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The Cohort-Size Sample-Size Conundrum: An Empirical Analysis and Assessment Using Homicide Arrest Data from 1960 to 1999

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A number of studies use the Age–Period–Cohort Characteristic (APCC) model to address the impact of cohort related factors on the age distribution of homicide offending. Several of these studies treat birth cohorts as spanning several years, an operationalization that most closely matches tenets of cohort theory, yet sharply reduces the number of observations available for analysis. Other studies define birth cohorts as those born within a single year, an operationalization that is theoretically problematic, but provides many more observations for analysis. We address the sample size problem by applying a time-series-cross-section model (panel model) with age-period-specific homicide arrest data from the United States for each year from 1960 to 1999, while operationalizing cohorts as five-year birth cohorts. Our panel model produces results that are very similar to those obtained from traditional multiyear APCC models. Substantively, the results provide a replication of work showing the importance of relative cohort size and cohort variations in family structure for explaining variations in age-period-specific homicide rates. The additional observations provided by our approach allow us to examine these relationships over time, and we find substantively important changes. The year-by-year estimates of the age distribution of homicide offending help us to examine the model during the epidemic of youth homicide.

KEY WORDS: pooled time series; Age–Period–Cohort Characteristic models; age distribution of homicide; cohort effects; epidemic of youth homicide.

1. INTRODUCTION

Analyses that attempt to examine cohort effects are now a common part of the social science literature. These analyses cover such wide ranging

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areas as political attitudes and voting (Weil, 1987; Firebaugh and Chen, 1995; Alwin and Krosnick, 1991), differences in verbal ability (Alwin, 1991), sex role attitudes (Mason and Lu, 1988), anti-black prejudice (Firebaugh and Davis, 1988), age at first marriage (Goldstein and Kenney, 2001), generational differences in income (Welch, 1979), and suicide rates (Barnes and Schober 1986; Pampel, 1996). Understanding the nature of cohort effects and how they are distinct from those related to age or period is important both for advancing theoretical understanding and for developing appropriate social policy, yet determining the nature of cohort effects can be methodologically challenging.

One of the more promising techniques used to assess cohort effects is the Age–Period–Cohort Characteristic (APCC) model (Mason *et al.*, 1973; O'Brien, 2000). Its promise lies in its ability to control for the effects of Age and Period while focusing on the reasons for cohort differences. A number of criminological studies concerned with cohort effects on age-period-specific crime rates have used this approach. Within this literature, some researchers have constructed cohorts consisting of multiple years, often five-year age groupings (O'Brien, 1989; O'Brien *et al.*, 1999; Savolainen, 2000). The use of multiple years in an APCC analysis has theoretical advantages and conforms most closely to Easterlin's advice that ''generations'' and ''cohorts'' are interchangeable. Easterlin states, ''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'' (Easterlin, 1987, p. 7). Easterlin emphasized the effects of a cohort's (generation's) impact on entry-level jobs when it reached maturity. With hordes of entry level job seekers flooding the job market over a number of years, there would not be enough entry-level jobs available and as a result there would be more unemployment, lower wages, delayed marriages, and other negative outcomes. A single year of inflated births would not likely impact the job market greatly, but a large group of individuals spanning several years would. Similarly schools could adjust to a single year bulge in the population of third graders by shifting a teacher to the third grade and then one to the fourth, but a five-year bulge in students would create a less tractable problem.

Yet these multiyear operationalizations reduce the number of observations available for analysis, creating a high methodological cost and making it difficult for those using the multiyear definition of cohorts to answer many interesting questions concerning the constancy of the effects of cohort characteristics across time and across ages. Those using the multiyear operationalization also face the challenge of deciding which multiyear operationalization to use (how many years the cohorts should span) and which set of periods to use (e.g., for five year cohorts should the periods be 1960, 1965, 1970, etc. or 1962, 1967, 1972, etc.).[4]

Other authors have chosen to conceptualize cohorts as involving only one birth year (e.g., Menard and Elliott, 1990; Steffensmeier, *et al.*, 1987; Steffensmeier, *et al.*, 1992). This conceptualization provides obvious methodological advantages, for it generates many more observations for analysis, avoids the charge of possible selection bias in terms of the periods used in the analysis, and with the extra observations can help facilitate the examination of various interaction effects. Yet while this operationalization of a birth cohort produces these methodological advantages, it has (from our point of view) a very high theoretical cost, for the identification of cohort with generation virtually disappears.

Thus while the multiyear cohort users adhere more closely to Easterlin's definition of a cohort as a generation—a possible theoretical advantage—they often face strict restrictions in sample size—a methodological disadvantage. While those using single-year cohorts have much larger sample sizes to work with, their conceptualization of a birth cohort as involving only one year runs counter to the classic writings in the field and can be seen as theoretically limited. We label this problem the "cohort-size sample-size conundrum." In this paper we try to move beyond this conundrum by using a method that retains the strength of a multiyear conception of cohorts, yet uses information from single-year periods. We assess the utility of this method and use it to replicate and extend previous analyses of cohort effects on the changing age distribution of homicide arrest rates.

2. RELATED LITERATURE

2.1. Cohort Analyses

Much macro-level and demographic literature on cohort effects derives from the work of Richard Easterlin (1980) and his examination of the effects of the relative size of birth cohorts in a population. As noted above many cohort effects require that a critical mass of people be impacted over a sufficient length of time. Thus, most of those who use the APCC model use multiyear cohorts, with age groups and periods that are spaced "appropriately" for the multiyear cohorts; for example, five-year cohorts (those born between 1930 and 1934, 1935 and 1939, etc.) with five-year age groups (15–19, 20–24, ..., 45–49), and five year spacing between periods (1950,

[4]Data availability helped to drive the choice of a five-year cohort used in this paper, since the UCR data on homicide arrests are aggregated in this manner. To use cohorts of less than five years would require estimating arrest data for these shorter periods (except for the ages 15 to 24, where single year arrest rates are available).

1955, . . . , 2000). The researcher then examines theoretically relevant characteristics of cohorts that might be related to the dependent variable, such as, the relative size of the birth cohort born between 1930 to 1934 when the cohort members were a particular age (15 to 19) or the sex ratio of the cohort that was born between 1930 and 1934 when it was 15–19. By including dummy variables for each of the age groups (except for a reference group) and each of the periods (except for a reference period) it is possible to isolate cohort effects that are independent of age and period. The inclusion of the dummy variables for age and period also provides very strong controls for variables that are associated with age groups (to the extent their effects are constant across periods) and variables associated with periods (to the extent their effects are constant across age groups). For example, to the extent that changes in medical technology affect the homicide rate in different periods for all age groups in the same manner, this effect is controlled for by the inclusion of dummy variables. With strong controls for period and appropriate selection of the cohort variables, the APCC model can be especially helpful in assessing changes in the age distribution of a phenomenon such as homicide rates. Finally, when tests of the APCC model use multiyear cohorts they provide insights into generational variations and generational change.

Yet, as noted above, the APCC model, as it is often applied with multiyear cohorts, has a distinct disadvantage. For instance, in the traditional APCC analysis, when researchers use five-year birth cohorts, they use five-year age groups and periods that are spaced five-years apart. Because of this, the number of observations available for analysis equals the number of periods times the number of age groups. If the researcher used single year cohorts with the same data set, the number of observations available would be greater by a factor of twenty-five (the age groups are only one year apart and the periods are only one year apart). Giving up a twenty-five-fold increase in observations is a high price to pay for using the multiyear conceptualization of a cohort, and because of it, many analyses using APCC models have few observations.[5] As a result analysts using this model have found it difficult to examine some theoretically important hypotheses regarding interaction effects and changes in the relationship of cohort characteristics to the dependent variable over time. In addition, because data are examined at only five-year intervals (periods spaced five-years apart), the choice of which set of periods to examine might be considered arbitrary and the results as only one of several possible sets of results that could be derived from the existing data.

[5]For example, Kahn and Mason (1987) have 96 cases; Savolainen (2000) has 56 cases; and O'Brien (1989) has only 42 cases.

In this paper we explore the use of a pooled time series analysis to examine the APCC model, retaining the strength of a multiyear conception of cohorts, but using information from single year periods. We retain the structure used in the traditional APCC model with five-year birth cohorts and five-year age groupings, but, instead of analyzing data only from periods spaced five-years apart (e.g., age-period-specific homicide rates for five-year age groups in 1960, 1965, 1970, etc.), we construct a data set that includes measures of these variables for each year (e.g., homicide rates for five-year age groups in 1960, 1961, 1962, etc.). In contrast to studying a set of discrete birth cohorts (e.g., people born in 1940–1944, those born in 1945–1949, etc.), we include a larger set of overlapping birth cohorts (e.g., people born in 1940–1944, 1941–1945, 1942–1946, etc.). Similarly, the same age group will contain some of the same people in it multiple times, e.g., in 1941 those 20 to 24 will contain some of the same people who were 20 to 24 in 1940.

This data set has five times more observations than the standard model using five-year cohorts, while retaining the notion of a cohort as a generation. Additionally, because information is provided about all possible five-year cohorts (data for one-year periods), it does not suffer the problem of arbitrary selection of periods.[6] Perhaps the most obvious limitation of this design is that the cohorts involved are overlapping and each age group contains some of the same people multiple times. We employ several statistical corrections used in time-series-cross-section analysis to address this issue (see, Beck and Katz, 1995; Pampel, forthcoming).

2.2. Changes in the Age Distribution of Homicides

Fifteen years ago one might convincingly have argued that there was little need to investigate shifts in the age distribution of homicide over time. After all, the age distribution of U.S. homicide had been remarkably stable since 1960 when the first reliable data for the age distribution of homicide offending in the U.S. became available.[7] True, the homicide rate itself had increased dramatically in the 1960s rising from a rate of 5.1 per 100,000 in 1960 to 7.9 in 1970 and peaking at 10.2 in 1980. But during that time span the proportion of homicides committed by different age groups appeared to be reasonably stable within each year. Thus, although the rates of homicide

[6]It does suffer the problem of having chosen a five-year rather than a ten-year or one-year cohort.

[7]Reasonably reliable data on homicide offenders broken down by age (predominantly five-year age groupings) for the United States as a whole first became available in 1960 for arrest data reported in the Uniform Crime Report (FBI, 1961).

in different age groups shifted upward or downward during the period, the shape of the age distribution did not shift dramatically.

In the early 1980s two prominent criminologists Travis Hirschi and Michael Gottfredson (1983) maintained that the age-distribution not only of homicide but also of crime in general was invariant. The mean rates of homicide might shift upward or downward, but the shape of the age distribution was "invariant." A small literature developed to challenge this invariance thesis (Britt, 1992; Farrington, 1986; Greenberg, 1985; Steffensmeier *et al.*, 1989). The response to this literature by Hirschi and Gottfredson (Gottfredson and Hirschi, 1990; Hirschi and Gottfredson, 1994) focused on how minor, trivial, or unimportant were the shifts that these authors cited.

Shortly after the middle of the 1980s, however, a remarkable change in the age distribution of homicide offending occurred. This dramatic shift came to be labeled the "epidemic of youth homicide" by researchers and public commentators alike (Cook and Laub, 1998). For example, in 1985 the homicide arrest rate for those 15 to 19 was 16.3 per 100,000, but the rates for that age group more than doubled to 36.5 by 1990. Further, the rate for those 20 to 24 years of age increased by nearly 40%. Over the same two periods the rates for each of the five-year age groups 25 and older declined.

The most popular explanation for this dramatic increase in youth homicides points to the rise of the crack cocaine markets in the mid to late 1980s. For instance, Blumstein and Cork (1996; also Cork, 1999), suggest that these markets did not have the kind of control and organization that characterized the more established cocaine or heroin markets. A hit of crack cocaine was relatively inexpensive, and youth were heavily involved in the marketing and distribution of the drug. They used weapons to establish and control their territories, and other youth armed themselves for protection. This sharp increase in the ownership and use of guns resulted in an increase in homicides among youth even at a time when the homicide rates for those 25 years and older were dropping. While intuitively appealing, this explanation is *ad hoc* in the sense that it was designed specifically to explain this dramatic upturn in youth homicide that occurred around the mid 1980s.

A second, less prominent, explanation is based on cohort theory of the type explored in this paper. Although the literature associated with Age–Period–Cohort models most often refers to shifts in age-period-specific rates (or means, proportions, etc.), such changes associated with cohort differences result in a shift in the age distribution. Thus, changes in cohorts can be used to explain shifts in the age distribution over time. For homicide offenses, O'Brien *et al.* (1999) and Savolainen (2000) specifically attributed the shift in the age distribution of homicides to changes in the characteristics of the cohorts that enter each of the age groups in different periods. This cohort-based explanation envisions shifts in the age distribution of homicide as resulting from replacements of a cohort in an age group by a different cohort entering that age group in a different period. The new cohort may be more or less prone to producing homicides than the cohort that previously occupied that age group. This change is reflected in shifts in the shape of the age distribution. Two cohort level variables have been most prominent in explaining these differences among cohorts: relative cohort size and cohort family structure.

2.2.1. Relative Cohort Size

The literature provides several explanations of the effects of relative cohort size on the age distribution of homicide. One explanation focuses on the financial disadvantages experienced by members of relatively large cohorts when they enter the labor market. Given the relatively large number of new entrants into the labor market, they are less likely to find a job, especially quality entry-level employment. Because of this, members of relatively large cohorts are more likely to put off marriage and child bearing. Savolainen (2000), citing Menard and Elliott (1990), notes that strain theory predicts that diminished chances of economic security will promote higher levels of deviant behavior. Another explanation emphasizes that members of larger cohorts, in comparison to smaller cohorts, overload institutions of social control simply because they don't have (per person) as many adult figures in their lives, such as parents, teachers, school counselors, or ministers (O'Brien, *et al.*, 1999; O'Brien, 1989; Steffensmeier *et al.*, 1992). As a result, they are less well socialized, exhibit lower degrees of social control and, thus, higher levels of deviant and criminal behavior. These authors have theorized that there should be a positive effect of relative cohort size on homicide rates.[8]

Several authors suggest that the effects of relative cohort size on outcomes as diverse as political alienation (Kahn and Mason, 1987) and homicide (Steffensmeier *et al.*, 1992) should be greatest for the young. It is at these ages that individuals are most vulnerable to the effects of relative cohort size: when they reach the job market, when they delay marriage, etc. Others (Holinger *et al.*, 1988; Pampel and Peters, 1995; Pampel and Williamson, 1985), note that there may be beneficial effects of being in a relatively large cohort when the cohort is old, since it may result in greater political power, increased legislation protecting the welfare of the old, and so on. We note, however, that our oldest age group is 45 to 49. The 45–49

[8]Other studies have found cohort effects using crime rates as the dependent variable but, from our perspective, used inadequate measures or failed to control for both age and period effects (Easterlin and Schapiro, 1979; Maxim, 1985; Smith, 1986). O'Brien (1989) addresses the inadequacy of these studies.

year age group may not be old enough for any advantage to cohorts that are relatively large in size when they are "old" to occur.

The effects of relative cohort size may also shift across time. Pampel and Peters (1995) in a comprehensive review of the literature suggest that the relationships between relative cohort size and a number of demographic variables (e.g., timing of marriage, unemployment, and entry level wages) were substantial from the end of WWII to the early 1980s. Since that time, however, there has been little if any relationship. They offer several potential explanations for this shift, including women's changing economic roles and increases in immigration.

2.2.2. Family Structure

Two studies have examined how changes in the composition of cohorts' childhood families are related to age-period-specific homicide rates. Savolainen (2000) operationalized his measure of cohorts' childhood families as the percentage of single parent households with children 5 to 9 years of age when the cohort was 5 to 9. This measure focuses on single parent families at a crucial life stage for cohort members. Savolainen (2000, p. 123) notes that childhood family structure is a major correlate of "economic disadvantage, which is the critical variable in the strain-theoretical explanation of crime." O'Brien, *et al.*'s (1999) measure of cohorts' childhood family structure is the percentage of children in the cohort born to unwed mothers—a measure highly correlated with Savolainen's measure.[9] O'Brien *et al.* (1999) note the financial disadvantages associated with such families, but extend their argument to encompass the central role of family structure in ensuring that children experience monitoring and supervision, key elements in both the development of internal social control (self-control) as well as a major source of external social control.

Both Savolainen (2000) and O'Brien *et al.* (1999) suggest that the effects of the composition of cohorts' childhood families will endure throughout the life span of the cohorts. This enduring effect reflects the long-lasting impact of both childhood economic disadvantage and lower levels of internal social control. Thus, unlike the expectations of some researchers regarding relative cohort size, the literature does not lead us to expect dif-

[9]Savolainen (2000) used Public Use Micro Sample (PUMS) census data for the percentage of those in five-year birth cohorts who lived in single parent families when they were ages 5 to 9. Because these data are only available for the years 1910, 1940, 1960, 1970, 1980, and 1990 he estimated this cohort characteristic for the years 1915–1939, 1941–1959, and for single years between the other decennial censuses. He then aggregated these estimates into figures for five-year birth cohorts. The correlation of O'Brien *et al.*'s (1999) measure of NMB with Savolainen's measure is 0.98. The correlation between the first differences of these two measures is 0.90.

ferences in the effects of cohort family structure across age groups. We will, however, test for such an age group by cohort family structure interaction to examine this possibility.

Initial evidence from O'Brien *et al.* (1999) suggested that family structure had similar influences on homicide rates from 1960 to 1985 and from 1960 to 1995. Yet their analyses did not compare distinct time periods. We will explicitly examine the extent to which the effects of family structure have shifted over time. Such an analysis is especially important given that changes in family structure have been substantially greater in recent years than in earlier periods.

Although questions about shifts in the relationship of RCS and of changes in cohorts' childhood family structure have received some attention in the literature, the small sample size resulting from the multiyear operationalization of cohorts has made it difficult for those using the multiyear definition of cohorts to answer many interesting questions concerning the constancy of the effects of these cohort characteristics over time and across ages. Is the relationship of RCS and homicide rates stronger for younger age groups than for older age groups? Has the relationship between cohorts' childhood family structure and age-period-specific homicide rates changed over time? With our use of pooled time series, we are able to address these questions in our analyses below.

To summarize, this paper addresses both methodological and substantive issues. Our first two major research questions are methodological in nature. First, we use data from five-year cohorts for multiple years to examine the extent to which using different sets of periods (spaced five-years apart) produces different sets of results. Second, we pool the data from these five different data sets, in the manner described in the methodology section, to assess the degree to which our pooled time-series-cross-section analysis provides results that are comparable to those obtained through the traditional method. Finally, we use the pooled data to replicate and extend the analyses of cohort effects on homicide rates and to test changes in these cohort effects over time and differences in these effects for the younger and older age groups. Our substantive results also address the controversy regarding the invariance of the age-distribution of homicide.

3. METHODOLOGY

3.1. Data and Measures

Data on the number of homicide arrests by age come from the Uniform Crime Reports (UCR) for the years 1960 to 1999 (FBI, 1961 to 2000). The UCR homicide arrest data provide the only data source that contains the number of homicide offenders broken down by age for the United States over this extended period of time. Since 1960 these reports have presented data for homicide arrests by each year of age from 15 to 24 and for five-year age groupings from 25–29 to 45–49. We do not use the UCR data before 1960. Before that year data for rural areas broken down by age were not included in the reports, and before 1958, no data on homicide arrest by age were included in the UCR. The Current Population Reports (CPR): Series P-25 (U.S. Bureau of the Census, various years) provide data for the number of U.S. residents in each of the age groups. These data were used in computing homicide offense rates per 100,000.

We adjusted these rates because not all of the law enforcement agencies that represent the total U.S. population reported homicide arrests to the FBI in each of the years. The adjustment involved multiplying each of the computed rates by the ratio of the CPR estimated total U.S. population to the number of U.S. residents covered by the law enforcement agencies reporting to the UCR. For each year, this simply adjusts upward (by a *constant proportion* for that year) the age-specific offense rates.[10] U.S. Government publications provide the data for Relative Cohort Size (RCS) and for the percentage of Non-Marital Births (NMB) that we use in this paper. Data for RCS are derived from the CPR (U.S. Bureau of the Census, various dates). *Vital Statistics of the United States* (U.S. Bureau of the Census, 1946, 1990) provide data for the percentage of births to unwed mothers.

The operationalization of RCS that we use throughout our analysis is the percentage of the population age 15 to 64 that was 15 to 19 when the birth cohort was 15 to 19.[11] We use this measure because it is similar to the measures most commonly used in the literature. It is identical to one of the measures used by O'Brien (1989) and to one of the two measures of RCS used in O'Brien *et al.* (1999).[12] Steffensmeier *et al.* (1992) operationalize RCS as the percentage of the population from 15–64 who were 18 when the cohort was 18. This corresponds to their single-year definition of birth cohorts. Nevertheless, Steffensmeier *et al.*'s (1992) definition of RCS is conceptually close to the one we use for five-year age grouping with regards to the

[10]This adjustment does not affect the substantive conclusions of our analysis with regard to the effects of the cohort characteristics on the age-period-specific homicide rates. The period effects fully capture these adjustments. For example, when we multiplied the homicide rates by 2 for 1990 and ran the analysis using the natural log of these rates the only coefficient or standard error of a coefficient to change was that for the period 1990.

[11]When we used a measure based closer to the cohort's year of birth—the percentage of the population age 0 to 49 that was 0 to 4 when the birth cohort is 0 to 4 the results are very similar.

[12]O'Brien *et al.* (1999) also operationalize RCS as the percentage of the population 15 to 64 that is in the cohort when it is at each of the age groups; for example, the percentage in the age group 30 to 34 when the cohort is 30 to 34. This measures changes for each cohort at each age level. The results using either operationalization, in this case, were similar.

ages compared: youth in the 15 to 19 year age category (albeit a single year) with the total population 15 to 64. Savolainen (2000, p. 125) measured RCS as the "number of people in ages 15 to 19 divided by the number of those in ages 20 to 64 in the year when the cohort members were 15 to 19 years old."[13]

To determine the percentage of non-marital births for a birth cohort, we compute the mean percentage of non-marital births over the five years that constitute the birth cohort. The percentage of all live births that were to unwed mothers is available yearly from 1917. For seven cohorts, those born between 1910–1914, 1911–1915, ..., 1916–1920, data on non-marital birth does not exist for each year of the five year birth cohort. In these cases we coded the percentage of non-marital births as missing. This reduced the number of observations for our analysis from 280 to 271.

3.2. Analysis

Figure 1 is a schematic presentation showing how we organized our data for analysis. The first column indicates the seven age-groupings used. Each age group spans five years and these seven groups cover the ages for which data are available from the UCR. The columns of the table represent the periods for which we have data from the UCR on homicide arrests by age. Each cell in the body of the table represents an age-period-specific homicide rate (APSHR): our dependent variable, and we have subscripted each of these rates with the birth cohort that is that age in that period. For example, in Fig. 1 we see that the age group 15 to 19 in 1960 corresponds to the birth cohort born between 1940 and 1944 (stippled entry in the second column). In 1965 an entry for this birth cohort appears again when it now corresponds to the age group that is 20 to 24 years of age. In the typical APCC analysis one would analyze, for example, the data for periods 1960, 1965, 1970, ..., 1995 (see, for example, Fig. 1 in O'Brien *et al.*, 1999).

Equation (1) represents the basic APCC model that we use in our analyses, although the formula is elaborated in some of our other analyses.

$$\ln(\text{APSHR})_{ij} = \mu + \alpha_i + \pi_j + \rho \ln(R_k) + \beta \ln(B_k) = e_{ij} \qquad (1)$$

Where APSHR_{ij} is the age-period-specific homicide rate (values in the cells represented in Fig. 1), μ is the intercept, α_i is the age effect for the ith age group, π_j is the period effect for the jth period, ρ is regression coefficient for relative cohort size, R_k is the relative cohort size for the kth cohort, β is the

[13]Savolainen used an odds ratio approach to the measurement of RCS, while O'Brien *et al.* (1999) use a percentage approach. Other than that, the two measures are equivalent. If we let p represent the percent measure used by O'Brien *et al.* (1999) and r represent the ratio measure used by Savolainen (2000), then we can easily transform p into r using the following formula: $r = p/(100 - p)$. The transformation is non-linear, so regression results using one or the other measure might differ.

AGE	PERIOD								
	1960	1961	1962	1963	1964	1965	1966	...	1999
15-19	**APSHR**$_{1940-1944}$	**APSHR**$_{1941-1945}$	**APSHR**$_{1942-1946}$	**APSHR**$_{1943-1947}$	**APSHR**$_{1944-1948}$	**APSHR**$_{1945-1949}$	**APSHR**$_{1950-1954}$	...	**APSHR**$_{1979-1983}$
20-24	**APSHR**$_{1935-1939}$	**APSHR**$_{1936-1940}$	**APSHR**$_{1937-1941}$	**APSHR**$_{1938-1942}$	**APSHR**$_{1939-1943}$	**APSHR**$_{1940-1944}$	**APSHR**$_{1941-1945}$	...	**APRHS**$_{1974-1978}$
25-29	**APSHR**$_{1930-1934}$	**APRHS**$_{1931-1935}$	**APSHR**$_{1932-1936}$	**APSHR**$_{1933-1937}$	**APSHR**$_{1934-1938}$	**APSHR**$_{1935-1939}$	**APSHR**$_{1936-1940}$	...	**APSHR**$_{1969-1973}$
30-34	**APSHR**$_{1925-1929}$	**APSHR**$_{1926-1930}$	**APSHR**$_{1927-1931}$	**APSHR**$_{1928-1932}$	**APSHR**$_{1929-1933}$	**APSHR**$_{1930-1934}$	**APSHR**$_{1931-1935}$	...	**APSHR**$_{1964-1968}$
35-39	**APSHR**$_{1920-1924}$	**APSHR**$_{1921-1925}$	**APSHR**$_{1922-1926}$	**APSHR**$_{1923-1927}$	**APSHR**$_{1924-1928}$	**APSHR**$_{1925-1929}$	**APSHR**$_{1926-1930}$	...	**APSHR**$_{1959-1963}$
40-44	**APSHR**$_{1915-1919}$	**APSHR**$_{1916-1920}$	**APSHR**$_{1917-1921}$	**APSHR**$_{1918-1922}$	**APSHR**$_{1919-1923}$	**APSHR**$_{1920-1924}$	**APSHR**$_{1921-1925}$	...	**APSHR**$_{1954-1958}$
45-49	**APSHR**$_{1910-1914}$	**APSHR**$_{1911-1915}$	**APSHR**$_{1912-1916}$	**APSHR**$_{1913-1917}$	**APRHS**$_{1914-1918}$	**APSHR**$_{1915-1919}$	**APSHR**$_{1916-1920}$	...	**APSHR**$_{1949-1953}$

Fig. 1. Schematic representation of the data set-up for Age–Period–Cohort Characteristic models and the time-series cross-section analysis.

regression coefficient for non-marital births, B_k is the percentage of non-marital births for the kth cohort, and e_{ij} is the error term for the ijth observation. The subscripts run from $i = 1, 2, \ldots, 7$ for the seven age groups; $j = 1, 2, \ldots, 40$ for the forty periods; and $k = 1, 2, \ldots, 46$ since there are 46 cohorts. We chose the oldest age group and the most recent period to be the reference categories and estimated the age and period effects using dummy variables for the age groups and periods.

To test our first research question regarding the extent to which the traditional APCC analysis of these data using different sets of periods spaced five-years apart might produce different sets of results, we conduct five separate APCC analyses of this type using models that contain the periods 1960, 1965, ..., 1995; the periods 1961, 1966, ..., 1996; the periods 1962, 1967, ..., 1997; the periods 1963, 1968, ..., 1998; and the periods 1964, 1969, ..., 1999. In these analyses Eq. 1 is modified to select the appropriate 8 periods and the appropriate 14 birth cohorts. These five analyses provide a degree of "replication" of the analyses based on previous research. The five analyses can be compared to the extent to which they produce similar results even though they vary in terms of the birth cohorts and periods analyzed.

To examine our second research question regarding the comparison of results using different analytic strategies, we pooled our data using time-series-cross-section analyses. The periods included in the analyses are spaced only one-year apart while the five-year age groups correspond to the appropriate five-year birth cohorts. Here we treat the age groups as the fixed units (we use age group dummy variables to estimate these fixed age group effects), and we treat each period as fixed. Each year (period) in the analysis constitutes a cross-section and as we move across the years we have the time series. Combining these data (schematized) in Fig. 1 into a single data set results in a pooled time series that can be analyzed as a time-series-cross-section model. An important issue in the analysis of these data involves overlapping cohorts—for instance, some of the members of the cohort born between 1920 and 1924 are members of the cohort born between 1921 and 1925. Further, the same people appear in the same age group more than once. This does not occur in the typical APCC analysis where the periods are spaced the same number of years as the age range for the cohorts and the age groups. If we examine Fig. 1, we see that these two problems of overlapping membership in cohorts and age groups will reduce the independence of the observations.[14]

[14]One reviewer noted "rhetorically" that if taking observations every year was better than every five-years, why not use monthly data. One answer is that the monthly observations do not give us 12 times as much information as the yearly observations, given that the observations are "less independent." That is, with monthly periods the overlap of the individuals within the age groups and within the cohort would be nearly complete even though we treated the additional observation as a new observation. As noted earlier, our choice of yearly data was influenced by the availability of yearly data on homicides, non-marital births, and population estimates.

When dealing with a pooled data set, the analyst needs to correct for "panel heteroskedasticity," "contemporaneous correlations," and "temporally correlated errors" (Beck and Katz, 1995). In our analysis panel heteroskedasticity would occur if the variances of the error differ from age group to age group. This is likely given that the rates of homicide from age group to age group vary greatly in magnitude. Contemporaneous correlations occur when the amount of error in one age group at one time is correlated with the amount of error in another specific age group. For example, this would occur if the amount of error in the estimated age-period-specific homicide rates were correlated across age groups within periods. Temporally correlated errors occur to the extent that a higher than expected (given the model) rate of homicide in an age group in one year is related to the deviation from the predicted value in the next year for that age group. The overlapping membership of cohorts and age groups lead us to expect such a correlation.

To address these issues, we employ analytic methods suggested by Beck and Katz (1995). They reported on a series of simulations that used OLS estimates of coefficients for a series of time-series-cross-section models and "panel corrected standard errors," which they had developed. The OLS estimates were almost as efficient as GLS estimates for the data and the standard errors were better estimates (the GLS estimates were on average almost 50% too small). STATA has implemented the procedures proposed by Beck and Katz. We follow their recommendations to correct the standard errors for panel heteroskedasticity, contemporaneously correlated errors, and temporally correlated errors, and we report the panel corrected standard errors.[15]

In addition to a time-series-cross-section model with corrections for panel heteroskedasticity and for contemporaneous and temporal correlations, we calculate a simple OLS model on the pooled data without these corrections. We compare this analysis to the corrected time-series-cross-section model and compare the results of both of these models to results from the five separate analyses using the traditional APCC models.

Our third research question involves substantive issues that extend previous analyses of the influence of cohort related variables on age-period-specific homicide arrest rates by examining the extent to which this influence

[15]Given that we have observations across a large number of periods, we allow the temporal correlations to differ for different age groups. The panel corrected standard error developed by Beck and Katz corrects for contemporaneously correlated errors and panel heteroskedasticity. We also analyzed our models using GLS estimates: the point estimates were very similar and the standard errors were only slightly smaller than those from the OLS analyses. The closeness of these standard errors to those based on Beck and Katz's (1995) panel corrected standard errors is likely due to the relatively large number of time periods in comparison to units in our analyses. In this situation Beck and Katz (1995) find that the underestimation of the standard errors using GLS is not as great as when the ratio of time points to units (age groups) is smaller.

varies over the early, middle, and latter years covered by our data and between younger and older age groups. In the process, we provide results that address the hypothesized age-invariance of homicide rates. To examine these issues we use the greater number of observations generated by pooling the five data sets.[16] To test for the statistical interaction of relative cohort size by age groups, we created interaction terms by multiplying the dummy variable for age (say the age group 15–19) times the relative cohort size for the cohort that was in that age group during that period. There were 40 of these multiplications—one for each of the forty periods. We did the same for the interaction of relative cohort size and those age 20–24, age 40–44, and age 45–49. The interactions for the youngest groups allow for a test of the hypothesis that the effects of relative cohort size are greatest when people are young. The two interactions for the oldest age groups allow for a test of the hypothesis that the effects of relative cohort size are less when people are old. Parallel constructions of interaction terms for NMB and age allow us to examine the hypothesis that the influence of NMB is constant throughout the lifespan.

To analyze possible period-cohort characteristic interactions, we created dummy variables for both the earlier years of data (1960–1972) and for the later years (1987–1999) and used these to create interactions terms for RCS for both the early and the late years. For the early years, we multiplied the dummy variable for early years by the relative cohort size measures. Multiplying the dummy variable for later years by the relative cohort size measures created the interaction terms for the later years. The same process produced interaction variables for NMB for the early years and for the later years. These interaction terms enabled us to test whether the effects of cohort related variables have changed systematically over time. The refer-

[16]For the analyses using five-year age groups and five-year spacing between periods, our data (as depicted in Fig. 1) consists of seven age groups and eight periods. There are thus 56 age-period-specific homicide rates that we are trying to "predict." With 6 age group dummy variables, 7 period dummy variables, and 2 cohort characteristics, there are 15 independent variables and 56 observations. This makes it extremely difficult to test for RCS by age-group interactions and changes in the effects of RCS and NMB from the early to later years. For example, when we create an interaction term for RCS by the age group 15–19, we are testing to see if that interaction is statistically significant with only eight observations: one for each period. When we conduct the time-series cross-section analysis, we have seven age groups and forty periods and thus 280 observations (271 observations considering missing data). There are 6 age-group dummy variables, 39 period dummy variables, and 2 cohort characteristics; that is, 47 variables and 280 observations. When we compute the interaction for RCS by the age group 15–19, it is based on 40 time periods and thus 40 observations. This test is a more powerful one for detecting this interaction. Similar statements hold for situations where we test changes in the magnitude of the coefficients for RCS and NMB over time. That is why we use the pooled data for these tests.

ence category for these interaction terms is the middle period from 1973 to 1986.

In all of our analyses the natural log of the age-period-specific homicide rate served as the dependent variable. We also logged the two cohort characteristics: relative cohort size and the percentage of non-marital births. This log-log transformation is a departure from past literature, but it is in line with the intention of O'Brien *et al.* (1999). They wanted to make sure that, after controlling for age and period effects, a doubling of the rate of homicide for those 20 to 24 was no more (or less) important than a doubling of the rate for those 45 to 49. Our operationalization accomplishes this and makes sure that proportionate shifts in the percentage of non-marital births (relative cohort size) are associated with proportionate shifts in homicide rates controlling for age and period. The log-log transformation also facilitates the interpretation of the coefficients associated with the cohort characteristics as elasticities: a one percent change in the independent variable is associated with a "b" percent change in the dependent variable.

3.2.1. Statistical Controls

We use many degrees of freedom in our analyses by including the dummy variables for each of the age groups and periods. We could avoid this loss by, for example, creating a linear term to represent age (with a value of the midpoint of each of the age-group categories). That, of course, would not be a wise choice to capture the obvious non-linear relationship between age and homicide rates. While adding a linear and a quadratic term to the equation to capture the age curve would be better than using only a linear term, we also rejected this option. Instead, we used the full set of age group dummy variables and period dummy variables because they do not assume that these effects are linear, quadratic, or some other function, but instead controlled for the effects of age groups and period as completely as possible. We wanted to detect the effects associated with cohorts and not have these effects confounded with uncontrolled effects of age and period. For that reason, we followed the tradition in APCC models of using dummy variables to code and control for the effects of age and period.[17]

The dummy variables for age and for period also control for the effects of many variables not explicitly included in our model. For example, to the extent that the effects of variables such as news coverage surrounding

[17]The importance of controlling for the main effects of age and period when examining cohort effects is highlighted in recent exchanges in the *American Sociological Review* on cohort effects and changes in vocabulary scores over time (Alwin and McCammon, 1999; Glenn 1999; Wilson and Gove, 1999a, 1999b).

homicides, increased criminal sanctions, changes in medical technology, or downturns in the economy are relatively constant across age groups, then the effects of these variables are controlled for by the period dummy variables.[18] The same argument holds for the age-group dummy variables. They control for factors associated with age that are constant across periods. Thus, including these controls provides a rigorous indication of the effect of cohort related variables. The inclusion of the set of age and period dummy variables also controls for any linear trends associated with cohort characteristics. As noted by O'Brien *et al.* (1999), this occurs because of the linear dependency of the cohort "number" on the full set of age and period dummy variables. For example, if a cohort characteristic were related to homicide rates simply because it was linearly related to the time of the cohorts' birth, this linear relationship would be controlled for.

3.2.2. Non-Independent Residuals Due to Cohorts

O'Brien *et al.* (1999) developed a test for "autocorrelation due to cohorts." They did this because most cohorts contribute multiple age-period-specific homicide rate entries. For example, in Fig. 1 data associated with the cohort that was born between 1940 and 1944 appears in the upper left-hand data cell for the age group 15–19 in 1960. Data for essentially the same group of people (granted that some have died, some emigrated, and some have immigrated) appears again for the 20–24 year old age group in 1965. By 1990 this cohort is 45–49 and makes its final appearance in the data set. Anything that makes this cohort more (or less) likely to commit homicide that is not predicted by the age and period dummy variables or by the two cohort characteristics will lead to the residuals in that cohort being more likely to be positive (or negative). O'Brien *et al.* (1999) labeled this phenomenon "autocorrelation due to cohorts."

Their correction involved inspection of the residuals after which they created a dummy variable for the cohort that appeared to contribute most to the autocorrelation due to cohorts and entered that dummy variable into the analysis. We use a different method to deal with non-independence within cohorts. The "cluster" option in STATA permits us to specify the cohorts as clusters. This option allows the analyst to conduct an OLS regression analysis without requiring that the observations be independent within the cohorts. The regression coefficients are the same as with a regular

[18]More technically, we control for variables associated with period that are constant across age groups and variables associated with age that are constant across periods. To the extent that some of these variables are only relatively constant, the control is incomplete. But the control extends to variables associated with period and age that are not explicitly included in the equation.

OLS analysis, but the standard errors of the coefficients are corrected for this non-independence within the cohorts.[19]

4. RESULTS

4.1. Descriptive Statistics

Table I presents descriptive statistics for the dependent variable and the two cohort characteristics. The mean homicide rate is based on the 280 age-period-specific rates. The mean of 14.0 is higher than the average rate for the country as a whole over this time period because it is based on those between 15 and 49 years of age. The rates for those 0 to 14 and 50 and older are considerably lower than the rates for those in the 15 to 49 year age range. The rates range from 2.7 per 100,000 (for those 45 to 49 in 1999) to 41.4 per 100,000 (for those 15 to 19 in 1993). The descriptive statistics for the two cohort characteristics are weighted by the number of times they appear in the analyses. For example, the most recent cohort has an entry only for those 15 to 19 in 1999 and enters into the regression analyses only once as a predictor of the logged age-period-specific homicide rate. On the other hand, the cohort born between 1920 and 1924 appears in the analysis seven times. The mean RCS is 13.1 with a range from 10.3 to 16.1. Thus there has been a fair amount of variability about the RCS mean. NMB has shown considerably more variability. Its mean is 5.1 and its values have ranged from 2.3 for the cohort born between 1917 and 1921 to 18.7 for cohort born between 1979 and 1983, although as noted in O'Brien *et al.* (1999), the increase in NMB over time has not been monotonic.

4.2. Consistency of Results with Different Cohort Groupings

Table II presents the results from the five separate APCC models that allow us to address our first research question regarding the consistency of results across cohort groupings based on different selections of periods. In these analyses the time between periods is the same as the time spanned by the age groups and the number of years covered by the birth cohorts. The results for the analysis labeled 1961:1996 in Table II, for example, are based on an APCC analysis of data for the periods 1961, 1966, . . . , 1996 and age groups 15–19, 20–24, . . . , 45–49. There are seven age groups, eight periods, and 14 birth cohorts: the first born between 1911 and 1915, the second born

[19] We use this method only for the OLS analyses in Table II and the pooled OLS analysis in Table III. This option is not available for models using the standard corrections for time-series-cross-section analyses.

Table I. Descriptive Statistics for Age-Period-Specific Homicide Death Rates, Relative Cohort Size, and Non-Marital Birth Rates

	Homicide rate	Relative cohort size	Non-marital birth rate
Mean	14.0	13.1	5.1
S.D.	7.67	1.7	2.9
Range	2.7–41.4	10.3–16.1	2.3–18.7

between 1916 and 1920, and the last born between 1976 and 1980. This last cohort was 15 to 19 in 1996.

To save space and because our major focus, like that of most analyses that use the APCC model, is on the effects of cohort characteristics after controlling for age and period, we do not report the coefficients for the dummy variables for age groups and periods in Table II. In each of the analyses the relationship of the logged values of RCS and NMB to the logged age-period-specific homicide rates is positive (as predicted) and statistically significant ($p < 0.05$, one-tailed test).[20] When examining the results altogether, the coefficients for both LNRCS and LNNMB seem to remain fairly stable throughout these analyses. When we conducted a t-test for the difference between two regression coefficients from independent samples, we found that the RCS coefficients and the NMB coefficients do not differ significantly from analysis to analysis.[21]

4.3. Comparing Multiyear and Single-Year Analyses

Table III presents results from analyses in which we pooled the data across all of the years while maintaining the five-year age groups and five-year birth cohorts. The first model, "OLS Model of Pooled Data," reports the results of a standard OLS analysis with no corrections for heteroskedasticity, contemporaneously correlated errors, and temporally correlated errors but using the cluster option for cohorts. We include this

[20]In each of these analyses we specified the birth cohorts as clusters to correct for heterogeneity due to cohorts and used the robust standard errors provided by STATA. The reported regression coefficients are the same as with an OLS analysis, but the standard errors are corrected for the heterogeneity.

[21]The test we use does not assume homogeneity of error variance for the two models. Thus it is based on an estimate of the standard error of the difference between two regression coefficients of the following form $\sqrt{(SEb_1)^2 + (SEb_2)^2}$. We used a two-tailed test, since we did not predict which sets of periods would yield the strongest relationships. This test is somewhat crude, since these data do not constitute fully independent samples. We investigate shifts in the effects of the cohort characteristics more rigorously in our pooled time-series analyses.

Table II. Age-Period-Cohort Characteristic Model Results Corrected for Heterogeneity Between Cohorts

Beginning and ending periods for the analysis	b	Cluster corrected robust standard error	t	model R-square
1960:1995				
LNRCS	1.309	0.684	1.91	
LNNMB	1.792	0.518	3.46	0.957
1961:1996				
LNRC	1.386	0.407	3.41	
LNNMB	1.920	0.360	5.33	0.960
1962:1997				
LNRCS	2.154	0.453	4.75	
LNNMB	2.365	0.275	8.59	0.961
1963:1998				
LNRCS	1.500	0.347	4.33	
LNNMB	1.919	0.229	8.40	0.965
1964:1999				
LNRCS	1.298	0.346	3.75	
LNNMB	1.750	0.261	6.72	0.965

analysis, so that we can compare results from it with the results of the five separate analyses reported in Table II. We also want to compare results from this pooled model that is not corrected for panel heteroskedasticity and contemporaneously and temporally correlated errors with the results of Model 1 in Table III, which employs these corrections. The coefficients for LNRCS and LNNMB in the OLS model of the pooled data of 1.495 and 1.921, respectively, are very similar to the average coefficients for the five separate models reported in Table II: 1.529 and 1.949, respectively. Thus, these pooled OLS results are similar to the average of the five un-pooled analyses that contain all of the data in the pooled model.

Model 1 in Table III employs the corrections for panel heteroskedasticity, and contemporaneously and temporally correlated errors and uses panel corrected standard errors.[22] The estimates of the coefficients for

[22]Specifically we use the STATA program XTGLS to analyze the data and use the following options: corr(psar1), pcse. We use the corr(psar1) option, which corrects for temporally correlated errors (lag 1) within age-groups rather than the corr(ar1) which uses a lag 1 correlation estimated across all of the age groups because we have a sufficient number of periods to estimate accurately this effect within each age group. The lag 1 error structure is the only one available in STATA and the one recommended by Beck and Katz (1995).

Table III. Age–Period–Cohort Characteristic Models Using Time-Series-Cross-Section Analysis: 1960–1999

	OLS Model of pooled data		Time-series cross section models											
			Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	B	*z*	*B*	*z*	*B*	*z*	*B*	*z*	*B*	*z*	*B*	*z*	*B*	*z*
Age														
15–10	−0.334	−2.60	0.193	1.50	0.159	1.28	0.15	0.15	0.500	4.68	3.883	4.51	−0.638	−4.00
20–24	0.252	2.40	0.673	7.49	0.647	7.36	0.521	6.53	0.918	11.39	1.901	2.15	−0.057	−0.48
25–29	0.253	2.94	0.563	6.34	0.540	6.02	0.453	6.04	0.778	9.42	0.183	0.29	0.467	3.95
30–34	0.215	3.32	0.446	6.88	0.431	6.81	0.370	6.52	0.617	11.28	0.034	0.05	0.247	2.17
35–39	0.204	4.54	0.364	5.93	0.355	5.97	0.311	6.78	0.472	12.08	−0.071	−0.011	0.085	0.72
40–44	0.127	4.32	0.217	4.73	0.212	4.73	0.183	5.48	0.258	9.72	0.005	0.01	0.373	2.55
44–49	0.000	0.00	0.000	0.00	0.000	0.00	0.000	0.00	0.000	0.00	0.000	0.00	0.000	0.00
LNRCS	1.495	6.91	0.791	4.50	0.831	4.78	0.983	5.98	0.420	2.56	0.959	6.06	0.236	1.45
LNNMB	1.921	12.17	1.240	9.70	1.283	10.22	1.450	12.34	0.663	5.26	1.087	10.31	0.594	4.69
Early LNRCS	—	—	—	—	−0.004	−0.20	—	—	0.143	3.09	—	—	—	—
Late LNRCS	—	—	—	—	−0.005	−0.25	—	—	−0.272	−6.36	—	—	—	—
Early LNNMB	—	—	—	—	—	—	−0.094	−2.50	−0.282	−3.42	—	—	—	—
Late LNNMB	—	—	—	—	—	—	0.099	3.69	0.360	6.85	—	—	—	—
LNRCS×15–19	—	—	—	—	—	—	—	—	—	—	−1.585	−4.95	—	—
LNRCS×20–24	—	—	—	—	—	—	—	—	—	—	−0.628	−2.17	—	—
LNRCS×40–44	—	—	—	—	—	—	—	—	—	—	−0.095	−0.32	—	—
LNRCS×45–49	—	—	—	—	—	—	—	—	—	—	−0.181	−0.71	—	—
LNNMB×15–19	—	—	—	—	—	—	—	—	—	—	—	—	0.479	5.91
LNNMB×20–24	—	—	—	—	—	—	—	—	—	—	—	—	0.405	6.27
LNNMB×40–44	—	—	—	—	—	—	—	—	—	—	—	—	−0.410	−3.68
LNNMB×45–49	—	—	—	—	—	—	—	—	—	—	—	—	−0.345	−3.47
R^2	0.960													
R^2-adjusted	0.954													

LNRCS and LNNMB are substantially smaller than those based on the OLS model that does not correct for these factors, this difference is due to the correction for temporally correlated errors.

4.4. Substantive Results

4.4.1. Extending Earlier Analyses

Our results extend earlier analyses by including data on homicide arrest rates through the end of the 20th century including a number of years past the height of the crack cocaine epidemic and the start of the rapid increase in youth violence. The results in both Tables II and III replicate earlier findings and indicate that even when the most recent time periods are included, the effects of relative cohort size and family structure on age-period-specific homicide arrest rates are both statistically significant and substantively strong. As in earlier analyses, the influence of family structure is stronger than that of relative cohort size in all analyses.

4.4.2. Interaction Effects

Models 2 through 6 in Table III take advantage of the increased number of observations in the pooled analyses and allow us to examine the hypothesized interaction effects. In Models 2 through 4, we introduce interaction terms designed to detect changes in the effect of the LNRCS and LNNMB on the log of age-period-specific homicide rates across three broad periods. The early period is from 1960 to 1972, the late period is from 1987 to 1999, and the period from 1973 to 1986 serves as the reference period. Results in Model 2, which only include the interactions of periods with LNRCS, would seem to indicate that the effects of LNRCS are fairly stable across the early, middle, and late periods. Model 3 includes the two interaction terms for period and LNNMB. The results indicate that LNNMB had a stronger relationship with homicide in the later period than in the earlier period. Both of these coefficients are statistically significantly different from the reference period and from each other.

Model 4 includes both sets of interactions that test for changes in the effects of LNRCS and LNNMB over time. The results for LNNMB are similar, in terms of the direction of the relationship, to those observed in Model 3, and indicate that LNNMB has become more important as a determinant of changes in the age distribution of homicide from the early to middle to late periods even when LNRCS is controlled. In contrast, the results for LNRCS are strikingly different than those reported in Model 2 and indicate that, once the influence of LNNMB is controlled, LNRCS has become less important as a determinant of age-period-specific homicide rates over time. This is in line with the observations of a number of researchers (reviewed by Pampel and Peters).

The coefficient associated with a one-unit shift in the percentage of non-marital births has risen from 0.381 (= 0.663 − 0.282) during the early period to 0.663 during the middle period to 1.023 (= 0.663 + 0.360) during the most recent period. The coefficients associated with a one-unit shift in the percentage of the population 15 to 64 who were in the cohort when the cohort is 15 to 19 dropped from 0.563 during the early period to 0.420 during the middle period to 0.148 during the later period. These coefficients can be interpreted as the percent change in the age-period-specific homicide rate for a percent change in the independent variable. For example, during the later period a one percent increase in non-marital births was associated with a 1.023% increase in the age-period-specific homicide rate.

Models 5 and 6 test for shifts in the relationship of LNRCS and LNNMB to logged age-period-specific homicide rates across age groups. As noted earlier several authors have suggested that the effects of relative cohort size on an outcome may differ for cohort members of different ages (Kahn and Mason, 1987; Pampel and Peters, 1995; and Steffensmeier *et al.*, 1992). Kahn and Mason (1987) and Steffensmeier *et al.* (1992) have suggested that the effects of RCS should be stronger for the youngest age groups. Pampel and Peters (1995) have suggested that as cohorts age, they may become relatively advantaged by being members of a relatively large cohort. The results from Model 5 do not support either of these hypotheses (albeit our older age group is not as old as the ones discussed by Pampel and Peters). The signs of the coefficients are opposite of those hypothesized for the younger age groups and are negative and close to zero for the oldest age groups. In contrast, the results from Model 6 indicate that the effects of LNNMB are greater for the younger age groups than for the older age groups. The relationships for those 15 to 19 and those 20 to 24 years old are statistically significantly greater than for those in the reference group (those 25 to 39) and the relationships for those in the 40 to 44 and 45 to 49 age groups are statistically significantly less than those for the reference group. These relationships are not predicted in the literature, and we offer two post-hoc explanations in the discussion section.

4.4.3. Multicollinearity

High degrees of multicollinearity can create problems in any regression model. Collinearity inflates the standard errors of regression coefficients and with high collinearity the solutions can be unstable. In our case, the degree of multicollinearity is high by traditional standards for most of the models in Table III. Using a conservative criterion many researchers (see, Rawlings, 1988; Snee and Marquardt, 1984), refer to multicollinearity that involves a Variance Inflation Factor (VIF) of 10 or less as not serious and one of over 10 as serious. Other researchers use a VIF of over 30 as the demarcation between a serious and a not serious degree of collinearity (see StataCorp, 1997). Using these criteria several of our models suffer from severe multicollinearity. We note, however, that VIF and other measures of collinearity are not affected by the sample size, by the amount of variance explained in the dependent variable, or by the variance of the independent variable for which the VIF is generated. This is appropriate, since these measures focus on the collinearity among the independent variables. But these other factors affect the size of the standard errors used to generate the *z*-scores in Table III (e.g., Fox, 1997). The estimated standard errors of the regression coefficients take these factors into consideration as well as the multicollinearity represented by the VIFs and calculates the standard errors accordingly; that is, our *z*-scores in Table III take into consideration the effects of multicollinearity (Goldberger, 1991).

When interpreting the potential damage done by multicollinearity to our conclusions regarding the statistical significance of the coefficients reported in Table III we use the following guidelines. (1) If we find a statistically significant relationship, we have done so in the face of collinearity that has inflated the standard error of the regression coefficient. (2) If we fail to find a statistically significant relationship when there is a high degree of collinearity, this may be due to the diminished power of our test due to excessive collinearity. (3) If we find a relationship that is in the opposite direction of our hypothesis and its *z*-score is over two in size, then this is an indication (even in the face of excessive collinearity) that we were wrong. This approach is fully consistent with the following advice from Belsley, Kuh, and Welsch (1980, p. 116) "Thus, for example, if an investigator is only interested in whether a given coefficient is significantly positive, and is able, even in the presence of collinearity to accept that hypothesis on the basis of the relevant *t*-test, then collinearity has caused no problem. Of course, the resulting forecasts or point estimates may have wider CIs than would be needed to satisfy a more ambitious researcher, but for the limited purpose of the test of significance initially proposed, collinearity has caused no practical harm... These cases serve to exemplify the pleasantly pragmatic philosophy that collinearity does not hurt so long as it does not bite." This is the case in most of our tests, and we take the pragmatic philosophy that in the cases where the interactions are statistically significant collinearity has not bitten.

Using these guidelines we note that the statistically insignificant findings in Model 2 for the interaction terms for RCS in the early and late periods may result from a lack of power. We note, however, that in the more

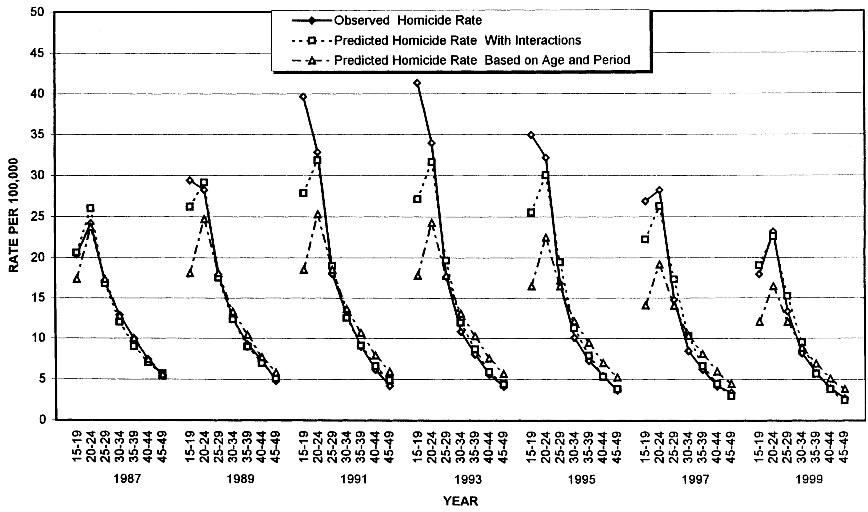

Fig. 2. Observed and model predicted rates of homocide.

complete specification of Model 4, where the interactions of periods and LNRCS and LNNMB are included in the equation, the results are statistically significant and in the predicted direction for all of the coefficients. This occurs in the face of a very high degree of multicollinearity. The VIFs associated with the early and late periods of LNRCS are 137.1 and 129.6, respectively; while the VIFs for the early and late periods for LNNMB are 76.7 and 64.1, respectively.

The only other model for which multicollinearity may have inflated the variance so much that the regression coefficients failed to reach conventional levels of statistical significance is Model 5. Here the degree of collinearity is indeed extreme. For LNRCS interacted with the age groups 15–19, 20–24, 40–44, and 45–49, the VIFs range from 520.2 to 709.8. Our pooling of the data are not responsible for the size of these VIFs, similarly high values of VIFs occur when we add these interactions to the unpooled series of Table II. In Model 5, none of these interaction terms were statistically significant, although if we had not incorrectly predicted the direction of the relationship for the youngest age groups, the *z*-scores would have led to rejection of the null hypotheses for these two age groups. So using the guidelines outlined above, we conclude that there is evidence against the enhanced effect of RCS for the two youngest groups. For the two oldest groups the high degree of multicollinearity may have reduced the power of significance test to such a degree that the results provide little evidence about the relationship between these interactions and age-period-specific rates of homicide.

4.4.4. The Invariance of Age

Finally, our analyses can address issues in the literature surrounding changes in the age distribution of homicide and the "epidemic of youth homicide." The age coefficients in each of the models in Table III that do not include age by cohort characteristic interactions conform to the shape of the "invariant" age distribution described by Hirschi and Gottfredson (1983) with the highest rates occurring for people in their twenties. Similar results occurred in the analysis of multiyear data reported in Table II (though the age coefficients were omitted to conserve space). In none of the models are the rates highest for those in their teens, a striking difference from the pattern observed in the raw data where from 1989 to 1996 those in the age group 15 to 19 had the highest rates. These results replicate the findings of O'Brien *et al.* (1999), which show that once the period effects and cohort characteristics related to age-period-specific homicide rates are controlled the invariant age pattern appears.

We conducted a stronger "test of invariance" by conducting a time-series-cross-section model (equivalent to Model 1 in Table III), with only the years 1989 to 1996 (the years in which the highest observed homicide rates were for the age group 15 to 19) and the results support the findings of O'Brien *et al.* (1999). With LNRCS, LNNMB and the period dummies in the model, the age group 20 to 24 had the highest predicted homicide rate (controlling for these other factors). In the words 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 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). In this case, the strong effect of cohort characteristics obscured the consistent age pattern associated with homicides.

The pooled time series analysis conducted in this paper allows us to take a closer look at the epidemic of youth homicide, typically thought to have occurred during the later half of the 1980s and into the 1990s. In these years the homicide rates of 15–19 year olds were substantially higher than the rates of earlier years, often the highest of any age category, a situation that was unique in the later part of the twentieth century. Our model demonstrates a remarkably strong fit to the data, R-squares averaging around 0.96. An inspection of the departure of the observed rates from the model predicted rates indicate that these departures were substantial only during this "epidemic." Importantly, however, both before and after this time period the model fits the data very well, and, during this period the cohort characteristics are significantly associated with shifts in the age distribution of homicides.

These patterns are illustrated in Fig. 2, which shows three sets of age-period-specific homicide rates for the periods 1987, 1989, ..., 1999: The observed rates (shown by diamonds); the rates predicted by Model 4,[23] which includes cohort effects and the interaction of cohort effects with period (shown by squares); and the rates predicted on the basis of the age and period dummy variables alone, which serve as a baseline showing predictions that occur without the two cohort variables (shown by triangles).[24] It can be seen that the model fits very well in 1987 and 1999, the beginning and the ending years depicted in the model. The model fit is worse from 1989 to 1997 and especially in 1991, 1993, and 1995, three years in which the age groups 15 to 19 had substantially higher homicide rates than other age groups. In other words, the years in which the model has the least fit coincide with the peak years of the "epidemic of youth homicide."

At the same time, it should be stressed that the model with the cohort characteristics provides a substantially better fit to the observed rates than does a model that only includes age and period as predictors and the only age group with a relatively poor fit is the youngest one: 15 to 19 year olds. For instance, examining the worst fitting year (1993), for the model without the cohort characteristics the prediction for those for those 20 to 24 years old was 24 per 100,000 using the model without cohort characteristics, 30 for the model with cohort characteristics, and the observed rate was 32. For those 15–19 years old, the observed rate was 41, the rate predicted with cohort characteristics was 27, and the rate without cohort characteristics was 18. We emphasize again that this short period is the only period in which the model with cohort characteristics did not provide an excellent fit to the observed age distribution of homicides. During this period the largest error using the model to predict the observed rate was 14.28 for those 15–19 in 1993, the largest error of prediction for any of the other age groups outside of the years 1987 to 1999 was 2.46.

Obviously something else (in addition to the cohort characteristics included in our analysis) affected homicide rates for the very young (those 15 to 19) during the epidemic of youth homicide. As noted in the introduction to this paper, many authors have suggested that part of the increase may be due to the crack cocaine epidemic that swept the country during this period (Blumstein and Cork, 1996; Cork, 1999). This is a plausible hypothesis. If this is the case, our model suggests that its major effect was on those in the 15 to 19 year old age group and that part of the increase during the epidemic

[23]The model without interactions (Model 1 in Table III) also fits the data substantially better than this baseline model. We present the model with interactions in Fig. 2, because our results in Table III indicate that this is a better specification of the model.

[24]We used every other year simply for convenience. A graph using every year from 1987 to 1999 is "crowded" and conveys information that was not needed in Fig. 2.

of youth homicide is associated with RCS and NMB. Importantly, these cohort characteristics are associated with shifts in the age distribution of homicides over the period that extends from 1960 to 1999.

5. SUMMARY AND DISCUSSION

This paper addresses a theoretical and methodological conundrum that has faced researchers who assess cohort effects. Theoretical work within this area generally and quite clearly implies that cohorts are "generational" or multiyear in nature, yet empirical work that has followed these guidelines, most notably the APCC model, has been hampered by having a relatively small number of observations for analysis. Empirical analyses that have used single year data as an operationalization of cohorts have avoided these statistical limitations, but face the challenge of defending their operationalization on theoretical grounds. Our analyses in this paper have tried to move beyond this conundrum by using a multiyear conception of cohorts and data from single-year periods. Specifically, we used a pooled time-series-cross-section analysis with periods spaced only one-year apart but age groups that correspond to five-year birth cohorts. We thus retain the theoretical advantage of a multiyear definition of cohorts as well as the statistical advantage of a larger sample size.[25]

Our first two major research questions are methodological in nature. First we examined our data set using five-year cohorts, but beginning in different periods to determine the extent to which using these different sets of periods produced different results. We found some minor differences in the results, but the coefficients for our measures of cohort characteristics (LNRCS and for LNNMB) did not differ significantly at the 0.05 level from analysis to analysis. Thus, at least for the analysis of age-period-specific homicide arrest rates in the latter half of the 20th century, the selection of specific groupings of periods did not greatly affect the results obtained.

The second question relates to how closely the time-series-cross-section analysis of the pooled data agrees with the results from the five separate traditional APCC analyses. This question is not independent of our first question. Given that we found that the five separate analyses yielded similar results it is not surprising that when we pool these data the results are

[25]We can characterize the sample size problem as follows: if the researcher has available rates by single years of age and for each year and defines cohorts as spanning x-years, then using the traditional approach to APCC models, the number of observations available for an analysis is $(Y \times A)/x^2$ (where Y is the number of years and A is the number of ages available). Using the pooled time-series-cross-section approach outlined in this paper, the number of observations available is $(Y \times A)/x$. Thus, if x is 5, the number of observations is reduced by a factor of 5 compared to a factor of 25. This makes the loss of observations far less severe when researchers use a multiyear operationalization of cohorts.

similar to those obtained from the separate analyses. With corrections employed in the time-series-cross-section analysis, the coefficients for LNRCS and LNNMB are smaller, yet still highly statistically significant.

The third research question involved several substantive issues. First, our data on homicide rates extended through the end of the 20th century and we wished to examine the extent to which our results would replicate earlier examinations of the influence of RCS and NMB on age-period-specific homicide arrest rates. Based on this earlier work, we expected a positive relationship between both LNRCS and LNNMB and the logged age-period-specific homicide rates after controlling for age groups and periods. In each of our analyses we found these positive and statistically significant relationships. As in the earlier work the influence of NMB was consistently stronger than that of RCS.

Our second substantive interest involved possible interaction effects of relative cohort size and family structure with both age and period. These results suggested that the influence of RCS on homicide rates diminished over time while the influence of NMB increased. In discussing the changing relationship between RCS and a variety of demographic variables Pampel and Peters (1995, see also Pampel, 2002) suggest that a declining influence of RCS could reflect factors such as changes in economic roles and levels of immigration.

Our analyses of the interactions of age with LNRCS and age with LNNMB produced mixed results. While the previous literature suggested that interactions with RCS would appear, our results were in the opposite direction predicted for young people and very small and insignificant for our two oldest groups. We note, however, that our oldest age group is 45 to 49, which is not as old as those groups discussed in the literature as benefiting from being members of relatively large cohorts. The relationship of LNNMB to age-period-specific homicide rates is stronger for the youngest age groups and weaker for the older age groups. This result was not anticipated in the literature, and we offer two possible explanations. It may be that the sharp upturn in NMB has so far only impacted the youngest cohorts and therefore only the youngest age groups. It is only these cohorts that have experienced the exceptionally high levels of NMB that characterized the past two- to three-decades. Other substantive explanations are likely to involve the idea that the impact of family structure is greater on youth and young adults before their assumption of more adult roles and relationships.

Our final substantive question involved the invariance of the age distribution of homicides rates over time. Our results support Hirschi and Gottfredson's suggestion that this age distribution is constant from one period to the next, *once the effects of cohort characteristics are controlled.* Without taking these characteristics into account, the observed age distribution of homicide rates went through a major shift from the mid-1980s to the mid-1990s. This shift is partially associated with changes in cohort levels of RCS and NMB. But as Fig. 2 indicates, this explanation is not complete during this relatively brief period. This is not surprising, since factors other than the characteristics of cohorts certainly can effect the age distribution of homicide offenses.

This study incorporates the longest series of data available on homicide offenses broken down by age for the United States. For reasons outlined earlier, a multiyear operationalization of cohorts is theoretically desirable for testing for the effects of RCS and NMB. When this has been done in previous studies, researchers have used only a portion of the available data. We have used all of the data available for each year while maintaining a multiyear conceptualization of cohorts. Comparisons of results from various types of analyses suggest that this method provides robust results as well as the flexibility to test various hypotheses regarding interactive effects.

The results of our analyses strongly support the conclusion of the importance over the entire period from 1960 to 1999 of non-marital births on cohorts' risk of homicide offending independent of the age and period. This effect appears to have gotten stronger over time. The results also support the conclusion that the effects of RCS have been weaker than those for NMB and that those effects have diminished over this period. The conclusions concerning the effects of RCS and NMB on age-period-specific homicide rates is consistent with theories that view both of these variables as associated with less social control, more strain, and fewer resources for the members of cohorts.

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