinois Department of Corrections Adult Admissions and Exits Database (1993–2008)	Youth incarceration		High school graduation ↓ 39 pp, adult incarceration ↑ 41 pp	Youth incarceration leads to ↓ 13 pp high school graduation and ↑ 23 pp adult incarceration
Ang (2021)	Los Angeles, US	Incident-level data on the universe of officer-involved killings in LA County, California, from 2002-16; home addresses and individual-level panel data for all high school students enrolled in the LA Unified School District	Police killings	Granular variation in how close students live to a killing		Exposure to police violence leads to ↓ 0.04 points in GPA, ↑ 15% incidence of emotional disturbance, ↓ 3.5% rates of high school completion and ↓ 2.5% college enrollment
Arteaga (2023)	Colombia	Colombia’s census of potential beneficiaries of welfare (SISBEN); Attorney General’s Office records; internet records scraped by the author	Parental incarceration	Judge IV		Parental incarceration leads to ↑ 0.78 years in educational attainment for children of convicted parents
Brown and Velásquez (2017)	Mexico	INEGI monthly homicide reports at the municipal level and Mexican Family Life Survey	Drug-related violence	Surge in drug-related crime		Increased local violence leads to ↓ 0.3 years of education, ↓ 8 pp likelihood of compulsory school completion, and ↑ likelihood of employment
Bruck et al. (2019)	West Bank	MOEHE administrative records from 2000-06; Israeli NGO B’Tselem (Israeli Information Center for Human Rights in the Occupied Territories); Palestinian Labor Forces Surveys (PLFS) for the period 2000–06	Effect of the Israeli–Palestinian conflict on education outcomes	Within school variation in the number of conflict-related Palestinian fatalities in the academic year		The conflict leads to ↓ probability of passing the final exam, ↓ in the total test score, and ↓ in the probability of being admitted to university
Carrell and Hoekstra (2010)	Florida, US	Confidential student-level panel dataset provided by the School Board of Alachua County in Florida on students in the third through fifth grades from 22 public elementary schools for the academic years 1995–1996 through 2002–2003; public records from the Alachua County Courthouse including the date filed and the names and addresses of	Exposure to children from troubled families	Family problems, as signaled by a request to the court for protection from domestic violence, used as exogenous source of variation in peer quality		Adding one more troubled boy peer to a classroom of 20 students leads to a ↓ nearly 2 percentile points (one-fifteenth of a standard deviation) in boys’ test scores, and to a ↑ 40% in the number of disciplinary infractions committed by boys

		individuals involved in domestic violence cases filed in civil court in Alachua County between January 1, 1993 and March 12, 2003				
Carrell et al. (2018)	Florida, US	Administrative records from Alachua County (Florida) primary schools from 1995–1996 and 2002–2003 from the Florida Department of Education (FLDOE). Domestic violence cases filed in civil court in Alachua County between January 1, 1993 and March 12, 2003; National Student Clearinghouse (NSC) records from 2012	Exposure to a disruptive peer in elementary school	Variation in cohort composition across time within school		Exposure to a disruptive peer in classes of 25 in elementary school leads to ↓ 3% earnings aged 24-28
Eren et al. (2017)	Louisiana, US	Administrative records of the Louisiana Department of Education (LDOE) from 1999-2012; Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice	Summer school and grade retention	Regression discontinuity design to study test-based promotion policy		Grade retention ↑ 3pp the propensity of a student to drop out of school
Eren and Mocan (2021)	Louisiana, US	Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice from 1996-2012; Louisiana Department of Public Safety and Corrections, Adult Services from 1996-2012	Impact of juvenile punishment on adult criminal recidivism and high school completion	Judge IV		Negative effect on high school completion for earlier cohorts, but no impact on later cohorts. Juvenile incarceration leads to ↑ 27 pp in the probability of adult conviction of a drug offense, null effect for violence
Foureaux-Koppensteiner and Menezes (2021)	São Paulo, Brazil	Brazilian school census data collected by Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira (INEP; National Institute for Educational Studies and Research “Anísio Teixeira”) on behalf of the Brazilian Ministry of Education; Sistema de Avaliação de Rendimento Escolar do Estado de São Paulo (SARESP; the education evaluation system of the state of São Paulo); and individual attendance records in all state schools from the São Paulo State	Effect of exposure to homicides around schools, students’ residences, and on way to school	Variation in homicides within a 25-meter radius around schools		Violence leads to ↓ 5% standard deviation test scores and to ↑ 20% dropout rates

		Secretariat of Education; Brazilian Ministry of Health records				
Harlow (2003)	US	Survey of Inmates in State and Federal Correctional Facilities 1991 and 1997; Survey of Inmates in Local Jails 1989 and 1996; Survey of Adults on Probation 1995; Current Population Survey 1997; National Adult Literacy Survey 1992	Education achievement of inmates		Inmates' education links with ↓ youth sentencing, as roughly 40% without high school diploma, 45% with a GED, 26% with a high school diploma, 21% with some college had prior youth sentences either to a facility or probation	
Hjalmarsson (2008)	US	National Longitudinal Survey of Youth 1979	Arrest, charge, conviction, incarceration at age 16 or younger	OLS	Arrests lead to roughly ↓ 11 pp likelihood of graduation, and incarcerations lead to roughly ↓ 26 pp likelihood of graduation	When correcting for unobservables, incarcerations still lead to roughly ↓ 13 pp likelihood of graduation
Michaelsen and Salardi (2020)	Mexico	Ministry of Health (Secretaría de Salud) records of violence; ENLACE - Evaluación Nacional del Logro Académico en Centros Escolares - data on performance of primary school students on national standardized exams from 2006-11	Exposure to violence	Exogenous variation generated by "War on Drugs"		Exposure to at least three homicides within a 2km radius in the week immediately prior to exams leads to ↓ 0.1 in standard deviation of test scores
Padilla-Romo and Peluffo (2023)	Mexico	ENLACE (National Assessment of Academic Achievement in Schools) data from 2005-13; INEGI (National Institute of Statistics and Geography of Mexico) data; CONAPO (National Population Council) data	Out-migration from violence-affected areas and peer exposure to violence	Mexican war on drugs		Adding a new peer who was exposed to local violence to a class of 20 students leads to ↓ 2% standard deviation in incumbents' academic performance
Pope and Zuo (2023)	Los Angeles, US	Student-level administrative data from the Los Angeles Unified School District	School suspension	Changes in school suspension policies		10 pp lower suspension rates lead to ↓ 0.040 standard deviation in math and ↓ 0.065 standard deviation in English test scores, ↓ 0.07 standard

						deviation in grade point averages and ↑ 15.1% absences
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Panel D. Policy and Interventions						
Anders et al. (2023)	North Carolina, US	Administrative conviction data from North Carolina's Department of Public Safety 1972-2018; Head Start and Smart Start funding information from the National Archives and Records Administration (NARA)	Early childhood education	Rollout of Head Start and Smart Start		Improvements to early childhood education led to \uparrow 20% reductions in the likelihood of a serious criminal conviction in adulthood
Anderson and Sabia (2018)	US	Youth Risk Behavior Surveys (YRBS) from 1993-2013	Youth gun carrying and mass shootings	Child access prevention (CAP) gun controls laws		CAP laws lead to a \downarrow 13% in the rate of past month gun carrying and a \downarrow 18% in the rate at which students report being threatened or injured with a weapon in school
Blattman et al. (2017)	Liberia	Survey data collected for the evaluation of the intervention	Cognitive behavioral therapy to foster self-regulation, patience, noncriminal identity, lifestyle, and \$200 grants	Randomized allocation of treatment		Cash after therapy led to \downarrow 0.31 standard deviation antisocial behavior for over a year
Foged et al. (2023)	Denmark	Administrative records on demographics and school and crime records of youth born in 1990 to 2001, still in Denmark at age 18, and with at least one parent granted asylum in Denmark within four years around 1st January 1999	Parental language training for refugees	Reform to expand language training for adult refugees		Parental language training of refugees leads to \downarrow 72.7% convictions and \downarrow 80.8% charges of male children aged 15-18
Gulesci et al. (2021)	Bolivia	Data collected for the purpose of the evaluation of this intervention	Impact of a youth empowerment programme on the reported prevalence of violence against girls			The youth empowerment programme led to \downarrow 10pp in the reported prevalence of violence against girls during the COVID-19 lockdown

Heller (2022)	Chicago, Philadelphia, US	Chicago and Philadelphia administrative police records of arrests; in Philadelphia, service records from the City's integrated data system, known as CARES, to measure juvenile incarceration (including both detention and prison) and related court ordered services	Summer youth employment programmes (SYEP)	Admission lotteries		In Philadelphia [Chicago], being offered the programme leads to ↓ 1 [9] arrest per 100 youth (i.e., 36%) [i.e., 52%]. Due to the size of first stage, the effect on compliers in Philadelphia is ↓ 3 arrests per 100 youth (i.e., 65%)
Heller et al. (2017)	Chicago, US	Longitudinal student-level CPS records, Illinois State Police (ISP) records and arrest data from the Chicago Police Department (CPD)	Becoming A Man (BAM) programme	Set of randomized controlled trials (RCTs)		The BAM programme reduced total arrests during the intervention by 28–35%, reduced violent-crime arrests by 45–50%, raised school engagement and graduation rates by 12–19%
Owens (2017)	US	National Incident Based Reporting System (NIBRS) from 1997-2007; COPS office from 1994-2007; Uniform Crime Reports Law Enforcement Officers Killed and Assaulted (LEOKA); Law Enforcement Management and Administrative Statistics (LEMAS)	Use of sworn School Resource Officers (SROs)	Federal hiring grant to place law enforcement in school		The average grant is linked with ↓ 1.1-1.9% disruptive criminal incidents in school
Rees et al. (2022)	US	Youth Risk Behavior Surveys (YRBS) data from 2009-17; National Vital Statistics System (NVSS) from 1993-2016	Bullying victimization	State-level anti-bullying laws (ABLs)		State-level anti-bullying laws (ABLs) lead to ↓ 2.6 pp in bullying victimization, ↓ 1.9 pp in depression, and ↓ 1.7 pp in suicidal ideation
Sabates and Feinstein (2008)	England and Wales	Home Office Offenders Index database (OI) records of criminal convictions in England and Wales for 1996-2002 samples	UK government initiatives: Reducing Burglary Initiative; and Educational Maintenance Allowance	OLS with difference in differences design		Areas that introduced both programmes faced ↓ 1.1 - 1.5 convictions for youth aged 16-18 years old for burglary per 1000 relative to areas where neither programme was introduced
Sorensen et al. (2023)	US	2013/2014 and 2017/2018 waves of the CRDC from all public schools in the US, except preschools and schools with <25 students; Freedom of Information Act (FOIA) request to the	School resource officers (SROs)	Fuzzy regression discontinuity design		SROs lead to ↓ 30% in violence in schools, but do not prevent gun-related incidents

		U.S. Department of Justice COPS office				
Weisburst (2019)	Texas, US	Texas Education Research Center (ERC) records, Texas Education Agency (TEA) records and Texas Higher Education Coordinating Board (THECB)	Funding for school police on student outcomes	Federal Community Oriented Policing Services (COPS) grants		Federal grants for police in schools lead to ↑ 6% middle school discipline rates, ↓ 2.5% in high school graduation rates and ↓ 4% in college enrolment rate