The effect of violent crime on economic mobility

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ABSTRACT

Recent evidence has found substantial geographic variation in the level of upward economic mobility across US states, metropolitan areas, commuting zones, and counties. However, minimal progress has been made in identifying the key mechanisms that help explain why some urban areas have low rates of upward mobility while others have rates of upward mobility that resemble the most mobile nations in the developed world. In this article we focus attention on one specific dimension of urban areas, the level of violent crime. Using longitudinal data and an array of empirical approaches, we find strong evidence that the level of violent crime in a county has a causal effect on the level of upward economic mobility among individuals raised in families at the 25th percentile of the income distribution. We find that a one standard deviation decline in violent crime as experienced during late adolescence increases the expected income rank in adulthood by at least 2 points. Similarly, a one standard deviation decline in the murder rate increases the expected income rank by roughly 1.5 points. These effect sizes are statistically and economically significant. Although we are limited in our capacity to provide evidence on the mechanisms explaining the link between crime and mobility, we present suggestive results showing that the decline in the violent crime rate reduced the prevalence of high school dropouts at the county level between 1990 and 2010.

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1. Introduction

Until recently, virtually all research on intergenerational economic mobility in the United States had focused on the transmission of economic advantage and disadvantage in the nation as a whole. Although substantial progress has been made in measuring levels and changes in economic mobility in the US, this literature has ignored the tremendous heterogeneity in levels of economic mobility across regions of the country, states, cities, and counties (Chetty et al., 2014a, 2014b; Chetty and Hendren, 2015; Economic Mobility Project, 2012; Graham and Sharkey, 2013). Chetty et al. (2014a) find that some commuting zones in the US have levels of mobility equal to the most mobile nations in Western Europe, and others have levels of mobility lower than any of the nations in the developed world. Further, this geographic variation in economic mobility appears to be a function of places themselves, rather than the people within them (Chetty and Hendren, 2015).

Although this evidence suggests a causal effect of places on economic mobility, minimal progress has been made in explaining what it is about those places that increases or reduces the chances for residents to move upward in the income distribution. In an initial attempt to shed light on the mechanisms for upward mobility, Chetty et al. (2014a) and Chetty and Hendren (2015) examine several characteristics of counties and commuting zones. The authors find that both commuting zones with the highest levels of absolute upward mobility and counties with the largest positive effects on earnings in adulthood have, on average, lower rates of residential segregation by income and race, lower levels of income inequality, better schools, lower rates of violent crime, and a larger share of two-parent households. However, the authors make clear that these associations are a first step and should not be thought of as causal.

In this article we push this literature forward by focusing our attention on one specific dimension of urban areas, the level of violent crime. Our focus on violent crime is driven by several strands of converging evidence showing that exposure to neighborhood violence may be a central mechanism by which growing up in areas of concentrated disadvantage affects the life chances of children (Burdick-Will et al., 2011; Harding, 2009; Harding et al., 2011; Sharkey, 2010). This argument is consistent with preliminary correlational evidence that shows a strong association between violent crime and upward mobility measured at the level of counties and commuting zones (Chetty et al., 2014a; Chetty and Hendren, 2015). In this study, we assess the robustness of the relationship between

violent crime and upward economic mobility, we exploit shocks on violent crime within counties to assess whether this relationship is causal, and we provide preliminary evidence on possible mechanisms linking violence and economic mobility.

Our estimates suggest that birth cohorts raised at the 25th percentile of the income distribution and exposed to levels of violent crime one standard deviation above the mean experienced a decline of 2 points in their expected income rank in adulthood. For the murder rate, we find an effect size of 1.5 points in expected income rank. In an attempt to shed light on potential mechanisms, we investigate the association between changes in violent crime and changes in college attendance and high school dropout rate, respectively. We find no evidence that violent crime affects rates of college attendance, but we find suggestive evidence that high-school dropout may be an important mechanism explaining the link between violence and economic mobility.

2. Violence and the life chances of children

Violence is unique among major public health problems in that it targets young people. Even after two decades of declining violent crime, homicide remains among the leading causes of death for all 15–34 year-olds and is the leading cause of death among African Americans in this age range (National Vital Statistics System, 2015). This feature of violence is important because it means that children living within dangerous communities are confronted with the threat of violence at an early stage in life, with consequences that can disrupt their developmental trajectories and limit their ability to accumulate human capital.

Ethnographic research focusing on the lives of young people within violent settings demonstrates the ways that youths are forced to navigate strategically through public spaces, shifting their schedules, their networks, and their routines in efforts to minimize the threat of victimization (Anderson, 2000; Edin et al., 2015; Harding, 2010; Jones, 2010). Parents and their children develop creative ways to manage the threat of violence, but they do so in ways that may limit their children's capacity to engage in public life within their communities and schools (Jarrett, 1999; Furstenberg, 1993). Instead of taking advantage of resources and activities that may be available in local schools or community centers, parents and children expend a great deal of energy on the more basic challenge of avoiding victimization.

Research on low-income families participating in housing mobility programs has shown that parents often make choices about important aspects of their children's lives, such as which school the child will attend, based on concerns about safety rather than concerns about school quality (Clampet-Lundquist et al., 2011; Darrah and DeLuca, 2014). Concerns about violence, drugs, and gangs are consistently found to be the primary reasons why low-income families choose to take part in residential mobility programs designed to offer families the chance to move out of public housing located in areas of concentrated poverty (Wilson and Mast, 2014).

An extensive quantitative literature shows a negative association between rates of community violence and children's academic and developmental outcomes (Bowen and Bowen, 1999; Delaney-Black et al., 2002; Grogger, 1997; Harding, 2009; Hurt et al., 2001), although much of this research does not account for selection into violent environments and does not allow for strong causal inferences (Aizer, 2007). Recently, however, several studies have utilized more convincing empirical designs to identify the effect of changes in community violence and have produced stronger evidence on the negative impact on children.

Research using child or neighborhood fixed effects specifications shows that school performance or standardized test scores fall among children who live in neighborhoods where violence rises, or in years in which children feel less safe or report violent victimization (Aizer, 2007; Burdick-Will, 2013; Lacoe, 2015). Other studies have exploited exogenous variation in exposure to specific incidents or episodes of violence, such as a school shooting or a series of random sniper shootings, and found that children closer in proximity to violence exhibit more extensive symptoms of post-traumatic stress and worse performance in school (Gershenson and Tekin, 2015; Nader et al., 1990). International research from Brazil and Mexico, respectively, shows that school failure rises and student test scores decline during periods of intense violence driven by gang warfare (Caudillo and Torche, 2014; Monteiro and Rocha, 2017). Lastly, a set of studies exploiting variation in the timing of local violence has shown that when children are given assessments of cognitive skills or school-based standardized tests in the immediate aftermath of extreme local violence, their performance declines relative to other children assessed at a time when no recent violence has taken place (Sharkey, 2010; Sharkey et al., 2012; Sharkey et al., 2014).

Although the individual-level mechanisms linking violence and upward mobility have received the most empirical attention, the spatial concentration of violence means that the impact of violent crime must be thought of not only at the individual level, but also at the community and city levels. Violent crime is concentrated in pockets of urban areas that frequently are characterized by poverty, joblessness, institutional decay, and racial and ethnic segregation (Massey, 1995; Peterson and Krivo, 2010; Sampson, 2012; Sampson and Wilson, 1995; Wilson, 1987). The presence of crime, or the perception that an area is dangerous, can accelerate a process of neighborhood decline, leading to outmigration of families, disinvestment by local businesses, and a deterioration of public life and economic conditions (Cullen and Levitt, 1999; Ellen and O'Regan, 2010; Sampson, 2012; Skogan, 1986). Violence thus has the capacity to undermine the institutions that are central to the lives of young people, eroding their opportunities to obtain quality schooling, to take advantages of local employment opportunities, and to utilize social networks in order to facilitate entrance in the labor force.

In sum, this body of evidence indicates that neighborhood crime is a salient attribute of children's environment that undermines their own development and compromises their ability to obtain the education and skills necessary for economic mobility. Furthermore, the spatial concentration of crime erodes functioning communities, leading to lower quality institutions like schools, fewer jobs, and lower quality networks that facilitate economic mobility.

3. Data and analytic approach

3.1. Measures of intergenerational economic mobility

Using administrative tax records of more than 40 million children and their parents, researchers from the Equality of Opportunity project (Chetty et al., 2014a, 2014b; Chetty and Hendren, 2015) characterized a child's expected rank in the national income distribution (at age 26) given her parents' rank in the income distribution (measured when the child was approximately 16 years old). We use this metric as our measure of absolute intergenerational mobility,[1] exploiting temporal variation across seven birth cohorts (1980–1986) and geographic variation across 1,355 counties.[2] Because children's county of residence at age 16 could be de-

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[1] Chetty et al. (2014a) provide a detailed discussion of the advantages of this rank–rank measure with respect to other alternative measures such as log income-log income. The key advantage of rank-based measures is that they are less sensitive to the presence of observations with zero or very small incomes.

[2] Measures beyond the 1986 birth cohort are not available because children's income distribution was constructed when the birth cohort was 26 years old (for the

termined, the measure allows us to link children's expected rank in the income distribution as young adults to the geographic context in which they were raised.

Chetty et al. (2014a) demonstrate that the relationship between the mean income rank of children and the mean income rank of parents is almost perfectly linear within counties. Rather than having to resort to non-parametric ways to characterize this rank–rank relationship, the linearity of the association allows them to summarize it with two parameters: a slope and an intercept. The slope captures the relative mobility in the county (i.e., the difference in expected income rank between children from families at the top of the income distribution and children from families at the bottom of the distribution). The intercept represents the expected rank in the children's income distribution for children of families at the bottom of the distribution.[3]

By combining the slope and the intercept, we can estimate the expected rank in the income distribution at age 26 for children whose parents were at any given percentile in the parents' income distribution. Our main set of analyses use the slope and the intercept such that the outcome variable measures children's expected rank in their income distribution at age 26 conditional on parents being at the bottom 25th percentile of the income distribution (measured when children were 16 years old).

We choose a measure of upward mobility rather than a measure of relative mobility because of our focus on violent crime, which is disproportionately concentrated in low-income communities. If violence affects economic mobility, the impact is likely to be strongest near the bottom of the income distribution.[4] However, any measure of intergenerational mobility that is based on ranks is not a true measure of absolute mobility because it inherently means that there is displacement of someone else in the distribution. In fact, at the national level, our metric of absolute upward mobility is mechanically associated with the measure of relative mobility obtained from a rank-rank regression. However, because the measure of absolute mobility is county-specific and incomes in a given county have a negligible impact on the national income distribution, the measure provides a reasonable way to describe patterns of absolute upward income mobility across geographic areas (Chetty et al., 2014a).[5]

While our key measure of upward income mobility is the expected rank in the adult income distribution for children who grew up in families that were at the 25th percentile of the income distribution, we also examine the impact of crime rates on children from families at other points in the income distribution. The county-level data available through the Equality of Opportunity project (Chetty et al., 2014a) only include estimates of the expected rank in the adult income distribution for children growing up in families that were at the 25th and 75th percentiles of the income distribution. Given the linearity property documented by Chetty et al. (2014a) and the fact that we know two points (25th and 75th percentiles) along the line that characterizes the expected income rank at all percentiles, we can obtain the slope and intercept for such line using a straightforward algebraic calculation. We carry out this calculation separately for each county and birth cohort and obtain the expected rank at all percentiles of the parents' income distribution for all counties and birth cohorts in our sample.

3.2. Measures of violent crime

We use FBI Uniform Crime Reports (UCR) to construct the average crime rate in the county when children were between 14 and 17 years old. The National Archive of Criminal Justice Data (NACJD) keeps a county-level record of arrests and reported crimes based on law enforcement agencies' records. We use these county aggregates to compute the county crime rates for each birth cohort at ages 14–17. For example, the measure of crime at age 14–17 for the 1980 birth cohort is computed by averaging crime rates in the county over years 1994–1997. We restrict our analysis to violent crimes (homicides, aggravated assaults, and robberies).[6]

One might argue that the effects of violent crime on cognitive outcomes and educational attainment are felt at ages younger than 14 years old. In fact, quantitative and qualitative studies have shown that both the acute incidents of neighborhood violence and continued exposure to highly violent residential contexts have meaningful developmental impacts on children of younger ages (Sharkey, 2010; Harding, 2010). In models available upon request, we use measures of county-level crime rates averaged over all years when children were between 5 and 17 years old and when

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youngest cohort, 1986, this means that income was measured in 2012. Chetty and Hendren (2015) provide measures of income mobility at age 24 for birth cohorts 1980–1988. We have run all models using mobility at age 26 and at age 24, and we obtain similar results. We prefer the measure of mobility at age 26 because at that age income is likely to be more stable than at age 24, when some young adults may still be completing their education.

[3] Formally, the county-specific measure of intergenerational mobility developed by Chetty et al. (2014a) is computed as follows:

$$R_{ic} = \alpha_c + \beta_c P_{ic} + \varepsilon_{ic}.$$

where R_{ic} is the rank in the children income distribution at age 26 for child i growing up in county c, and P_{ic} is the rank in the parents' income distribution for parents of child i in county c (measured when the child was 16 years old). The estimates for parameters α_c and β_c provide the measures of absolute and relative mobility, respectively, for county c. These county-level statistics are the two measures that are made publicly available and that we exploit in this analysis. The temporal variation across birth cohorts is obtained by carrying out this estimation separately for birth cohorts 1980–1986.

[4] More generally we agree with the argument for measures of absolute mobility put forth by Chetty et al. (2014a): "increases in relative mobility could be undesirable if they are caused by worse outcomes for the rich. In contrast, increases in absolute mobility at a given income level, holding fixed absolute mobility at other income levels, unambiguously increase welfare." For an interdisciplinary review of alternative approaches to measuring intergenerational mobility see Torche (2015).

[5] In our panel with repeated measures of intergenerational mobility for different birth cohorts across counties, 84% of the variation in intergenerational mobility is between counties and 16% is within counties over time. While we acknowledge that most of the action is taking place across counties, we believe that exploiting the temporal variation within counties has the potential to yield valuable insights. One of the key findings from the most recent literature on intergenerational mobility is

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the lack of a temporal trend (Chetty et al. 2014b). Nationally, the intergenerational income elasticity has remained constant at around 0.3 for birth cohorts born between 1971 and 1993. Keeping this in mind, we argue that a story of a flat trend in the national estimate of intergenerational mobility could be consistent with a story of offsetting trends across different counties. We also argue that the temporal variation that we exploit is not an artifact of noisy measures of intergenerational mobility. When plotting trends over time for specific counties, we observe clear upward and downward trajectories that are far from being a result of noisy estimates. Further, if the temporal variation in mobility was in fact random noise or the estimates were measured with error, this would impose a challenge in the estimation and inference that would be reflected in larger confidence intervals around our point estimates.

[6] Because the reporting patterns of the different agencies vary across time and counties, some have cautioned against the use of UCR data in longitudinal analyses (Maltz and Targonski, 2002). In 1994, NACJD modified the algorithm that imputes missing data when one or more agencies fail to report their crime records. As a result, crime reports prior to 1994 are not comparable to crime reports for years 1994 and onwards. Because we start measuring crime in 1994 (when the 1980 cohort was 14 years old), our crime estimates are comparable across all years included in the analyses. Despite the consistency that the NACJD imputation algorithm provides for post-1994 data, any measure of crime will still be sensitive to measurement error issues. For example, if the missing data that were imputed came from relatively low-crime jurisdictions (relative to the rest of the county), the county-level crime rates would be upwardly biased. Conversely, if the imputed data were from relatively high-crime jurisdictions, the county-level crime rates would be biased downwards. We account for this potential source of bias by adding a coverage indicator variable that measures the proportion of agencies for which NACJD imputed data in a given year. Most importantly, the instrumental variable approach that we propose will mitigate the bias associated with measurement error in crime rates.

children were between 10 and 17 years old. Results are highly consistent with the findings presented below. We prefer measuring crime at ages 14–17 because the quality of crime data improves significantly after 1994 and because the measures of intergenerational economic mobility capture children's county of residence at age 16 (Chetty et al., 2014a). If we were to use measures of county crime rates at earlier ages we would have to make stronger assumptions about how long children have lived in the same counties.

3.3. Demographic variables

In all models, we control for time-varying characteristics of the population and the local labor market that are predictive of crime rates and of intergenerational mobility. The choice of these control variables is motivated by an extensive literature on neighborhood disadvantage and crime (Peterson and Krivo, 2010) and by the correlates of intergenerational mobility that Chetty et al. (2014a) report. The set of county-level demographic controls includes proportion of non-Hispanic African-American residents, proportion of Hispanic residents, proportion of non-Hispanic White residents, proportion of foreign-born residents, proportion of families living in poverty, proportion of residents in the labor force who are unemployed, proportion of female-headed households, proportion of residents 25 years and older without high-school diploma, and proportion of residents 25 years and older with a college degree or more. These time-varying covariates are measured contemporaneously with crime rates (i.e., averaged over the years when children were 14–17 years old).[7] Data on these socio-demographic indicators are obtained from the 1990 Census, the 2000 Census, and the 2006–2010 American Community Survey.

Among the 3,138 counties in the United States, longitudinal data on economic mobility are only available for 1,451 of them. We restrict our sample to the 1,382 counties that have measures of economic mobility for at least three birth cohorts. Among those, 47 counties have missing or unreliable crime data. The final sample includes 1,335 counties with measures of mobility and crime for at least three birth cohorts, producing a panel with 8,867 county-cohort observations.[8] The sample of counties included in our analyses was home to more than 277 million residents in 2010 (roughly 91% of the total US population living in the 49 continental states).[9]

Table 1 shows the mean and standard deviation of absolute upward mobility, crime rates, and demographic characteristics for the 1980 and 1986 birth cohorts in our sample. On average, children born in 1980 experienced a violent crime rate of 628 crimes per 100,000 residents at ages 14–17. For children born in 1986, this figure decreases to 479. Similarly, children born in 1980 experienced a murder rate of 7.67 murders per 100,000 residents at ages 14–17, while children born six years later experienced a murder rate of 5.72 during their adolescence. Relative to the 1980 birth cohort, the 1986 birth cohort experienced violent crime and murder rates that were 24% and 25% lower, respectively. In some cities that experienced more dramatic declines in violent crime during this period, like New York and Chicago, the violent crime rate for the 1986 birth cohort was more than 40% lower than the violent crime rate for the 1980 birth cohort. The standard deviations reported in Table 1 show a substantial reduction in the variability

Table 1

Descriptive statistics for 1980 and 1986 birth cohorts (N = 1,355 counties).

	Cohort 1980		Cohort 1986	
	Mean	SD	Mean	SD
Expected income rank at age 26	44.26	4.69	44.20	4.18
Violent crime rate	628.08	454.68	479.22	320.68
Murder rate	7.67	7.16	5.72	5.27
Property crime rate	4,419.69	1,850.92	3,636.43	1,591.56
% Non-Hispanic White	70.96	19.89	66.92	21.02
% Hispanic	11.88	14.43	14.03	15.53
% Non-Hispanic Black	12.08	12.24	12.49	12.66
% Foreign-born	10.54	10.28	12.32	11.13
% Poverty	12.31	5.55	12.52	5.28
% College or more	23.53	8.43	26.04	9.15
% Less than high school	21.29	7.21	18.42	6.80
% Female-headed households	8.16	2.58	8.59	2.65
% Unemployed	3.85	1.07	4.04	1.10

All figures have been computed using county population weights. Economic mobility data are from the Equality of Opportunity project (Chetty et al. 2014a, 2014b). The metric of absolute upward income mobility characterizes a child's expected rank in the national income distribution at age 26, conditioning on parents' rank in the income distribution when the child was approximately 16 years old. The figures shown in this table correspond to children born to families that were at the 25th percentile. Crime rates are in crimes per 100,000 residents. Violent crimes include aggravated assaults, robberies, and murders. Property crimes include burglaries, larcenies, and motor-vehicle thefts. The victims from the 9/11 terrorist attacks have been subtracted from the counts (proportionally to the population of each of the 5 New York city counties).

of crimes rates over time. Not only were crime rates substantially lower when the 1986 cohort reached adolescence, but fewer counties had extreme crime rates. Our analysis will exploit the spatial and temporal variation in crime rates that children experienced during adolescence to predict income mobility in adulthood.

3.4. Difference-in-differences estimation

We exploit variation in measures of economic mobility and crime rates across counties and over time for the seven birth cohorts in our data in a two-way fixed effects framework. Specifically, we regress the measure of absolute upward income mobility on the log of average violent crime rate measured at ages 14–17, demographic attributes of the county at age 14–17, a set of county fixed effects, and a set of cohort fixed effects. This two-way fixed effects specification is effectively a difference-in-differences model that exploits variation within counties in birth cohorts' exposure to violent crime, accounts for all time-invariant attributes of the counties, and controls for temporal trends that are common to all counties. The model takes the following form:

$$Y_{it} = \delta\, Crime_{it} + \boldsymbol{X}_{it}'\boldsymbol{\beta} + \boldsymbol{Z}_i'\boldsymbol{\gamma} + \boldsymbol{W}_t'\boldsymbol{\theta} + e_{it} \tag{1}$$

In Eq. (1), Y_{it} is the expected rank in the income distribution at age 26 for children from families at the 25th percentile in county i for birth cohort t; $Crime_{it}$ is the log of the average crime rate in county i for cohort t measured when children were 14–17 years old; $\boldsymbol{X}_{it}'$ is a vector of demographic covariates that vary over time within counties, as described in Section 3.3; $\boldsymbol{Z}_i'$ is a vector of dummy indicators for each county; $\boldsymbol{W}_t'$ is a vector of dummy indicators for each birth cohort; and e_{it} is an idiosyncratic error term for county i and cohort t. δ is the parameter of interest and provides an estimate of the association between violent crime and upward income mobility.[10] We estimate parameters in

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[7] We use linear interpolation to impute data between Census years.

[8] Among the 1335 counties in the final sample, 1215 have mobility data for all seven birth cohorts, 37 have mobility data for six birth cohorts, 36 have mobility data for five birth cohorts, 18 have mobility data for four birth cohorts, and 29 have mobility data for three birth cohorts.

[9] In a separate set of analyses, we estimate models at the level of commuting zones (CZ). The CZ-level estimates are in line with the county-level results. Results are available upon request.

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[10] In an alternate fixed effects specification, we replace the set of cohort fixed effects, $\boldsymbol{W}_t'$, with a set of cohort-by-region fixed effects. This specification relaxes the assumption that trends over time were the same for all counties in all Census regions. All results that we present are robust to using this alternate fixed effects specification.

Eq. (1) via Ordinary Least Squares (OLS), weighting counties by population and clustering standard errors by county. Because crime data from larger counties are more reliable, giving higher weights to more populous counties improves the precision of our estimates. Similarly, by clustering the standard errors by county we account for serial correlation in the measure of economic mobility within counties and for heteroskedasticity across clusters.

3.5. Instrumental variable estimation

The strategy described in the previous section relies on the assumption of zero correlation between crime rates and the error term in Eq. (1). Formally, to ensure that δ is an unbiased and consistent estimate of the causal effect of crime on economic mobility, the following condition must hold: $Cov[Crime_{it}, e_{it} \mid X_{it}, Z_i, W_t] = 0$. This condition will be violated in the presence of unmeasured time-varying confounders that are correlated with changes in crime rates and economic mobility. For example, if more public and private investment in neighborhood revitalization reduced violent crime and improved the economic opportunities, failing to control for this and other unmeasured time-varying characteristics of the counties will bias the estimates of the effect of violent crime on economic mobility. A special case of this kind of confounding would be the presence of reverse causality, whereby improvements in economic mobility lead to reductions in crime rates and not the other way around. Similarly, measurement error in crime rates may introduce attenuation bias in the estimation of δ.

To address the possibility that the association between within-county changes in violent crime and changes in economic mobility may be driven by unobserved, time-varying characteristics of the county or its population, we propose an instrumental variable (IV) estimation strategy that exploits the timing of grants that law enforcement agencies received under the Community Oriented Policing Service (COPS) program. The COPS program was established in 1994 as part of the Violent Crime Control and Law Enforcement Act. Through the COPS Universal Hiring Program, police departments that applied for grants received funding to cover 75% of the cost of hiring and re-hiring entry-level career law enforcement officers. By end of fiscal year 2004, the COPS program had distributed $11.3 billion in grants, with $5 billion of these funds being spent to hire 64,000 new police officers (Evans and Owens, 2007). By 2016, the COPS program had distributed approximately $14.9 billion in grants across 13,000 law enforcement agencies (Office of Community Oriented Policing Services, 2017).

Evans and Owens (2007) documented that 90% of cities with population between 25,000 and 100,000 residents and 98% of cities with population over 250,000 received COPS grants. The authors found that the addition of police officers through the COPS grants program generated statistically significant reductions in violent and property crimes. They also showed that although the total grant amount received over the 1994–2002 period was correlated with the size of the police force and crime levels in 1993, it was uncorrelated with changes in crime rates that predated the start of the COPS program. Qualitative evidence gathered by Evans and Owens through conversations with police agencies indicated that agencies faced low barriers to apply and a simple application process.[11] They go on to present robust evidence across different model specifications that there was no correlation between the timing of the receipt of the grants and prior crime trends. This feature of the COPS program makes the hiring of police officers through the COPS grants a candidate for an instrument for crime

rates in the context of the fixed effects specification that we have described previously.

The instrument is constructed by averaging the number of officers per capita[12] hired through the COPS grants when children were 13–15 years old, thus allowing for a lagged effect of the COPS grants on crime rates.[13] We carry out the instrumental variable estimation using the following system of equations:

$$Crime_{it} = \pi_1 \, COPS_{it} + X'_{it}\beta + Z'_i\gamma + W'_t\theta + \eta_{it} \qquad (2.1)$$

$$Y_{it} = \pi_2 \, COPS_{it} + X'_{it}\beta + Z'_i\gamma + W'_t\theta + e_{it} \qquad (2.2)$$

In Eq. (2.1), the first stage, $Crime_{it}$ is the log of the average crime rate in county i for birth cohort t measured when children were 14–17 years old; $COPS_{it}$ is the log of the number of police officers per capita hired through the COPS program in county i for birth cohort t, measured when the birth cohort was 13–15 years old. In Eq. (2.2), the reduced form, Y_{it} is the expected rank in the income distribution at age 26 for children from families at the 25th percentile in county i for birth cohort t. X'_{it}, Z'_i, and W'_t have the same interpretation than in Eq. (1). The instrumental variable estimate of crime on economic mobility, δ_{IV}, is computed by dividing the estimated coefficient on COPS from the reduced form over estimated coefficient on COPS from the first stage ($\delta_{IV} = \pi_2/\pi_1$). Both equations include population weights and clustered standard errors by county.

Two conditions must hold in the instrumental variable estimation for δ_{IV} to recover the causal effect of crime on economic mobility. First, the instrument must induce a change in the endogenous variable in the direction that theory predicts. In our case, crime rates should have declined more rapidly in counties that hired more police officers through the COPS grants. This is one of the main findings in Evans and Owens (2007), who report relatively larger declines in crime rates in jurisdictions that hired more police officers through the grants. To validate this assumption in our study, we report the results of the first-stage relationship in the sample of counties that feature in our analyses.

In a supplemental analysis shown in Fig. A1 in the Online Appendix, we investigate the first-stage relationship in more depth by adding lags and leads to the COPS instrument. We find that the hiring of police officers through the COPS program only leads to significant reductions in crime rates when the hiring predates the age at which we measure birth cohorts' exposure to violent crime. The results in Fig. A1, which are explained in more depth in the Online Appendix, provide the basis for our decision to use the average number of police officers per capita hired through the COPS grant over ages 13–15 as our instrument.

Second, the exclusion restriction requires the instrument to be independent of the potential outcomes, conditional on the covariates included in the model.[14] This assumption can be divided in two parts (Angrist and Pischke, 2008): first, the instrument is as

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[11] Evans and Owens (2007) note that the applications that law enforcement agencies submitted were often shorter than 300 words and that virtually every agency that submitted an application received a grant.

[12] Evans and Owens (2007) show that police agencies were able to retain the police officers that were hired through the COPS grants from year to year. Given this finding, our instrument is a running sum of the number of officers hired up to a given year. Although this mechanically increases the number of police officers over time, we argue that this is an accurate way of constructing the instrument because it captures the actual number of sworn officers in duty in a given year. In a separate set of models, we instrument crime rates at ages 14–17 with just the average number of new police officers hired through the grants at ages 13–15. We find the same results both in the first and second stages of the IV models (results are available upon request).

[13] Results are robust to alternate specifications that average the number of police officers hired through the grants over ages 13–14, 14–15, and 15–16.

[14] Including the set of covariates X'_{it} is not necessary to guarantee the validity of the exclusion restriction. If we exclude these controls from our model, we obtain similar point estimates (although less precisely estimated). The purpose of controlling for changes in other attributes of the county is to obtain a stronger first stage by reducing residual variation in crime rates.

good as randomly assigned, and second, the instrument has no effect on economic mobility other than through its effect on crime rates. The first part of this assumption implies that the number of police officers hired through the COPS grants should be orthogonal to prior trends in crime and prior trends in economic mobility. Orthogonality with respect to prior trends in crime is one of the findings in Evans and Owens (2007), who show that the grants were not systematically distributed to law enforcement agencies operating in cities where crime rates had started to change. Orthogonality with respect to prior trends in economic mobility requires that the grants did not systematically target counties where economic mobility was already changing.

Although we don't have data on economic mobility prior to the years that our study examines, we can test whether the receipt of COPS grants correlates with prior trends in economic conditions in the county such as median and mean income, poverty rate, unemployment rate, and share of college educated residents.[15] As we show in Table A1 in the Online Appendix, we find no meaningful association between changes in these economic conditions and the number of police officers per capita that were hired through the COPS program a decade later. While not conclusive, these results are consistent with Evans and Owens (2007) and provide additional evidence that the COPS grants did not systematically target counties based on either recent changes in crime or improving or declining economic conditions.

The second part of the exclusion restriction requires that the number of police officers per capita hired through the COPS grants should affect economic mobility only through its effect on crime rates. While the validity of this assumption cannot be tested, we can provide circumstantial evidence showing that the effect of the COPS grants on economic mobility was zero in samples for which the receipt of the COPS grants was unrelated to changes in crime rates. In Fig. A2 in the Online Appendix, we conduct a placebo test in which we examine the impact of the COPS grants on economic mobility for birth cohorts that were younger and older than 16 when more police officers were added through the COPS grants program. Results from this test, which is described in more depth in the Online Appendix, suggest that the instrument did not impact economic mobility among birth cohorts that were too old to experience the change in crime rates induced by the instrument. While not conclusive, the results from this exercise provide indirect support for the exclusion restriction.[16]

If the valid first stage and exclusion restriction conditions are met, our instrumental variable strategy will identify the local average treatment effect (LATE) of violent crime and murder rates on economic mobility.[17] We distinguish δ from δ_{IV} to emphasize the notion that the LATE may differ from the average treatment

Fig. 1. Cross-sectional association between upward economic mobility and violent crime rate (1980–1986 cohorts).

Fig. 2. Cross-sectional association between upward economic mobility and murder rate (1980–1986 cohorts).

effect for the entire population (Angrist et al., 1996). Differences in the point estimates of δ and δ_{IV} may arise because the IV strategy helps address the omitted variable and measurement error problems, and because the IV strategy identifies the causal effect only for the set of counties for which the instrument induces a change in crime rates. Depending on where in the distribution of possible casual effects the LATE falls, the difference between δ and δ_{IV} may be substantial. This distinction should be kept in mind when comparing the regression estimates from the OLS and IV models.[18]

4. Results

4.1. OLS evidence on the relationship between crime and economic mobility

To motivate the analyses of within-county changes in crime and economic mobility, Figs. 1 and 2 show the cross-sectional association between upward mobility and violent crime and murder rates, respectively. To produce these figures, we pool repeated

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[15] We thank a reviewer for this suggestion.

[16] A similar placebo test is proposed by Angrist and Pischke (2008, pp. 131) in the context of Angrist's (1990) analysis of the effect of veteran status on civilian earnings that uses the Vietnam draft lottery as an instrument for veteran status. Angrist and Pischke suggest that one indirect way to test the validity of the exclusion restriction in Angrist's instrumental variable design is by looking at the impact of the Vietnam draft lottery that took place in 1970 on 1969 earnings. Because 1969 earnings predate the time when the instrument induces a change in the probability of serving in the military, we should not expect any association between the instrument and earnings. They show that this is indeed the case in Angrist's data. In another variation of this test, they propose looking at the impact of the draft lottery on earnings for birth cohorts that were too young to be drafted by the time the draft had officially ended. Again, because the instrument can only impact earnings in samples where a change in the probability of serving has been induced, we should not expect any relationship between the instrument and earnings when the first-stage relationship is zero. They show that this also holds in Angrist's study.

[17] In the interest of brevity, we omit a detailed discussion of the monotonicity assumption. We argue that it is implausible to believe that a county would experience a decline in crime rates when less police officers are hired but an increase in crime rates if more police officers were added instead. In other words, we assume that there are not any "defiers" in our sample.
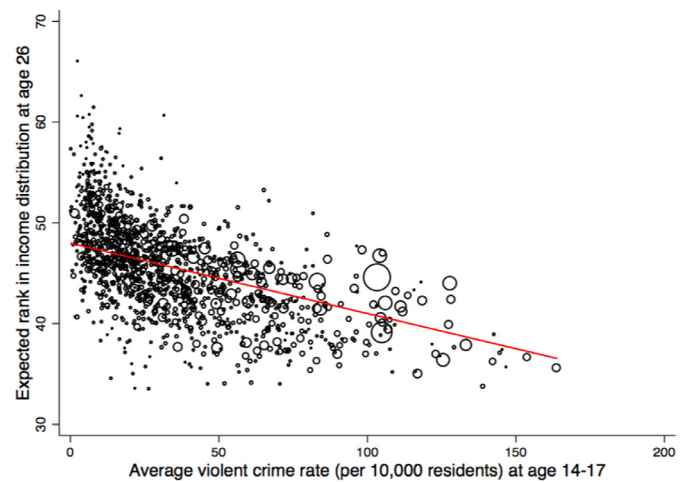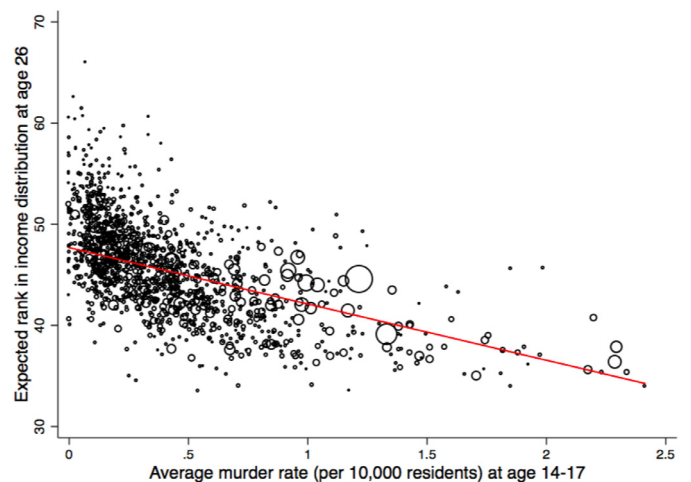
[18] In the presence of heterogenous treatment effects, δ_{IV} recovers a local average treatment effect that is specific to the instrument being used. Technically, the notation for δ_{IV} should allow for this heterogeneity of effects. However, for simplicity, we do not reflect this in our notation.

cross-sections from the seven birth cohorts and compute the average violent crime and murder rates averaged over ages 14–17 and the average expected rank in the income distribution at age 26 for children who started at the 25th percentile in the parents' income distribution.

The population-weighted scatterplots reveal a strong bivariate relationship between both measures of violence and upward mobility, consistent with the findings presented in Chetty et al. (2014a). In a regression of mean expected rank on log mean violent crime rate, the coefficient on log mean violent crime is −3.08 ($R^2 = 0.35$). This means that children growing up in a county with violent crime rate one standard deviation above the national mean exhibit an expected rank in the income distribution at age 26 that is 2.55 points lower (0.59 standard deviations of the cross-sectional distribution of expected income rank). Similarly, a regression of mean expected rank on log mean murder rate yields a coefficient of −9.41 on log murder rate ($R^2 = 0.40$). In standard deviation units, this represents that children growing up in a county with murder rate one standard deviation above the national mean exhibit an expected rank in the income distribution at age 26 that is 2.73 points lower (0.63 standard deviations of the cross-sectional distribution of expected income rank). Both of these correlations hold when we add a rich set of controls that account for socioeconomic, labor market, and schooling conditions in the county.[19]

Although they show a strong bivariate association, the patterns shown in Figs. 1 and 2 may be driven by attributes of the counties that we are unable to measure in our data. The next set of analyses use data on birth cohorts from 1980 to 1986 to assess whether changes in rates of violent crimes and murders across successive birth cohorts, as experienced over the period of late adolescence from age 14–17, are associated with changes in upward economic mobility. Although only seven birth cohorts are available to study income mobility, individuals in these seven birth cohorts lived through a period when the level of violent crime was changing rapidly. In our sample of 1,355 counties, there were, on average, 628 violent crimes for every 100,000 Americans when the 1980 birth cohort was 14–17 years old, and 479 violent crimes per 100,000 Americans when the 1986 birth cohort was the same age. Within some urban counties there were declines in violent crime that were substantially larger.

The OLS results in Column 1 of Table 2 show no association between change in violent crime within counties and change in upward economic mobility. The slope of this relationship is weakly negative and not close to statistically significant. There is, however, a strong, statistically significant relationship between changes in upward economic mobility and changes in the murder rate within a county. From Column 2, we find that a one percent increase in the murder rate is associated with a 0.02 point reduction in the expected income rank at age 26 after controlling for observed time-varying characteristics of counties. A one standard deviation increase in the murder rate experienced in the county from ages

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Table 2

OLS estimates of the effect of crime on upward economic mobility at the 25th percentile.

	Expected rank at age 26	
	(1)	(2)
Log violent crime rate	-0.039	
	(0.156)	
Log murder rate		-1.709***
		(0.421)
% Non-Hispanic White	0.844***	0.871***
	(0.189)	(0.190)
% Hispanic	0.318*	0.337**
	(0.172)	(0.169)
% Non-Hispanic Black	0.892***	0.948***
	(0.206)	(0.206)
% Foreign-born	0.621***	0.612***
	(0.199)	(0.207)
% Poverty	-0.368***	-0.360***
	(0.111)	(0.109)
% Less than high school	-0.071	-0.075
	(0.086)	(0.085)
% College or more	-0.242**	-0.218*
	(0.112)	(0.111)
% Female-headed households	0.990***	0.960***
	(0.369)	(0.363)
% Unemployed	-0.477	-0.518
	(0.332)	(0.324)
Adjusted R^2	0.913	0.914
County fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Observations	8,867	8,867

Standard errors clustered by county in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. All models include county population weights.

14–17 is associated with a 0.23 point decline in expected rank in adulthood (a 0.10 standard deviation decline in the expected income rank).

4.2. Instrumental variable evidence on the relationship between crime and economic mobility

To push the analysis further, we use the receipt of COPS grants as an instrument for changes in violent crime and homicide rates. We report estimates from the first and second stages in Table 3. Columns 1 and 3 show that a 10% increase in the number of police officers per capita hired through the COPS grants (measured when the birth cohort was 13–15 years old) led to a 1.54% decrease in violent crime rate and to a 0.7% decrease in murder rate. A Wald test on the excluded instrument in each of these first stage regressions yields a F-statistic of 12.53 in the violent crime regression and 13.31 in the murder regression. These first-stage results suggest a strong correlation between the instrument and the endogenous variable.

The second stage estimates shown in Columns 2 and 4 indicate statistically and economically significant impacts of changes in crime rates on changes in economic mobility. The measure of economic mobility is the expected rank in the income distribution at age 26 for children who started in the 25th percentile of the parents' income distribution. We find that a 10% increase in the violent crime rate leads to a reduction of 0.5 points in the expected income rank at age 26. Based on this estimate, a one standard deviation increase in the violent crime rate in the county reduces the expected income rank in adulthood by 1.79 points (or 0.73 standard deviations of the distribution of within-county changes in economic mobility). Similarly, a 10% increase in the murder rate leads to a reduction of 1.2 points in expected income rank at age 26. Based on this estimate, a one standard deviation increase in the murder rate in the county reduces the expected income rank

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[19] We generate these additional controls from the Census and from the ancillary data in Chetty et al. (2014a). The cross-section regression with the largest set of controls includes the following: share of non-Hispanic African-American residents, share of Hispanic residents, share of non-Hispanic White residents, share of foreign-born residents, share of families living in poverty, share of residents in the labor force who are unemployed, share of female-headed households, share of residents 25 years and older without high-school diploma, share of residents 25 years and older with a college degree or more, share of employed residents working in manufacturing jobs, Theil index of racial segregation, Theil index of income segregation, Gini index of income inequality, local tax rate, tax progressivity (i.e., difference between the top state income tax rate and the state income tax rate for individuals with taxable income of $20,000 in 2008), mean state EITC top-up rate between 1980 and 2001, average expenditures per student in public schools (computed from the NCES Common Core Data from 1996 to 1997 Financial Survey), and share of workers with commute longer than 15 min.

Table 3
IV estimates of the effect of crime on upward economic mobility at the 25th percentile.

	1st stage Log violent (1)	2nd stage Expected rank (2)	1st stage Log murder (3)	2nd stage Expected rank (4)
Log violent crime rate		-5.493*** (2.117)		
Log murder rate				-12.137*** (4.040)
Log COPS per capita	-0.154*** (0.047)		-0.070*** (0.021)	
% Non-Hispanic White	0.053*** (0.018)	1.057*** (0.251)	0.023** (0.009)	1.049*** (0.251)
% Hispanic	0.020 (0.018)	0.467** (0.200)	0.008 (0.009)	0.457** (0.183)
% Non-Hispanic Black	0.042** (0.018)	1.111*** (0.248)	0.035*** (0.011)	1.303*** (0.303)
% Foreign-born	0.036 (0.024)	0.684** (0.267)	0.006 (0.013)	0.561** (0.277)
% Poverty	-0.024 (0.017)	-0.550*** (0.149)	0.008 (0.009)	-0.318** (0.134)
% Less than HS	-0.028** (0.012)	-0.222* (0.116)	-0.003 (0.006)	-0.102 (0.100)
% College or more	-0.008 (0.014)	-0.269** (0.133)	0.012** (0.006)	-0.074 (0.126)
% Female-headed HH	0.123** (0.051)	1.449*** (0.463)	0.002 (0.025)	0.803** (0.408)
% Unemployed	-0.029 (0.038)	-0.614 (0.380)	-0.026* (0.015)	-0.776** (0.319)
Adjusted R^2	0.965		0.960	
County fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
F-test 1st stage	12.53		13.31	
Observations	8,867	8,867	8,867	8,867

Standard errors clustered by county in parentheses $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. All models include county population weights.

in adulthood by 1.57 points (or 0.64 standard deviations of the distribution of within-county changes in economic mobility).

In an effort to evaluate the robustness of the estimates shown in Table 3, we estimate another set of IV models using a different instrument that exploits the timing of the crack cocaine epidemic in the state where children were raised. Using an index constructed by Fryer et al. (2013) that captures the severity of the crack epidemic, we generate a dummy indicator for whether the crack cocaine epidemic reached its peak in the state when children of a given birth cohort were between 14 and 17 years old. The assumptions made are described in more detail in the Online Appendix, along with model results.

Results using the timing of the crack epidemic as an instrument show that a 10% increase in the violent crime rate leads to a reduction of 0.7 points in the expected income rank at age 26, and a 10% increase in the murder rate leads to a reduction of 1.6 points in the expected income rank at age 26. Although we consider the timing of the crack epidemic to be a weaker instrument than the timing of COPS grants in terms of its validity, the consistency of results using both a "positive shock" to violent crime (from the crack epidemic) and a "negative shock" to violent crime (from the receipt of COPS grants) is reassuring.

4.3. Income mobility at the top of the distribution

Our results indicate a negative effect of violent crime on mobility for children of the most disadvantaged families in the nation. This is consistent with prior evidence that links the spatial concentration of violence and disadvantage with the life chances of children growing up in these environments. In this section, we examine whether the association between crime and mobility also holds for children from families near the top of the income distribution. Although the decline of violence has been experienced in

both poor and non-poor communities, the evidence available indicates that the largest declines have occurred in the poorest urban neighborhoods and the strongest impacts of the decline of violence are likely to be felt by young people closer to the bottom of the income distribution (Friedson and Sharkey, 2015).

We take advantage of the fact that the relationship between mean child income ranks and parent income ranks is linear within counties (Chetty et al., 2014a), and we compute the expected rank in the income distribution for children who grew up in families at other percentiles of the income distribution. In Table 4 we show IV estimates of the impact of crime on expected rank in the adult income distribution for children who grew up in families that were at the 1st, 75th, and 100th percentiles of the income distribution. For comparison, we also report estimates from our main results at the 25th percentile. Columns 1 and 2 show stronger effects of violent crime and murder on economic mobility among children who were raised at the lowest percentile of the income distribution. For children starting from the 1st percentile of the income distribution, a 10% increase in the violent crime rate leads to a reduction of 0.7 points in the expected income rank at age 26, and that a 10% increase in the murder rate leads to a reduction of 1.6 points in the expected income rank at age 26. Conversely, estimates at the 75th and 100th percentiles (Columns 5–8) show weaker and non-statistically significant relationships between violence and upward economic mobility for children whose parents were close to the top of the income distribution.

All results presented so far have looked at the impact of violent crime on a measure of income mobility that approximates absolute income mobility.[20] To unpack what may be driving differences in the impact of crime along the income distribution from

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[20] See Chetty et al. (2014a) and our discussion of the economic mobility data for an explanation of how the expected rank approximates absolute income mobility.

Table 4
IV estimates at different percentiles of the income distribution.

	Effects at 1st pctile.		Effects at 25th pctile.		Effects at 75th pctile.		Effects at 100th pctile.		Effects on relative immobility	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log violent crime rate	-6.905*** (2.647)		-5.493*** (2.117)		-2.669 (1.644)		-1.257 (1.855)		5.648* (2.949)	
Log murder rate		-15.256*** (4.747)		-12.137*** (4.040)		-5.898 (3.754)		-2.778 (4.252)		12.478** (5.214)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,867	8,867	8,867	8,867	8,867	8,867	8,867	8,867	8,867	8,867

Standard errors clustered by county in parentheses $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. All models include county population weights and the same set of controls than those in Table 3.

which children start, we examine the impact of changes in crime rates on changes in relative income mobility. As opposed to the expected rank, relative mobility is not conditional on the percentile from which children start. It is instead a feature that is common to everyone in the same birth cohort and county. Lower levels of relative mobility characterize a more rigid process in which the income rank from which children start is more deterministic of the income rank that they will achieve as adults. In Table 4 we have labeled columns 9 and 10 "relative immobility" to make clear that higher values reflect less relative mobility. Our IV estimates in Columns 9 and 10 in Table 4 indicate a positive effect of violent crime on relative immobility; in other words, relative mobility increased in counties that became less violent. Another way to interpret relative mobility in the Chetty et al. (2014a) is as a measure of the gap in the expected income rank as adults between children starting from the 1st and 100th percentiles. Therefore, results in Columns 9 and 10 can also be read as showing that falling crime rates led to a narrowing of the gap between the poorest and the wealthiest children in the county.

4.4. Educational attainment as a potential mechanism

Results presented to this point show a strong negative effect of violent crime on income mobility of children whose families began toward the bottom of the income distribution. In this section, we present suggestive evidence to better understand why violence affects the prospects for economic mobility, focusing our attention on educational attainment as a potential mechanism. We examine two measures of educational attainment: changes in the college attendance rate using the Chetty et al. (2014a) data and changes in the high school dropout rate using data from the Census and the National Center for Education Statistics.

We use data on college attendance rates, made publicly available by Chetty et al. (2014a), to examine whether cohorts that experienced lower crime rates during adolescence were more likely to attend college. In addition to characterizing a child's expected rank in the national income distribution at age 26, Chetty and colleagues predicted college attendance rates at age 18–21 for children whose parents were at the 25th percentile of the national family income distribution. We use variation over time across birth cohorts (1980–1986) and across counties. Using the same two-way fixed effects approach and the COPS instrument, we examine whether changes in violent crime within counties lead to changes in the probability of attending college by age 18–21. As before, exposure to crime is measured at ages 14–17.

Results from the analyses of college attendance using the COPS instrument are shown in Table 5. Contrary to expectations, we do not find a negative association between changes in violent crime and changes in college attendance rates. The coefficient is positive although imprecisely estimated, preventing us from drawing

Table 5
IV estimates of effect of crime on college attendance at 25th percentile.

	(1) College attendance	(2) College attendance
Log violent crime rate	7.180 (4.546)	
Log murder rate		13.419 (8.937)
County fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Demographic controls	Yes	Yes
Observations	8,867	8,867

Standard errors clustered by county in parentheses $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. All models include county population weights and the same set of controls than those in Table 3.

a definitive conclusion about the relationship between the drop in violence and changes in college attendance.[21] The absence of a negative effect of crime on college attendance could mean that the children who benefited most from the drop in violence are not candidates for college enrollment. Recall that children raised in families at the 75th and 100th percentiles of the income distribution did not experience significant improvements in income mobility as a result of changes in violent crime.

In the last set of analyses, we focus our attention on an alternative measure of educational attainment that is arguably more relevant for young people originating at the bottom of the income distribution: high school dropout. We measure changes in high school dropout rates using two data sources. The first source is the Local Education Agency Universe Survey Dropout and Completion Data from the National Center for Education Statistics (NCES). These data include the number of dropouts, high school diploma recipients, and other high school completers for all local education agencies (school districts) reporting these data in agreement with NCES. We take NCES's estimates of dropout rates for grades 9 to 12 from each school district and assign districts to counties based on the ZIP code of the district's mailing address listed in the Common Core of Data (CCD) files. We average dropout rates over all districts in each county and compute the change in the average

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[21] Separate results using the timing of the crack epidemic show a negative relationship between violent crime and college attendance, but the estimates are extremely imprecise and are not close to statistically significant. We suspect that college completion rates might be an equally interesting measure to consider, given the fact that attendance rates have been rising over time while completion rates have been falling (Bound et al., 2010).

Table 6
OLS estimates of the association between changes in crime rates and high school dropout rates.

	NCES (1997-2009)		Census (1990-2010)	
	(1)	(2)	(3)	(4)
Δ Log violent crime rate	0.264***		0.517**	
	(0.090)		(0.205)	
Δ Log murder rate		0.542***		0.868***
		(0.157)		(0.235)
Δ Non-Hispanic White	0.197	0.183	0.281***	0.270***
	(0.164)	(0.161)	(0.106)	(0.103)
Δ Hispanic	0.165	0.144	-0.134**	-0.145**
	(0.119)	(0.117)	(0.068)	(0.065)
Δ Non-Hispanic Black	0.265*	0.236*	0.322***	0.296***
	(0.147)	(0.144)	(0.115)	(0.110)
Δ Foreign-born	0.259	0.244	0.264*	0.276*
	(0.265)	(0.264)	(0.149)	(0.149)
Δ Poverty rate	-0.072	-0.066	0.072*	0.058
	(0.064)	(0.063)	(0.039)	(0.037)
Δ College or more	0.117	0.107	-0.030	-0.051*
	(0.073)	(0.072)	(0.030)	(0.030)
Δ Less than high school	0.008	0.007	0.224***	0.213***
	(0.068)	(0.068)	(0.033)	(0.034)
Δ Female-headed households	-0.079	-0.061	0.070	0.076
	(0.159)	(0.161)	(0.100)	(0.098)
Δ Unemployment	-0.272***	-0.256***	-0.519***	-0.526***
	(0.093)	(0.092)	(0.087)	(0.085)
Constant	-1.090*	-1.033*	0.128	0.292
	(0.593)	(0.587)	(0.542)	(0.521)
Observations	2,154	2,154	2,983	2,983
Adjusted R^2	0.041	0.042	0.255	0.263

Heteroskedasticity-robust standard errors in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. All models include county population weights. Models in Columns (1) and (2) use data on high-school dropouts from the NCES Local Education Agency Universe Survey Dropout and Completion Data from years 1997 and 2009. Models in Columns (3) and (4) use data on high-school dropouts from the 1990 Census and the 2006-2010 American Community Survey.

dropout rate in the county from 1997 to 2009.[22] The second source is the 1990 Census and the 2006–2010 American Community Survey, from which we measure the change from 1990 to 2010 in the percentage of adolescents between 16 and 19 years old who were not enrolled in school and were not high school graduates.

In Table 6, we examine how changes in violent crime and murder rates correlate with changes in the high school dropout rate between 1990 and 2010. Columns 1 and 2 show results using the NCES estimates of dropout rates, and Columns 3 and 4 show results using Census and ACS estimates. In all models, we control for other demographic changes taking place in the county during that period. Because data on high school dropouts, crime, and demographics are available for almost all counties in the nation, the analyses of high school dropout using NCES and Census data include 2,154 counties and 2,983 counties, respectively. In models available upon request, we find similar results when we restrict the sample to the 1,355 counties that we use in the economic mobility analyses. We find that a 10% increase in the violent crime rate is associated with a 0.03 percentage point increase in the high school dropout rate when using the NCES estimates and a 0.05 percentage point increase in the high school dropout rate when using the Census estimates. Similarly, a 10% increase in the murder rate is associated with a 0.05 percentage point increase in the high school dropout rate when using the NCES estimates and a 0.09 percentage point increase in the high school dropout rate when using the Census estimates. These effect sizes are modest and likely suffer from attenuation bias due to measurement error in the crime data. Nonetheless, these estimates are in line

with prior findings on the impact of crime on high school graduation rates (Evans et al., 2012), and they suggest that one potential mechanism through which declining crime rates led to improvements in economic mobility is by reducing the likelihood of dropping out from high school.[23]

5. Conclusion

Recent research documenting variation in economic mobility within the United States opens up the possibility for major advances in understanding the mechanisms for upward mobility. Although preliminary evidence points to specific features of places that are correlated with upward mobility—such as residential segregation, social capital, and family structure—minimal progress has been made in identifying the causal effects of these different dimensions of places on upward mobility. This article represents an attempt to push the literature forward by investigating one particularly salient dimension of urban areas, the level of violent crime, and attempting to determine whether the association between violent crime and upward mobility is causal.

Our focus on violent crime is motivated by several different strands of evidence suggesting that community violence has damaging effects on children's academic and developmental trajectories and is a central reason why growing up in disadvantaged residential environments has such substantial effects on the life chances of children (Sharkey and Sampson, 2015). An extensive literature documents the way that violent crime undermines community life, leading to disinvestment from residents, public officials, and potential businesses (Klinenberg, 1995; Skogan, 1986). In areas where public spaces are seen as dangerous, economic activity declines and economic opportunities become less prevalent (Wilson, 1996). Thus, the effects of community violence are diverse and operate at multiple levels. Violence undermines children's developmental trajectories, but also undermines public life and economic activity across entire communities and cities.

Despite the extensive theory and evidence that we have reviewed, the literature on community violence has not considered whether the impact of violence as experienced in childhood extends further into early adulthood, altering the economic trajectories of individuals raised in violent environments. This article attempts to fill this gap in the literature. We examine the relationship between violent crime and upward economic mobility by exploiting variation in violent crime within counties over time, and variation in violent crime arising from exogenous shocks driven by the timing of grants from the federal COPS program.

Our preferred specifications, which are based on within-county change using plausibly exogenous changes in violent crime, indicate that a one standard deviation decline in violent crime as experienced during late adolescence increases the expected income rank in adulthood by roughly 2 points, and a one standard deviation decline in the murder rate increases the expected income rank

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[22] To stabilize the impact of outliers, the 1997 dropout rate is the average of 1997, 1998, and 1999. Similarly, the 2009 dropout rate is the average of 2007, 2008, and 2009.

[23] In a set of models available upon request, we estimate the effect of changes in violent crime on changes in the high school dropout rate using the intensity of the crack epidemic as an instrument for crime and controlling for the same demographic factors than those listed in Table 6. We find a strong positive relationship between changes in crime and changes in the high school dropout rate. One difference with respect to the previous analyses of economic mobility is that these models exploit the change in the severity of the crack epidemic, rather than its timing. The key assumption underlying this approach is that changes in the severity of the crack epidemic affected the high school dropout rate only through its effect on changes in the crime rates. Although there are certainly a number of unmeasured factors that our strategy fails to account for, we believe that the covariates included in our models block other backdoor pathways through which changes in the severity of the crack cocaine epidemic could have affected changes in the dropout rate. Nonetheless, we recognize that this IV strategy relies on strong assumptions. We consider this analysis an attempt to generate additional suggestive evidence on one plausible pathway for our main results, rather than a definitive causal analysis.

by roughly 1.5 points. These effect sizes are statistically significant and substantively meaningful. A 2-point increase in the expected income rank in adulthood represents, for example, the difference between being raised in Denver, CO (expected rank = 41.7) versus being raised in Chicago, IL (expected rank = 39.5).

Although this basic conclusion is straightforward, interpreting the mechanisms linking violence with upward mobility is more complicated. As expected, we find that the impact of violence is greater for young people beginning near the bottom of the income distribution rather than the top. We have provided only tentative evidence on the mechanisms explaining the link between violence and economic mobility, however. Exploratory evidence suggests that violent crime may increase the prevalence of high school dropout, providing one potential mechanism for impacts on upward economic mobility. We find no evidence that changes in violent crime affected county-level rates of college attendance.

The core findings from the article take on added importance when one considers the decline in violence that has occurred in the United States since the early 1990s. Since 1991, when the national homicide rate reached its latest peak, both the rate of homicides and all violent crimes have been cut roughly in half. Violent victimization has dropped for every racial and ethnic group, at all levels of income (Bureau of Justice Statistics, 2015), and the available data indicate that the cities and neighborhoods where violence was most severe in the 1990s have experienced the greatest changes since then (Friedson and Sharkey, 2015). In many urban areas where crime has declined, being poor no longer means living in an intensely violent residential environment. Despite this decline, violent crime remains highly concentrated in predominantly African-American and Hispanic neighborhoods (Sampson, 2012). Future research should examine whether the effects that we document here vary across children of different racial and ethnic backgrounds.

Considering all of the evidence showing the impact of community violence on children, it is natural to ask whether the decline of violence has changed the economic trajectories of children who lived through what Zimring (2006) calls "The Great American Crime Decline". Our analysis does not cover the entire period during which crime has fallen, but it does include birth cohorts that experienced substantial changes in exposure to violence. The 1980 birth cohort reached adolescence at an extremely violent time in the nation's history, while the 1986 cohort reached adolescence at a time when violence had begun to decline quickly.

Results from the analysis indicate that these differences in the experiences of successive birth cohorts have led to meaningful differences in upward mobility. Children raised in families at the 25th percentile who reached the period of late adolescence during a more peaceful time could expect to move upward in the income distribution relative to other children, from the same counties, who lived through a more violent period. The drop in violent crime is an important trend in its own right, but the findings in this article suggest that this trend has also substantially improved the economic outcomes of children beginning near the bottom of the income distribution.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2017.07.001.

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