

The Birth Lottery of History: Arrest over the Life Course of Multiple Cohorts Coming of Age, 1995–2018¹

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This article advances and tests hypotheses on arrest in the lives of 1,057 individuals from an original longitudinal study of multiple birth cohorts who came of age during a period of considerable social change in the last quarter-century. The authors show that large cohort differences in the course of arrest arise from changing macrohistorical environments rather than dispositional, demographic, socioeconomic, or neighborhood differences in childhood. Further, the impact of two leading explanations of crime—socioeconomic disadvantage and low self-control—depends on the historical timing of when children reach late adolescence and early adulthood. Cohort fortunes diverge mainly as a result of when both crime rates and police enforcement—especially for drug offenses—unexpectedly fell. The results quantify the power of social change and contribute a new understanding of inter- and intracohort inequalities in growing up during the era of mass incarceration and the great American crime decline.

Social change is one of the great questions of sociology, and the concept of cohort is foundational to its study. Nearly a century ago, Mannheim ([1928]

¹ We are grateful for the comments of many colleagues, but special thanks go to Stephen Raudenbush, Christopher Winship, Alix Winter, John Laub, Orlando Patterson, Bruce Western, and the *AJS* reviewers for their generous comments or assistance on this project. Financial support from a John Simon Guggenheim Foundation Fellowship is gratefully acknowledged. The authors contributed equally to this article. Direct correspondence to Robert J. Sampson, Department of Sociology, Harvard University, Cambridge, Massachusetts 02138. Email: rsampson@wjh.harvard.edu

1952) focused on how the shared sociohistorical environment of young people gave rise to a distinct generational experience with lasting influence. Ryder (1965) later articulated what has been called the “principle of cohort differences in aging” (Riley 1987, p. 4). As we age, our individual experiences are inevitably bound up with social change, and when that change is substantial, it distinguishes the life experiences of different age cohorts.

These insights inspired some of sociology’s classic works, such as Glen Elder Jr.’s (1974) *Children of the Great Depression: Social Change in Life Experience*, which showed how historical change has the power to reshape the life experiences of entire cohorts. The effects of the Depression on socialization were differentially experienced by children born just eight years apart, for example, and divorce rates were higher for men who entered World War II later in life compared to veterans who were younger at the time of service (Pavalko and Elder 1990). Scholars have also examined how trajectories of health, political attitudes, and work interact with large-scale economic transitions and cultural change (e.g., Alwin, Cohen, and Newcomb 1991; Zhou and Hou 1999; Chen, Yang, and Liu 2010). But as Elder and George (2016, p. 65) observe, despite a rich theoretical tradition, studies that follow multiple birth cohorts over time to compare how social change differentiates their life experiences are relatively rare. Rarer still are prospective studies with the rich measurement needed to assess whether diverging cohort fortunes reflect similar people coming of age in a changed world as opposed to compositional or early life differences between cohorts.

In this article, we study inter- and intracohort variations in becoming arrested as individuals came of age during some of the largest social changes of recent times—the rise of mass incarceration and proactive policing (Travis, Western, and Redburn 2014) and what has been labeled the “great American crime decline” (Zimring 2006). These substantial changes have been surprisingly neglected in the study of age, crime, and the life course, due to both data and theoretical limitations. Indeed, a large and influential literature examines age, criminal behavior, and criminal justice contact, but few studies prioritize the sociohistorical contexts through which individuals from different cohorts age or account for competing hypotheses on what produces cohort differentiation. We therefore advance a theoretical framework, statistical model, and original longitudinal data to redirect our focus to how shared historical circumstance imprints its effects on the criminalization of multiple birth cohorts growing up in a changing social world and how key individual and socioeconomic characteristics interact with historical change.

We accomplish our goals by analyzing longitudinal data from a multi-cohort study that originated in Chicago. One cohort is based on a representative sample of children born around the study’s start in 1995, on whom data were collected through childhood and into adolescence. We pair this cohort with data on three cohorts from the same study who were born in the

1980s—ages 9, 12, and 15 at the study's start and again representative of their respective ages—who were studied into their late 20s and early 30s. The information collected allows us to dig deep into the characteristics of individuals, their families, and early life neighborhood conditions, dimensions along which the cohorts differ. In addition, criminal history records were collected for all participants through 2018, allowing a study of arrest over more than a 20-year span, from childhood through emerging adulthood and beyond. Integrating these long-term data with macrosocial trends, notably in crime rates and criminal justice enforcement, we examine the mechanisms shaping how each cohort aged through a distinct historical era at the prime ages for arrest and during the eventual decline in arrests.

The results from three sets of analyses demonstrate how social change has altered criminal justice contact in adolescence and early adulthood, including how social change interacts with two of the leading causes of crime put forth in prior research. First, we demonstrate large intercohort differences in trajectories of arrest, particularly much lower chances of arrest during the peak ages of delinquency in adolescence followed by faster subsequent declines in adulthood for the younger birth cohorts. Importantly, rather than reflecting differences between birth cohorts in individual disposition, family background, economic status, or neighborhood environments in childhood, these differentials arise from the distinct sociohistorical environments through which cohorts aged.

Second, we examine how the consequences for arrest of intracohort variation in two major concepts in theories of crime—*socioeconomic disadvantage* and *low self-control*—depend on historical context. Societal changes have been so large that they rendered socioeconomically disadvantaged and low self-control individuals of recent cohorts nearly indistinguishable from socioeconomically advantaged and high self-control individuals of cohorts born just one decade earlier. The changes in arrest trajectories across cohorts are especially pronounced by socioeconomic disadvantage, such that the disadvantaged members of younger cohorts have benefitted the most from social change—the influence of disadvantage on arrest is thus in part a function of the birth lottery of history.

Third, we disentangle the specific historical changes that took place in Chicago as our cohorts came of age, with a focus on the proximate mechanisms of law enforcement practices and criminal behavior. Disorderly conduct and drug arrests fell substantially in the period we study, while police size and the prevalence of drug use were relatively constant. These results indicate that the intercohort differences in arrest at the same ages that we demonstrate are not driven by police aggressiveness in the era of mass incarceration at the magnitude commonly hypothesized, nor even in the direction commonly hypothesized. In addition, property and violent crimes fell substantially. Taken together, this last set of analyses identifies plausible behavioral

and institutional pathways of social change that made a person's birth era so consequential for subsequent patterns of arrest.

The results establish the value of a historically focused examination of both inter- and intracohort differences in trajectories of arrest, calling for a reconsideration of standard accounts of age, crime, and criminal justice. Changes in the larger social environment that are shared by members of the same birth cohorts influence the life course of adolescents and adults in meaningful ways, in turn rendering explanations based on individual differences and many traditional social contexts, like socioeconomic status and the family, fundamentally incomplete. In addition to contributing to debates on age, crime, and the life course, our approach and findings offer a framework for studying social change and cohort differentiation in other areas of sociology and human development, such as addiction, mortality (e.g., "deaths of despair"), employment, and protest.

AGE, CRIME, AND SOCIAL CHANGE

A major debate in the study of crime turns on conflicting theories of age and criminal behavior. Why does crime tend to increase steeply in adolescence followed by a less steep but nearly continuous decline throughout adulthood? In an influential article, Hirschi and Gottfredson (1983) argued that the effect of age on criminal behavior, peaking in adolescence, is invariant across social and historical conditions. Gottfredson and Hirschi (1990, p. 232) went on to argue that low self-control emerges early in life and is stable thereafter—a direct cause of crime at all ages. A surge of studies emerged to test whether the effect of age on crime was independent of social factors and the extent to which early individual differences, especially in low self-control, predicted crime at all ages.²

Another school of thought uses a life-course framework for the study of age and crime, emphasizing both stability and change in crime at different ages or life stages and the turning points that shift people into and out of crime (Hagan and Palloni 1988; Sampson and Laub 1992). Sampson and Laub (1993), for example, argued that patterns of crime commission in adolescence and desistance from crime in adulthood can be explained by an age-graded theory of informal social control across the life course. In this viewpoint, events like marriage, military service, and unemployment serve as potential turning points that can redirect pathways of crime, net of individual differences that emerge early in life. A large body of research has emerged to

² The age and crime debate reached a broad audience, with articles in sociology, criminology, psychology, economics, and law. In a follow-up, Gottfredson and Hirschi (2019) offer a new look at the expansive literature on age, self-control, and crime, holding to their main age-invariance thesis.

test theories of turning points, informal social control, and desistance from crime (Bersani and Doherty 2018).

Notwithstanding the contributions of this research, we argue that the age, crime, and life-course debate, and consequently much of the empirical research related to it, are unduly narrow in their treatment of the larger socio-historical environment. A chief reason is theoretical. The micro- or individual-level focus in longitudinal research on testing the invariance of the age and crime relationship has implicitly held history, or social change, at bay. Consider the large body of research examining the heterogeneity of individual age-crime trajectories. Over 100 studies have been published trying to determine how many trajectory groups underlie the aggregate age-crime curve (Piquero 2008; Morizot 2019), with a focus on testing the typology of life-course persistent and adolescent-limited offenders proposed by Moffitt (1993) that contrasts with the age-invariance argument of Hirschi and Gottfredson (1983). Other longitudinal research has tested whether age effects are direct or are instead mediated by social factors such as employment, marriage, exposure to antisocial peers, and poverty (e.g., Shavit and Rattner 1988; Sweeten, Piquero, and Steinberg 2013).

Regardless of the resolution of these particular debates, theoretical concern with social change is largely absent, with many longitudinal studies reading as if they could be about any cohort aging through any historical period (see also Laub and Sampson 2020). As Raudenbush (2005, p. 132) comments on this literature: “What is lost is the notion of development as an ongoing interplay between individual action and social intervention.” Historical change, we suggest, is the ultimate intervention, an argument consistent with the recognition among life-course scholars that social change can alter trajectories of crime and criminal justice contact. For example, Laub and Sampson’s longitudinal study of Boston men in the mid-twentieth century highlighted turning points in relation to historical events such as the New Deal, World War II, the Korean War, and the G.I. Bill (Laub and Sampson 1995; Sampson and Laub 1995; Laub and Sampson 2003). More recently, Farrall, Gray, and Jones (2020) argue that economic restructuring and welfare state retrenchment in England in the 1980s influenced the crime trajectories of a sample of Britons born in 1970.

Recognizing the role of historical change, however, is different from studying how it shapes the life course of various cohorts. For example, the Boston study—like nearly all criminological work in the life-course tradition—was focused primarily on intracohort variation in transitions and turning points, and it did not empirically examine how social change interacted with aging to shape life-course patterns. Nor could it have, as the study was based on individuals who were born at most a few years apart. Such a data structure, in addition to those like the British study that followed a single cohort, is characteristic of work in this tradition. Moreover, these studies

predate the major social changes in crime and justice of recent decades that motivate our study—changes like mass incarceration, proactive policing, and the large declines in crime since the 1990s.

COHORT DIFFERENTIATION AND THE LIFE COURSE

A multicohort approach has strong foundations in the life-course perspective. Ryder's (1965) conceptual account focused on understanding cohort differentiation in relation to macrosocial change rather than identifying additive age, period, and cohort effects, and he emphasized that adolescence and emerging adulthood are especially turbulent times, subject to lasting historical influence.³ Elder's (1974, 1994, 1998, p. 3) framework similarly positions historical time and place as the first principle of the life-course perspective, proposing that "the life course of individuals is embedded in and shaped by the historical times and places they experience over their lifetime." A second principle, timing in lives, states that the impact of an event is contingent on when it occurs in a person's life. Combining these two principles, even for cohorts alive at the same time, the age-graded impacts of contextual influences on human development mean that social changes may produce substantial cohort differentiation (see also Smith 2020). We are thus concerned with the effect of social change in this specific way: how the age-graded course of crime and criminalization depends upon the historical period in which it occurs.

Cohorts are heterogeneous groups, however (Ryder 1965, p. 847; Elder and George 2016, p. 75), and intracohort variation in terms of race, class, and other key dimensions of stratification mean that social changes are not likely to be experienced in the same way for all members of the same cohorts. Put differently, the nature of intercohort differentiation may depend on which cohort members are examined. An additional implication is that the effects of intracohort variation in individual, family, or neighborhood characteristics on life-course outcomes may be dependent on historical context. Chen et al. (2010), for example, showed how the extent to which socioeconomic factors predict health trajectories in China depends upon the macrosocial context through which different cohorts age. Because inter- and intracohort differences may interact, a proper understanding of either necessitates a joint examination of both.

³ A standard interpretation is that age effects refer to changes over the life course of individuals, period effects to changes arising from historical events at a particular time that influence all ages, and cohort effects to changes that arise from the replacement of older cohorts of individuals with younger ones with different characteristics (or cohort composition). A well-known statistical problem in estimating additive effects of all three terms, which a large literature addresses (e.g., Mason and Fienberg 1985; Yang et al. 2008; Yang and Land 2013; Fosse and Winship 2019; Smith 2020) and to which we return, is the linear dependence created by the fact that $\text{Age} = \text{Period} - \text{Cohort}$.

Yet, the focus of most efforts in the study of age and crime has been on how cohort and period effects influence aggregate trends in crime rates and the aggregate age-crime distribution (Kim, Bushway, and Tsao 2016; Baumer, Vélez, and Rosenfeld 2018; O'Brien 2019). Greenberg (1985) was one of the first to highlight how age-crime distributions could reflect historical period and cohort effects. Farrington (1986) similarly argued that age-crime curves exhibit variation across historical eras, one of several studies to do so (e.g., see Steffensmeier et al. 1989; Greenberg 1994). O'Brien, Stockard, and Isaacson (1999) also found that cohort characteristics are related to homicide rates after controlling for age and period, while O'Brien and Stockard (2009) found cohort replacement is an important reason that the aggregate age-crime distribution has changed over time. This focus is complementary but distinct from our own, which seeks to understand how social change produces cohort differentiation in life-course patterns of crime.

A handful of longitudinal studies compare the offending patterns of multiple cohorts. Tracy, Wolfgang, and Figlio (1990) studied two Philadelphia cohorts that were born in 1945 and 1958. While briefly acknowledging the role of changing social contexts, they argued that criminal justice practices were mostly unchanging as the cohorts aged, and they found small differences in various metrics of criminal careers across the cohorts. Raudenbush (2005) used a small set of compositional controls in a longitudinal study of adolescents in Chicago in the 1990s and found cohort differences in self-reported violent crime over five years that he attributed to historical effects. Fabio et al. (2006) compared two cohorts of males in Pittsburgh from the 1980s and found that the older cohort was more violent at a given age after controlling for several cohort characteristics, a difference attributed to historical period. Farrall, Gray, and Jones (2020*b*) found that those from a British cohort born in 1970 were more disengaged from school than those born in 1958, a difference attributed to the economic restructuring of the 1980s, one which they argue resulted in elevated offending levels among the 1970 birth cohort. Finally, Payne and Piquero (2020) compared 1984 and 1994 Australian birth cohorts using officially adjudicated offense data to measure criminal offending to age 21. They found a lower prevalence of offending among the younger cohort, a difference that emerged in early adolescence and grew as the cohorts aged.

These findings are broadly consistent with the life-course cohort approach we advance, but we study a successive series of birth cohorts over a longer portion of the life course and during a period of rapid social change over the last quarter-century. Unlike prior research, we also examine a wide set of theoretically motivated measures of cohort composition and childhood experiences to adjudicate competing hypotheses, and we examine both the proximate mechanisms of social change and historical influences on intracohort inequalities for two major explanations of crime—disadvantage and low

self-control. Further, we unite cohort differentiation as a window on historical change with the study of official criminalization across the life course.

Criminal Justice Contact

Although most research on age and crime in the wake of Hirschi and Gottfredson's (1983) intervention refers to criminal behavior, longitudinal studies typically rely on official measures such as arrest or convictions. This choice is understandable given the lack of alternatives and a long-standing body of research showing that unofficial measures of criminal behavior predict arrest at the individual level (Blumstein et al. 1986; Gottfredson and Hirschi 2019). However, it means that societal and institutional practices of crime control that vary historically are potentially powerful explanations of cohort variations in arrest patterns. This distinction is particularly salient considering recent societal transformations in crime and the American criminal justice system. Crime exploded in the 1960s and 1970s and stayed at high levels until the mid-1990s, when it began a remarkable and unexpected decline (Baumer et al. 2018). This period also saw the rise of mass incarceration and the development of novel policing practices, many of them aggressive and proactive in nature. But many community policing programs emerged as well, such as the Chicago Alternative Policing Strategy (Skogan 2006). While crime and violence are near historically low levels and arrests for many crimes have declined, imprisonment rates remain high (Travis et al. 2014; Wakefield and Apel 2016).

Western (2006) and Pettit and Western (2004) examined inequality in mass incarceration by using demographic methods on aggregate data to estimate trends in imprisonment by birth cohort. They showed that the rapid growth in incarceration not only reshaped the life course of recent cohorts, it also made going to prison an expected transition to adulthood for black men with low education. This finding illustrates how historical changes do not necessarily affect all members of birth cohorts uniformly—it was the least educated black men who experienced the largest cohort changes in the prevalence of incarceration.

The work of Western and colleagues influenced other studies of incarceration from a life-course perspective. Wakefield and Wildeman (2013) employed two longitudinal data sets to examine the consequences of parental incarceration for early child outcomes in *Children of the Prison Boom*. Hagan and Foster (2012) similarly examined intergenerational effects of parental incarceration in the early life course (for an extensive review of the intergenerational literature, see Wildeman [2020]). At the more macro level, Porter et al. (2016) showed that changes in the age distribution of the prison population is primarily driven by birth cohorts who became young adults—the prime ages of both crime and incarceration—when substance use was at its

peak. Relatedly, Shen et al. (2020) found that the birth cohorts that became young adults during the 1990s crime wave have elevated rates of incarceration throughout their observed life course. The key mechanism for elevated incarceration rates decades after the crime wave is the accumulation of extended criminal histories under a sentencing structure that systematically escalated punishment for those with prior convictions.

These findings underscore the importance of examining cohort differentiation in arrest—the trigger generating a criminal record in the first place. Yet surprisingly little is known about the competing reasons for cohort variability in arrest over the life course, a lack that is also notable given that the negative consequences of a criminal record, even for low-level arrests, are broad and enduring (Pager 2009; Uggen et al. 2014). Using national survey data from a 1980s birth cohort up to 2008, Brame et al. (2012) found the prevalence of arrest to age 23 had increased compared to previous estimates from the 1960s. These prevalence estimates are valuable, but they do not adjudicate competing mechanisms of cohort variability in arrest trajectories. Weaver et al. (2019) used data on cohorts from both the 1979 and 1997 versions of the National Longitudinal Survey of Youth to examine how the relationship between self-reported crime and self-reported arrest varied with the rise of mass incarceration. Their finding of a decoupling between offending and arrest among the more recent cohorts, possibly due to policing practices, constitutes a potential reason that arrest patterns may vary across cohorts. However, the most recent cohort was born in 1984, and no data past 2002 were analyzed. Sociohistorical or cohort compositional changes since then may differentiate the arrest patterns of more recent birth cohorts. Additionally, both Weaver et al. (2019) and Brame et al. (2012) rely on self-reported arrest; in light of the validity problems with this metric (Kirk 2006), it is important to study official records. We do so, using long-term data through 2018 to examine cohort differences in arrest patterns over the life course, to assess rival explanations for why such differences exist and to disentangle macrosocial changes related to crime and justice.

STRATEGY AND HYPOTHESES

Drawing on the sociological life-course tradition and elevating the study of contemporary historical change in the age-crime debate, we examine both inter- and intracohort differentiation in arrest, the crucial first stage of criminalization on which incarceration depends. Instead of trying to decompose the extent to which changes in aggregate crime rates reflect age, period, or cohort effects, our aim is to unite the macrolevel study of history with patterns of arrest across the life course of individuals.

Studying social change in relation to age and arrest over the life course requires a strategy that imposes demanding data requirements. For one, it

requires a multicohort study. While social science including criminology is now richer in the supply of longer-term longitudinal studies, these studies are typically based on a single-cohort design, which cannot disentangle the effects of age and history since the two are perfectly collinear, or on multiple cohorts that are adjacent temporally such that historical context does not vary in meaningful ways. By contrast, our data extend the Project on Human Development in Chicago Neighborhoods (PHDCN), which employed a sequential-cohort longitudinal design. As Piquero, Farrington, and Blumstein (2003, p. 410) have noted, using the PHDCN as their example, such designs “present an unusual opportunity to examine period effects since successive cohorts will reach specific ages (e.g., age twelve) in different years and their life experiences can be compared.” Using PHDCN data, Raudenbush (2005) began to do just this, but with only five years of available data. Examining crime across a substantial portion of the life course or stages of historical import with PHDCN data was impossible, until now.

With data collected on PHDCN respondents through 2018, we can assess patterns of arrest across a good deal of the life course, for cohorts whose birth is separated by up to 17 years—1979–96. We specifically assess the role of historical change and its interaction with aging in three sets of empirical tests that derive from our theoretical focus. In the first, we estimate the degree of intercohort differentiation in arrest trajectories and then assess how much it is driven by historical change as opposed to what Ryder (1965, p. 845) calls cohorts’ “distinctive composition and character” reflecting the circumstances of their unique origination. In the model we describe later, the historical process of cohort differentiation is estimated as the interaction of age and period. While compositional differences that are fixed at birth or differences in childhoods between cohorts could produce an interaction between age and period, we argue instead that the interaction between age and period is produced by the changing contexts in which coming of age occurs.

We therefore measure a wide-ranging group of theoretically motivated concepts and formulate a statistical model to test the hypothesis that neither differences in demographic composition, in stable individual traits, nor differences in the early life family and neighborhood characteristics and experiences explain intercohort differences in arrest over the life course. The need to control for these factors is another way in which our strategy imposes stringent data requirements, but the rich measurement on individuals and the context in which their development occurred makes the PHDCN well suited for the task. In particular, cohort effects on crime rates have been hypothesized to arise from different cohorts’ demographic structure and childhood exposure to violence, drugs, family problems, lead toxicity, and the emergence of juvenile “super predators” from the crack generation, all leading to changing propensities to commit crime (e.g., see Cook and Laub 2002). We account for these and many other competing factors (e.g., socioeconomic

and neighborhood) that vary by cohort in order to isolate how historical change influences trajectories of arrest.

Although we highlight the role of broader societal change, we are not arguing that individual, family, or neighborhood factors are unimportant. To the contrary, in our second set of analyses we unite such factors with historical change by examining intracohort variation in patterns of arrest by age and how it interacts with period for theoretically important groups. We specifically examine inequality in criminal justice contact by testing the hypothesis that not all groups experience historical change in the same way, focusing on two of the most influential concepts in the study of crime—socioeconomic disadvantage and low self-control.

There are several reasons to test whether intracohort variations in socioeconomic disadvantage interact with intercohort inequalities in arrest. Empirically, the importance of class differences across cohorts in incarceration (Pettit and Western 2004) suggests the importance of a similar inquiry for the prior stage of arrest. Theoretically, strain, social disorganization, and social control explanations of crime all point to the importance of socioeconomic disadvantage in motivating and regulating crime (Kornhauser 1978). Several scholars, for example, highlight contentious peer encounters among disadvantaged minority groups and their conflictual interactions with the police, especially in segregated environments (Anderson 1990, 2000; Harding 2010; Patterson 2015). Research on economic deindustrialization (Wilson 1987), the growth of urban violence (Sharkey 2010, 2018), and punitive crime policies (Western 2006; Alexander 2012; Hinton 2016) also points to the vulnerability of socioeconomically disadvantaged adolescents, especially minorities, who grew up during the height of social dislocations like the peaking of the drug war and violence (Contreras 2012). Yet social changes—such as in policing and incarceration, forms of urban inequality and revitalization, and falling violence—have since altered those contexts. For these reasons we hypothesize that disadvantaged members of birth cohorts who came of age more recently will face substantially improved prospects of avoiding the snare of arrest over the life course relative to individuals who were born a decade or more earlier but who are otherwise similar in all other respects. To our knowledge, this hypothesis has not been tested before.

Second, we test the hypothesis that the link between self-control and arrest interacts with social change. Low self-control has been posited as “*the* individual-level cause of crime” (Gottfredson and Hirschi 1990, p. 232; emphasis in original), and a large body of research has established low self-control as a strong predictor not just of criminal behavior but also a wide range of problem behaviors in adulthood (Moffitt et al. 2011; Gottfredson and Hirschi 2019). Based on this research, we expect low self-control to predict arrest at all ages. But our theoretical framework leads us to further expect that the manifestations of low self-control depend on historical

context. Wikström and colleagues (Wikström et al. 2012; Wikström 2020) similarly draw attention to the interaction of self-control with the environments in which individuals are embedded, in their case microenvironments like situations or neighborhoods. We shift the focus upward by proposing that low self-control will lead to differing chances of arrest depending on the changing social environments in which aging occurs.

In our case, low self-control individuals in older birth cohorts, even if they were of the same socioeconomic position and otherwise similar to those individuals from younger cohorts, were especially disadvantaged in their exposure to societal contexts of high crime and violence, and in particular to disputatious encounters (such as threats and insults) with peers during the vulnerable ages of adolescence and early adulthood. The chance that impulsive outbursts in interactions with authorities will result in arrests is also likely to depend on school and police “zero tolerance” policies, which vary historically. We thus hypothesize that the conversion of tendencies to crime fostered by low self-control into a criminal record is dependent on historical context (see also Wakefield and Apel 2016; Turney and Wakefield 2019; Weaver et al. 2019). In this sense, whether low self-control leads to arrest is shaped by macrosocial changes that go well beyond individual-level explanation. As with socioeconomic disadvantage, this individual and macrohistorical linkage has not, to our knowledge, been previously tested.

Our third set of analyses drill down on mechanisms that plausibly account for how social change produces intercohort differentiation, with a focus on social and institutional factors most proximate to arrest. In one test, we examine intercohort variations in police practices by disaggregating police charging behaviors over time, including by type of offense. Given prior expectations in the literature that the drug war was the main cause of increasing criminalization (Alexander 2012; Hinton 2016), we should see distinct cohort differences in the proportion of arrests that are drug related, which we can directly assess. We also examine trends in adolescent drug-using behavior to help tease out the nature of cohort differences. In another set of tests, we look at multiple aspects of social change over the period of our study. We focus on crime and social control practices by creating measures of yearly changes from 1996 to 2018 in reported crime, arrests for drugs, disorderly conduct, violence, and property crimes, the rate of arrests per reported offense, and police size in Chicago. In the discussion, we also consider more distal factors such as the deconcentration of public housing starting in the mid-1990s, immigration, technology, and community anticrime organizations.

RESEARCH DESIGN AND DATA

The data in this article center on an original follow-up of four different age cohorts from the PHDCN. The design of the PHDCN is based on a two-stage

procedure. First, a representative sample of 80 neighborhoods was selected in the mid-1990s. These neighborhoods were chosen to represent the wide variability, by race and class, in Chicago. An array of data was collected from each neighborhood, including independent surveys of residents and systematic observations. Second, a representative sample of children, rather than one selected from administrative records or on a specific trait, was drawn from these neighborhoods and studied over the course of about six years, from the mid-1990s to the early 2000s. Dwelling units were systematically selected from a random start within enumerated blocks and then all households, approximately 35,000, were screened for children of eligible age. Children falling within seven age cohorts (0, 3, 6, 9, 12, 15, and 18—numbers indicating respondents' age around the study's start in 1995) were then sampled from randomly selected households.

Because of these procedures, the PHDCN sample was broadly representative of children and adolescents living in a wide range of Chicago neighborhoods in the mid-1990s. The first round, or "wave," of the study included just over 6,200 children who were visited for extensive in-home interviews or assessments, along with interviews with their primary caregivers, over the course of three years, starting in late 1994 and running through 1996. Then, at roughly 2.5-year intervals, two more waves of data were collected by the PHDCN research team (wave 2 was concentrated in 1997–1999, and wave 3 in 1999–2001). Although all children were living in Chicago at wave 1, and most stayed in Illinois or nearby (Indiana or Wisconsin), participants were followed no matter where they moved. Fortunately, the participation at wave 1 and retention over time were comparatively high for a contemporary urban sample, 78% and 75%, respectively.

Long-Term Follow-Up

In 2011–13, a random sample of those last contacted at wave 3 of the PHDCN in the 0, 9, 12, and 15 age cohorts was located and reinterviewed, here labeled "wave 4."⁴ These cohorts were selected to maximize variation in life-course experiences and exposure to social change at different ages. Despite the long time that had elapsed since the last contact at wave 3 and the difficulty of reaching people in an era of caller ID and nearly extinct home phones among young adults, the follow-up located and collected data on 1,057 members of the original PHDCN study, a response rate of 67% of eligible cases in the youngest cohort and 63% in the older cohorts. Ranging

⁴ These four PHDCN age cohorts were born over 13 different years. People can be the same integer age if they are born in adjacent years (e.g., cohort 15 is almost entirely born in 1980 and 1981). Moreover, data collection for the first wave occurred on a rolling basis over time. We analyze respondents' actual birth years rather than conventional PHDCN age cohorts.

between ages 24 and 33 at wave 4, there are 226 respondents in the 9-year-old cohort, 236 in the 12-year-old cohort, and 217 in the 15-year-old cohort. There are 378 children born at the study's start (i.e., members of cohort 0), who were on average 16–17 years old at wave 4. The sample is nearly evenly split by gender (51% female, 49% male) and diverse by race/ethnicity (19% white, 36.5% black, 40.3% Hispanic, and 4.2% other). Parallel to waves 1–3, detailed information was gathered from members of cohorts 9–15 and the caretakers of cohort 0 members. Also, the caretakers of cohort 0 members were asked a battery of items measuring the behavior and circumstances of their children during adolescence, including measures of aggression, antisocial behavior, low self-control, and internalizing behaviors like anxiety and depression, all of which were previously measured in the older cohorts.

Criminal Histories

Many population-based studies rely on self-reports of official contact instead of criminal justice histories that can be reliably placed in time. It is not just that people's memory falters: an earlier analysis of the PHDCN based on arrests in the late 1990s found that a sizable number of adolescents, almost a quarter, reported an arrest that did not exist officially. More worrisome, nearly half of adolescents with an arrest record failed to report that they had been arrested (Kirk 2006). What is needed is a dynamic flow of data that captures the timing of arrest across the life course.

To meet this objective, we carried out three waves of data collection from the criminal history record information (CHRI) reported to the state of Illinois and housed by the Illinois State Police. All PHDCN wave 4 respondents (cohorts 0, 9, 12, 15) were first matched by name (including aliases) and date of birth with all CHRI records for the entire state of Illinois in August 2015. At that time, cohort 0 members were almost 20 years old, on average, and cohort 15 members were in their early 30s. The official criminal history records cover all jurisdictions in Illinois and include detailed information, by date, on arrests and charges.⁵ Sometimes, however, it takes

⁵ Probabilistic matching algorithms from the Merge Tool Box (MTB) software were used (Devitt and Hughes 2016). Over 80% of the matches yielded a probability of 100% accuracy, and over 90% were 99% or higher. The remaining cases were individually checked for accuracy. Because the CHRI database reaches back to the mid-1990s, the search process captured the period before the age of first arrest in both the 0 and 9 age cohorts, permitting construction of a full history of arrests for these cohorts, and arrests after approximately age 12 and 15, respectively, for the two older age cohorts. Among the younger cohorts for whom we have full data, only 0.7% were arrested before 12 and 4.2% were arrested before 15, indicating that there are likely few left-censored arrests among the older cohorts. Still, we do not use the oldest cohorts in models where such left censoring could be problematic, such as in survival models.

time for a given case to be recorded, and the agency periodically updates its records, meaning that we might have undercounted arrest in our first search. Moreover, study members, especially in the 0 and 9 age cohorts, were accumulating potential criminal activity after 2015. To circumvent these issues, we repeated the search process twice more, once in 2017 and the last time in January of 2019. The records were integrated with the 2015 search and an updated sequence was created, yielding a final 23-year record of arrest histories through 2018.

These data still have limitations. They are based on Illinois criminal records, although the proportion of our sample that lived in Illinois at wave 4 was substantial (88%). Others lived in Illinois for many years just up to wave 4, while others moved out in earlier waves and then moved back to the state *after* wave 4, some of whom were in fact arrested. To avoid undercounting those at risk of arrest in the state, we only consider criminal records to be missing among cohort members who moved out of Illinois at wave 2 and never returned. Fortunately, this is less than 2% of the sample, and the results do not change if we use less restrictive or more restrictive definitions, such as focusing only on those who lived in Illinois their entire lives. While some individuals may have expunged their juvenile records, less than 0.005% of juvenile records in Illinois were expunged between 2004 and 2014. Moreover, even if later expunged, our earlier record checks retain the original information, allowing us to piece together respondents' full criminal histories. Overall, then, there is no reason to believe that missing juvenile arrests are more than a handful, and we engage in several robustness checks on missingness in data by residential location.

ANALYTICAL STRATEGY

Our analysis proceeds in three steps, corresponding to our theoretical framework and three sets of proposed tests. The first step is to assess how the relationship between age and arrest varies as a function of historical period. To do so, we employ two models. The first estimates age-arrest curves but allows them to vary by period. Formally, let Y_{it} indicate whether participant i was arrested in time t , where $t \in [1, 2, \dots, T_i]$ is the one-year measurement period for that participant, who is observed a total of T_i times. Let $E(Y_{it}) = P(Y_{it} = 1) = \mu_{it}$ denote the probability of arrest for participant i at time t , which is estimated with the marginal model:

$$\begin{aligned} \text{logit}(\mu_{it}) = & \beta_0 + \beta_1 a_{it} + \beta_2 a_{it}^2 \\ & + (\beta_3 + \beta_4 a_{it} + \beta_5 a_{it}^2) p_t \\ & + (\beta_6 + \beta_7 a_{it} + \beta_8 a_{it}^2) p_t^2, \end{aligned} \tag{1}$$

where a_{it} is the age of a participant i at time t , and p is the period (i.e., the year of time t). Thus, we use logistic regression to estimate whether respondents are arrested in a given year as a function of their age, the period, and the interaction of age and period. Although we impose a quadratic functional form on the age and period terms, results are not sensitive to these assumptions, as will be shown. The model in equation (1), henceforth model 1, allows for an initial estimate of the nature and degree of intercohort differentiation in arrest trajectories. Because birth year (which determines cohort membership) is equal to the year for a given measurement period minus a respondents' age at that measurement period, the age and period terms in equation (1) are sufficient to examine how arrest trajectories vary by cohort.⁶

While model 1 can provide insights into the existence and form of intercohort differentiation in arrest, it cannot distinguish between competing explanations for any such differentiation. The differences could reflect how aging through a changing world dynamically shapes arrest patterns through adolescence and into adulthood—our theoretical focus. Conversely, they may reflect that cohorts differ in characteristics that are fixed at birth, or that are determined in childhood, and which carry persistent consequences for arrest patterns across the life course. To adjudicate between these explanations, we estimate model 2 as

$$\begin{aligned} \text{logit}(\mu_{it}) = & \beta_0 + \beta_1 a_{it} + \beta_2 a_{it}^2 \\ & + (\beta_3 + \beta_4 a_{it} + \beta_5 a_{it}^2) p_t \\ & + (\beta_6 + \beta_7 a_{it} + \beta_8 a_{it}^2) p_t^2 \\ & + \pi \cdot M_i, \end{aligned} \tag{2}$$

the form of which is identical to equation (1), except for two terms. Specifically, π is a coefficient vector that corresponds to M , a vector of theoretically relevant covariates that capture compositional and early life differences between cohorts. Under the assumption that these covariates succeed in adjusting for such differences, any interaction between age and period will reflect intercohort differentiation in arrest patterns that arise from coming of age during different periods of history (see also Raudenbush 2005, p. 143).

Age, period, and cohort are linearly dependent, an identification problem that rules out adding a linear cohort term to this model (Fosse and Winship 2019). However, in light of our focus, doing so is not of theoretical interest even if mathematically possible. As Luo and Hodges (2020, p. 36) argue, the type of cohort effect estimated by additive age, period, and cohort accounting

⁶ The analysis of latent-class models, or group-based trajectory models (e.g., Nagin 2005), is a natural extension of our analytic framework but is beyond the scope of the present article.

methods is assumed to occur independently of social changes (indexed by period) and aging, without age-graded effects of social changes (see also Lu and Luo 2020). Yet, our focus is precisely the consequences of aging through a specific slice of history. This focus is a form of cohort effect (captured by age-period interactions), and so too is the alternative explanation that observed cohort differentiation reflects compositional or early life differences. By conditioning directly on such differences, we are able to tease apart competing explanations of cohort differentiation. In this way, identification of the parameters of theoretical interest is achieved without needing a linear (or nonlinear) cohort term to the model.

We estimate these models with generalized estimating equations (GEE) to account for the nested nature of the data; that is, observations from the same respondents with correlated errors (Hardin and Hilbe 2003).⁷ Specifically, we use the population-averaged version of GEE introduced by Liang and Zeger (1986).⁸ Multiple imputation is employed to account for missing data.⁹ Five imputed data sets were created using Amelia II (Honaker, King, and Blackwell 2011), results were estimated separately for each data set, and using Rubin's rules (Rubin 1987), these results were combined into one overall result.¹⁰

Nonlinear models with interaction terms cannot be interpreted in the way interaction terms in linear models often are, by examining tabular output (Mize 2019). As such, our presentation of results focuses on visualizing expected values and carrying out significance tests on the relevant marginal effects (with tabular regression output found in online app. table A1). Because logistic regression models are inherently nonlinear, the expected values obtained depend on what values the control variables are set to (Hanmer and Kalkan 2013). As such, for each age and period combination for which we wanted an expected value, we created a replica data set that

⁷ For all models fit using GEE, we assume a first-order autoregressive working correlation matrix and use robust standard errors in case the working matrix is misspecified. While multilevel models are a viable alternative, random effects would need to be integrated out in order to yield population-average estimates. As such, we avoid fitting random effects in the first place.

⁸ As a result, estimates from our model indicate the average probability of arrest for individuals with a given set of covariate values in the population, rather than subject-specific estimates, which are not of interest for current purposes.

⁹ Missing data are not common: 59% of observations are missing no data, and 79% are missing data on three or fewer variables. No variable has missing data on over 13% of observations.

¹⁰ The combined point estimate is the mean of the within-imputation point estimates, i.e., the average of the estimates from the five imputed data sets. The combined variance is the mean of the within-imputation variance plus a component that accounts for between-imputation variability. This imputation strategy assumes the data are missing at random (MAR), i.e., that the data are missing only as a function of the observed data that are used in the imputation procedure.

has exactly the same values as the actual data set for the control variables, calculated an expected value for each observation, and then averaged these respondent-level expected values to obtain a single point estimate.¹¹ In order to calculate the standard errors of these estimates, we simulated 10,000 sets of parameters from a multivariate normal distribution that accounts for the variance and covariance of the coefficients. For any given age and period selected, multiplying these simulated parameter values by the values of the replica data set will produce a distribution of values, the standard deviation of which is the standard error of that expected value.

The second and third steps of the analysis employ variants of this model combined with other types of models and data. For ease of comprehension, these are described when presented.

MEASURES

The main dependent variable is whether a respondent was arrested in a given year, as measured in the official criminal history. In some analyses, we further break down this variable by type of arrest, specifically whether it contained a drug, property crime, or violent crime charge. Similarly, following from equations (1) and (2), the two focal independent variables are age and period. Period is defined as calendar year, whereas age is defined as calendar year minus birth year.

In addition to the sequential cohort design and long-term nature of the study, both necessary to properly carry out the models described above, a key advantage of the data employed is the rich information with which to adjust for potentially salient early life and compositional differences between cohorts. Prior research and theory led us to identify six sets of potential differences: *demographics*, *subject behavioral troubles*, *family structure and socioeconomic status*, *family troubles*, *neighborhood structure and social processes*, and *neighborhood crime and criminal justice*. A description of each domain follows, proceeding in a hierarchical fashion from the individual, to their family, and their early life neighborhood context, with further details in table 1, including timing of measurement by cohort.

Demographics

We include three basic demographic factors that have well-established links to criminality and criminalization: sex, race/ethnicity, and immigrant generational status. While sex composition has been relatively constant over the study period, immigration has been remaking the composition of cities like

¹¹ Hanmer and Kalkan (2013) call this the observed-value approach, and they convincingly argue that it produces a superior estimate of an average treatment effect compared to alternatives, such as holding controls at their mean.

Chicago. Specifically, Hispanic and immigrant populations—two things generally associated with lower crime rates, even at the individual level—have been growing. As Ryder (1965) presciently observed, immigration may be a major differentiator of cohorts.

Subject Behavioral Troubles

Early life behavioral problems may set in motion a cycle of events that perpetuate persistent behavioral problems (Sampson and Laub 1993). They may also signal stable individual tendencies to criminality (Farrington 1998). Gottfredson and Hirschi (1990, p. 232; 2019) argue that low self-control is, for all intents and purposes, the individual-level cause of crime, a capacity that is presumed stable from childhood or early adolescence onward. Accordingly, we control for both low self-control/impulsivity and aggression/antisocial behavior from the Child Behavior Checklist, or CBCL (Achenbach 1997). The CBCL, based on parental interviews, is a widely used instrument for identifying childhood emotional and behavioral problems.¹² If there were something like a superpredator or crime-prone generation, these measures of early predispositions and behavioral tendencies to delinquency would likely tap it.¹³ In addition, we assess anxiety and depression, which research has linked to adolescent crime (Elliott, Huizinga, and Menard 1989).

¹² For the first set of analyses comparing all cohorts, we measure all three CBCL scales around age 15 (see table 1). For the older cohorts, a longer list of items was available and thus utilized. In a later and more focused analysis, we examine low self-control even earlier (around age 12) in comparing age cohorts 0 and 9. For cohort 0, we took the average of assessments at waves 3 and 4 (circa ages 5–6 and 16), whereas for cohort 9 we used the assessment at wave 2 (around ages 11–12). For both the age 12 and age 15 measures and all analyses, self-control is therefore assessed either before the onset of arrest or before the peak ages of arrest. The scales are standardized, weighted by the number of items included in each wave's scale.

¹³ Self-reports of delinquency or low self-control are a possible alternative, but our theoretical focus requires that all early life cohort covariates predate crime and arrests, meaning childhood or early adolescence, when self-reports are less common or unavailable across all cohorts, as here. There is also evidence that the validity of self-reports varies by age (Lauritsen 1998; Kim and Bushway 2018). For these reasons, parental reports of childhood behavior are highly desirable, and as we have noted, there is considerable evidence in the literature that between-individual differences in childhood antisocial behavior and low self-control are relatively persistent over time. Furthermore, although data limitations preclude us from measuring the CBCL in the same exact way across cohorts, the low self-control and antisocial behavior measures nonetheless similarly predict overall arrest in the full sample and within each cohort, consistent with past research and supporting construct validity. For example, despite measurement differences in items and ages, low self-control strongly predicts arrest virtually identically across cohorts—OR = 1.464 ($P = 0.003$) for age cohort 0, controlling sex and race/ethnicity; the corresponding coefficient for the older cohorts (9, 12, 15) is 1.428 ($P < 0.001$).

TABLE 1
DESCRIPTION AND DATA SOURCES FOR VARIABLES USED IN ANALYSIS

Variable	Description	Data Source
Dependent:		
Arrest	Whether SP was arrested in a given time period. ^a	CHRI
Focal independent:		
Age	SP's age in a given time period. Defined as birth year minus the calendar year of that time period.	W1
Period	The calendar year of a given time period.	NA
Cohort controls:		
Demographics:		
Race/ethnicity	Factor: whether SP is white, black, Hispanic, or other.	W1
Sex	Whether SP is male or female.	W1
Immigrant generation	Factor: whether PC is first, second, or higher generation immigrant.	W1
Subject behavioral troubles:		
Anxiety/depression	Standardized scale, parent report from CBLC checklist. ^b	W1–W4
Low self-control/impulsivity	Standardized scale, parent report from CBLC checklist. ^b	W1–W4
Aggression/antisocial behavior	Standardized scale, parent report from CBLC checklist. ^b	W1–W4
Family structure and SES:		
Parental age	PC's age at SP's birth.	W1
Parental employment	Whether PC was working. ^{c,d}	W1, W3
Public assistance	Whether PC was on TANF. ^{c,d}	W1, W3
Parental education	Factor: PC's educational attainment is < HS, HS/GED, more than HS but less than BA, BA or more. ^{c,d}	W1, W3
Household income	Factor: seven levels ranging from < \$5,000 to > \$50,000. ^{c,d}	W1, W3
Parental relationship status	Factor: whether PC was single, cohabitating, or married. ^e	W1, W3
Family residential stability	No. years at current address. ^e	W1, W3
Household size	No. people who reside in SP's household.	W1, W3
Homeownership	Whether PC owns own home. ^e	W1, W3
Family troubles:		
Parental depression	Whether PC is depressed.	W1
Parental exposure to violence	Whether PC has seen someone shot, shot at, or stabbed in past year. ^e	W1, W3
Parent arrest	Whether a parent had trouble with police or been arrested.	W1
Family arrest	No. family members that have had trouble with police or been arrested.	W1
Family incarceration	No. family members currently incarcerated.	W1
Family institutional troubles	No. family members that have had frequent trouble with the law, holding a job, getting into fights, or school discipline.	W1

TABLE 1 (Continued)

Variable	Description	Data Source
Family alcohol problems	No. family members with alcohol use that led to trouble with family, job, law, or health.	W1
Family drug problems	No. family members with drug use that led to trouble with family, job, law, or health.	W1
Family in treatment	No. family members treated for emotional, drug, or alcohol use problems.	W1
Neighborhood structure and social processes:		
%Black	% of tract's residents that were Black when SP was age 9.	CN
%Hispanic	% of tract's residents that were Hispanic when SP was 9.	CN
%foreign born	% of tract's residents that were foreign born when SP was age 9.	CN
Concentrated poverty	Tract-level poverty rate when SP was age 9.	CN
Unemployment rate	Tract-level unemployment rate when SP was age 9.	CN
Youth population	Tract-level fraction of households with children when SP was age 9.	CN
Female-headed households	Tract-level fraction of households headed by a female only when SP was age 9.	CN
Owner-occupied rate	Tract-level fraction of households that were owner-occupied when SP was age 9.	CN
College education	% of tract's adults who were college-educated when SP was age 9.	CN
Lead exposure	% of children < 5 tested by the city and % of children < 5 with blood-lead levels > 5 µg/dL when SP was between ages 6 (1995 cohort) and ages 9–15 for older cohorts.	CDPH
Collective efficacy	Tract-level social cohesion and trust, measured by combining responses to 10 Likert-style scales, 1995. See Sampson, Raudenbush, and Earls (1997) for further details.	CS
Physical/social disorder	Tract-level, first principal component of systematic social observation data that loads onto physical and social disorder, and degraded physical condition of buildings, 1995	SSO
Alcohol/commercial Establishments	Tract-level second principal component of systematic social observation data that loads onto observed alcohol sale points, alcohol/tobacco advertising, and building security, 1995.	SSO
Neighborhood crime and criminal justice:		
Homicide rate	Tract-level rate of homicides per 100,000 residents when SP was between ages 6 and 10.	CPD
Robbery rate	Tract-level rate of robberies per 100,000 residents when SP was between ages 6 and 10.	CPD
Weapon offense rate	Tract-level rate of weapon offenses per 100,000 residents when SP was between ages 6 and 10.	CPD
Incarceration rate	Tract-level rate of prison admissions per 100,000 residents when SP was between ages 6 and 10.	State of Illinois

TABLE 1 (Continued)

Variable	Description	Data Source
Neighborhood policing intensity	Tract-level, first principal component of the ratio of all arrests to offenses and rate of part 2 arrests per population, 1999 ^e	CPD

NOTE.—SP = study participant (respondent); PC = primary caregiver (usually parent). Data sources: CHRI = Illinois criminal history record information; W1 = PHDCN Longitudinal Cohort Study (LCS) wave 1; W3 = PHDCN LCS wave 3, W4 = PHDCN LCS wave 4; CS = PHDCN Community Survey; CN = Census; SSO = systematic social observation; CDPH = Chicago Department of Public Health; CPD = Chicago Police Department. Unlike for the longitudinal study and the census, the CDPH, CPD, State of Illinois, CS, and SSO data sources are not available after wave 1 for participants who moved out of Chicago.

^a In the section “Linking Macrosocial Change to Life-Course Patterns,” this variable is further broken down into types of arrests: those with drug, property, or violent crime charges. Additionally, that section introduces different types of information on arrests, which are described therein.

^b Measured at an average age of 12 in the self-control analysis of the section on intracohort variations, and average age 15 in the first section of results on intercohort differentiation.

^c These variables are measured at W1 for cohorts 9, 12, and 15 and at W3 for the birth cohort, so that they are measured at ages that are as similar as possible across cohorts given the data structure. Only 5.8% of the youngest cohort shared a household with older cohorts; using different waves captures within-household, between-cohort differences in these measures.

^d These variables are used to create an index of disadvantage used in the second results section, as described in the text.

^e Part 2 arrests include offenses such as disorderly conduct, prostitution, vandalism, drunkenness, drug abuse, and simple assaults. See <https://ucr.fbi.gov/crime-in-the-u.s/1999/99sec7.pdf>, p. 405.

Family Structure and Socioeconomic Status

Early life characteristics related to family structure and socioeconomic status, while not destiny, are well established as important sources of criminality across the life course, whether for developmental or sociological reasons (Sampson and Laub 1993). As such, we measure parental age at the time of participant’s birth, whether parents were employed, whether parents were on welfare (TANF), their educational attainment, their relationship status, household size, household income, the family’s residential stability, and home ownership.

Family Troubles

Basic family structure variables do not give a wholly satisfactory picture of early life family context, especially when the goal is to measure things that may ultimately prove to be criminogenic, or at least criminalizing. We therefore measure an array of problems that beset families, which research suggests impact children’s subsequent life-course patterns of crime (Loeber and Stouthamer-Loeber 1986; Sampson and Laub 1993; Farrington et al. 2001; Wakefield and Apel 2016), and which may vary by cohort. We specifically

measure the extent to which family members have had trouble with the police or other core institutions, have been arrested and incarcerated, had trouble with alcohol, had drug problems, and were treated for substance abuse or emotional problems. Additionally, we measure parents' exposure to violence, whether parents have had trouble with the police or been arrested, and whether they suffered from depression. Many of these criminally relevant parent- and family-level factors differ across cohorts during a period that saw the rise of mass incarceration, the fall in violence, and the rise and fall of the crack epidemic.

Neighborhood Structure and Social Processes

Early life development occurs not just within a family, but within a larger neighborhood context. Put simply, where people are born and grow up matters for their subsequent life course (Sampson 2012; Chetty and Hendren 2018). Yet, neighborhoods are not static entities but are themselves subject to change over time, and Chicago, like many contemporary American cities, has changed in recent decades. The implication is that the changing nature of neighborhoods may constitute a cohort difference that materializes as differences in lifetime patterns of arrest. Thus, we adjust for neighborhood structures measured early in respondents' lives. Because children were followed no matter where they moved, we were able to geocode addresses and create census tract measures of the percentage of residents in poverty, foreign born, black, Hispanic, unemployed, and college educated, as well as the percentage of housing units that were owner occupied and the percentage of households with children that were female headed. Each of these variables was measured at age 9 for every cohort, before the onset of first arrest and before the onset of the social changes whose effects on the age-arrest relationship is our focus. These controls constitute elements of neighborhood structure and composition that consistently predict crime, violence, and arrest in Chicago (Kirk 2008; Sampson 2012), as elsewhere.

Similarly, we adjust for several environmental factors, including age-graded lead exposure as measured by the average blood-lead levels of children in each of Chicago's neighborhoods (see table 1 for further details). Lead exposure has been linked to crime, and children's exposure to it has declined markedly in recent decades (Muller, Sampson, and Winter 2018). Although we do not have the precision to measure at the same or similar ages across cohorts, we also adjust for signs of physical disorder (e.g., graffiti, vacant buildings) and social disorder (e.g., drinking or fighting in public) that may trigger police responses, as well as the extent of alcohol stores and secured buildings, determined through systematic observations (Raudenbush and Sampson 1999). Finally, we adjust for collective efficacy, a neighborhood social process shown to predict crime (Sampson, Raudenbush, and Earls 1997).

Neighborhood Crime and Criminal Justice

We separate out early life neighborhood crime and criminal justice reactions for two reasons. The first is substantive: these are pertinent features of neighborhoods that varied greatly across the childhoods of the cohorts. This is particularly true of crime, which began its precipitous decline in Chicago in the early to mid-1990s, just before the younger cohorts were born. We focus on violence given the detrimental consequences of exposure to violence for children's development (Sharkey 2010, 2018). Similarly, while the ascent of incarceration was already underway when the oldest cohort was born, incarceration rates continued to climb throughout the 1990s, making childhood exposure to neighborhood incarceration also vary by cohort. Given the spatial concentration of incarceration in Chicago, these changes are particularly pronounced for a subset of neighborhoods (Sampson 2012, p. 114). Mass incarceration may erode the fabric of communities in ways that are detrimental to the development of their children, potentially creating consequences that persist across the life course (Hagan and Dinovitzer 1999).

To tap such neighborhood differences in childhood exposure by birth cohort, we gathered tract-level incarceration rates (admissions per 100,000) and violent crime rates per 100,000 for three types of crimes—homicide, robbery, and weapons offenses—that were available for the city of Chicago from the early 1990s to the early 2000s. Each measure refers to the neighborhoods where children were living when they were between 6 and 12 years old.¹⁴ Hence, the second reason we separate out these variables from the rest is that we do not have neighborhood incarceration or crime incident data for children born in the 1990s who moved out of Chicago before the early 2000s (about 23% of cohort 0). This is nonrandom missingness, so rather than impute those values, we restrict analyses that use these measures to the individuals of all cohorts who spent their childhood in Chicago. Finally, while we cannot measure it at such similar ages, we adjust for the intensity of neighborhood policing, a composite measure based on the ratio of Chicago Police Department (CPD) arrests to offenses in neighborhoods that were available in the late 1990s.

INTERCOHORT INEQUALITIES IN ARREST

We first assess the extent to which aging through different periods leads to differing arrest trajectories across birth cohorts. We begin with model 1, which estimates arrest as a function of age, period, and their interaction,

¹⁴ Measures for cohorts 0 and 9 pertain to exposure at age 6, whereas those for cohorts 12 and 15 pertain to the ages of about 9 and 11. Tract measures prior to the 1990s are unavailable for Chicago.

with no additional control variables. Figure 1(a) presents the visual results for the 1980, 1985, and 1995 birth cohorts (full regression output for model 1 can be found in online app. table A1).¹⁵ Each line represents the average probability of arrest for individuals born in a certain year when they reached a given age. We denote the specific years in which the cohorts reached various ages to clarify the historical periods through which they aged. For example, people born in 1995 had a 0.054 probability of arrest when they were 21 years old, in 2016.

The sequential-cohort design permits us to see the distinctive experience of each cohort, revealing large differences in arrest trajectories between the 1995 cohort and those born in the 1980s. These differences emerge in the midteens, with members of the 1995 birth cohort being less likely to be arrested as they reached adulthood. All differences from the age of 17 onward in figure 1(a) between the 1995 cohort and the other two cohorts are significant ($P < .05$). While these results suggest that intercohort differentiation in arrest trajectories may arise from the interaction of age and period, these differences—or at least part of them—may reflect compositional or early life differences between the cohorts. Thus, it is necessary to remove theoretically relevant differences to isolate how the impact of age depends on period.

Figure 2 demonstrates that nontrivial differences do in fact exist between the cohorts born in the 1990s and those born before then, which is where the largest intercohort differences in arrests patterns are found.¹⁶ Notably, the 1990s birth cohorts were systematically advantaged in their exposure to potentially criminogenic early life conditions compared to the older cohorts. Their parents were 43.3% less likely to be arrested but 69.5% more likely to have a university degree, and their early life neighborhoods exposed them to less poverty, lead, violence, and concentrated incarceration. Depending on the offense type, neighborhood violence rates in childhood were anywhere from 82.8% (homicide) to 202.6% (weapon offenses) higher among the older cohorts. The observed intercohort differentiation in arrest trajectories plausibly reflects the influence of these and other factors that are determined at birth or during childhood rather than the dynamic interplay of aging through a changing historical context during late adolescence and early adulthood.

¹⁵ Computerized arrest record keeping in the mid-1990s was not as complete as in later years, which mainly affects the oldest (age 15) cohort. We thus adjust for this difference with a dummy variable indicating whether an observation occurred before 2000 or not. In addition, while the models are fit using data on 13 birth cohorts, we focus on the 1980, 1985, and 1995 birth cohorts here since the vast majority of respondents were born on or adjacent to these years, and because it almost entirely spans the range of observed birth years. Discussing and visualizing all birth cohorts would lead to the same conclusions, but because of the added complexity, would be far harder to understand.

¹⁶ Differences in means across cohorts for all the covariates adjusted for in this analysis can be found in figure A1 of the online appendix.

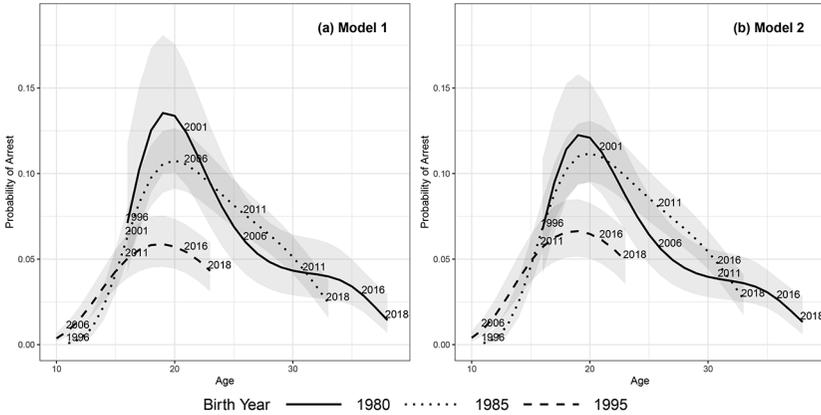


FIG. 1.—Probability of arrest at a given age for three birth cohorts (a) before and (b) after adjusting for differences between cohorts in composition and early life experiences (see table 1). Shading indicates 95% confidence intervals.

To assess these competing explanations of the results in figure 1(a), model 2 adjusts for five sets of differences in composition and early life experiences between cohorts: demographics; respondents’ low self-control, aggression, and anxiety/depression levels; family structure and status; family problems; and early life neighborhood social structure and processes (see table 1). The results from model 2 are visualized in figure 1(b), and the full regression details are in online appendix table A1. The age-arrest curves for the same cohorts are plotted as in figure 1(a).

Strikingly, comparing figure 1 panels (a) and (b) reveals no substantive difference in the general pattern.¹⁷ This is the case despite the general importance of the set of control variables in the study of crime and criminal justice, and despite variability in many of these controls across birth cohorts. The implication is that cohort differences in arrest trajectories are not confounded by the characteristics of the subjects, their families, or their early life neighborhoods, but rather are due to the strongly divergent historical periods through which the cohorts have aged. For all three cohorts, figure 1 also reveals the familiar shape of age-crime curves (Hirschi and Gottfredson 1983) and the rapid increase in arrest during early adolescence is almost indistinguishable across cohorts.

¹⁷ As a check, a similar model was run in which several of the variables—sex, race/ethnicity, parental education, immigrant generation, and household income—were interacted with the age terms. These variables are included in this list because they are thought to be particularly important for crime trajectories on the basis of prior research, or because they have a strong main (i.e., linear) effect. Whereas the earlier models control for the possibility that these confounders vary by cohort and shift the odds of arrest in the same way at all ages, this model allows for the possibility that they shift the odds of arrest more so at some ages than others. The results were virtually identical to fig. 1(b).

The Birth Lottery of History

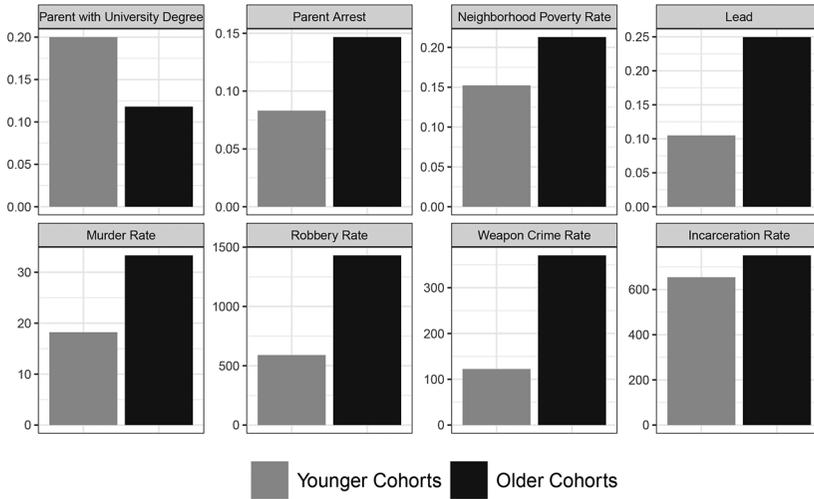


FIG. 2.—Select differences in composition and early life exposure between the cohorts born in the 1980s and those born in the mid-1990s. Shading indicates 95% confidence intervals.

Simultaneously, however, there is clear evidence of meaningful historical variation in arrest trajectories despite cohort controls. From figure 1(b), around the age of 16 there is a large divergence, with the probability of arrest for the 1980s birth cohorts rising sharply (albeit to different degrees), reaching a peak of 0.122 when the 1980 birth cohort was 19. In contrast, the 1995 cohort has an age-arrest curve that barely increases after 16, reaching a peak probability of arrest of 0.066 at age 19, before quickly returning to the levels they were in earlier adolescence. Thus, the extent of arrest at peak ages of criminal offending differs by a factor of nearly two between certain birth cohorts. All differences in the probability of arrest from ages 18 to 23 between the 1995 birth cohort and the 1980 and 1985 birth cohorts are statistically significant ($P < .05$).

Figure 1(b) also reveals that the 1995 cohort can be expected to be arrested in their early 20s at a rate that the older cohorts did not experience until they were in their 30s. For instance, at the age of 15 both the 1985 and 1995 birth cohorts had an arrest probability between 0.045 and 0.05. Yet the 1985 cohort did not experience this same probability again until it reached age 31, in sharp contrast to the 1995 cohort who did so at the age of 23. Put simply, there is an eight-year gap between the older cohorts and the youngest cohorts in the time it takes for arrest to lower to a given point. These large differences in the level and age-graded pattern of criminalization over the life course are brought about by the differing sociohistorical contexts through which individuals aged.

The results of model 2 in figure 1(b) are highly robust. In the supplementary online appendix, we demonstrate that results are not sensitive to assumptions about functional form by presenting a semiparametric alternative to the model. This analysis weights observations to balance the distributions of the compositional and childhood covariates across cohorts, by using stable balancing weights (Zubizarreta 2015), and employs splines to flexibly model the age-arrest relationships. We also demonstrate that the same conclusions are arrived at when incorporating sampling design and attrition weights. Furthermore, the online appendix reports on a formal sensitivity analysis demonstrating that the findings are robust to the possibility that there are unmeasured differences between the cohorts.

Model 3 is identical to model 2 except that it adds the sixth set of covariates, those measuring early life neighborhood crime and criminal justice context. To recall, we assess these separately to restrict the sample to people who stayed in Chicago throughout their childhood, about whom we have more complete measurement, and because these factors were changing dramatically over the period in which various cohorts were children.¹⁸ This is attested to by the large differences between the 1980s and 1990s birth cohorts seen in figure 2 in neighborhood crime and criminal justice–related variables. Overall, however, the results from model 3 presented in figure 3 are similar to those in figure 1(b) even though cohort differences in the probability of arrest at the same ages are narrowed somewhat. The 1980s birth cohorts, for example, still have arrest probabilities in adolescence and early adulthood that are nearly twice the size of those of the 1995 birth cohort. Despite the magnitude of changes in neighborhood crime and criminal justice context across the 1980s and 1990s, then, cohort differences in childhood exposure do not explain why some cohorts went on to be arrested at such persistently higher levels than others. This similarity with earlier results further demonstrates that the findings of this section are not driven by the subset of respondents who moved out of Chicago in their childhood.

INTRACOHORT INEQUALITIES AND HISTORICAL CHANGE

Although we have established large intercohort differences in the probability of arrest in late adolescence and emerging adulthood that arise from differences in the historical contexts in which aging occurred, these differences average over potentially important intracohort heterogeneity. In this section, we test for intracohort variation in the impact of historical changes, focusing on socioeconomic disadvantage and low self-control for the theoretical reasons given earlier.

¹⁸ Similar results are obtained if these variables are imputed for all children. As also shown in model 2, the results are robust to whether childhood lead exposure for cohort 0 is imputed or not.

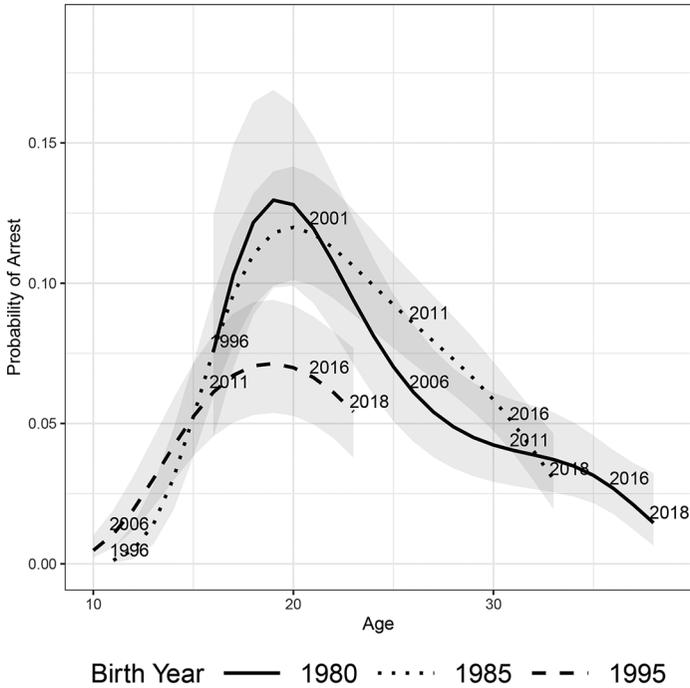


FIG. 3.—Probability of arrest at a given age for three birth cohorts, using the subsample who stayed in Chicago and further adjusting for differences in early life neighborhood crime and criminal justice context in addition to all other compositional and early life control variables. Shading indicates 95% confidence intervals.

Extending model 2, we first examine how arrest trajectories vary across cohorts for people who come from a given level of disadvantage and who are otherwise similar. An index of socioeconomic disadvantage was created using principal component analysis by combining household income, parental education, parental work status, and whether parents received TANF. The first component, onto which the different measures loaded about equally (with TANF receipt in the expected opposite direction as the others), accounted for the majority of the variation.¹⁹ This first component, our measure of (dis)advantage, was interacted with all the age and period terms and their

¹⁹ We focus on welfare usage, employment, income, and education given how fundamental these are in constituting individuals' socioeconomic (dis)advantage. Though clearly related, neighborhood disadvantage is a different concept, so we do not incorporate measures of it on theoretical grounds. Nonetheless, results are consistent when neighborhood poverty is incorporated into our index of disadvantage. Repeating the survival analysis presented below to contrast individuals who are disadvantaged or advantaged in terms of both individual and neighborhood poverty leads to similar substantive conclusions as well.

interactions.²⁰ Formally, then, this model allows the age-arrest relationship to vary as both a function of period, disadvantage, and their interaction, independent of all controls.

The results are visualized in figure 4. The 1995 cohort is again similar to the 1980s cohorts until around the age of 16, after which it diverges on a much flatter, less criminal trajectory. However, there are large differences in the extent of this divergence according to how disadvantaged individuals' backgrounds were. In this figure, we define disadvantaged individuals as one standard deviation below the mean and advantaged as one standard deviation above the mean. Figure 4(b) reveals that at the age of 19, for example, advantaged individuals from the 1980 birth cohort had a 0.107 probability of arrest, over 80% higher than the 0.059 probability for their otherwise similar counterparts born in 1995. Although substantial, that gap is smaller than that for those from disadvantaged backgrounds, shown in figure 4(a), for whom the 1995 birth cohort had a 0.062 probability of arrest at age 19 compared to 0.137 for the 1980 cohort, more than 120% higher.²¹ All differences from ages 18 to 22 between the disadvantaged 1995 birth cohort and the two 1980s birth cohorts plotted are significant ($P < .05$). In contrast, none of the differences between the advantaged 1995 birth cohort and those two 1980s birth cohorts at the same ages are significant ($P > .05$). That there are larger intercohort differences for disadvantaged individuals than advantaged individuals means that the former are particularly important in driving the overall intercohort differences reported in figure 1. A related, yet distinct implication of figure 4, as evidenced by the fact that the arrest curves do vary within each panel, is that coming from a given level of disadvantage does not carry static implications for subsequent life-course patterns of crime and criminalization. Rather, the relationship between disadvantage and arrest is historically contingent.²²

²⁰ The four variables used to create the socioeconomic status variable were also omitted, but beyond these alterations the model is identical to model 2.

²¹ This interaction is not an artifact of the logistic transformation, which is inherently interactive. We subset the data according to level of disadvantage and fit the curves separately for each subset data set. The results for the analogue of model 1 are qualitatively the same. In other models, we avoided the logistic transformation entirely by fitting the curves with smoothing splines or polynomials in linear models. The curves look very similar to those presented. Moreover, the survival analyses presented in this section, which show a different albeit related interaction, do not involve a logistic transformation. Similarly, the same robustness checks indicate that the results of the self-control analysis presented below are not affected by this issue.

²² As in model 3, restricting the sample to those who stayed in Chicago for their childhood, which enables using neighborhood crime and incarceration variables, leads to the same substantive results. The same is true for the analogous model for self-control that is discussed below.

The Birth Lottery of History

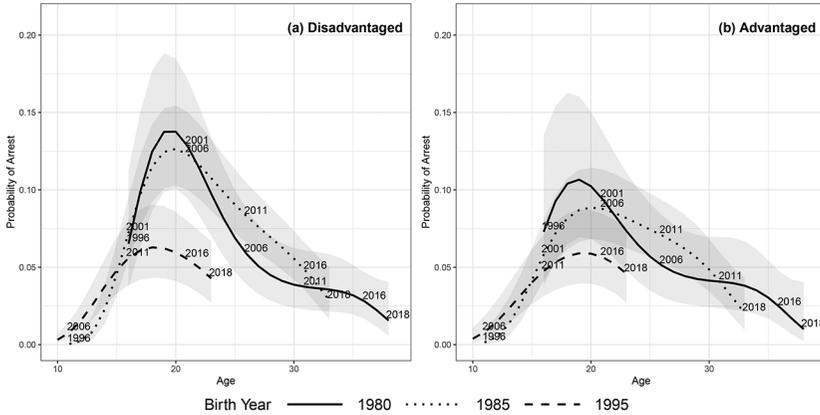


FIG. 4.—Probability of arrest at a given age for three birth cohorts, by socioeconomic status. Estimates for (a) socioeconomically disadvantaged individuals and (b) advantaged individuals. Shading indicates 95% confidence intervals.

A different way of understanding inter- and intracohort inequalities is to examine the extent to which different groups are ever arrested, and in the life stages during which first arrests tend to happen. We do so using Kaplan-Meier (1958) survival curves, which are used to estimate the survival function—in our case, the cumulative proportion of individuals who have never been arrested—at every age observed. We estimated these curves separately across cohorts and levels of disadvantage. The results, in figure 5, are even more pronounced than in figure 4.²³ The cohort born in the mid-1990s has a relatively low prevalence of arrest up to 2018, with about 25% having been arrested by their mid-20s, though this number is a few percentage points larger for the disadvantaged members of this cohort. Those born into advantaged families in the mid-1980s have similar patterns as those born in the 1990s—these three curves are nearly indistinguishable at the observed ages for which they overlap. In sharp contrast, fully 70% of individuals born in the mid-1980s to disadvantaged families had been arrested by their mid-20s. In early adolescence, arrest prevalence was similar for this group compared to the others, before quickly diverging around age 14. Compared to their

²³ In this plot, disadvantaged is defined as being in the first quartile and advantaged as in the fourth quartile of our disadvantage measure. Estimating survival curves requires stratifying the sample by disadvantage rather than plotting estimates held at specific values, as was done in fig. 4. Thus, the exact subpopulations being assessed vary between these analyses, in addition to the quantity being estimated (e.g., the same person being arrested at multiple ages can influence fig. 4 but not fig. 5). We do not include estimates from the oldest two age cohorts (cohorts 12 and 15) due to concerns about left censoring; that is, that some people were arrested before data collection began or before criminal justice record keeping became reliable.

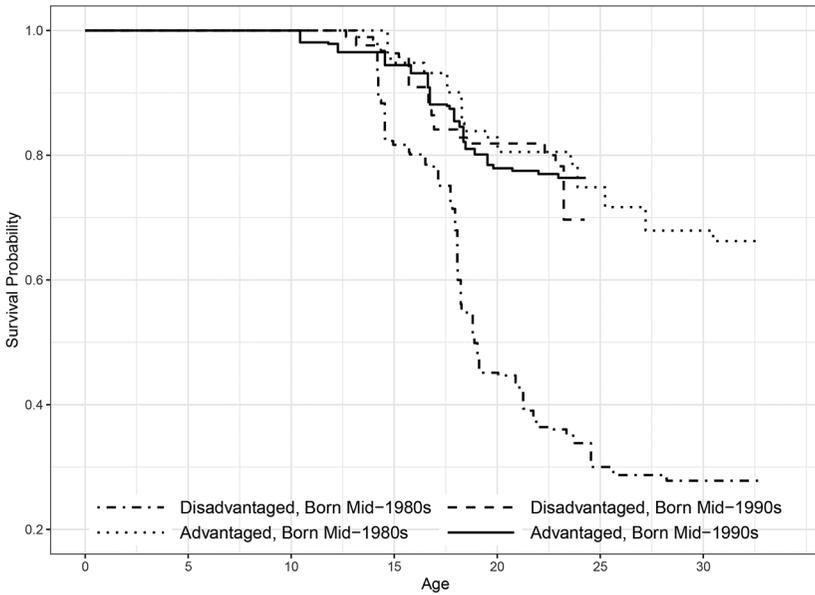


FIG. 5.—Kaplan-Meier survival curves for arrest, comparing cohorts born in the mid-1980s to mid-1990s and those in the bottom versus top quartile of socioeconomic advantage.

advantaged contemporaries, this divergence continued to grow until around age 25. Thus, while advantaged individuals exhibit no material variability in the prevalence of arrest across cohorts, and relatively modest variability in their trajectories of crime, disadvantaged individuals of different cohorts have very different criminal trajectories and cumulative prevalence of criminalization as a result of the changing historical contexts through which they have aged.

A similar but more nuanced conclusion emerges when examining race/ethnicity instead of disadvantage. Survival analysis reveals that the prevalence of arrests has dropped most for African-Americans, even though arrest is still more prevalent among the more recent black cohorts than among the older white and Hispanic cohorts. Although we lack the statistical power for a precise test of racial and ethnic differences, due to the relatively small sample size of whites and their relatively low arrest levels, the disadvantage results are not confounded by racial and ethnic differences. In particular, repeating the analysis for only blacks and Hispanics shows that more disadvantaged individuals from more recent cohorts have seen the largest reductions in arrests compared to similar members of older cohorts, which would not be true if the disadvantage findings were driven by racial/ethnic differences alone. That socioeconomic disadvantage is strongly concentrated among the black

and Hispanic population of Chicago means that the weakening of its relationship with arrest has benefited these populations the most.²⁴

We now examine how the relationship between low self-control and the course of arrest depends on the historical periods through which individuals age. We employed a variant of model 2 that allows cohorts' age-crime relationship to vary as a function of self-control, measured at an average age of 12, before the peak ages of arrest. This model relies solely on age cohorts 0 and 9—cohorts born in the mid-1990s and mid-1980s, respectively—for which the measures are most comparable.²⁵ Figure 6(a) presents the results for those with high self-control, defined as those individuals one standard deviation above the mean. The arrest trajectories begin to diverge at age 14 and grow to a maximum percentage point difference of 6.3 at age 22. Whereas the differences in these curves before the age of 19 are insignificant ($P > .05$), those from age 19 onward are significant ($P < .05$).

Figure 6(b) plots the analogous arrest trajectories among those with low self-control, defined as one standard deviation below the mean. Consistent with Gottfredson and Hirschi (1990), the arrest trajectories for those with low self-control are generally higher. That is, compared to their high self-control contemporaries, arrest is more likely at all ages after early adolescence. Comparing the curves of figure 6(b) to each other, we nonetheless see that the relationship between low self-control and arrest exhibits substantively meaningful historical contingencies. For example, whereas the probability of arrest at age 20 was 0.159 for those with low self-control born in 1986, individuals who were otherwise the same but born in 1996 had an arrest probability of 0.084. The differences between the curves from ages 20 to 23 are significant ($P < .05$), though insignificant before these ages ($P > .05$). This decline across birth cohorts separated by just one decade is not only statistically significant but also substantively large. This is most clearly illustrated by the fact that the arrest trajectories of *low* self-control individuals

²⁴ While there are also intracohort differences in arrest trajectories by immigrant status and sex, the extent to which these trajectories have changed across cohorts mirrors the results in fig. 1(b).

²⁵ The measure of low self-control used here is correlated with the socioeconomic status measure from the previous analysis at only -0.14, meaning that these two analyses are capturing distinct patterns, not simply using different labels for the same underlying phenomenon. We also repeated all analyses using the measure of low self-control assessed at age 15, with similar results, suggesting that measurement differences cannot account for the results. The other differences from model 2 are that we do not simultaneously control for childhood antisocial behavior because it taps a concept similar to low self-control, and we do not adjust for potential underreporting of arrests prior to age 15 since that is not a concern for these cohorts, though including it does not change the point estimates. Additionally, the birth years 1986 and 1996 are used instead of 1985 and 1995 because fewer cohort 9 individuals were born in 1985, making estimates for that birth cohort less precise.

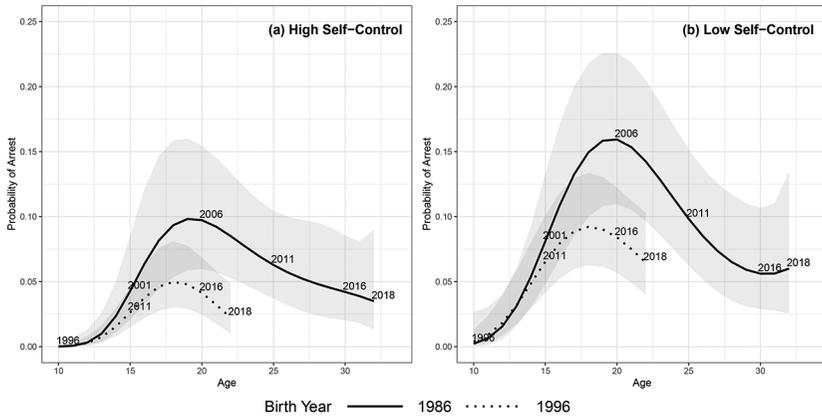


FIG. 6.—Probability of arrest at a given age for two birth cohorts, by self-control levels. Estimates for (a) high self-control individuals and (b) low self-control individuals. Shading indicates 95% confidence intervals.

born in 1996 are nearly identical to those of *high* self-control individuals born in 1986, seen in panels (b) and (a) of figure 6, respectively. However, that the arrest trajectories are lower for the 1996 cohort in both panels of figure 6 indicates that individuals at all levels of self-control contribute to the intercohort variation in arrest patterns.

Figure 7 presents Kaplan-Meier survival curves for individuals classified by whether they were born in the mid-1980s or mid-1990s and whether they were in the first quartile or fourth quartile of self-control. There are differences in the extent to which those with high self-control have experienced arrest. For example, at age 23, 36.7% of the older cohort had been arrested compared to 31.5% of the younger cohort. But these are small differences compared to those for individuals with low self-control: whereas 30.4% of such individuals in the younger cohort were arrested by the time they turned 23, 53.9% of the older cohort had been arrested by this age. Among the older cohort, differences in self-control translate into large differences in the prevalence of criminalization; among the younger cohort, they do not. Again, as was the case with the age-arrest trajectories, we see that low self-control individuals are about as well-off as high self-control individuals born one decade earlier in terms of the prevalence of arrest.

LINKING MACROSOCIAL CHANGE TO LIFE-COURSE PATTERNS

We have established important patterns of inter- and intracohort differentiation in arrest that are not compositional in nature, but we have not assessed

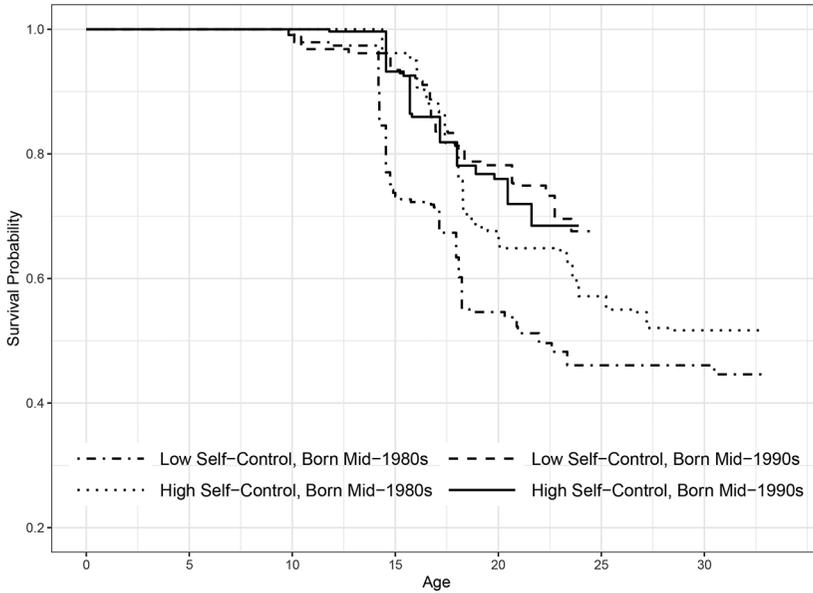


FIG. 7.—Kaplan-Meier survival curves for arrest, comparing cohorts born in the mid-1980s to mid-1990s and those in the bottom versus top quartile of self-control.

what it is about aging through different historical periods that drives the observed differences. Here, we turn to the question of whether the cohort differences in arrest trajectories reflect macrosocial changes in the proximate mechanisms of law enforcement, criminal behavior, or some combination of both.

As the intense attention given to the drug war has made clear, epitomized in the argument of Alexander (2012), increasing drug enforcement is widely thought to be the primary driver of mass incarceration. Because the police are the front end of the criminal justice system, this influential account of criminalization rests heavily on the drug war policing hypothesis. But this argument has been challenged (Travis et al. 2014; Forman 2017; Garland 2020), and its implications for our findings are especially unclear since, despite differing childhood exposure to neighborhood incarceration, everyone in the study came of age after incarceration’s ascent. This fact deepens the puzzle because it means that if changing drug enforcement patterns are part of the story, then large changes toward less aggressive enforcement by the police are possible within the era of mass incarceration, on a level that most theorizing on the American criminal justice system struggles to account for.

We begin our assessment by disaggregating arrests according to whether they contain a drug, property, or violence charge and repeating the main analysis, the analogues of model 2, as was presented in figure 1(b) for total

arrests.²⁶ Results are presented in the panels (a), (b), and (c) of figure 8. Comparing these panels, we see the same general pattern: the youngest cohort exhibits a much lower and flatter arrest trajectory.²⁷ That this is true of all offense types means that intercohort differentiation in arrest trajectories is not simply due to historically variable drug enforcement practices. Additionally, whereas the older cohorts' property and violence arrest patterns converge with the younger cohort's patterns by their early to mid-20s, no such convergence is seen for drug arrests. Our offense-specific analysis thus indicates that changing drug enforcement cannot explain away the gap in arrests at peak offending ages, though it is certainly part of the explanation.

We next take advantage of the granular nature of the arrest data to compare the arrests and charges of individuals born in 1980s to those born in the 1990s, when they were both between ages 18 and 22—the ages where the gap in arrests is most sizable. There were 96% more arrests per person among the older cohorts than younger cohorts.²⁸ Disaggregating these data further allows us to assess in a different way the extent to which this difference is driven by drug arrests. One answer comes from simply ignoring arrests with a drug charge and examining the remaining gap in arrests. Applying this restriction, arrests are 67% higher for the older cohort, again indicating that drug arrests account for some of the gap in criminalization, but far from all of it. However, this finding does not reveal how large the arrest gap would be if drug charging practices had stayed the same for the 1990s cohorts as they were for the 1980s cohorts. Answering this question requires calculating how many more drug arrests the 1990s cohorts would have experienced if they had faced the 1980s cohorts' drug arrest rates instead of their observed drug arrest rates, combining that figure with the 1990s cohorts' observed nondrug arrests, and comparing the resulting hypothetical arrest rate to the observed arrest rate for the 1980s cohorts. Estimating this counterfactual, the arrest gap is 43% higher for the 1980s cohorts, meaning that 55% of the arrest gap between ages 18 and 22 is explained by changing drug arrest patterns.²⁹

²⁶ An arrest may contain a charge of a specific type even if the arrest was fundamentally for a different reason, as we elaborate upon below. However, as the preceding analysis affirms, the conclusions drawn from the offense-type plots are not misleading as a result.

²⁷ As before, the same substantive conclusions hold if we restrict the sample to those who spent their childhood in Chicago and include the neighborhood crime and incarceration variables.

²⁸ The number of arrests per arrestee is the same across these groups, meaning that the arrest gap reflects differences in the prevalence, rather than incidence of arrests.

²⁹ Calculated as $\frac{96\% - 43\%}{96\%} \approx 55\%$. This figure assumes that any arrest with a drug charge was truly a drug arrest and not incidental to other charges. The data support this assumption. Among the 1980s cohorts' drug arrests, 95% did not contain a charge for violent, property, or weapon crimes, a figure which is 76% for the 1990s cohorts. The lower number for the 1990s cohorts does suggest their drug arrests were more often incidental, but such small differences do not alter the main conclusion. Assuming 95% of the 1980s cohorts'

Another possible mechanism is that our results reflect changing patterns of drug use, rather than changing law enforcement practices or priorities. Comparing drug-use trends among Chicago adolescents to CPD drug arrest patterns, we see that this is not the case. Figure 8(d) plots the number of drug arrests (scaled to be relative to 2004, the year CPD drug arrests peaked) against representative estimates drawn from the CDC's Youth Risk Behavioral Survey (YBRS) of the percentage of Chicago high schoolers who have ever used a variety of drugs. The prevalence of most types of drug use is not only quite low but also stable going back multiple decades. If anything, there is a slight upward trend in marijuana use, but no evidence of falling drug use among Chicago teens.³⁰ Conversely, the CPD began to make far fewer drug arrests starting in the mid-2000s, when the mid-1990s birth cohorts were still children. Comparing 2003 to 2013—when people born in 1985 and 1995 would have been 18, respectively—all drug-use levels among teenagers are similar, but none are lower among the 1990s cohorts, whereas CPD drug arrests fell by 38.3% between these years. From these patterns, we conclude that changing drug enforcement, not drug use, explains cohort differences in drug arrests.

Drug enforcement patterns are not the only aspect of policing in Chicago that has varied in recent decades. Figure 9, which shows various policing and crime trends at the city level over time since 1996, makes this clear.³¹ Shown in panel (a), disorder arrests constitute a particularly striking example: since 1996—a year in which they made up 22.2% of all CPD arrests—they have fallen by over 96%. This dramatic decline exceeds even drug arrests, which fell by 76% since 1996. Disorder arrests are known to be more discretionary than arrests for most other types of crime. In fact, broken-windows-style policing was rooted in the idea of increasing the discretionary policing of disorder—aptly named “order maintenance” (Wilson and Kelling 1982). In the era of mass incarceration and a presumed culture of control,

drug arrests were not incidental as opposed to 76% of the 1990s cohorts' drug arrests would mean that changing drug arrest patterns accounted for 58.3% of the arrest gap.

³⁰ While these data are not limited to the PHDCN sample, both surveys drew representative samples from the same population at the same ages, so we see little reason to think the PHDCN children would be particularly different in drug-use trends. Illinois-wide drug use data from the YBRS yield the same conclusions, despite drug-use levels differing slightly from those in Chicago alone. Among Chicago teenagers, inhalant, ecstasy, and injected drug use (not plotted) follow patterns similar to the hard drugs shown in fig. 8(d), i.e., relatively rare usage with only small trends in usage over time.

³¹ Data for this plot are drawn from the FBI's Uniform Crime Reporting (UCR) crime statistics and the CPD's annual reports. In cases where both sources have data on the same indicator in the same years, they tend to agree quite closely. The major exception is disorder arrests in 1999, for which the CPD report figure is nearly 100% larger than the UCR figure. The latter is the figure used in fig. 9.

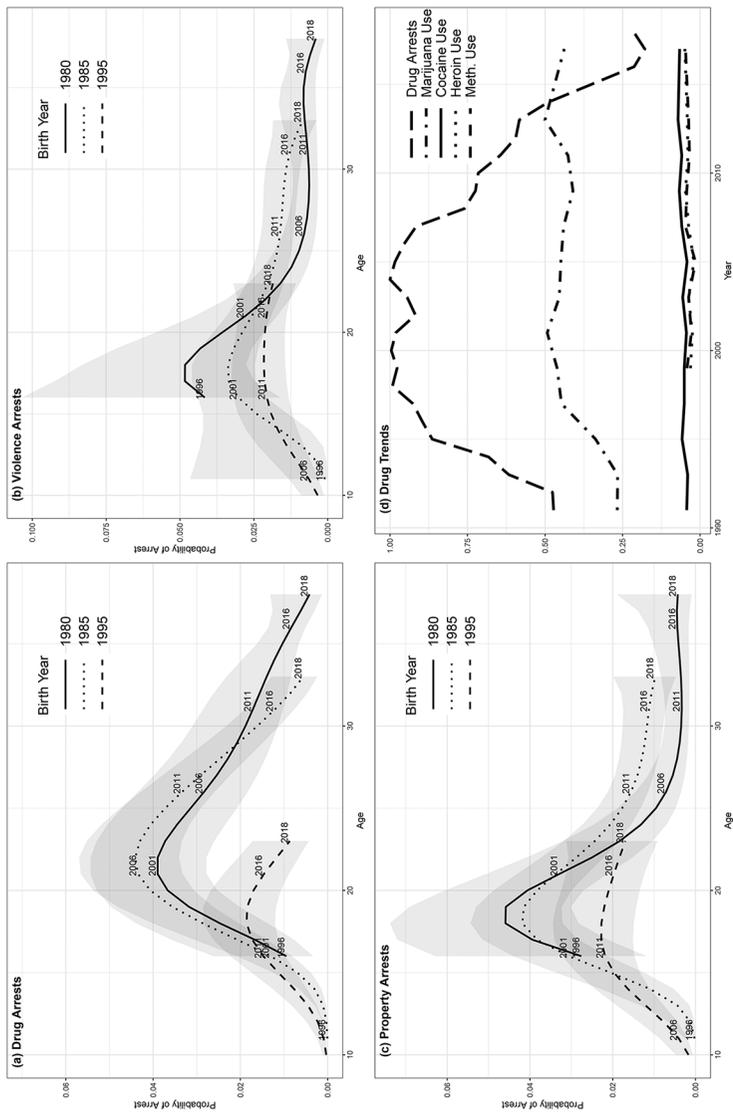


FIG. 8.—Probability of arrest at a given age for three birth cohorts, conditional on compositional and early life differences, for (a) drug arrests, (b) violence arrests, and (c) property arrests. (d) The number of drug arrests made by the CPD—scaled relative to 2004, the year drug arrests peaked—compared to the prevalence of select drug use among Chicago high schoolers. Shading indicates 95% confidence intervals.

such a dramatic and stable decline is unanticipated, and it is unlikely to be the product of actual disorderly conduct rather than changing policing styles. Reflecting the aggregate trend, no 1990s birth cohort member was arrested for disorderly conduct between ages 18 and 22, compared to the 1980s cohorts for which they made up 3.5% of arrests at these ages. Because disorder arrests made up only 3.5% of the total, they cannot be a particularly important driver of the overall cohort arrest gap, but the overall decline in disorder arrests nonetheless reflects a large change in the CPD's enforcement practices.

That changing drug arrest patterns account for more of the arrest gap than disorder arrests, despite the latter having fallen more, makes sense when thinking more precisely about the historical contexts in which these cohorts reached peak offending ages. As seen in figure 8(d), drug arrests did not begin falling until 2004, by which point disorder arrests had already fallen by 72% since 1996 (plotted in fig. 9[a]). In other words, most of the decline in disorder arrests happened before anyone was older than their early teens, whereas most of the decline in drug arrests happened between when the 1980s and 1990s cohorts were at the ages of peak arrest.

Figure 9(b) shows that the number of officers employed by the CPD has been relatively stable from 1996 to 2018. Another complexity is that violence index and property index arrests have fallen substantially as well, as seen in figures 9(c) and 9(d), respectively. Violence arrests have fallen by 66% and property arrests by 78% since 1996. Notably, the downward trends in these types of arrests track the decline in the number of violent and property offenses that were reported to the CPD, with the violent index crime falling by 60% and property index crime by 55% since 1996.³² The declines in arrests and offending as indexed by crime rates are not exactly proportional, however. Between 2006 and 2016—at which point the 1985 and 1995 cohorts would have been 21 years old—the rate of arrests per offense declined. That there were 12% fewer officers in 2016 compared to 2006 (see fig. 9[b]) is one possible explanation why arrests fell faster than offenses. That said, the extent to which the trends in both agree with each other indicates that changing property and violence arrest patterns mostly reflect changing offense patterns.

To evaluate this argument more formally, we conduct a counterfactual test by examining how arrest patterns would have changed in two hypothetical worlds: one in which the intensity of policing stayed at 1996 levels and crime varied in the way it actually did, and another in which crime stayed at

³² Several respondents moved out of Chicago proper after the study's start, but overwhelmingly within Illinois, particularly to metropolitan Chicago. In this regard, it is worth noting that violent and property crime rates for Illinois as a whole track Chicago trends almost exactly ($r > 0.99$).

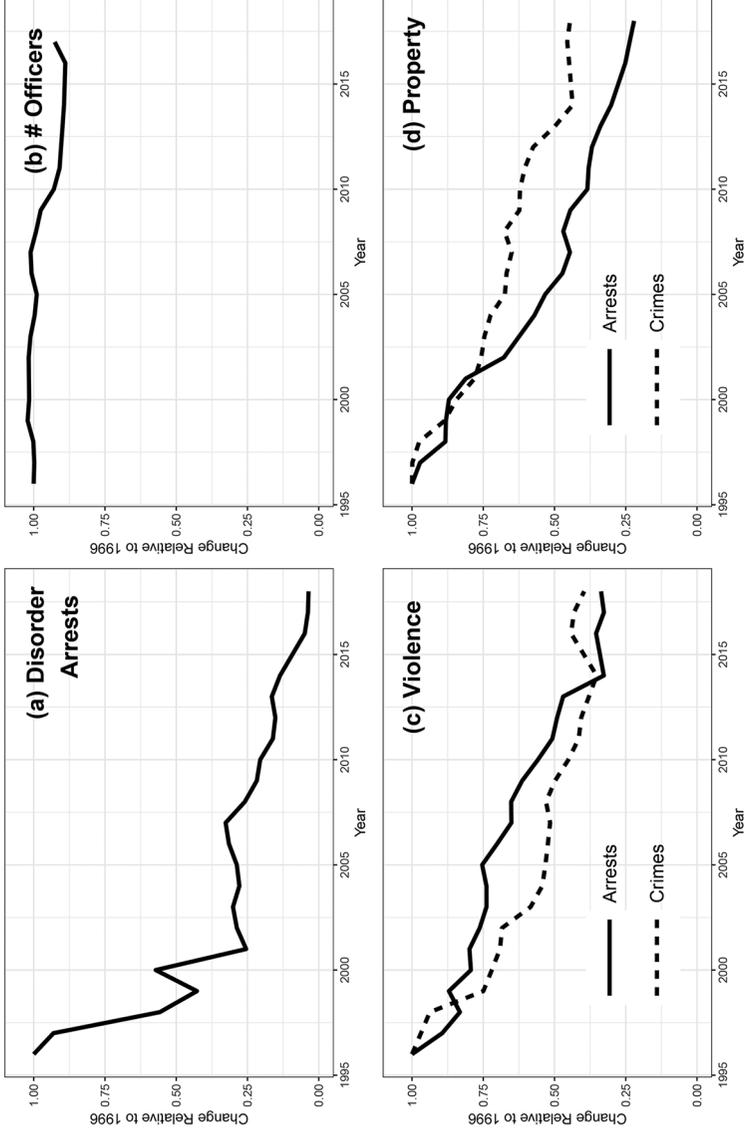


FIG. 9.—Changes in crime and enforcement patterns in Chicago relative to 1996 levels: (a) disorder arrests; (b) the number of sworn police officers; (c) violence offenses and arrests; and (d) property offenses and arrests.

1996 levels but policing intensity varied in the way it actually did. This test rests on the following simple model of how arrests are generated: for a given type of crime, the number of arrests observed is equal to the number of crimes multiplied by the average probability that a crime will result in an arrest, which we refer to as “intensity” since it represents the chance that police will make an arrest for a given crime. We know the value of both inputs—crime and intensity; in order to generate hypothetical arrest figures, below we alter these inputs in ways that differ from reality.

We conduct this test separately for violence and property index arrests, with results presented in panels (a) and (b) of figure 10, respectively. The solid lines represent the amount by which violence and property arrests fell since 1996. In figure 10(a), the dashed line represents how much arrests would have fallen by if police intensity stayed the same and violent crime fell as it did, whereas the dotted line represents what would have happened if crime stayed the same, but intensity varied as it did. Clearly, the decline is driven primarily by changing crime patterns. For much of the period, intensity increased as crime fell. Even in 2018, where intensity was at lower levels than 1996, violence arrests have fallen by 66% since 1996, compared to the 60% drop predicted in a world where only crime was changing, and to the 15% drop predicted in a world where intensity varied but not crime.³³ For property arrests, as shown in figure 10(b), both hypothetical worlds predict less of a decline than occurred, and both are quite close to each other going back to 1996. Overall, then, the results indicate that both changing crime and the intensity of police enforcement contributed about equally to the decline in property arrests, with changing crime being slightly more important.

DISCUSSION AND IMPLICATIONS

The members of any cohort are entitled to participate in only one slice of life—their unique location in the stream of history. Because it embodies a temporally specific version of the heritage, each cohort is differentiated from all others. (Ryder 1965, p. 844)

What if individuals born into the same socioeconomic conditions or who experience similar childhoods nonetheless face significantly different social worlds growing up or entering early adulthood? What if two groups of individuals, whose only early life difference is the year in which they are born, have immensely varied experiences over the course of their lives, not because of *who* they are, but because of *when* they are? The sociohistorical environments of young people potentially give rise to a distinctive

³³ Using 2006 as the benchmark year instead of 1996 also leads to the conclusion that both mattered, but intensity and crime both become important explanations.

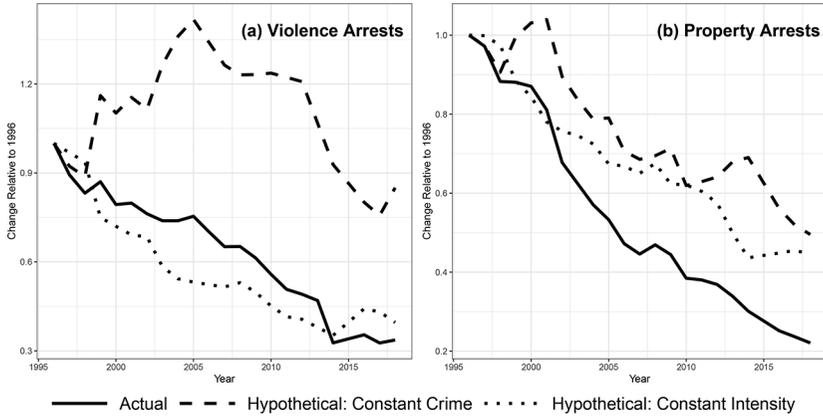


FIG. 10.—Arrest patterns compared with two hypothetical worlds, one with constant crime and the other constant law enforcement intensity for (a) violence arrests and (b) property arrests.

shared experience, one with lasting influence for their future life trajectories. In this way, social change may produce important cohort differentiation in coming of age (Ryder 1965; Elder 1974), motivating a different perspective on human development.

Despite some of the largest social changes of recent times—the rise of mass incarceration, aggressive policing tactics, and the “great American crime decline”—this core idea has been neglected in the study of age, crime, and the life course, which remains individualized in style, data, and theory. By contrast, in this article we systematically examined cohort differentiation in the critical first stage of entry into the criminal justice system—arrest. We focused on how and why the age-graded course of arrest depends upon the historical period in which it occurs and, consequently, how this gives rise to cohort differentiation. Cohorts also vary internally, implying that historical change is not necessarily uniform—its influence on life experience is likely to interact with features of social stratification. We thus sought to unite a macrosocial focus on historical changes that differentiate the life experience of birth cohorts with inequalities in arrest over the life course by socioeconomic disadvantage and low self-control, two of the major hypothesized causes of crime.

To study these dynamic processes, we introduced an original multicohort design of over 1,000 children originally from Chicago, born up to 17 years apart, who were studied from 1995 through 2018. The sequential cohort design, combined with the multiple stages of data collection, rich measurement, and an analytical strategy to make full use of the data, offer a unique vantage point from which to examine life-course trajectories during a time

of rapid social change, including both intra- and intercohort variation in the life-course consequences of social change.

The results indicate how social change altered the experience of criminal justice contact in adolescence and early adulthood in meaningful ways. The probability of being arrested is nearly twice as large during the peak ages of delinquency in adolescence for cohorts born in the early to mid-1980s compared to younger cohorts born in the mid-1990s. We also find a much faster rate of decline in the probability of arrest in early adulthood for the younger birth cohorts, yielding an eight-year gap compared to older cohorts. Crucially, we show that these findings are not driven by differences between birth cohorts in alternative explanations such as individual dispositions, demographic and family background, economic status, or early life neighborhood environments, several of which were large. Rather, the substantial cohort differentials in arrest in late adolescence and the course of desistance in adulthood arise from the distinct sociohistorical environments through which each cohort aged. The results thus quantify the power of social change on patterns of human development and indicate in new ways how widely divergent early life circumstances are far from destiny and arguably secondary to social change.

Perhaps paradoxically given these results, the general shape of Hirschi and Gottfredson's (1983, p. 555) "brute fact" of age and crime is still evident, especially the escalation of arrest in early adolescence and its later decline. At the same time, large cohort differences in the chances of arrest at peak ages of offending and in subsequent rates of decline that cannot be explained by developmental or "risk exposure" differences originating earlier in childhood call for a renewed focus on the importance of how shared historical environments shape life trajectories. While the life-course perspective is frequently invoked to make sense of patterns of criminal behavior (Sampson and Laub 1993), it is only by broadening its approach to the study of each birth cohort's unique location in the stream of history that such patterns can be fully revealed and understood.

It would be a mistake, however, to view our approach as denying the role of more microlevel individual, family, or neighborhood factors. For one, we showed that those from advantaged socioeconomic backgrounds have seen relatively modest intercohort variation in arrest over the life course, whereas those from disadvantaged backgrounds have seen large changes. Remarkably, 70% of individuals born in the mid-1980s to disadvantaged families had been arrested by their mid-20s, compared to about a quarter of the disadvantaged members of the younger cohort from the mid-1990s. The implication that follows is that the experiences of those from disadvantaged socioeconomic backgrounds are disproportionately responsible for driving intercohort variation in the age-crime relationship. This varying disadvantage penalty—the extent to which disadvantage translates into

criminality and criminalization as a function of historical context—is testable in future research.

Another implication bears on a dominant concept in studies of crime—self-control. Although there is some evidence that low self-control individuals have been more important in creating intercohort differentiation in arrest than high self-control individuals—particularly reflected in their larger cohort differences in the prevalence of arrest—individuals of all levels of self-control have contributed to intercohort differences in the probability of arrest at a given age. Put differently, there is minimal statistical interaction between self-control and intercohort inequalities in arrest. In this case, the impact of low self-control on crime theorized by Gottfredson and Hirschi (1990, 2019) is supported in all cohorts.

Looked at differently, however, we can ask another question. What if individuals with similarly low levels of self-control have enormously varied outcomes as a function of when they reach the vulnerable ages associated with crime? We believe the answer is telling: reductions in the chances of arrest have been so large that they have made the *low self-control people of one cohort nearly indistinguishable from the high self-control people of a cohort born just one decade earlier*. The role of self-control therefore depends on age-graded historical context, such that knowing self-control levels alone is not particularly informative about the magnitude of future arrest trajectories.

The implication that speaks to the long-standing debate between individual difference (or propensity) and life-course theorists is that one cannot fully understand how criminal tendencies or even early life socioeconomic opportunities or turning points relate to trajectories of crime until cohort differentiation is accounted for. We should be looking not only at what was wrong (or virtuous) with the individuals of a particular cohort, as is common, but also what was wrong (or virtuous) with the larger social environment during the historical period through which they happened to come of age. By this way of analyzing the life course, individual characteristics like self-control are less stable or, in effect, less individual than commonly conceived, with manifestations that vary by sociohistorical context. Socioeconomic disadvantage, a mainstay of sociological theories of crime, is even more dependent on history. Individual differences and social inequality in life experience are thus fundamentally in interaction with the birth lottery of history and should be studied as such.

Disentangling Social Change

The usual suspects examined in the rapid growth of literature on mass incarceration, such as the drug war and aggressive policing, do not explain major differences in arrest patterns by cohort in the ways that are likely expected.

To be sure, changing drug arrest patterns do help to explain cohort differences in criminalization, and differences in drug arrests are the result of historically variable drug enforcement practices rather than cohort variation in drug-use patterns. However, these practices do not neatly account for cohort differences in criminalization over the life course. By any reasonable assumption, drugs account for a little over half of the gap in arrests at the peak ages of offending, leaving half the story unexplained. Moreover, despite the standard drug war account, where after being declared in the 1970s and ramping up in the 1980s and 1990s people have been living in the era of control and aggressive policing, we see that drug enforcement is becoming *less* aggressive on a scale that translates into sizable differences in criminalization over the life course. Within the drug war era and more broadly the era of mass incarceration, locally and temporally specific enforcement patterns yield real consequences.

Drug enforcement is not the only way in which enforcement patterns changed. From 1996 to 2018, nondrug arrest patterns declined roughly as much as drug arrests, and more so for disorderly conduct, which declined over 90% despite the expectations of broken-windows policing and the common interpretation of policing in recent decades as increasingly aggressive. We also ruled out police force size as a major factor given its relatively constant state over the decades. The fact that property and violence arrest patterns are so responsive to reported crimes further indicates that changing arrests for them are largely, though by no means entirely, driven by changes in criminal behavior. It follows that cohort differences in property and violent offenses at least partially reflect real behavioral differences between cohorts at the same age and not just criminal justice reactions.

Changes in criminal behavior and policing patterns, in turn, may be driven by other social changes that differentiated the contexts through which the study's cohorts came of age. Although rates of poverty from the mid-1990s to 2018 did not vary greatly, parts of Chicago have undergone urban revitalization, including from immigration, repopulation, and gentrification in select areas (Sampson 2012). Even neighborhoods largely bypassed by such processes have seen important changes. For example, many of Chicago's largest, economically distressed, and most troubled public housing projects were replaced with mixed-income housing in nearby communities (Chaskin and Joseph 2015), with potential lasting consequences for criminal behavior. Moving beyond economic explanations, Sharkey, Torrati-Espinosa, and Takyara (2017) show how community-based organizations emerged in the United States as an endogenous response to neighborhood violence, proactively intervening with youth programs in a collective effort to bring down crime rates. Part of the crime reduction and increase in the safety of public spaces for the younger cohorts in our study is thus arguably due to the collective efficacy of residents and local organizations (see also Sampson 2012,

pp. 394–404; Sharkey 2018). The rise of private security forces and household security measures may have further reduced opportunities for crime, and the rise of technologies such as smartphones, video games, the internet, and social media have transformed the lives of young people. The amount of time engaged with these technologies may reduce potentially criminogenic situations such as unstructured socializing (Baumer, Cundiff, and Luo 2021), and the rise of social media may have substituted digital conflict for physical confrontations (Stuart 2020).

Social change—whatever the ultimate source—not only produces cohort differentiation, but this differentiation in turn can be a powerful force producing further social change (Riley 1987, p. 4). Our findings imply that such change occurs not just because of changing age distributions or pure period effects, or through cohort differences in composition or early life experiences, such as lead exposure, but also because adolescence and young adulthood are strongly shaped by the historical context in which they occur. Scholars seeking to make sense of social changes must therefore incorporate age-specific period effects over the life course into their analyses (see also Cook and Laub 2002, pp. 20–22; Lu and Luo 2020; Luo and Hodges 2020; Shen et al. 2020).

Conclusion

Although our sample is limited to birth cohorts originally from Chicago, the historical changes we have examined are not unique to the city, and the reductions in arrest among the younger cohorts relative to the older cohorts mirror a more general pattern of reduced problem behaviors among today's youth (Twenge and Park 2019; Laub and Sampson 2020, p. 166). The importance of this broader transformation has gone largely unappreciated, and the reasons for it remain to be precisely specified. Thus, while our sample is limited to one outcome among cohorts from one city, the social changes at play and the life-course consequences extend far beyond Chicago. For these reasons, we do not view our study as a definitive account of the mechanisms driving intercohort differentiation in arrest but as a framework and call to those studying crime and criminal justice to take this line of inquiry as a frontier in life-course and historically motivated research.

Another potential limitation is that our power to study historical change is limited by the temporal window of just over 20 years. Looked at differently, however, that we demonstrated large differences in such a short temporal window of change is important in its own right given the more usual focus in the social sciences on the *longue durée* of history (Patterson 2012). Sometimes social change is abrupt, producing large disparities in a relatively short period. These have been eventful times indeed (Clemens 2020). The past quarter-century alone has brought forth noteworthy changes, in some cases

turning almost on a dime, in violent crime, incarceration, policing, and, recently, widespread protests against police brutality. Adolescence and young adulthood are turbulent times when such social change matters greatly. In the past two decades, “deaths of despair” from suicide, drug overdose, and alcoholism have also risen dramatically (Case and Deaton 2020), and more recently the COVID-19 pandemic has upended many aspects of everyday life, such as the nature of work. Coming of age and beyond has been transformed for successive birth cohorts differentially experiencing these relatively short-term but nonetheless substantial historical changes, imparting enduring consequences over the life course that will in turn spur new transformations.

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