The Enduring Effects of Cohort Characteristics on Age-Specific Homicide Rates, 1960–1995[1]

Robert M. O'Brien, Jean Stockard, and Lynne Isaacson
University of Oregon

In the past decade, young people in the United States have been two to three times more likely than in the two previous decades to commit homicides, while those 25 years and older have been less likely to commit homicides than were members of their age groups in the earlier time period. These changes in youth homicide rates are associated with two cohort characteristics that are theoretically linked to criminality: relative size of cohorts and the percentage of cohort members born to unwed mothers. These effects persist throughout the life span, are independent of age and historical period, and can explain fluctuations in homicide arrest rates before the recent upturn.

INTRODUCTION

Until the mid-1980s, it appeared that the relationship between age and homicide was relatively stable, so stable that some scholars described it as "invariant" (Hirschi and Gottfredson 1983). This relationship, however, changed markedly in the past decade as the likelihood of a young person in the United States committing homicide increased two- to threefold. For those 14–17 years of age, the homicide rate climbed from 6.2 per 100,000 in 1984 to 19.1 per 100,000 in 1994, and, for those ages 18–24, the rate rose from 15.3 per 100,000 in 1984 to 25.3 per 100,000 in 1994. In contrast to the pattern for younger cohorts, homicide rates for people 25 years old and older declined during this same period, with rates dropping from 6.3

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per 100,000 in 1984 to 4.7 per 100,000 in 1994 (Maquire and Pastore 1996).[2]

This epidemic of youth homicide has received much attention, with most explanations focusing on the introduction of crack cocaine and the ensuing increase in the possession of guns by adolescents (Blumstein 1995; Blumstein and Cork 1996; Cook and Laub 1997).[3] While such explanations seem plausible, they essentially are ad hoc. A more comprehensive explanation would apply not just to the period after the mid-1980s but to earlier historical periods as well.

In this article, we explain the relative increase in youth homicides in comparison to those age 25 and over without invoking an explanation specific to the period after the mid-1980s. We build on the traditions of cohort theory and research as well as research in crime and delinquency and examine changes in the characteristics of cohorts born during 1915–19 and 1975–79. We account not only for the relative increase in youth homicides since 1985 but for more subtle changes in the relationship between age and homicide rates between 1960 and 1985. Changes in these cohort characteristics are associated with the homicide rates of each of the cohorts throughout their life spans independent of the effects of age and historical period.

RELATED LITERATURE

Over 30 years ago, Norman Ryder (1965) described how birth cohorts move in a two-dimensional space of time and age. This makes one cohort's experiences different from those of any other's. For instance, infants experience historical events, such as an economic depression or war, differently than those entering the labor force, those of middle age, and those who have retired. A major tenet of cohort theory and research asserts that certain events can produce *lasting changes* in the attitudes and behaviors of cohort members. These changes depend upon cohort members' ages when the events occur, and these effects are analytically distinct from those associated with age and historical period. Thus, living through the Depression produced lasting changes, which varied from one birth

[2] Homicide rates for the total population of the United States rose dramatically from 1962 (4.6 per 100,000) to 1974 (9.8 per 100,000) then fluctuated moderately, reaching a high of 10.2 per 100,000 in 1980 and a low of 7.9 per 100,000 in 1985 and 1986. From 1974 to the present, homicide rates have fluctuated up and down but do not appear to be systematically increasing or decreasing (O'Brien 1996).

[3] The entire 1996 fall issue of *Law and Contemporary Problems,* entitled "Kids, Guns, and Public Policy," was devoted to this epidemic. The 1995 fall issue of the *Journal of Criminal Law and Criminology,* entitled "Guns and Violence Symposium," also contained several articles focused on this problem.

cohort to another (e.g., Elder 1974, 1979; Elder, Modell, and Parke 1993). Such cohort effects have been found in a variety of areas such as political orientation and voting (Firebaugh and Chen 1995; Alwin and Krosnick 1991), racial and sex role attitudes (Mason and Lu 1988), intellectual skills (Alwin 1991), parental values (Alwin 1990), and criminal behavior (O'Brien 1989). Some of these effects may reflect changes in strategic styles—variations in behavioral strategies including criminality (Vila 1994).

Determining that cohorts vary is different than explaining why these effects occur. One way of considering this question is to view the historical periods in which cohorts are born as opportunity structures, settings that provide both opportunities and constraints for members of a birth cohort (e.g., Elder 1996). These opportunities and constraints may result from historical events, such as wars or economic depressions. They may also derive from demographic characteristics associated with birth cohorts such as sex ratios, relative size, and family structures. Two demographic aspects of cohorts that are theoretically linked to criminal behavior are relative cohort size and family structure.

Relative Cohort Size

Many analyses of cohort variations involve Richard Easterlin's (1978, 1987) hypothesis that members of relatively large cohorts experience disadvantages that persist throughout their life spans. Some of these disadvantages involve economic opportunities—the relatively greater number of workers in these cohorts leads to a buyers' market for employers and thus to lower wages and greater unemployment for members of these cohorts. These in turn may lead to delayed marriage and smaller families (Easterlin 1987). These consequences of relative cohort size might be labeled job-market effects.

We theorize that other effects of relative cohort size are related to community and family resources. Relatively large cohorts overload institutions of social control and stretch the family and community resources available to cohort members. Members of these cohorts grow up with more children per parent, more children per classroom, and more children per counselor (O'Brien 1989). Just as children in families with excessive demands relative to their caregivers' resources receive fewer family resources, children in relatively large birth cohorts have available to them diminished community resources. This involves lower levels of supervision and attention from parents, teachers, counselors, and other adults as well as more crowded homes and schools.

Researchers have found some evidence of a relationship between relative cohort size and homicide arrest rates (O'Brien 1989; Steffensmeier, Streifel, and Shihadeh 1992). Given this evidence, and because of job market, community, and family resource effects, we hypothesize that relatively large cohorts will have higher crime rates in comparison to relatively small cohorts, once the effects of age and period are controlled.

Family Structure

At the individual level, the relationship of family interactions and structure to delinquent behavior is well documented. Research indicates that children from homes with insufficient supervision and attention are much more likely to be delinquent, and because it is harder for one parent to provide as much supervision, consistent discipline, or attention as two can provide, children from single-parent homes are more at risk than those from two-parent homes (e.g., Dishion, Patterson, Stoolmiller, and Skinner 1991; Farrington 1989; Furstenberg and Hughes 1995; Hetherington, Cox, and Cox 1978; Huesmann, Eron, Lefkowitz, and Walder 1984; Loeber 1982; Nagin and Paternoster 1991; McLanahan and Sandefur 1994; Patterson, DeBaryshe, and Ramsey 1989). A number of contemporary theories based in sociology, criminology, and developmental psychology have incorporated the links between ineffective child-rearing practices, the development of low self-control, and later criminal propensities or behavioral strategies (e.g., Gottfredson and Hirschi 1990; Sampson and Laub 1993; Moffitt 1993; Patterson and Yoerger 1993; Vila 1994).

A literature within the broad perspective of social structure and personality further supports the emphasis on the relationship between childhood family relations and criminal behavior but focuses on males and aggression. These theorists point to the special role of fathers in helping young boys control aggressive tendencies and develop a secure gender identity. They suggest that when fathers are absent from their lives, boys are more apt to exhibit signs of "compulsive masculinity," including heightened levels of aggression (Coltrane 1988, 1992; Holter 1970; Parsons [1947] 1954). In support of this approach, cross-cultural data indicate that males in subcultures and societies with less father-son contact, especially in the early years of life, are much more likely to display characteristics typical of compulsive masculinity and hyperaggression (Tiller 1958; Coltrane 1988, 1992).

Researchers have also highlighted the relationship between single-parent families and poverty. Especially important from our perspective is the relationship between single-parent families and youths growing up in poverty. For instance, in 1994, the rate of poverty for children under 18 who lived in married-couple families was 8.3 per 100. For children who lived in female-headed households, the rate was 44.0 per 100 (O'Hare

1996). These children are less likely to live in "safe and desirable" neighborhoods, to obtain adequate medical care, to be successful in school, and to have adequate day care and after-school care (National Research Council 1993; McLanahan and Sandefur 1994). As more members of a cohort grow up in poverty, more children in that cohort are likely to have peers who have experienced poverty.

At the macrolevel, a number of studies document the relationship between family disruption and crime (Blau and Blau 1982; Jacobs and Helms 1996; Huff-Corzine, Corzine, and Moore 1986; Messner 1983; Messner and Sampson 1991; Sampson 1985, 1986; Sampson and Groves 1989; Williams 1984; Williams and Flewelling 1988) and find that the relationship is especially strong for juvenile crime (Sampson 1987). Sampson (1987, pp. 352–53) accounts for this relationship with a theory of community social organization, noting that increased family disruption decreases the effectiveness of both formal and informal social controls within the community. This contextual effect is independent of the influence of individual family composition, since all youngsters in a community are influenced by the decreased social control or "social disorganization" (see esp. Sampson and Wilson 1995, p. 44).

None of this research explores the influence of family disruption on cohort variations in criminal behavior across the life span of cohorts, although several authors have hypothesized that family disruption in an earlier period may result in increases in criminal behavior in later periods. For instance, Sampson and Wilson (1995) state that "the roots of urban violence among today's 15–21-year-old cohort may stem from childhood socialization that took place in the late 1970s and early 1980s" (p. 53), a time when family disruption began increasing sharply. Similarly, Vila (1994, p. 342) hypothesizes that individual life experiences, such as those related to family life, nurturance, and social control, will have a lagged effect on criminality and crime rates. Although their analysis focused on other issues, results in Jacobs and Helms (1996) indicate that a five-year moving average of the percentage of children born out of wedlock, which is lagged 19 years, is related to prison admissions in the United States from 1950 to 1990.

Based on both the microlevel and macrolevel analyses and theorizing, it is reasonable to expect that an increase in the proportion of a cohort growing up in single-parent homes is associated with an increase in that cohort's criminogenic tendencies. This relationship is produced in at least two contexts. First, within the single-parent family, there is an increased likelihood of poverty, which diminishes a family's abilities to provide adequate day care, after-school care, and other valuable resources for its children. There also is a decreased likelihood of the parental-child contact and parental monitoring of children's activities that facilitates the development of self-control. We anticipate a rising level of criminal violence to accompany the rise of single-parent families, especially single-parent families that contain young children (children in their first 8–10 years). This simply reflects the aggregated experiences of those growing up in single-parent families.

Second, building on Sampson and Wilson's (1995, p. 44) analysis of community contextual effects and Coleman's (1990) discussion of "closure" and social capital, we suggest that all children within cohorts with relatively more single-parent families experience less social control and social capital, whether or not they are members of single-parent families.[4] For example, two-parent families are more likely to contain an adult (parent) who knows one or more parents of their children's friends. This increases closure (social organization) of the community and more specifically the system made up of parents and children. As a result, the monitoring and supervisory abilities of parents, which are crucial in the development of self-control in children, are increased.

In addition, in settings with fewer intact families, the importance of peers relative to parents and other adults increases. The larger proportion of children who experience poverty and the diminished parental contact and supervision in the home alter interactions in peer groups and the nature of peer-based socialization experienced by all children, regardless of their own individual family background. Peer groups would be more likely to include others with a greater probability of having antisocial tendencies, and peer-group socialization influences the behavior of cohort members, whether or not they are from a single-parent family.[5]

These forms of social control and social capital are important aspects of community and family resources. Our use of these terms is akin to Sampson and Wilson's (1995) use of the phrase "structural dimensions of community disorganization" to refer to "the prevalence and interdepen-

[4] Coleman (1990, p. 300), describing Loury's (1977, 1987) initial use of the term *social capital,* refers to it as "the set of resources that inhere in family relations and in community social organizations and that are useful for the cognitive or social development of a child or young person." Most analysts today would expand this definition to include more than those relationships that are useful to children and young people. Adults, too, can benefit from increases in social capital (Coleman 1990).

[5] A recent study of contextual effects on student achievement lends support to this contention. Pong (1998) found that children who attend schools where a greater proportion of students come from single-parent homes tend to have lower levels of academic achievement than would be predicted given other characteristics such as their socioeconomic status and their own family structure. Pong found that this contextual effect can be explained, in part, by social capital, particularly parents' involvement in the school.

dence of social networks in a community." These include formal and informal networks and "the span of collective supervision that the community directs toward local problems" (p. 45). We suggest that it is useful to conceive of social capital (those resources that inhere in family relations and social organization) as residing not only on the microlevel of individual relationships or at the macrolevel of the community but also as a characteristic of birth cohorts.

Predictions

To summarize, we expect cohort effects on homicide rates due to both the aggregation of the effects of families with more children and single-parent families on the individuals from those families *and* because of the effects of relatively more children and the availability of relatively less parental and community resources for the members of those cohorts whether or not these members are from such a family.

We test the hypothesis that variations in age-period-specific violent criminal behavior, specifically homicide, are associated with variations in two cohort characteristics: one designed to examine the Easterlin (1978, 1987) hypothesis and another to assess (*and serve as a proxy for*) several related changes in family structure. Specifically, we examine the relationship of the homicide arrest rates for cohorts throughout their life spans to the cohorts' size relative to other cohorts and the percentage of births to unwed mothers. Both of these cohort characteristics relate to factors that contribute to the community and family resources available to cohorts, such as the number of parental figures per child, the likelihood that parents of children's friends know each other, and the number of students per teacher. We hypothesize that both relative cohort size and the percentage of births to unwed mothers are positively related to the homicide arrest rates for cohorts and that the effect of these cohort characteristics is independent of both age and period effects.[6]

Finally, while we expect the effects of relative cohort size and the percentage of births to unwed mothers to occur throughout the life span, it is possible that these effects will be most marked at younger ages. For instance, some authors (Kahn and Mason 1987; Steffensmeier et al. 1992) argue that the effects of relative cohort size should be especially pronounced for those who are young, when they are most in need of support

[6] We realize that discussions of difficulties faced by children of single mothers have produced political controversy in the past. No single study can establish whether such difficulties do or do not exist. If they do not, there is no problem. If they do, then it is important that researchers identify the nature of these problems so they can be addressed.

from family and communities. Similarly, while noting the importance of early family experiences in promoting criminal behavior, Sampson and Laub (1993) note that some young people who engage in crime in adolescence and young adulthood are able to alter these behavior patterns when they are older. We thus test the possibility that the effects of these cohort-related characteristics are especially marked at younger ages.

DETECTING COHORT EFFECTS

A close reading of Easterlin (1987) reveals a number of criteria to use when looking for the effects of relative cohort size specified in his theory. We extend this logic to the examination of nonmarital births.

Easterlin emphasizes that any examination of the effects of cohort characteristics must involve cohort-specific dependent variables, such as age-period-specific homicide rates. As Easterlin says, "[T]he changing age structures of the population—technically dubbed 'age-composition effects'—are important, but are not my concern. . . . My primary interest, in other words, is in the conditions specific to the group whose number is changing—what are called 'age-specific' effects" (Easterlin 1987, p. 6).

Second, it is important to examine characteristics of cohorts relative to other cohorts within the population, for instance, the relative size of the cohort and the relative number of children born to unwed mothers—not the absolute size of the cohorts or the absolute number of cohort members who were born to unwed mothers. Easterlin (1987) argues that it is the "bulge" created by the cohort as it passes through the system that creates problems for the cohort. If the cohort is much bigger (relatively) than the cohorts that immediately precede it through the system, then there is not enough capacity for it in terms of teachers or counselors per cohort member, and when they enter the job market there are not enough jobs per cohort member. For a relatively small cohort, there is an excess capacity in schools and a greater demand for their skills when they enter the job market. In the case of cohort members born to unwed mothers, we suggest that it is their relative number that directly affects the relative rates of homicide. In addition, it is their relative number that is important in operationalizing group processes, such as peer socialization or the average amount of closure, that diffuse beyond the individuals who were born to unwed mothers.

Third, cohorts should be based on the grouping of individuals born in more than a single year. Easterlin states that, "I use 'generation' and 'cohort' interchangeably. . . . My interest is primarily in those born in periods of low or high birth rates, rather than in any one high or low year" (1987, p. 7). Easterlin's reasoning suggests that the schools can absorb a one-year bulge in the population or the labor market but not a five-year bulge.

Similarly, an increased percentage of nonmarital births in a single year should not have as great an impact as a high percentage for several years. With a high percentage for several years, not only do relatively more children grow up in families in which they were born to unwed mothers, but a larger proportion of the children in that cohort engage in peer socialization with a broad group of peers from these families.[7]

Fourth, relative cohort size affects cohorts throughout their life spans. "The effects of generation size, good or bad, persist throughout the life cycle. Every generation follows the pattern of below-average earnings in early working life and above-average earnings later. But the earnings pattern of a small generation is more favorable throughout its career than that of a large generation—during early working life, the earnings of a small generation are not so far below average; at mid-career, the earnings are further above average" (Easterlin 1987, pp. 29–30). We predict lower homicide arrest rates for cohorts that are relatively small and that have lower percentages of nonmarital births. These decreased rates should persist throughout the life spans of the cohort members.

Fifth, Easterlin (1987) suggests that the effect of relative cohort size is most likely for data from the United States for the period after World War II. He selected this period, in part, because of its large variation in relative cohort size and relatively low level of immigration. Thus, for models that include relative cohort size, the test should ideally involve data from that period, as our data do.

Finally, it is crucial to control for the effects of period and age before testing for the effects of cohort characteristics on age-period-specific rates. When these controls are ignored, it becomes difficult to disentangle the effects of cohort characteristics from age and period effects.

We meet each of these criteria. (1) The dependent variable is cohort specific, that is, age-period-specific homicide arrest rates. (2) We use relative measures for the independent variables: relative cohort size and the percentage of nonmarital births. (3) Our age groupings cover five-year periods. (4) We examine the effects of relative cohort size and the percentage of nonmarital births on homicide rates throughout the life span of the cohorts. (5) Our data cover a period after World War II. (6) We control for both age and period effects.

[7] It is perhaps worth mentioning that we are not contending that all children who are born to unwed mothers are poorly socialized (see also Furstenberg, Brooks-Gun, and Morgan 1987; Furstenberg and Hughes 1995). We do maintain that it is easier for families and communities to provide adequate supervision and consistent discipline when two-parent families (or those with two other responsible adults) are more common, and that the percentage of nonmarital births is related to the number of parents or parent substitutes available within the cohort.

DATA AND MEASURES

Homicide Arrests

We would have liked to use the rates of homicides, known to the police, that were committed by different age groups in different years. However, these data, broken down by age groups, are not available over an extended time period. We instead use arrest rates. Homicide rates, fortunately, are not only the best reported Part 1 Uniform Crime Reports (UCR) crime rates, they are likely to be the ones for which crimes known to the police and arrest data are the most consistent.[8] We examined this relationship for the years covered in our analysis, correlating five-year moving averages for total homicide rates and total homicide arrest rates for the years 1960 to 1995. The resulting correlation was .99.[9]

Our data for homicide arrests come from the UCR (FBI 1961, 1966, 1971, . . . , 1996) and are based on five-year age groupings. This particular grouping of ages results, in part, from the way the published reports group arrest rates. From 1960 until the present, the UCR grouped arrest rates for those ages 25–29, 30–34, 35–39, 40–44, and 45–49. The ages 15, 16, 17, through 24 have been reported separately for arrests since 1960, and we grouped them to obtain equal-size age groups that would correspond to equal-length periods (five-year periods).

The series begins in 1960 because that is the earliest year for which data are reported on homicide arrests based on a reasonably representative group of law enforcement agencies using a fairly consistent basis for reported arrests over the entire period. For example, in 1950, fingerprint records kept by the Federal Bureau of Investigation (FBI) formed the basis for the data on arrests by age groups, but by 1955, reports to the FBI and not fingerprints formed the basis of the arrest by age-group counts. Unfortunately, data were reported only for cities with populations over 2,500. Since 1960, data based on the UCR system have been reported for both rural areas and cities, separately or combined.

The arrest data taken from the FBI need to be corrected for the population of the United States covered in a given year, since the arrest figures for each age group are based on the number of reporting agencies, which varies from year to year. We computed a correction factor by dividing the total population of the United States by the number of residents in the areas reporting to the FBI that year. This ratio was multiplied by the

[8] The age-period-specific arrest data in the UCR are not broken down by race or gender. For that reason, all of our analyses use arrest data broken down by age group for the entire population.

[9] The simple correlation between yearly homicide rates and homicide arrest rates was .97.

number of homicide arrests in each of the age groups. This corrected number of homicide arrests was then divided by the number of U.S. residents in the particular age group and multiplied by 100,000 to obtain an estimate of the number of homicide arrests in the age group per 100,000 individuals in the age group. Data on the number of U.S. residents in each age group were drawn from the Current Population Surveys: Series P-25 (U.S. Bureau of the Census, various dates). We logged the homicide arrest rates (natural log) for two reasons. First, the distribution of these rates was markedly skewed to the right without such a transformation. Second, we felt that it was important that a doubling of the homicide rate for those ages 45–49 should be weighted as much as a doubling for the homicide rates for those ages 15–19. The log transformation provides such weighting when the dummy variables for age groups are in the equations.

Cohort Characteristics

Both of the variables that we use to characterize cohorts are clearly associated with each birth cohort: the percentage of nonmarital births (%NB) and relative cohort size (RCS). Although we have provided theoretical linkages between the percentage of a cohort reared by single parents during early childhood and the homicide rate for that cohort compared with other cohorts, we need to mention why we chose to represent this family structure by the %NB. The main reason, in addition to the partial fit of this variable with what we would ideally like to measure, is the practical one of data availability. Data on single-parent families with young children (say, 0–9 years of age) does not exist for the range of cohorts that we examine, but data are available on %NB for all but the cohort born between 1910 and 1914. This is not a perfect measure, but it does indicate the likelihood that young children were raised by a single parent during at least part of their first 8–10 years. As we will see later, the %NB works quite well in predicting the age-period-specific homicide arrest rates of cohorts.

Data for %NB (the number of births to unwed mothers per 100 live births) were obtained from two different volumes of *Vital Statistics of the United States* (U.S. Bureau of the Census 1946, 1990). Data for the years 1917–40 were taken from the earlier volume, and data for the remaining years were drawn from the second volume. To obtain %NB for each cohort, we summed the appropriate percents and divided by the number of years.[10] For example, to obtain %NB for those who were ages 15–19 in

[10] Nonmarital birth data in *Vital Statistics of the United States* are presented as rates per 1,000, so we first converted these to percentages before we summed them. In these census publications these rates are called "illegitimacy ratios." We averaged the percentages across the five years in each birth cohort rather than calculating a "grand"

1960, we summed %NB for the years 1940–44 and divided by five. The %NB for the oldest birth cohort was not available, since that cohort was born between 1910 and 1914. The %NB for the second oldest cohort was derived by summing %NB for 1917, 1918, and 1919 and dividing by three. All other %NB values were based on five years of data.

RCS is operationalized in two ways: (1) the percentage of the population age 15–64 that is in the cohort when the cohort is age 15–19, and (2) the percentage of the population age 15–64 that is in the age group 15–19 when the cohort is 15–19, the percentage of this population that is age 20–24 when the cohort is 20–24, and so on throughout the age range covered in the analysis. Both of these operationalizations have been used before (O'Brien 1989). We use these operationalizations in separate analyses to determine if using them makes any substantive differences in the results. We label the first operationalization as the fixed operationalization for RCS, because its value is constant for each cohort at all ages. The second is labeled as the variable operationalization for RCS, because its value varies for a cohort at different ages. Data for the RCS measures were obtained from the Current Population Surveys: Series P-25 (U.S. Bureau of the Census, various dates).[11]

ANALYSIS

We analyzed the data using a modified age-period-cohort model. Figure 1 depicts an age-period-cohort model corresponding to our situation and

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percentage across the five years, because the data for the years before 1938 are available in the published reports only as ratios for the *reporting areas* rather than as estimates for the nation as a whole. In 1951, estimates for the nation as a whole were made for the number of nonmarital births (and nonmarital birth ratios) for the years 1938–50. After that year, estimates were reported for the United States as a whole regardless of the reporting areas. Given these shifts, and the difficulty of tabulating the number of live births in the areas that reported nonmarital births for each of the years since 1917, we decided to average the percentages based on the *reporting areas* or (in later years when only estimates for the nation as a whole were available) the percentages based on the nation as a whole, across the appropriate five-year cohorts.

[11] O'Brien (1989) notes, in the context of testing for the Easterlin effect, that it would be inappropriate to include variables such as cohort unemployment rates, delays in marriage, declines in cohort wages, and so forth that Easterlin (1978, 1987) predicts are affected by RCS as separate independent variables in the age-period-cohort-characteristic model designed to predict age-period-specific homicide rates. To the extent that these variables are caused by RCS and %NB, which are arguably causally prior, we would then be partialling the effects of these variables from themselves. An agenda for future work could include a thorough examination of the way in which these variables may intervene in the relationship between relative cohort size and homicide rates.

	15-19	20-24	25-29	30-34	35-39	40-44	45-49	
1960	12.43 **7** 8.98	10.14 **6** 14.00	10.09 **5** 13.45	11.10 **4** 10.73	11.64 **3** 9:37	10.85 **2** 6.48	10.17 **1** 5.71	
1965	14.62 **8** 9.07	11.58 **7** 15.18	9.70 **6** 14.69	9.54 **5** 11.70	10.32 **4** 9.76	10.70 **3** 7.41	9.81 **2** 5.56	14.40 **1** N.A
1970	15.27 **9** 17.22	13.16 **8** 23.76	10.80 **7** 20.09	9.14 **6** 16.00	8.80 **5** 13.13	9.50 **4** 10.10	9.64 **3** 7.51	13.89 **2** 2.10
1975	15.33 **10** 17.54	13.96 **9** 25.62	12.41 **8** 21.05	10.21 **7** 15.81	8.37 **6** 12.83	8.07 **5** 10.52	8.51 **4** 7.32	13.69 **3** 2.57
1980	14.03 **11** 18.02	14.25 **10** 23.95	13.09 **9** 18.91	11.82 **8** 15.22	9.38 **7** 12.31	7.81 **6** 8.79	7.35 **5** 6.76	12.39 **4** 2.93
1985	11.72 **12** 16.32	13.27 **11** 21.11	13.74 **10** 16.79	12.81 **9** 12.59	11.19 **8** 9.60	8.88 **7** 7.50	7.36 **6** 5.31	10.80 **5** 3.92
1990	10.82 **13** 36.52	11.66 **12** 29.10	12.94 **11** 17.99	13.35 **10** 12.44	12.18 **9** 9.38	10.84 **8** 6.81	8.42 **7** 5.17	10.87 **6** 4.08
1995	10.53 **14** 35.24	10.43 **13** 32.34	11.08 **12** 16.75	12.75 **11** 10.05	12.97 **10** 7.27	11.79 **9** 5.48	10.17 **8** 3.67	12.43 **7** 3.62
		10.53 **14** 15.59	10.82 **13** 12.11	11.72 **12** 8.97	14.03 **11** 5.99	15.33 **10** 4.82	15.27 **9** 4.06	14.62 **8** 3.82

FIG. 1.—Age-period-cohort model with cohorts numbered in the cells (cohort 7 is the cohort born between 1940 and 1944). Within the cells: top, RCS of the cohort at each age; middle, cohort number; bottom, homicide rate. In the margins: top, RCS when the cohort was ages 15–19; middle, cohort number; bottom, percentage born out of wedlock.

provides the data used in our analyses. The rows and columns indicate period and age; each of the cells indicates the experiences of a particular cohort at a given age and year. Different birth cohorts are identified by number (the boldface number). Cohort 1 was born between 1910 and 1914, cohort 2 between 1915 and 1919, and so on. By following a particular cohort diagonally through the table, one can see the way in which cohorts move through the time/age space described by Ryder (1965). The top and bottom entries in each cell represent, respectively, the *variable* RCS measure (the percentage of the population ages 15–64 who were in the cohort during the period represented by the row) and the age-period-specific homicide arrest rate for the cohort during that period. The margins contain values that remain the same for a cohort over time. The top and bottom entries represent, respectively, the *fixed* RCS measure (the percentage of the population ages 15–64 who were in the cohort when the cohort was 15–19) and the percentage of the cohort members who were born to unwed mothers.

Inspection of figure 1 indicates substantial variation on each of our measures. Age-period-specific homicide rates range from a low of 3.7 for cohort 8 (those born between 1945 and 1949) in 1995 (when they were 45–49 years of age) to a high of 36.5 for cohort 13 (those born between 1970 and 1974) in 1990 (when they were 15–19 years old). The %NB ranges from 2.1% for cohort 2 to 15.6% for cohort 14. Note, however, that the increase in this variable is not strictly linear, with cohort 6 (born between 1935 and 1939) having a value that is higher than that for the cohorts 7, 8, and 9 (born between 1940 and 1954). Finally, the measure of RCS varies from 10.5 to 15.3 for the fixed measure and 7.4 and 15.3 for the variable measure. The cohorts with the smallest relative size are 5 and 6 (born in the depression years of 1930–39) and cohorts 13 and 14 (born between 1970 and 1979). The cohorts with the largest relative size are those in the post–World War II baby boom (cohorts 8, 9, and 10, born between 1945 and 1959).

In our analysis, the set of age-period-specific homicide arrest rates associated with each cohort in each cell is the dependent variable; the various time periods, age groupings, and measures of RCS and %NB associated with each cohort are the independent variables. This approach allows us to associate changes in the dependent variable (age-period-specific homicide arrest rates) with particular cohort characteristics while controlling for both age and period.[12] Examining figure 1, we see that, for an analysis

[12] Including variables that measure cohort characteristics rather than dummy variables for each cohort avoids the problem of linear dependency that would occur if one included dummy variables for $T - 1$ of the time periods, $A - 1$ of the age group-

involving age-group dummy variables, period dummy variables, RCS, and %NB, there are [(7 − 1) + (8 − 1) + 1 + 1 =] 15 independent variables to predict [(7 × 8) =] 56 age-period-specific homicide arrest rates. We test this model with both measures of RCS to see if our results are consistent across both of these operationalizations. We also test our model for data from 1960 through 1995 and for the period 1960–85. Examination of this shorter and earlier time period allows us to determine the extent to which our model can be generalized to an earlier time period that does not overlap with the epidemic in youth homicides.

Test for Autocorrelation

A feature of the age-period-cohort-characteristic model that has not been considered previously is the possibility of autocorrelation generated by cohorts. A cohort's homicide arrest rate "moves diagonally" from the upper left to the lower right in figure 1. The cohort labeled 7 in figure 1, for example, contributes seven observations to the data: the first in 1960 when it was ages 15–19 and the final one in 1990 when it was ages 45–49. This raises an important issue—the possibility that the residuals for cohorts may be correlated. Systematic differences in even a single cohort that affect its age-period-specific homicide arrest rates and are not predicted by the independent variables in the model will result in residuals for that cohort that exhibit a systematic relationship to each other. Anything about a particular cohort that makes its age-period-specific homicide arrest rates higher than expected, given the independent variables, will result in its residuals being positive.

We test for autocorrelation due to cohorts using the following procedure. There are some observations on cohorts that cannot be checked for autocorrelation. For example, both cohort 1 and 14 have only one observation, so we cannot check how much the residuals for cohort 1 are correlated with each other or how much the residuals of cohort 14 are correlated with each other. For all of the other cohorts, we can examine the correlation between one or more pairs of residuals. We do this by examining the correlation of the cohort residuals at time t with those at time $t - 1$. Using this procedure, we can correlate a pair of residuals for cohort 2 and a pair for cohort 13, two pairs of residuals for cohort 3 and two pairs for cohort 12, and so on. The total number of pairs of residuals correlated is $[(A \times T) - (A + T - 1)] = 42$, where A is the number of age groups and T is the number of periods. If there is a systematic under-

ings, and dummy variables for each of the $C - 1$ cohorts, a problem noted by Mason et al. (1973).

or overprediction of the homicide arrest rates for different cohorts, this procedure should result in a positive correlation between residuals.

Control Variables

An important strength of the age-period-cohort-characteristic model is the inclusion of dummy variables for both age groups and periods. From our perspective, these two sets of dummy variables are control variables. They control for age and period directly, and they control for other variables that are associated with age and period, thus providing a clearer indication of the effect of cohort-related variables. For instance, by including a dummy variable for period, we control for the effects of many variables not explicitly included in our model, such as the yearly unemployment rate, the number of police officers per capita, changes in media, and improvements in medical technology that might prevent deaths. The period dummies serve as a proxy for these and other variables that change over time. The same argument applies to the age-group dummy variables, which control for factors that are associated with age, such as physical strength, social maturity, and life stage.[13]

The inclusion of these dummy variables also controls for effects of variables associated with linear trends of cohort characteristics. This occurs because the age group and period dummy variables perfectly predict the cohort numbers (1, 2, . . . , 14) in figure 1. Thus, we have, in essence, included the time period in which the cohort was born as a control variable. If a cohort characteristic were related to the homicide arrest rate simply because both of these variables were linearly related to the time of the cohorts' birth, that effect would be controlled for. Together, the age dummies, period dummies, and the implicit inclusion of the time of the cohorts' births provide a very strong set of controls. As we shall see, these independent variables explain a large portion of the variation in age-period-specific homicide arrest rates without introducing our two cohort characteristics.

Two additional control variables are used in later analyses to test the robustness of our results. Since males are more likely than females to commit homicides, we add the age-period-specific sex ratio (number of males

[13] More technically, we control for the effects of variables associated with periods that are constant across age groups and control for the effects of variables that are associated with age group that are constant across periods. To the extent that effects of some of these variables are only relatively constant across age groups (in the case of effects associated with periods) or across periods (in the case of effects associated with age groups), the control is incomplete. The controls extend to the effects of variables associated with period and age group that are not explicitly included in the equation.

per 100 females) for each age group and period as a control variable. Similarly, since African-Americans have higher homicide rates than other groups, we add the age-period-specific percentage of the population that is African-American as a control variable.

The confounding of age, period, and cohort makes it *necessary* to control for age and period in order to untangle the effects of cohorts. The best way to achieve this control is through dummy variable coding. This requires a set of 13 "control variables" and might lead to the charge that we have overfit our data, even in the basic model in which we include these dummy variables and just two variables representing cohort characteristics. We do not find the use of these 15 variables to be problematic. The 13 dummy variables were used as a set to allow us to examine the effect of cohort characteristics. We did not try different combinations of these variables in an attempt to provide support for our theory. Nor did we engage in a "search" for significant results by trying to fit our model with a number of cohort characteristics that we did not report. Controlling age and period effects using dummy variables makes it more difficult to find cohort effects.

Most important, controlling for age and period is essential to testing our theory, and we think that it is usually worse to leave out theoretically relevant variables than to include too many variables. As Johnston (1984) notes, "It is more serious to omit relevant variables than to include irrelevant variables since in the former case the coefficients will be biased, the disturbance variance overestimated, and the conventional inference procedures rendered invalid, while in the latter coefficients will be unbiased, the disturbance variance properly estimated, and the inference procedures properly estimated" (p. 262). In our case, we believe that all of the 15 variables in our basic model are relevant variables.

RESULTS

Table 1 presents the results of regressing the natural log of the age-period-specific homicide arrest rates on age group and period dummy variables, %NB, and the fixed version of RCS. Because age group and period are orthogonal in this design, the amount of variance that they explain in the homicide arrest rates is not dependent on which of the two sets of dummy variables is entered first into the regression equation. The R^2 associated with the set of period dummy variables is .09 ($P > .20$). The R^2 associated with the age-group dummy variables is .76 ($P < .001$). Together, the age and period dummy variables explain nearly 84.7% of the variance in logged age-period-specific homicide arrest rates (adjusted $R^2 = .79$). Adding %NB to the regression equation that contains only age and period dummy variables increases the explained variance to 97.6%, while adding

TABLE 1

Regression Analysis of Logged Age-Period-Specific Homicide Arrest Rates, 1960–95

Variables	Model 1		Model 2	
	B	t	B	t
Intercept	−.200	−1.241	−.345	2.031
Period:				
1960	.712	10.819***	.750	11.379***
1965	.732	12.244***	.767	12.810***
1970	1.044	18.442***	1.074	19.055***
1975	.989	18.641***	1.015	19.319***
1980	.825	16.876***	.846	17.603***
1985	.515	11.748***	.529	12.398***
1990	.450	11.765***	.454	12.339***
1995	.000[a]	.000[a]	.000[a]	.000[a]
Age:				
15–19	.331	5.794***	.291	5.002***
20–24	.832	17.760***	.805	17.153***
25–29	.767	18.840***	.749	18.648***
30–34	.609	16.217***	.596	16.289***
35–39	.464	12.896***	.455	13.069***
40–44	.251	7.185***	.246	7.324***
45–49	.000[a]	.000[a]	.000[a]	.000[a]
RCS	.054	6.533***	.061	7.057***
%NB	.186	19.078***	.194	19.002***
Cohort 7	—	—	.059	2.060*
R^2	.989		.995	
Adjusted R^2	.984		.990	

Note.—N (number of cells minus one cell with missing data) = 55.
[a] Dummy variable omitted for purposes of estimation.
* $P < .05$ (for two-tailed test).
*** $P < .001$.

the fixed version of RCS to the equation containing the age and period dummies increases the explained variance to only 87.4%. Each of these increases in explained variance is statistically significant ($P < .01$). Adding both %NB and RCS increases the explained variance to 98.9%.[14]

Controlling for the period and age-group dummy variables, both RCS and %NB are positively and statistically significantly related to the age-

[14] One way to think about explaining this variance is to think of fig. 1 as a "contingency table." Note that we are explaining the age-period-specific homicide arrest rates or "cell rates." We do this with the "main effects" of the columns (age groups) and rows (periods) and explain the "interaction" with two cohort characteristics.

period-specific homicide arrest rates. To gain a sense of the extent of the relationship represented by these coefficients, we can transform the coefficients into "effective rates of return" (Stolzenberg 1979) or percentage change form. The RCS coefficient of .054 means a change of one unit in RCS is associated with a change in the natural log of the homicide arrest rate of .054. This is equivalent to an $(e^{.054} - 1) \times 100\%$ increase or a 5.6% increase.[15] Since RCS is measured as the number in the cohort per 100 people in the age range 15–64 when the cohort was ages 15–19, we can say a one point increase in RCS (say from the cohort making up 8% of the population to 9%) is associated with a 5.6% increase in the homicide arrest rate (controlling for the other variables in the model). A similar interpretation is available for the coefficient associated with %NB. The regression coefficient of .186 means that an increase of one person born to an unwed mother per 100 live births is associated with an increase in the age-period-specific homicide arrest rate of $[(e^{.186} - 1) \times 100\% =]$ 20.4%.

We checked the residuals from model 1 for autocorrelation due to cohorts and found that the correlation between the lagged residuals (lag one) was .20. As described earlier, we calculated the lag-one correlations between all pairs of residuals where there was a preceding residual within the cohort: 42 pairs of residuals. An examination of the residuals indicated that this autocorrelation mainly was due to the residuals for cohort 7 (the cohort born between 1940 and 1944). All of the residuals for this cohort were positive; that is, the homicide arrest rates based on age, period, %NB, and RCS were underpredicted for this cohort in all seven periods. This suggests that there is something special about cohort 7 that made its homicide arrest rates higher than expected given the independent variables in model 1.[16]

In model 2, we included a dummy variable for cohort 7. This provided a test to determine if the under prediction for cohort 7 was statistically significant (albeit, we selected this cohort dummy variable on the basis of the residuals). The results for model 2 in table 1 indicate that the regres-

[15] For example, if the age-period-specific homicide rate is 10, the natural log of that rate is 2.303. The unstandardized regression coefficient of .054 indicates that the predicted natural log of the homicide rate would increase by .054, or in this case it would be (2.303 + .054 =) 2.357. In terms of antilogs, the increase is from 10 to 10.55 or a 5.55% increase. Although we used an age-period-specific homicide rate of 10 in this case, we could have just as easily used a rate of 20. A .054 increase in the natural log is always a 5.55% increase.

[16] The positive residuals associated with the 1940–44 birth cohort are consistent with our theoretical contentions that alterations in family structure can lead to diminished social capital for children that affect them throughout their life spans. This cohort may have experienced a high rate of father absence during infancy and early childhood due to the disruptions associated with World War II. But this, admittedly, is an ad hoc explanation.

sion coefficient associated with the dummy variable for cohort 7 is statistically significant at the .05 level. If this cohort is mainly responsible for the correlated residuals, this cohort dummy variable should substantially reduce the autocorrelation. This is important, because the correlated residuals bring into question the significance tests used in model 1. When we checked the autocorrelation due to cohorts for model 2, the correlation between the residuals was only .04. It is important to note that other regression coefficients in model 2 and their associated *t*-values were affected only slightly by the inclusion of the dummy variable for cohort 7.

The relationship between homicide arrest rates and age from 1960 to 1995 is depicted in figure 2, where the solid line represents the observed homicide arrest rates. Note the near doubling of rates for those ages 15–19 between 1985 and 1990 and the startling increase for those ages 20–24 between 1985 and 1990. These rates remained at nearly as high a level in 1995.

The dashed line in figure 2 indicates the predicted value of the homicide arrest rates based on a regression equation containing only the age and period dummy variables.[17] If the relationship between age and homicide arrest rates were invariant across periods (i.e., if the relative rate of homicide arrests remained constant for the different age groups), then these predictions would perfectly fit the observed rates. The estimated values dramatically under predict the homicide arrest rates for those ages 15–19 and ages 20–24 in 1990 and 1995. Otherwise, the predictions do not seem to be "too bad" based on a visual inspection. Graphically, this is the problem faced by scholars who attempt to explain the sudden increase in youth homicide and why that sudden increase has been referred to as an epidemic (Zimring 1996): the relationship between age and homicide rates had been very stable up until 1985.

The stippled line in figure 2 represents the predictions based on model 1 in table 1—the model that includes the age and period dummies in addition to relative cohort size and the percentage of nonmarital births. This model does a much better job of predicting the sudden increase in youth homicides, although it underestimates the rate of 15- to 19-year-olds in 1990 by about 6 per 100,000. Nevertheless, the estimated and observed rates are quite close. Note that including RCS and %NB improves predictions of rates throughout *all* of the age groups covered in our analysis, not just the youngest age groups. For example, the predictions for age groups over 30 in 1995 are much better given estimates based on the full model as are those for the young age groups in the earlier periods.

[17] We took antilogs of the predicted values from our regression model to transform our logged predicted values into predicted homicide arrest rates per 100,000.

FIGURE 2: PREDICTED AND OBSERVED HOMICIDE ARREST RATES FOR AGE GROUPS FROM 1960 TO 1995

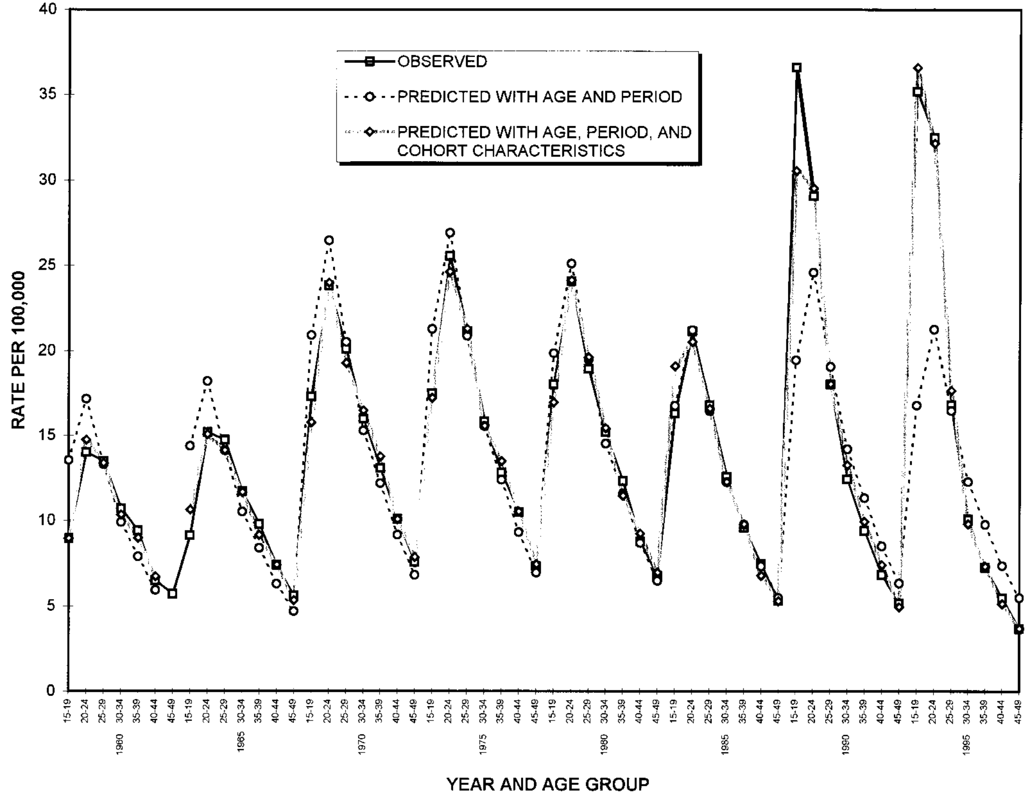

FIG. 2.—Predicted and observed homicide arrest rates for age groups from 1960 to 1995

Some Additional Tests

Kahn and Mason (1987) and Steffensmeier et al. (1992) have argued that the effects of RCS should be especially pronounced for those who are young, although our theory is mute on this point. We tested this by interacting ages 15–19 and ages 20–24 with RCS and with %NB. This resulted in four interactions that indicate whether the effects of RCS and %NB are especially large for those in the two youngest age groups. All four of these interactions are positive. The coefficients for the interactions of ages 15–19 and ages 20–24 with %NB were statistically significant at the .05 level (one-tailed tests), and the main effects for RCS and %NB remained statistically significant ($P < .005$) when the interaction effects were included. This means that although the effects of RCS and %NB are stronger for those ages 15–24, these effects remain positive and are statistically significant for the remaining age groups.

When we conducted the same series of analyses reported in table 1 but used the variable measure of RCS, the percentage of the population from ages 15–64 during a particular period, that is, in the cohort's five-year age group during that period, the results were nearly identical. The %NB and RCS were both strongly related to the log of the age-period-specific homicide arrest rates, the cohort variable for those born from 1940 to 1944 was statistically significant, and the same four interactions described in the preceding paragraph were positive. The coefficients for ages 15–19 and for ages 20–24, interacted with %NB, were both statistically significant at the .05 level, and the main effects for RCS and %NB remained statistically significant ($P < .005$) even with these interaction terms in the model.

We tested two other specifications for our model using data from the 1960 to 1995 periods. Using age-period-specific homicide rates in unlogged form yielded results that were substantively similar to those reported in table 1. Again, RCS and %NB were statistically significant in the full model, and the dummy variable for cohort 7 was statistically significant. We also added age-period-specific measures of the sex ratio and the percent African-American as independent variables in the full model (model 2 of table 1). As noted above, both of these variables might well account for variations in age-period-specific homicide arrest rates, since males are more likely to commit homicides than females and African-Americans have higher homicide rates than other groups. Controlling for age, period, RCS, and %NB, neither the sex ratio nor the percent African-American was a statistically significant predictor of homicide arrest rates in this full model. More important, including these two additional control variables did not substantively change the results reported in table 1. The effects of both RCS and %NB remained statistically significant, and the unstandardized regression coefficients were of nearly the same magnitude. (These results are available from the authors on request.)

We do wish to insert a note of caution regarding the results obtained when the interaction effects and these additional control variables are added to our basic model. While there is not a problem with multicollinearity in our basic model, the addition of the other controls such as the sex ratio and the percentage black produce regression diagnostics that indicate the presence of serious multicollinearity. The introduction of the interactions of age groups with our two cohort measures produces what we believe is an excessive amount of multicollinearity.[18] Thus, while the main effects of RCS and %NB remain consistent and strong across all of our analyses, the coefficients associated with the additional control variables should be interpreted with care.

Finally, and perhaps most important, if our suppositions concerning the effects of RCS and %NB on homicide arrest rates are correct, we should find these relationships prior to the dramatic increases in youth homicides that occurred in the mid-1980s. The relationships might be weaker (in terms of statistical significance) given both the reduction of variability in homicide arrest rates and the number of cases that accompany such a truncation of the data, but they should be present. To test this, we used our model to predict homicide arrest rates for the years preceding the dramatic recent increase in youth homicide arrest rates: the years from 1960 to 1985. The results appear in table 2.

[18] We assess multicollinearity with the variance inflation factor [VIF $= 1/(1 - R_k^2)$] and its square root. The square root of VIF indicates the degree to which the SE of an independent variable's regression coefficient is inflated due to multicollinearity. If the kth independent variable had a squared multiple correlation of .75, when regressed on the other independent variables in the analysis, it would have a VIF of 4 [$=1/(1 - .75)$]. This indicates that the SE of the regression coefficient for the kth variable is twice as large as it would have been if there had been no multicollinearity between it and the other independent variables. In our basic model (model 1 of tables 1 and 2), the greatest inflation of a SE due to multicollinearity is 2.77: associated with nonmarital births. Squaring this coefficient yields a VIF of 7.67. Although this degree of collinearity indicates some degree of inefficiency in our estimation, Rawlings (1988, p. 277) and others cited in Rawlings suggest that serious collinearity problems do not occur when the VIFs are less than 10. The VIFs do not exceed 10 for any of the models in tables 1 or 2. Adding the other controls results in serious multicollinearity. When the sex ratio and the percentage black are added to model 2 in table 1, the VIF for %NB rises to 11.39. Of more concern, the VIF for percentage black is 17.43. This degree of inflation might help explain why the coefficient associated with this variable is not statistically significant. When the interactions for age groups by %NB and age groups by RCS for the two youngest age groups are added to model 2 in table 1, the VIFs are greater than 100 for the two youngest age groups and for both the age group by RCS interactions. The VIF for %NB in this model is 62.40. As argued by O'Brien and Gwartney-Gibbs (1989), the use of these sorts of interactions raises difficult issues concerning the interpretation of the partial regression coefficients.

TABLE 2

Regression Analysis of Logged Age-Period-Specific Homicide Arrest Rates, 1960–85

Variables	Model 1		Model 2	
	B	t	B	t
Intercept	.567	3.167**	.553	3.591**
Period:				
1960	.100	1.736	.075	1.501
1965	.135	2.687**	.124	2.872**
1970	.460	10.250***	.451	11.655***
1975	.420	10.684***	.413	12.178***
1980	.278	8.245***	.273	9.403***
1985	.000[a]	.000[a]	.000[a]	.000[a]
Age:				
15–19	.426	6.463***	.453	7.913***
20–24	.915	17.529***	.937	20.646***
25–29	.840	18.754***	.861	22.051***
30–34	.662	16.852***	.683	19.866***
35–39	.509	13.950***	.528	16.547***
40–44	.274	7.871***	.278	9.270***
45–49	.000[a]	.000[a]	.000[a]	.000[a]
RCS	.048	5.675***	.052	7.014***
%NB	.142	7.116***	.134	7.760***
Cohort 8	—	—	−.084	3.245***
R^2	.988		.991	
Adjusted R^2	.982		.987	

Note.—N (number of cells minus one cell with missing data) = 41.
[a] Dummy variable omitted for purposes of estimation.
* $P < .05$ (for two-tailed test).
** $P < .01$.
*** $P < .001$.

Unlike the full data set, the adjusted R^2 for the age and period dummies explains a greater percentage of the variance in homicide arrest rates for these data: adjusted $R^2 = .95$. This is not surprising given the data shown in figure 2, where the relationship between age and homicide arrest rates is quite consistent across periods until 1990. Thus, the age and period dummy variables are able to “predict” homicide arrest rates rather well during this limited time frame. However, both RCS and %NB are positively and statistically significantly ($P < .001$) related to the age-period-specific homicide arrest rates. Again, the model fits the data extremely well (adjusted $R^2 = .98$). This means that even in this era the relationship between age and the homicide arrest rate was not invariant and that we can account for a statistically significant portion of the variance in that relationship using RCS and %NB.

One difference between the 1960–95 period and the 1960–85 period is that in the latter period the autocorrelation due to cohorts is generated by cohort 8, the cohort born between 1945 and 1949. Members of this cohort have lower homicide arrest rates than expected given the independent variables in model 1.[19] Including the dummy variable for this cohort in the model reduced the autocorrelation from .21 to .10. The results from this analysis are presented as model 2 in table 2. It is important to note that the size of the relationships of RCS and %NB to the homicide arrest rate, as well as the statistical significance of those relationships, shows little change from model 1 to model 2 in table 2.

DISCUSSION

Age groups and periods explain much of the variation among age-period-specific homicide arrest rates from 1960 to 1995 (adjusted $R^2 = .79$). This is not surprising, given the well-known relationship between age and crime. A greater challenge to researchers involves explaining *changes* in the relationship between age-period-specific homicide arrest rates and age across periods. Our results indicate that two characteristics of birth cohorts can account for changes in the relationship between homicide arrest rates and age from 1960 to 1995. A model that contains age group and period dummy variables, in addition to the percent nonmarital births and relative cohort size, results in an adjusted R^2 in excess of .98. The effects of these two cohort characteristics are important, even after controlling for age and period effects. Further, these two cohort characteristics help explain age-period-specific homicide arrest rates both before and after the dramatic increase in youth homicide, indicating that our model is not specific to the most recent period.

Theoretical Issues

Family structure and crime.—These results lend support to both macrolevel and microlevel theories regarding the importance of alterations in family structure in explaining criminal propensities. At a macrolevel, when birth cohorts include relatively more children born to unwed mothers or when birth cohorts are relatively large, families, schools, churches, and neighborhoods must stretch their resources. Most children within these birth cohorts, no matter how large their own family or the marital

[19] By 1985, not enough time had passed for either cohort 7 or 8 to have traced their life courses as represented in fig. 1. Cohort 7 is 40–44, and cohort 8 is only 35–39. Yet, as shown in fig. 1, cohort 8 had the lowest age-period-specific homicide arrest rate for any time and period included in our analysis (3.67 in 1995).

status of their parents, are likely to feel some impact of the diminished family and community resources available to members of their cohort. Just as all children who live in communities with higher levels of social disorganization are affected by higher rates of family disruption and other indicators of social disorganization (see Sampson and Wilson 1995, p. 44), all children in these birth cohorts seem to be at higher risk. To use Vila's (1994) terminology, people born into these birth cohorts may be more likely than people in other birth cohorts to adopt a criminal strategic style because they experience different social environments (see esp. p. 324).

At the microlevel, it is not easy to separate the effects of childhood poverty, adult supervision and monitoring, and other factors that may account for the strong association between nonmarital births and homicide rates. Poverty certainly plays a role in denying resources to children for day care, after-school care, medical care, and other necessities. Our results are consistent with the self-control aspects of the theories of Gottfredson and Hirschi (1990), Moffitt (1993), Patterson and Yoerger (1993), and Sampson and Laub (1993) that stress the importance of early socialization within the family. The results also fit well with social structure and personality theories that point to the importance of father-son ties in stemming inappropriate aggressive tendencies among males (Parsons 1954; Coltrane 1988, 1992). We suggest that at the individual level peer socialization and a lack of closure can play an important role in reinforcing and extending the effects of RCS and %NB beyond the families in which these births occur.

Cohort characteristics.—While both of our cohort characteristics are significantly related to age-period-specific homicide arrest rates, %NB has a much stronger association. For example, in model 1 of table 1, a change of one unit in the %NB results in a 20% change in the estimated homicide arrest rate, while a change of one unit in the percentage of those ages 15–64 who were ages 15–19 when the cohort was 15–19 results in a 6% change. In terms of the impact that these two variables have had from 1960 to 1995, it is important to note that the range of variation of RCS is much smaller than the range of variation in %NB. Across the cohorts in our sample, RCS has ranged from 10.53 to 15.33 (with an SD of 1.77), while %NB has ranged from 2.10 to 15.59 (with an SD of 4.05). Thus, an SD change in RCS is associated with an increase in the predicted homicide arrest rate of 10% [= $(e^{.054\times1.77} - 1) \times 100$]. A one-SD change in %NB is associated with an increase in the predicted homicide arrest rate of 115% [= $(e^{.186\times4.05} - 1) \times 100$].

Both RCS and %NB reflect changes in family structure. Yet, following the work of Easterlin (1978, 1987), most analyses of cohort variation focus on RCS and exclude other cohort-related variables. Our analysis raises the possibility that these other cohort characteristics, such as %NB and conceivably others, may provide even more effective explanations of variations between cohorts. It is possible that these other cohort characteristics are especially important in explaining traits that are closely linked to childhood experiences, such as criminal behavior, academic achievement, and occupational aspirations. Given the strength of the relationship between %NB and the age-period-specific homicide arrest rate in our analysis, examining the relationship between RCS and this rate, without including %NB, is likely to be an incorrect specification of the model underlying this relationship.

Age and crime.—Our results also speak to the controversy surrounding Hirschi and Gottfredson's (1990) hypothesis regarding the invariant relationship between age and crime: "The age effect is everywhere and at all times the same" (p. 124). Controversy has surrounded this hypothesis (Cohen and Land 1987; Greenberg 1985; Farrington 1986; Blumstein, Cohen, and Farrington 1988; Steffensmeier et al. 1989) since first introduced in its most extreme form by Hirschi and Gottfredson in 1983.[20]

If we compare the homicide arrest rates by age for each of the periods depicted in figure 2 with a simple conception of the invariance hypothesis, we would conclude that the invariance hypothesis is not supported. After all, in 1960, 1965, 1970, 1975, 1980, and 1985, the highest arrest rates for homicide were for those ages 20–24, but this changed dramatically in 1990 and 1995, when the highest rates were for those ages 15–19. This same shift in the age-period-specific homicide arrest rate relationship is seen for the age-period-specific homicide rate data based on crimes known to the police from the UCR. These data contributed to the initial concern about the *epidemic* of youth violence. Further, if we compare rates in 1960 and 1995 for those ages 45–49, we see a slight drop for this age group, but there is a dramatic increase in the rates from 1960 to 1995 for those ages 15–19. To the extent that the validity of the invariance hypothesis hinges on the observed rank order of age-specific homicide rates (arrests or homicides known to police) or the observed ratio of rates between age groups across periods, these results do not support the invariance hypothesis.

From a different perspective, however, the results from our analyses may be seen as consistent with the invariance hypothesis, especially with the following caveat of Gottfredson and Hirschi (1990): "So, although we may find conditions in which age does not have as strong an effect as usual, the isolation of such conditions does not lead to the conclusion that

[20] It is clear from their discussion that this invariance concerns the distribution of crime rates and not their absolute size. Many of their examples of invariance involve graphs of age-period-specific arrest rates, age-period-specific conviction rates, and so on, because of the unavailability, in many cases, of age-specific rates for offenders.

age effects may be accounted for by such conditions. On the contrary, it leads to the conclusion that in particular cases the age effect may be to some extent obscured by countervailing crime factors" (p. 128). When we control for period, %NB, and RCS (and other variables in the different models presented in this paper), the age effects follow a familiar pattern: the greatest effect is for 20- to 24-year-olds. Beyond that age, the rates decrease for each age group in our data.[21] From this perspective, shifts in the observed distribution of age-period-specific homicide arrest rates are associated with cohort characteristics that obscure the age effects. Thus, once the effects of RCS, %NB, and period are controlled, as in our model, the pattern of age effects in recent years conforms to that predicted by Hirschi and Gottfredson (1983).

Future Research and Limitations

It always is important to replicate research, but this may be particularly crucial for the research reported in this paper. The importance of RCS and %NB in explaining age-period-specific rates of homicide arrests may depend on the unusual amount of variation in both of these variables over the time period covered in this research. The postwar baby boom was much larger in the United States than in most European countries. In addition, the United States has relatively high rates of single parenthood, especially among young women and women who are not in stable relationships. An important question involves the extent to which our model would fit patterns of age-period-specific homicide rates in societies with different underlying patterns on these independent variables.

Additionally, we can ask whether, with the same amount of variation in RCS and %NB, the effects of these variables would be as strong in other settings. Pampel and his associates (e.g., Pampel and Peters 1995; Pampel and Gartner 1995) have described conditions under which the importance of cohort size may rise or fall in response to other forces of social change. These include the sexual division of labor in the family, immigration, the role of governments in smoothing business cycles, and the extent to which nations adopt collectivist policies regarding social benefits. Pampel and Gartner (1995) provide evidence that the relationship between the age structure of the population and the homicide rate is weaker in societies that have stronger collectivist social institutions (see also Gartner and Parker 1990).

Part of this cross-national variability in political practices involves the

[21] This conclusion is based on the size of the coefficients associated with the dummy variables for age groups in each of the models in tables 1 and 2.

extent to which societies provide public support for children. For instance, most industrialized countries provide some sort of family child allowance and higher welfare payments, resulting in much lower levels of child poverty than in the United States. Many of these countries also have more extensive programs of paid maternity leaves, often for a period of several years (Bergmann 1993, 1996; Hewlett 1992; Pampel 1994). It is quite possible that in such societies %NB will have less of an impact on the homicide rates of cohorts.

On the other hand, given the results of studies of income maintenance programs in the United States (Hannan, Tuma, and Groeneveld 1977, 1978) and commentaries on the decline of the family in the Swedish welfare state (Popenoe 1987, 1988, 1991), it is also possible that such governmental programs cannot provide the resources that help foster the higher levels of both informal social control and self-control needed to combat criminogenic tendencies within birth cohorts. To test these competing views, it is important to replicate our model in societies in which governments attempt to provide greater resources for children.

As noted earlier, discussions of the consequences of RCS have focused on both job market effects and those more directly related to family and community resources and support. The recent increases in homicide arrest rates have generally involved people too young to have much experience within the job market. In addition, our findings indicated that the effects of RCS and %NB persist throughout the life course, including years when relatively few cohort members are actively searching for jobs. As a result, we suggest that our results generally lend support to theories regarding the importance of alterations in family structure for the years examined in our analysis; however, further research should be done that separates job market effects from those effects more directly related to family and community support.

Policy Implications

The youngest cohort in our analysis was born between 1975 and 1979. This particular birth cohort was not especially large relative to others in the sample (RCS = 10.5) but had the largest %NB (%NB = 15.6). It also had the second highest age-period-specific homicide arrest rate (35.2). Cohorts born since 1979 have been somewhat larger and have had substantially higher percentages of nonmarital births. For instance, 18% of all births in the United States in 1980 were to unmarried women, but this figure increased to 22% by 1985 and to 27% by 1990. The %NB for those born between 1990 and 1994 was 30. Given the continued growth in %NB and slightly increased size of younger cohorts, we expect the relatively recent pattern of an extremely large gap between the younger and the older age groups with regard to homicide will continue in the near future.

What might be done to ameliorate the loss of community and family resources available to younger cohorts? Our analysis appears to lend support to those who propose that "nurturant strategies," which focus on childhood, may be much more effective in the long-range control of criminality than either "protection" or "deterrent strategies" (see Vila 1994, p. 337; Sampson and Wilson 1995). It is perhaps worth quoting from Gottfredson and Hirschi (1990): "In our view, the origins of criminality and low self-control are to be found in the first six or eight years of life, during which time the child remains under the supervision of the family or a familial institution. Apart from the limited benefits that can be achieved by making specific criminal acts more difficult, policies directed toward enhancement of the ability of familial institutions to socialize children are the only realistic long-term state policies with potential for substantial crime reduction" (p. 272). These policies might include some sort of family child allowance, higher welfare payments, more extensive programs of paid maternity leaves, and subsidized, high-quality child care. Schools also serve as a "familial institution," and increased support for schools and other institutions that provide support for children could be beneficial.

A different strategy involves creating the conditions that might lead to fewer single-parent families. A number of studies attribute the sharp increase in nonmarital births since the 1970s to the declining wages and high unemployment rates of young men, especially those with low levels of education (Cready, Fosset, and Kiecolt 1997; Lichter, LeClere, and McLaughlin 1991; Lichter et al. 1992; South 1996; Tucker and Mitchell-Kernan 1995; Wilson 1980, 1987).

Summary

Our results provide a precise and parsimonious explanation of recent changes in age-period-specific homicide rates, and explaining these changes motivated this research. We suggest that cohorts may experience different levels of family and community resources. Two measures that tap family and community resources are used in an age-period-cohort-characteristic model. The model controls for the effects of age groups and periods and variables that are associated with these categories as well as the linear effects of the time at which the cohorts were born (1910–14 to 1975–79). The results suggest that past and recent changes in age-period-specific homicide arrest rates are closely associated with cohorts' relative size and the percentage of nonmarital births. The effects of these cohort characteristics are both statistically and substantively large. In addition, the data indicate that their effect on homicide arrest rates lasts throughout the cohort's life course.

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