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# Cohort Variations and Changes in Age-Specific Suicide Rates over Time: Explaining Variations in Youth Suicide*
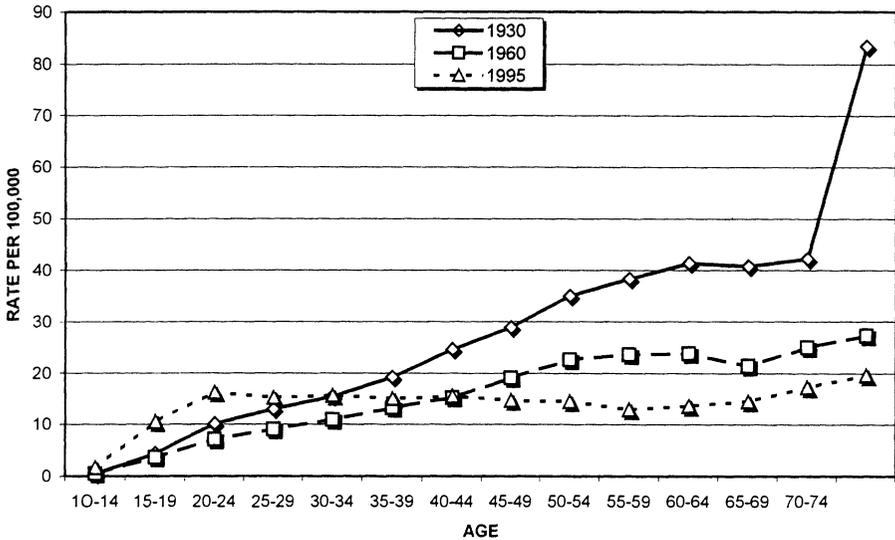
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*Abstract*

*Dramatic changes in the age distribution of suicide in the U.S. are associated with variations in the demographic characteristics of birth cohorts. Using an age-period-cohort-characteristic model, we show that cohort characteristics theoretically linked to integration and regulation have substantively strong and statistically significant relationships with changes in age-specific suicide rates from 1930 to 1995. Members of relatively large cohorts and of cohorts with higher percentages of nonmarital births are at greater risk for suicide throughout their life spans. These results appear for the total population and for race-sex subgroups, even though the age distributions of suicide differ substantially across these demographic groups. They can account for recent sharp increases in youth suicide, as well as more moderate increases in earlier decades.*

The social foundations of suicide — seemingly the most individual of all behaviors — have fascinated sociologists since Durkheim's classic, *Le Suicide*, appeared a century ago. Like scholars before and since, Durkheim noted the apparent invariant relationship of suicide with age: "not only is suicide very rare during childhood but it reaches its height only in old age, and during the interval grows steadily from age to age" (Durkheim [1897] 1951:101). Until recently, age trends in suicide rates for the U.S. followed this pattern. The lowest rates appeared among the young and the highest among the elderly. In recent years this pattern changed substantially. While rates for the very young remained much lower than those for adults, the pattern of a gradual rise in

FIGURE 1:    Age Distributions of Suicide Rates for the Total Population:
             1930, 1960, and 1995



the incidence of suicide from youth to adulthood shifted to a pattern of sharp
increase to the early twenties, slightly lower rates through middle age, and an
increase to higher levels only among those in their seventh decade of life.
Suicides among children reached unprecedented levels: between 1980 and 1995
the suicide rate for children ages 10–14 doubled.

The dramatic nature of this shift in the age distribution of suicides is shown
in Figure 1.[1] In 1930 the suicide rate for the total population showed a sharp
and nearly monotonic increase with age. In 1960, while rates for older groups
were generally substantially smaller than they had been for groups of similar
age in 1930, those for people under 20 years of age were virtually the same as
in the earlier period. This reflects a relative rise in the youth suicide rate that
began around 1960 and continued through later years. Still, in 1960, the suicide
rates continued to show a nearly monotonic increase with age. By 1995 the
pattern changed dramatically. Suicides rose quickly to age 20–24, reaching levels
generally unprecedented in earlier periods, remained fairly constant or
declined slightly until age 55–59, and then increased. In 1995 the rate for 20–
24-year-olds was not exceeded until age 70–74.

Even though public health officials and scholars within the medical
community have paid careful attention to the greater incidence of youth

suicide (e.g., Diekstra & Hawton 1987; Hendin 1995; Holinger et al. 1994), surprisingly little sociological research has dealt with this trend or with the more general issue of the changing age distribution of suicide. In contrast, youth homicide, which also increased substantially in recent years, has received much more theoretical and research attention from sociologists. In recently published articles (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999), we show how changes over time in the age distributions of homicide offending and victimization are associated with two characteristics of birth cohorts — relative cohort size and the percentage of live births in a cohort that are nonmarital births — arguing that these cohort variables are related to the family and community resources available to members of different birth cohorts. Our results provide strong evidence that changes in these cohort characteristics can account for the most recent upturn in homicides committed by the young as well as subtler changes in the relationship between age and homicide rates in earlier years.

In this article we synthesize our earlier theoretical reasoning (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999) and general tenets of Durkheimian thought to produce a framework for analyzing changes in age-specific suicide rates from 1930 through 1995 among age groups from 10 to 79. We suggest that these changes can be explained by the extent to which members of birth cohorts experience social integration and regulation. Our results hold for both the total population and across race and sex groups and lead us to propose that general theoretical understandings of both homicide offending and suicide death (lethal violence directed toward both others and self) could be enhanced if we employed a more unified framework that uses notions of both social integration and regulation.

Our theoretical rationale and hypotheses are based on cohort theory and research and the Durkheimian research tradition on suicide. In the following sections we discuss cohort theory and research regarding suicide and then describe the elements of the Durkheimian tradition of control theory and suicide research that form the basis of our hypotheses and analysis.

## Cohort Theory and Research

Birth cohorts move in a two-dimensional space of time and age (Ryder 1965), and cohort theory and research can be used to examine how this movement affects the outcome variables in various periods and age groups. Two major tenets of cohort theory and research are the life-stage principle and the lasting-effects principle. The life-stage principle posits that the experiences of members of one cohort differ from those of another because they experience historical events at different ages or developmental periods. Infants experience historical

events, such as the establishment of universal suffrage or the transition to a market economy, differently than those who are 21 years old, those of middle age, and those who have retired (Elder 1974, 1979; Elder & Caspi 1990; Elder, Modell & Parke 1993; Firebaugh & Chen 1995). The second major tenet suggests that certain events can produce lasting changes in the attitudes and behaviors of cohort members. These changes must be analytically distinct from those associated with age and period to be labeled cohort effects. Cohort effects have been found in a number of areas of research, for example, criminal behavior (O'Brien 1989; O'Brien, Stockard & Isaacson 1999; Savolainen 2000), antiblack prejudice (Firebaugh & Davis 1988), opinions on democracy and Nazism (Weil 1987), parental values (Alwin 1990), political orientation and voting (Alwin & Krosnick 1991; Firebaugh & Chen 1995), sex-role attitudes (Mason & Lu 1988), and intellectual skills (Alwin 1991).

While researchers can examine whether cohorts vary by controlling for historical period and age, the theoretical issue of most importance involves accounting for any differences among cohorts. One can view the historical periods in which cohorts are born as "opportunity structures" (Elder 1996:5). These structures may result from historical events, such as wars and revolutions, economic depression, transformations of economic systems, or opportunities to participate in the electoral process. Demographic differences between cohorts such as increases in immigration, changes in sex ratios, relative size, and variations in family structure also generate differences in opportunity structures. Whatever the genesis of these opportunity structures, they affect the resources of, and constraints on, the members of birth cohorts.

We suggest that birth cohorts, because they experience different opportunity structures, vary in the amount of social integration and regulation that they experience, variables that Durkheim saw as important in promoting or deterring suicide within a society.[2] Two demographic aspects of cohorts that may produce such cohort-related variations are relative cohort size and family structure. As described below, we hypothesize that both these factors are related to the incidence of death from suicide.

RELATIVE COHORT SIZE

Easterlin's (1978, 1980, 1987) hypothesis that members of relatively large cohorts experience disadvantages that persist throughout their life spans represents perhaps the most commonly examined hypothesis related to cohort effects. He emphasizes the relative deprivation that members of large birth cohorts experience as they face diminished economic opportunities — especially when they enter a job market crowded with a large number of those seeking relatively rare entry-level jobs.

We suggest that relatively large cohorts are less socially integrated and regulated. This lower level of integration and regulation results from several fac-

tors. Members of large cohorts may be more likely to interact with and be responsive to members of their own cohort rather than with others when they are young. This promotes the development of a "youth culture" that is "relatively insulated from the influence of older generations" (Holinger et al. 1994:70). Relatively large cohorts also overload institutions of social support and control, stretching 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), thus potentially providing less stable and less integrative relationships with adults. In short, children in relatively large birth cohorts may obtain insufficient social integration and regulation because they receive less attention and supervision from parents, teachers, counselors, and other adults within the community. Even though peers may provide social support, it is doubtful that they furnish the same integrative or regulative forces that strong ties to parents and community members provide.

Although his analysis predated the recent increase in youth suicide, Easterlin already provided evidence to support the hypothesized positive relationship between cohort size and self-destructive behaviors such as suicide, homicide, and drug use (Easterlin 1980:104–6; see also Ahlburg & Schapiro 1984; Freeman 1998). Similarly, using data from 1933 through 1978 and 1982, Holinger and his associates (Holinger & Offer 1982; Holinger, Offer & Ostrov 1987) found significant positive relationships between the suicide rates of adolescents (ages 15–19) and changes in the proportion of adolescents in the total population. Pampel (1998) found the same general effect across a diverse group of nations (but the effect was positive for younger groups and negative for older groups).

FAMILY STRUCTURE

A long tradition of work, beginning with Durkheim, documents the association of family integration and suicide, usually through showing a positive relationship between divorce rates and suicide rates (e.g., Breault 1986; Breault & Barkey 1982; Danigelis & Pope 1979; Durkheim [1897] 1951:259–76; Maris 1969; Pescosolido & Mendelsohn 1986; Stack 1982, 1989, 1990a; Trovato 1987; Wasserman 1984). Others have obtained similar results in large-scale individual-level analyses of mortality records (e.g., Kposowa, Breault & Singh 1995; Stack 1990b) or suicidal orientations (Thorlindsson & Bjarnason 1998). Similarly, much of the clinical literature focuses on family-related variables. For instance, Kerfoot (1987) found that 80% of young people who attempted suicide cited relationship problems within their families as the main reason for the attempt. Pfeffer (1987) indicates that suicidal young people (compared to both depressed and nondepressed controls) are much more likely to have experienced, among other characteristics, family stress, including disturbances

in family relationships and changes in family structure and continuity (see also Stillion, McDowell & May 1989).

Some suggest that early family experiences influence the possibility of suicide throughout the life course. Based on his years of clinical work with suicidal patients of all ages, Hendin (1995) contends that the probability of an individual's committing suicide, even in later years, is influenced by disturbed relationships and unresolved difficulties within the parental family: "The case evidence suggests . . . that if life . . . provides the stress, the vulnerability to a response by suicide usually has a lifelong history" (102). Using survey methodology and a much larger sample than Hendin's clinical population, Maris (1981) reports similar findings. Maris found that people who attempted suicide and those who completed suicide were more likely to have experienced early family trauma than those who died a natural death. He in fact suggests that suicides should be viewed as "careers," such that the ultimate act of self-destruction is the culmination of many earlier life events.

Though family resources can be conceptualized at the micro level (as in individual-level studies or clinical work) or at the aggregate societal level (as in many sociological studies), they also can be viewed at the cohort level. Children from cohorts with relatively more single-parent families have fewer family and community resources. Some of these resources are monetary. For instance, in 1997 the rate of poverty for children under 6 who lived in married-couple families was 10.6%; for children who lived in female-headed households the rate was 59.1% — almost 6 times as high (Dalaker & Naifeh 1998). In part because of this, these children are less likely to live in safe and "desirable" neighborhoods, obtain adequate medical care, be successful in school, or have adequate day care and after-school care (Duncan & Brooks-Gunn 1997; Duncan et al. 1998; Hogan & Lichter 1995; McLanahan & Sandefur 1994; National Research Council 1993). Although not all the effects of growing up in poverty are related to lower levels of social integration and regulation, children who are less successful in school and have less adequate day care and after-school care are likely to be less socially integrated and regulated. The effects of family poverty, however, are not limited to children growing up in those families. With more members of a cohort growing up in poverty, more children in that cohort will associate with peers who have grown up with fewer family and community resources, regardless of their own financial situation.

Other resources are nonmonetary. Two-parent families and smaller families are more likely to be able to supervise and monitor their children (ceteris paribus). They are more likely to contain an adult who knows one or more parents of their children's friends and who has the time and opportunity to be acquainted with teachers. Two-parent families increase the potential range of network ties and closure with others in the community. Cohorts with more two-parent families are thus more likely to have "closed systems" (Coleman 1990), denser networks with greater connections among parents, teachers, and

other community members and thus the capability of providing both more integration and regulation of behavior.

We emphasize that the decreased social integration and regulation of cohorts with larger proportions of single-parent families comes from at least two sources. First, it simply reflects the aggregated experience of those growing up in single-parent families. Second, we suggest that all children from these cohorts experience the effects of less social integration and closure of social networks. Regardless of their own individual family background, children in cohorts with more single-parent families would be more likely to be involved in networks that have less closure — where parents would be less likely to be acquainted with each other and provide substantial stability and integration. Peer groups would also be more likely to include others with fewer family and community resources; and peer group socialization influences the behavior of cohort members, whether or not they are from a single-parent family.

## The Durkheimian Tradition and Lethal Violence

The theoretical approach used in our earlier work on homicide offending and victimization (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999), summarized above, and our theoretical discussions relating to suicide can be integrated within a broadly defined Durkheimian tradition by building on Durkheim's observation that the causes of suicide and homicide in complex modern societies are "parallel" and stem from anomie. Writing in the context of a discussion of the moral statisticians Durkheim ([1897] 1951) stated, "there exists today, especially in great centers and regions of intense civilization, a certain parallelism between the development of homicide and that of suicide. It is because anomy is in an acute state there" (358). It is interesting to note that we know of no contemporary research that has explored the implications of this statement; that is, investigated the parallel effects of integration and regulation on suicide and homicide rates. Studies of suicide and homicide have developed in different subdisciplines and tend to be published in separate venues.[3]

In analyses of homicide, the tradition of control theory builds upon Durkheim, often quite explicitly, in explaining deviant and criminal behavior. Quite simply, control theory emphasizes the importance of external and internal social control for maintaining low levels of deviant behavior. The more social ties that individuals have with conforming others, the more likely they will be to conform themselves. Strong social ties with parents in the formative years help promote internal social control. In short, social relationships that provide high levels of integration and regulation, and thus lower levels of anomie, are crucial for the development of self-regulation and effective control by others within the environment.

Analyses of suicide following Durkheim's classic work generally support his contention that suicide rates are higher in situations with less social integration and regulation (e.g., Cutright & Fernquist 2000; Fernquist & Cutright 1998; Gibbs & Martin 1958, 1964; Johnson 1965; Maris 1969; Pescosolido & Georgianna 1989; Pope 1976).[4] Similar results occur on the individual level of analysis. For instance, in their recent analysis of youth suicidality in Iceland, Thorlindsson and Bjarnason (1998) found that lower levels of family integration and, to a lesser extent, lower levels of parental regulation enhanced the probability that young people would express suicidal intentions. In one of the few sociological analyses to examine teen suicide, Bearman (1991) describes how lower levels of integration and regulation may affect young people. He notes that the "teen today is often a member of two separate societies, the family of origin and the peer group" (517). The teen is integrated into these "societies," but these social worlds are usually independent of one another and produce conflicting social demands. "The normative dissonance experienced by the teen is the same as anomie."

Thus, both the tradition of control theory and Durkheimian analyses of suicide suggest that rates of lethal violence — homicides and suicides — will be higher in situations with less integration and regulation.[5] Both integration and regulation reflect the presence of strong social ties and social networks that have greater closure. To the extent that individuals interact in social systems that embody stronger social relationships, they will experience greater integration *and* regulation and will be less likely to experience any type of lethal violence. Building on the empirical literature reviewed above, we suggest that the integrative and regulative ties of an individual to society can come from both community and family resources and that birth cohorts vary in the degree to which they experience these resources.

Below we summarize the reasoning that underlies our hypotheses. We begin, appropriately, with Durkheim's statement that "suicide varies inversely with the degree of integration of the social groups of which the individual forms a part" (209). Given our discussion we expect suicide rates to be lower in populations that have higher levels of social integration and regulation, that is, populations with greater levels of social control through shared values and sentiments and denser linkages with others in the population.

Perhaps the relationships strongest in stability and durability in modern society are family relationships — especially in one's family of birth. The greater the stability, durability, and density of social relationships that involve this important social institution, the greater the social integration and regulation are likely to be. In addition, strong and stable ties within the broader social community promote integration and regulation. Thus, building on work in the Durkheimian tradition, we argue that community and familial resources that

create more stability, durability, and density of social relationships are related to lower levels of suicide.

We assume that patterns of social integration and regulation are, in large part, set during the early years of life. In this sense, the family and community resources available to people when they are growing up can affect their behavior in later life, a result documented by decades of research on the role of early life experiences in areas as varied as status attainment, criminal activity, and educational success. In making this assertion we do not deny that later experiences with more integration and regulation can affect choices and behaviors (see Sampson & Laub 1993). Instead, we assert that early life experiences are most crucial precisely because they not only influence the development of resiliency and internal social control, but also because they help set the pathway that individuals will follow, including the probability that they will encounter sufficient sources of social support throughout the life span (see also Cairns & Cairns 1994; Maris 1981).

Finally, we note that populations such as birth cohorts may vary in the extent to which they possess community and familial resources that are associated with higher levels of social integration and regulation. In this sense we see birth cohorts as entities that may be analyzed in their own right. We assume that birth cohorts can vary in the extent to which they possess the familial and community resources necessary for social integration and regulation.

Thus we hypothesize that cohort-related differences in community and familial resources are related to age period-specific suicide rates. We borrow from our previous work (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999) to suggest that birth cohorts that are larger and have more single-parent families are more likely to have less adult supervision of children. This leads to a lower likelihood of closure among parents of children, between parents and teachers, and so on. In general the smaller number of family and community resources available to the members of these cohorts would result in less integration and regulation and thus a greater propensity to commit suicide.[6]

Specifically, in this article we test the hypothesis that changes in age-period-specific suicide rates are related to these cohort characteristics and that changes in these cohort characteristics will result in changes in the age distribution of suicides for the population as a whole and for separate race and sex groups. These population-specific analyses are important because suicide rates differ markedly between the sex groups and racial-ethnic groups. Men have higher suicide rates than women do; and whites have higher rates than nonwhites do. In addition, the age pattern of suicide varies between these groups, with white men traditionally exhibiting the highest rates at older ages, and the other groups have highest rates at earlier ages. Nevertheless, both men and women and whites and nonwhites have experienced similar *changes* in cohort-related

sources of integration and regulation. We expect these changes to affect shifts in the age distributions of suicide rates for these groups in similar ways.

Given the importance of the formative years in the development of self-regulation and control as well as the promotion of patterns of integration throughout life, we also expect that cohort-related community and familial resources will affect suicide rates throughout the life span. It is, however, possible that these effects will be different at earlier ages than at later ages. For instance, some authors (Kahn & Mason 1987; Steffensmeier, Streifel & Shihadeh 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 from families and communities. Similarly, Diekstra (1995) notes a "growing consensus in the literature that the associations between social problems and mental ill-health are generally stronger among adolescents than among adults" (236, citing Platt 1984 and Rutter 1980) and that "adolescents are more vulnerable to social and interpersonal adversities than are adults." At the same time, Pampel (1998) notes that larger cohorts may have more political clout when they age and thus secure advantages that would make suicide rates lower in relatively large older cohorts. In addition, larger cohorts may provide more social and same-age peer support in later years than smaller cohorts may (see also McCall & Land 1994). We therefore test the possibility that the effect of cohort size varies at both earlier and later age ranges.[7]

## Measures

All our data come from publicly available government documents and describe the population of the U.S. born between 1915 and 1995. In all cases, except for the analysis of the total population, we employ race-specific measures for the cohort characteristics and age-period-race-sex-specific measures of suicide and homicide victimization.

### COHORT CHARACTERISTICS

Following O'Brien, Stockard, and Isaacson (1999) and O'Brien and Stockard (2002), we use two variables related to the community and family resources available to birth cohorts: the percentage of nonmarital births within a birth cohort (percent NB) and the size of a birth cohort relative to others (relative cohort size or RCS). We use race-specific measures for each subgroup analysis; e.g., the percentage of nonmarital births for nonwhites for each cohort when the analyses involve nonwhite males or nonwhite females. Thus, each of our cohort characteristics is clearly associated with each birth cohort and, we hypothesize, produces different opportunity structures with different degrees of social integration and regulation.

Perhaps the ideal measure of family structure, from our point of view, would be the average number of years children growing up in a cohort lived with both parents from the time of birth to the age of 10 or 12. The timing of disruption for two-parent families during those periods would also be relevant. Unfortunately, such data do not exist for the nation as a whole now, and no data remotely approaching this ideal are available for cohorts born earlier in the twentieth century. We use the percentage of nonmarital births associated with a birth cohort as a proxy measure. While not perfect, it does overlap conceptually with the characteristics of family structure that we would like to measure, since it indicates the *likelihood* that young children were raised by a single parent during at least part of their early years. It is important to note that this indicator is available for birth cohorts beginning with the 1915–19 cohort to the present for the nation as a whole, for whites, and for nonwhites.[8]

Data for the number of births to unwed mothers per 100 live births (percent NB) come from two volumes of *Vital Statistics of the United States* (U.S. Bureau of the Census 1946, 1990). The 1946 volume supplied data for the years 1917–40 and data for the remaining years were drawn from the 1990 volume. To obtain the percent NB for each cohort, we summed the appropriate percentages and divided by the number of years. For example, to obtain the percent NB for those 20–24 years old in 1950, we summed the percentage of nonmarital births for the years 1925 through 1929 and divided by five. Because data on the percentage of nonmarital births were not available for those born in 1915 and 1916, the percent NB for the cohort born between 1915 and 1919 was obtained by summing the percent NB for 1917, 1918, and 1919 and dividing by three. We based all other percent NB values on five years of data. We chose this operationalization, rather than a weighted mean of the rates for the five years, for reasons outlined in O'Brien, Stockard, and Isaacson (1999).[9]

Our confidence in this measure is enhanced by a comparison of our measure to Jukka Savolainen's (2000) estimates of the percentage of cohort members growing up in single-parent families from 1910 to 1990. Savolainen operationalized changes in family structure as "the percentage of those in the five-year birth cohort who lived in a single-parent household from ages 5 to 9" (125). He obtained these data by interpolating and extrapolating Public Use Micro Sample census data from 1910, 1940, 1960, 1970, 1980, and 1990. Even though he had to interpolate values from 1911 to 1939 and for other years between decennial censuses, and even though his measure is distinct from ours, his estimates correlate very highly with our own measure ($r = .98$). More impressive is the fact that his measure and ours correlate highly ($r = .90$) after they have been first-differenced. Thus, changes in his measure of single-parent families are highly correlated with changes in our measure of percent NB for cohorts. We have chosen to use our measure of nonmarital births, rather than Savolainen's estimate, because it is available for the total population and for

whites and nonwhites and comes directly from birth records rather than from interpolations (sometimes over long periods) from census data.

We operationalize relative cohort size as the percentage of the population age 15 to 64 that the cohort represents when the cohort is age 15 to 19.[10] This measure attempts to capture the percentage of the population who are young as the cohort enters the job market as well as the relative number of adults to children and resources per child that were available to the birth cohort in its formative years. The *Current Population Surveys: Series P-25* (U.S. Bureau of the Census, various dates) supplied the data for calculating RCS.[11] Again, we calculated these percentages for the total population and for whites and for nonwhites separately.[12]

SUICIDE RATES

Data on age-period-specific suicide rates per 100,000 come from the U.S. Department of Health, Education, and Welfare (various years). We use data from 1930 to the present for five-year age groups from 10 to 79 for the total population, white males, white females, nonwhite males, and nonwhite females. We begin the series in 1930 for the age group 10 to 14. This corresponds with the availability of data on nonmarital births, which first became available for the cohort born between 1915 and 1919 (those who were 10–14 in 1930).

Figure 2 provides the data used in our analysis of the age-period-specific suicide rates for the total population. The triangular shape of this matrix results from data on nonmarital births not being available for cohorts born before 1915. The rows and columns indicate period and age and each cell contains the age-period-specific suicide rate. Cohort 1 was born between 1915 and 1919, cohort 2 between 1920 and 1924, and so on. The last cohort in our analysis (cohort 14) was born between 1980 and 1984. By following a particular cohort diagonally through the table, one can see the way in which cohorts move through the space of time and age. (Cohort 1 is the uppermost diagonal, cohort 2 is the second diagonal, and so on, with cohort 14 represented by the bottom left cell.) The marginal at the bottom of the table contains two values that remain the same for each cohort over time. The top and bottom entries represent, respectively, the relative cohort size and the percentage of the cohort members who were born to unwed mothers.

We examine these data and those for nonwhite women, nonwhite men, white women, and white men in several separate analyses. First, we analyze all the data in the triangular data matrix for each group, with one case from 1930 and fourteen cases from 1995. This analysis includes all the cases for which we have the appropriate available data for the U.S. Second, we use a subset of these data to examine the periods after 1945 and the age groups under 60. This provides a test of the sensitivity of our results to having so few cases in the older age categories and the earlier periods.

FIGURE 2: Age-Period-Cohort Characteristics Model with Suicide Data for the U.S. (Lower Entry in Each Cell) and Cohort Number (Upper Entry in Each Cells — Cohort 1 Was Born between 1915 and 1919)[a]

| Year | 10–14 | 15–19 | 20–24 | 25–29 | 30–34 | 35–39 | 40–44 | 45–49 | 50–54 | 55–59 | 60–64 | 65–69 | 70–74 | 75–79 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1930 | *1* .4 | | | | | | | | | | | | | |
| 1935 | *2* .4 | *1* 4.3 | | | | | | | | | | | | |
| 1940 | *3* .4 | *2* 3.5 | *1* 8.8 | | | | | | | | | | | |
| 1945 | *4* .4 | *3* 2.8 | *2* 7.0 | *1* 8.6 | | | | | | | | | | |
| 1950 | *5* .3 | *4* 2.7 | *3* 6.2 | *2* 8.1 | *1* 10.1 | | | | | | | | | |
| 1955 | *6* .3 | *5* 2.6 | *4* 5.5 | *3* 7.9 | *2* 8.9 | *1* 10.5 | | | | | | | | |
| 1960 | *7* .5 | *6* 3.6 | *5* 7.1 | *4* 9.0 | *3* 10.9 | *2* 13.2 | *1* 15.2 | | | | | | | |
| 1965 | *8* .5 | *7* 4.0 | *6* 8.9 | *5* 11.3 | *4* 13.3 | *3* 15.8 | *2* 17.5 | *1* 18.7 | | | | | | |
| 1970 | *9* .6 | *8* 5.9 | *7* 12.2 | *6* 13.9 | *5* 14.3 | *4* 15.9 | *3* 17.8 | *2* 19.5 | *1* 20.5 | | | | | |
| 1975 | *10* .8 | *9* 7.6 | *8* 16.5 | *7* 16.5 | *6* 16.2 | *5* 16.2 | *4* 18.6 | *3* 19.9 | *2* 20.2 | *1* 20.6 | | | | |
| 1980 | *11* .8 | *10* 8.5 | *9* 16.1 | *8* 16.5 | *7* 15.3 | *6* 15.4 | *5* 15.3 | *4* 15.3 | *3* 16.4 | *2* 16.3 | *1* 15.5 | | | |
| 1985 | *12* 1.6 | *11* 10.0 | *10* 15.6 | *9* 15.5 | *8* 14.9 | *7* 14.3 | *6* 14.9 | *5* 15.5 | *4* 15.8 | *3* 17.0 | *2* 16.3 | *1* 16.8 | | |
| 1990 | *13* 1.5 | *12* 11.1 | *11* 15.1 | *10* 15.0 | *9* 15.4 | *8* 15.6 | *7* 14.9 | *6* 15.0 | *5* 14.7 | *4* 16.1 | *3* 15.9 | *2* 16.6 | *1* 19.6 | |
| 1995 | *14* 1.7 | *13* 10.5 | *12* 16.2 | *11* 15.2 | *10* 15.6 | *9* 15.0 | *8* 15.5 | *7* 14.7 | *6* 14.5 | *5* 12.9 | *4* 13.6 | *3* 14.5 | *2* 17.3 | *1* 19.6 |
| | 10.42 | 10.53 | 10.82 | 11.72 | 14.03 | 15.33 | 15.27 | 14.62 | 12.43 | 10.87 | 10.80 | 12.39 | 13.69 | 13.89 |
| | **14** | **13** | **12** | **11** | **10** | **9** | **8** | **7** | **6** | **5** | **4** | **3** | **2** | **1** |
| | 19.61 | 15.59 | 12.11 | 8.97 | 5.99 | 4.82 | 4.06 | 3.82 | 3.62 | 4.08 | 3.92 | 2.93 | 2.57 | 2.10 |

[a] In the bottom marginal the top number is the relative cohort size when the cohort was 15 to 19, the middle number (bolded number) is the cohort number, and the bottom number is the percentage born out of wedlock.

The accuracy of suicide rates has long been challenged. Jack Douglas's *The Social Meaning of Suicide* (1967) provides a classic formulation of the thesis that the patterns observed by Durkheim, especially with regard to religion, might well reflect biases in the reporting and recording of suicide rates. More recently, van Poppel and Day (1996) argue that no relationship exists between religious affiliation (specifically between Catholicism and Protestantism) and suicide rates, once differential reporting and recording of suicides are taken into account. They conclude that their "finding raises doubts not only about Durkheim's theory but also about other causal theories concerning suicide that rely on a sociological rather than a psychological (or even idiosyncratic) explanation" (van Poppel & Day 1996:500).

Biases that parallel the independent variables used in our analysis could present a problem. Given the types of analysis we use, however, the patterns of relationships observed would be very difficult to explain on the basis of many plausible sources of bias. We control relationships for age and time period and then look for patterns of relationship between suicide rates and relative cohort size and the percentage of each cohort born to unwed mothers. To explain the patterns that we observe on the basis of recording errors, improved medical technology, or some other common explanation would require a series of bizarre interactions.[12]

## Analysis

We use an age-period-cohort-characteristic (APCC) model to analyze these data. The model derives from the work of Mason, Mason, Winsborough, and Poole (1973) and has been refined and extended in the work of O'Brien, Stockard, and Isaacson (1999) O'Brien (2000), and O'Brien and Stockard (2002). The use of dummy variables to code ages and periods in APCC models helps assure that purported cohort effects are not merely uncontrolled main effects of period and age. The method not only identifies cohort effects but also attempts to explain them by examining the influence of theoretically identified cohort characteristics.

We use the natural log of the age-period-specific death rates for suicide as the dependent variable in all analyses. Logging the rates provides a better means of understanding changes in the relative size of the rates over time. For instance, we are as interested in the doubling of the suicide rates for those 10 to 14 as for those 55 to 59. Yet, because rates for those in the 10 to 14 age group are often much lower in magnitude, examining the raw data would not allow us to capture this similarity. In logged form a doubling of the raw rates is of the same magnitude whether the rate increases from 0.5 to 1.0 or from 10 to 20 per 100,000, given that in the analyses these changes are conditioned on age and period.
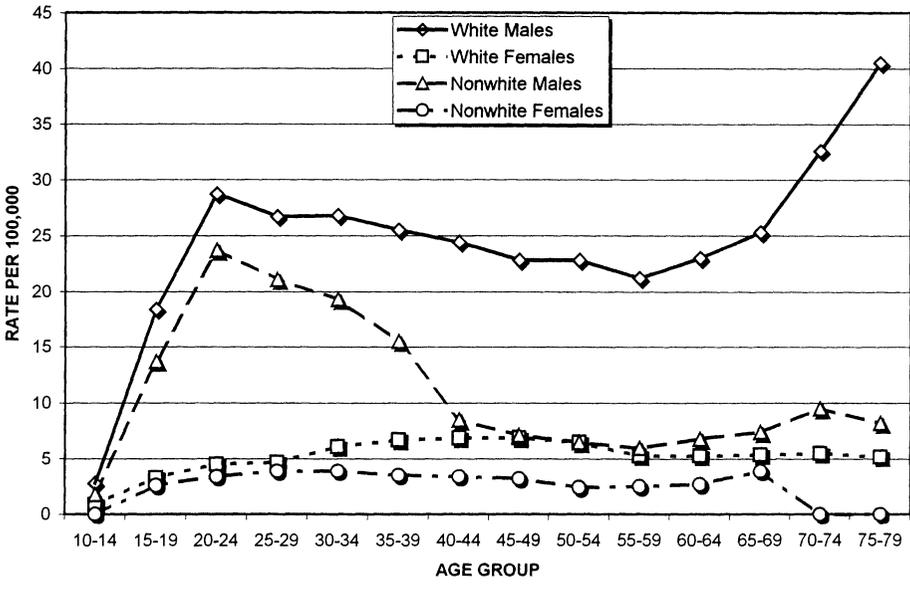
The set of logged age-period-specific suicide rates associated with each cohort in each cell is the dependent variable, and the various time periods, age groupings, and the measures of RCS and percent NB associated with each cohort provide the independent variables. This approach allows us to associate changes in the dependent variable, the age-period-specific suicide rate, with particular cohort characteristics while controlling for both age and period.[13] Data in Figure 2 indicate that for an analysis involving age-group dummy variables, period dummy variables, relative cohort size, and the percentage of nonmarital births, there are $[(14 - 1) + (14 - 1) + 1 + 1]$, or 28, independent variables to predict $[(14 \times 15) \div 2]$, or 105, age-period-specific rates. After testing our model with all these observations, we test our model eliminating observations prior to 1950 and for ages 60 and above (leaving 85 observed cases). We do this to see if the small number of cases that happen to fall in these periods and age groups affect our results. Because the repeated observations on cohorts should not be treated as independent observations, we use the "cluster" option in STATA (StataCorp 1997) that allows us to specify cohorts as clusters. This allows us to conduct an OLS regression analysis without requiring the observations to be independent within cohorts (see O'Brien & Stockard 2002).

CONTROL VARIABLES

An important strength of the APCC model is the inclusion of dummy variables for both age groups and periods. The use of dummy variables provides strong controls for the effects of age and period (Pampel & Peters 1995). Factors that change over time and whose effects are constant across age groups, such as variations in the media, income inequality, frequency of divorce, political strife, and improvements in medical technology that might prevent deaths, are controlled through the period dummy variables. Factors related to age that are constant across periods, such as the tendency for older people to commit suicide more than the young, are controlled through the use of dummy variables for age categories. The interaction of these variables with periods is not controlled by the dummy variables for age, but these dummy variables do control for the main effects of a large number of variables that are not included in the model to the extent that they are related to either period or age. Thus, including these controls provides a clearer indication of the effect of cohort-related variables.[14]

As noted in O'Brien, Stockard, and Isaacson (1999), the inclusion of these dummy variables also controls for any linear effects (linear trends) of cohort characteristics. Their inclusion implicitly includes the time period in which the cohort was born as a control variable. If a cohort characteristic were related to suicide rates simply because both of these variables were linearly related to the time of the cohorts' birth, that effect would be controlled for. Thus, to the

FIGURE 3:    Suicide Rates by Age Group 1995



extent that our cohort characteristics are linearly related to time of cohorts' birth, these effects are controlled for. In the case of percent NB, this linear relationship is quite strong, for changes in the rate of nonmarital birth are strongly associated with changes in other aspects of family structure. Because our model controls for such periodic changes, we can be more confident that our results reflect cohort effects.

We take into account the effects of race (white–nonwhite) and gender by running separate analyses for combinations of these two variables. Such additional analyses are important. As described in more detail below, the suicide rates of these groups differ significantly both in magnitude and in their age distributions. If our model is robust, it should hold for each of these data sets. Thus, analyses using disaggregated data provide a way to further check the model's validity.

DETECTING COHORT EFFECTS

Many of the specifications in our APCC analysis derive from the insights of Easterlin (1987) combined with some additional conceptualizations of cohort processes (O'Brien 1989; O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999). We follow five suggestions found in O'Brien, Stockard, and Isaacson (1999) for specifying APCC models. (1) The dependent variable should be cohort-specific. In our case we use age-period-specific suicide rates. (2) Relative measures should be used for the independent variables; we employ

TABLE 1: Means, Standard Deviations, and Range of Age-Period Specific Suicide Rates, Nonmarital Birth Rates, and Relative Cohort Size[a]

| | | White | | Nonwhite | |
|---|---|---|---|---|---|
| Variables | Total | Male | Female | Male | Female |
| Suicide rate | | | | | |
| Mean | 11.68 | 18.92 | 6.15 | 11.68 | 2.95 |
| S.D. | 6.06 | 10.16 | 3.72 | 6.52 | 1.74 |
| Range | .28–20.60 | .41–43.00 | .10–14.00 | 0.00–27.60 | .00–6.80 |
| | | | | | |
| Nonmarital birth rate | | | | | |
| Mean | 4.34 | 2.26 | | 17.70 | |
| S.D. | 2.96 | 1.61 | | 7.57 | |
| Range | 2.1–19.6 | 1.30–12.18 | | 11.86–49.28 | |
| | | | | | |
| Relative cohort size | | | | | |
| Mean | 12.93 | 12.64 | | 15.03 | |
| S.D. | 1.57 | 1.58 | | 1.76 | |
| Range | 10.42–15.33 | 10.10–14.90 | | 12.10–18.60 | |

[a] The summary statistics for the age-period-specific suicide death rates for the total population are based on the 105 cell entries in Figure 2 and those for each race race-sex group are calculated in the same way using race-sex-specific data. For RCS and %NB, the summary statistics are based on the fourteen cohorts in Figure 2.

the relative cohort size and the percentage of nonmarital births. (3) Age groups should be wider than a single year or two; our age groupings cover five-year periods. (4) Effects of the cohort characteristics should be examined throughout the life span of the cohorts. We examine such effects throughout the life span of cohorts and correct for nonindependence of observations within cohorts. (5) In order to disentangle the effects of cohort characteristics, it is necessary to control for both age and period effects, which we do in our APCC model.

## Results

Table 1 provides descriptive statistics on the variables in our analysis for the total group and each race-gender subgroup. There is substantial variation on each of our measures. For the total group, age-period-specific suicide rates range from a low of .28 for those 10 to 14 years old in 1950 to a high of 20.60 for those 55 to 59 years old in 1975: a range of 20.32. The range is greater for white males (42.59) and nonwhite males (27.6) and less for white females (13.9) and nonwhite females (6.8). The levels of age-period-specific suicide rates vary dramatically from one race-sex group to another. The mean suicide rate for white males (the average over all the age-period-specific suicide rates in our

sample for white males) of 18.92 is more than 50% larger than the average rate for nonwhite males, 11.68, and more than three times as large as that for white females, 6.15. Rates for nonwhite females are extremely low, with a mean of only 2.95. The suicide rates for nonwhite females are so low that the government did not report age-period-specific rates for a number of years. These include the years (1990 and 1995) for which data are available for ages 70 to 74 and 75 to 79 and five of the fourteen periods that provide data for ages 10 to 14. Rates were also too low to report for one period for nonwhite males in the 10 to 14 age group.[15]

In addition to variations in the magnitude of suicide rates, there are substantial differences in the age groups at which suicides occur across the four race-sex groups. Figure 3 shows the age distributions for suicide in 1995 for white males, white females, nonwhite males, and nonwhite females. These curves differ dramatically. The curve for white males climbs steeply to age 20–24 and then drops slowly to age 55–59 and then climbs to new highs. For nonwhite males the suicide rate climbs steeply to age 20–24 and then drops fairly steeply until age 40–44 and decreases more slowly until age 55–59. The age curve for white females increases slowly until age 45–49 and then remains fairly stable. For nonwhite women, the suicide rate remains low relative to all other groups and peaks first at age 25–29 and then again at 65–69. As demonstrated below, when our model is tested on data for these four race-sex groups, it can account for changes in the age distributions of suicide over time for each of these populations.

With the exception of one cell for nonwhite males, missing data is a problem only for suicides involving nonwhite females (see note 15). We addressed the problem of missing suicide data for nonwhite females in three ways. (1) We treated the data as missing observations and thus eliminated the "cases" from our analysis. (2) We substituted the lowest suicide rate observed for the race-sex group for the unreported rates. (3) We treated the unreported rates as zero and then added one to all of the rates before taking the natural log. Our results were substantively similar no matter which of these three ways we treated the missing data, and thus we report only the analyses in which we treated the missing data as missing observations. (Results from the other analyses are available on request.) Given the relative infrequency of suicides for nonwhite women and the relatively small size of their population, suicide rates for nonwhite women are the least reliable of those we analyze.

The percentage of nonmarital births for the total population ranges from 2.1% for cohort 1 to 19.6% for cohort 14. Note, however, as shown in Figure 2, the increase in this variable is not linear or even monotonic, with cohort 5 (born between 1935 and 1939) having a value that is higher than cohorts 6, 7, and 8 (born between 1940 and 1954). There are dramatic differences in the magnitude of this variable between whites and nonwhites, with the percentages

TABLE 2: Logged Age-Specific Suicide Rates Regressed on Age Dummies, Period Dummies, Relative Cohort Size, and the Percentage of Nonmarital Births and Corrected for Cohort Heterogeneity

| Variables | Total | | White Male | | Female | | Nonwhite Male | | Female | |
|---|---|---|---|---|---|---|---|---|---|---|
| | b | t | b | t | b | t | b | t | b | t |
| Intercept | −2.021 | −11.381 | −1.547 | −5.815 | −2.285 | −12.292 | −2.235 | −8.231 | −2.384 | −9.197 |
| Period | | | | | | | | | | |
| 1930 | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] |
| 1935 | .053 | 1.078 | −.090 | −2.035 | .395 | 2.840 | −.061 | −.204 | .452 | 2.377 |
| 1940 | −.039 | −.640 | .020 | .175 | −.133 | −.574 | .053 | .104 | .470 | 2.038 |
| 1945 | −.184 | −1.541 | .108 | .849 | −.240 | −2.116 | −.668 | −2.502 | −.609 | −1.006 |
| 1950 | −.305 | −7.640 | −.084 | −1.253 | −.447 | −3.638 | −.500 | −2.427 | −.189 | −.955 |
| 1955 | −.389 | −8.271 | −.145 | −1.464 | −.584 | −4.209 | −.495 | −2.664 | −.275 | −1.352 |
| 1960 | −.124 | −2.607 | .221 | 3.726 | −.421 | −3.360 | −.418 | −1.659 | −.079 | −.308 |
| 1965 | .005 | .063 | .323 | 3.750 | −.247 | −1.130 | −.151 | −.679 | .299 | 1.517 |
| 1970 | .116 | 1.527 | .457 | 6.179 | .022 | .163 | −.184 | −.775 | .422 | 1.998 |
| 1975 | .198 | 2.762 | .588 | 6.725 | .036 | .282 | −.177 | −.551 | .376 | 1.913 |
| 1980 | .023 | .250 | .497 | 4.483 | −.268 | −1.832 | −.276 | −.288 | .001 | .005 |
| 1985 | −.016 | −.144 | .531 | 4.528 | −.355 | −1.841 | −.309 | −1.165 | −.015 | −.065 |
| 1990 | −.144 | −1.343 | .466 | 3.876 | −.557 | −3.410 | −.420 | −1.441 | −.082 | −.304 |
| 1995 | −.333 | −3.064 | .337 | 2.776 | −.862 | −5.772 | −.587 | −1.925 | −.238 | −.973 |
| Age | | | | | | | | | | |
| 10–14 | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] | .000[a] |
| 15–19 | 2.239 | 32.326 | 2.115 | 23.841 | 2.386 | 16.968 | 2.532 | 11.365 | 2.102 | 8.505 |
| 20–24 | 3.044 | 42.344 | 2.915 | 42.319 | 3.063 | 20.078 | 3.704 | 15.652 | 2.914 | 13.422 |
| 25–29 | 3.258 | 52.121 | 3.031 | 42.058 | 3.420 | 23.481 | 3.924 | 16.532 | 3.052 | 12.189 |
| 30–34 | 3.397 | 64.518 | 3.098 | 38.706 | 3.702 | 28.884 | 4.020 | 19.276 | 3.227 | 16.200 |
| 35–39 | 3.503 | 46.879 | 3.167 | 30.475 | 3.885 | 24.065 | 3.942 | 18.192 | 3.079 | 13.922 |
| 40–44 | 3.603 | 41.648 | 3.232 | 30.292 | 4.018 | 22.479 | 3.900 | 15.908 | 3.110 | 13.185 |
| 45–49 | 3.664 | 34.726 | 3.262 | 26.926 | 4.066 | 25.790 | 3.922 | 16.042 | 3.017 | 13.238 |
| 50–54 | 3.702 | 32.963 | 3.302 | 24.355 | 4.053 | 28.820 | 3.827 | 15.100 | 3.077 | 12.342 |
| 55–59 | 3.715 | 39.653 | 3.322 | 27.504 | 4.016 | 27.611 | 3.920 | 14.639 | 2.943 | 12.785 |
| 60–64 | 3.714 | 46.578 | 3.313 | 34.642 | 3.943 | 25.479 | 3.968 | 14.015 | 3.005 | 11.854 |
| 65–69 | 3.803 | 40.411 | 3.409 | 32.016 | 3.911 | 21.433 | 4.134 | 15.736 | 3.221 | 10.163 |
| 70–74 | 4.021 | 39.399 | 3.641 | 30.561 | 4.033 | 27.114 | 4.533 | 14.125 | c | c |
| 75–79 | 4.203 | 38.651 | 3.883 | 31.964 | 4.022 | 26.933 | 4.577 | 15.000 | c | c |
| RCS[b] | .063 | 5.452 | .064 | 3.757 | .038 | 2.923 | .024 | 1.739 | .010 | .786 |
| Percent NB[b] | .123 | 9.424 | .156 | 4.812 | .231 | 11.343 | .058 | 7.896 | .028 | 2.908 |
| N | 105 | | 105 | | 105 | | 104 | | 95 | |
| S.E. | .1143 | | .1418 | | .1972 | | .2554 | | .2828 | |
| $R^2$ age and period | .973 | | .974 | | .951 | | .949 | | .918 | |
| $R^2$ full model | .993 | | .989 | | .980 | | .967 | | .924 | |
| Adjusted $R^2$ | .990 | | .984 | | .972 | | .955 | | .895 | |

[a] Dummy variable omitted for purposes of estimation.
[b] Whether these measures are based on the total population, whites, or nonwhites depends on the dependent variable.
[c] All cases for nonwhite female suicides in these age groups had missing data.

for whites ranging from 1.30 to 12.18 and for nonwhites from 11.86 to 49.28. Despite these differences, the pattern of fluctuations in the percentage of nonmarital births over time is similar across these groups.

Finally, the measure of cohort size relative to the total population varies from 10.4 to 15.3. The cohorts with the smallest relative sizes are 12, 13, and 14 (born in the 1970s and early 1980s) and cohorts 4 and 5 (born in the depression years of 1930 to 1939). The cohorts with the largest relative sizes are those in the post–World War II baby boom (cohorts 7, 8, and 9 born between 1945 and 1959). Nonwhites have higher values of RCS than whites, with a range from 12.1 to 18.6. This reflects slightly higher birth rates, but also higher death rates, which reduce the number of older adults within the population. Again, the pattern of fluctuation in these rates for whites and nonwhites is quite similar, although their magnitudes differ.

## THE RELATIONSHIP OF AGE AND PERIOD TO SUICIDE RATES

We began our analysis by regressing the log of age-period-specific suicide rates on the age and period dummy variables. The $R^2$ ($R^2_{\text{Age and Period}}$) for the total population and for the demographically disaggregated data appear in Table 2. For each set of rates, age and period explain much of the variation, ranging from .97 for white males and the total population to .92 for nonwhite females. Most of this explanatory power is due to the age dummy variables. For example, for the total population the $R^2$ for the period dummy variables as a set is .32 (adjusted $R^2$ = .22), which is statistically significant. When the age dummy variables are added to the equation, the $R^2$ increases to .97 (adjusted $R^2$ = .96). This pattern is repeated for the race-sex-specific analyses.

## THE INFLUENCE OF COHORT CHARACTERISTICS

One way of viewing the age-period model (the model including only these dummy variable sets) is that it constrains the age distribution of suicide to have the same form across periods (although periods may differ in levels). We add cohort characteristics to the model to examine whether we can explain changes in the shapes of the age curves over periods, such as the changes shown in Figure 1.[16]

Examining Table 2, we note that when the two cohort characteristics are added to the model, the $R^2$ values are extremely high: .99 for the total group and white males, .98 for white females, .97 for nonwhite males, and .92 for nonwhite females. In each of the analyses, the changes in $R^2$ due to adding the two cohort characteristics as a set are statistically significant ($p < .001$). The statistical significance of the change in $R^2$ by adding each cohort characteristic as the "final variable" to the equation is identical to the significance of the $t$-test for their regression coefficients reported in Table 2. As hypothesized, the

coefficients associated with the measures of both cohort characteristics are positive for each analysis.

The coefficients associated with nonmarital births are statistically significant in all cases, indicating that there is a consistent tendency for race-sex-specific cohorts with larger numbers of nonmarital births to have higher rates of suicide even after controlling for their ages, the historical periods in which they live, and their relative sizes. The greatest effect of nonmarital births is for white females, with an unstandardized regression coefficient of .231. This means that a one-unit change (a 1 percentage point change, since we measure nonmarital births as the percentage of live births that are to unmarried women) is associated with a .231 change in the natural log of the age-period-specific suicide rate. This is an awkward interpretation, but we can transform this regression coefficient to obtain the following interpretation: A unit change in the percentage of nonmarital births is associated with a $[100\% \times (e^{.231} - 1)]$, or 26.1%, increase in the age-period-specific suicide rate.[17] The smallest coefficient is for nonwhite females, with a regression coefficient of .028, which means that a one-unit change (1 percentage point change) in the percentage born out of wedlock is associated with a $[100\% \times (e^{.028} - 1)]$, or 2.8%, increase in the age-period-specific suicide rate. Although this coefficient is statistically significant at the .01 level, the size of the regression coefficient and that for nonwhite males is smaller than those for white males and females.

The coefficients associated with relative cohort size, while in the expected direction, are significant in only three of the five analyses: for the total group and for white males and white females. The size of these coefficients ranges from .065 for white males to .010 for nonwhite females. Again, since the dependent variable is the natural log of the suicide rate, a regression coefficient of .065 means that a one-point change in relative cohort size (e.g., a change from 11 to 12% of the population 15 to 64 being in the cohort when the cohort is 15 to 19) is associated with a 6.7% increase in the age-period-specific suicide rates for the total population.[18]

When we compared the coefficients for percent NB and RCS from one group to another to see if the differences between them were statistically significant, we found that for percent NB all the coefficients, except the comparison between white males and white females, were significantly different from each other. Specifically, the coefficient associated with percent NB for white males was significantly stronger than the coefficient for nonwhite males $(t = 2.95)$ and nonwhite females $(t = 3.78)$; the coefficient for white females was significantly stronger than that for nonwhite males $(t = 7.99)$ and nonwhite females $(t = 9.01)$, and the coefficient for nonwhite males was significantly stronger than the one for nonwhite females $(t = 2.48)$. For the coefficients associated with RCS, only one difference in coefficients was significant, that between white males and nonwhite females $(t = 2.54)$.[19]

TABLE 3: Logged Age-Specific Suicide Rates Regressed on Age Dummies, Period Dummies, Relative Cohort Size, and Corrected for Cohort Heterogeneity[a]

| | White | | | | | | Nonwhite | | | |
| | Total | | Male | | Female | | Male | | Female | |
| | b | t | b | t | b | t | b | t | b | t |
|---|---|---|---|---|---|---|---|---|---|---|
| RCS[b] | .073 | 6.430 | .073 | 3.980 | .036 | 2.406 | .041 | 2.681 | .006 | .462 |
| Percent NB[b] | .131 | 10.287 | .167 | 5.123 | .228 | 9.820 | .069 | 8.924 | .025 | 2.417 |
| N | 85 | | 85 | | 85 | | 85 | | 81 | |
| S.E. | .1129 | | .1393 | | .2006 | | .2072 | | .2321 | |
| $R^2$ | .991 | | .986 | | .976 | | .977 | | .937 | |
| Adjusted $R^2$ | .988 | | .981 | | .968 | | .969 | | .911 | |

[a] The data are truncated to exclude the periods 1945 and earlier and the age groups 60 and above.
[b] Whether these measures are based on the total population, whites, or nonwhites depends on the dependent variable.

To assess the effects of multicollinearity in the analyses in Table 2, we calculated variance inflation factors (VIF). Rawlings (1988) and others cited in Rawlings suggest that serious collinearity problems do not occur when VIF values are less than 10. Using this criterion, some of the dummy variables for age and period have serious collinearity problems and thus seriously inflated standard errors. The highest VIF values associated with any of the relative cohort size measures in any of the analyses, however, are less than 1.8, and the highest VIF values associated with any of the percent NB measures are less than 7.5.

INTERACTIONS

Because of earlier literature predicting an interaction between relative cohort size and age (Kahn & Mason 1987; Pampel 1996), we checked for interactions between age and relative cohort size for both the younger groups and the older groups. We created product terms based on the dummy variable for an appropriate age group times the relevant race-specific relative cohort size measure for that cohort when it was that age. For example, for the total population and the age group 15 to 19 in 1940 (members of cohort 2), we multiplied the relative cohort size for their cohort (13.69) times 1. For those 15 to 19 in 1945 (members of cohort 3) we multiplied 12.39 times 1. This process was continued for all the observations in the age group 15 to 19. In this way we developed an interaction term for the interaction of the age group 15 to 19 with relative cohort size. We then ran the full regression models presented in Table 2 with interaction terms for age group multiplied by relative cohort size for the three age groups from 15 to 30. In a second regression

analysis we included these three interaction terms and added four more for the oldest groups: those 60 and above.[20]

The major finding is that *none* of the sets of interactions when added to the equations in Table 2 is statistically significant, although in a few instances some of the individual interaction terms reach conventional levels of statistical significance. Here, we follow Cohen and Cohen's (1983) "protected *t*-test" strategy and do not examine the individual coefficients unless the coefficient of the set in which they figure is significant. It is no surprise that adding the seven interaction terms of age with RCS increases the multicollinearity and reduces the statistical significance of the main effect of RCS in our analyses.[21] Pedhazur (1982:260) cautions that "even the staunchest advocates of such an approach have warned that the simultaneous analyses of vectors and their cross products 'result in general in the distortion of partial regression coefficients' (Cohen 1978) associated with the vectors from which the cross products were generated . . . thereby resulting in the latter appropriating some (often much) of the variance of the former" (861). Still in all cases the coefficient for the main effect of RCS remains positive. One more of the RCS coefficients becomes insignificant (the one for white females) while those for nonwhite males and females remain insignificant as they were in Table 2. We seem to be adding only multicollinearity to the analysis through this procedure, and our substantive conclusions concerning the effects of percent NB and RCS change only slightly. It should be noted, however, that the tests for the interactions between age and RCS lack power because of the high levels of collinearity associated with these interaction terms.

## ANALYSIS FOR THE DATA EXCLUDING PERIODS BEFORE 1950 AND AGE GROUPS OVER 60

To examine the robustness of our analyses that use the full set of data in our triangular data matrix, we decided to eliminate those cases before 1950 and those cases in age groups 60 to 64 and older. Our concern was the possible effects of having so few cases in the periods before 1950 and in the age groups 60 to 64 and above. For example, there is only one case in the age category 75 to 79 and one case in the period 1930.[22] It is not surprising that these cases are perfectly predicted by our model.

The results from applying our model to this more limited data set are reported in Table 3. (Coefficients associated with age and period are eliminated to save space and to ease reading of the table; these coefficients are available on request.) The results are very similar to those found with the triangular matrix. The coefficients associated with cohort characteristics are all in the hypothesized direction. As in Table 2, the coefficients associated with the measure of nonmarital births are significant for the total group and all race-sex groups. The magnitudes of the coefficients are very similar to those reported in Table 2, as are the $R^2$ values. The major exception involves the influence of

RCS for nonwhite males, which becomes larger and statistically significant when the truncated data set is used in the analysis. Statistical comparisons of the significance of differences between the coefficients for the race-sex groups are, however, identical to those obtained with the full data set.

## Discussion

FINDINGS

The results provide strong support for our theoretical model. Independent of age and period, and with an implicit control for the linear effect of cohort time of birth, cohort characteristics that are theoretically related to integration and regulation have substantively strong and statistically significant relationships to age-period-specific rates of suicide. These results parallel earlier applications of this model to age variations in homicide arrest rates (O'Brien, Stockard & Isaacson 1999) and homicide deaths (O'Brien & Stockard 2002). Members of cohorts that are relatively large and that have larger numbers of nonmarital births are at higher risk of both suicide and homicide. These cohort characteristics are associated with the recent upturn in both youth suicide and homicide rates. Moreover, these cohort characteristics, which are theoretically related to less integration and regulation, are associated with higher suicide rates throughout the life cycle (at least for the age groups we examine: 10–14 to 75–79).

It is important that we explain shifts in the age distribution of suicide rates across periods — not the level of suicide rates in any period. Factors such as wars, economic prosperity, and even the weather, all of which can be plausibly linked to suicide, are not included in our model but may well be associated with the frequency of suicide in a given period. The period dummy variables included in our analysis control for average differences in the levels of suicide across periods. In the same manner, we do not explain the differences between male and female suicide rates nor the differences between rates of whites and nonwhites.[23] What we do examine is whether shifts in the age distribution of these rates within each race-sex group can be explained by changes in cohort characteristics. Our results show that cohort characteristics are strongly related to these changing age patterns.

Our results consistently indicate that family structure, as measured by the rate of nonmarital births, is a more important influence than relative cohort size on changes in age-specific suicide rates, a finding that parallels our findings in our analyses of homicide arrest and death rates (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999). As shown in Figure 2, more recent cohorts have the greatest number of nonmarital births and some of the smallest relative cohort sizes. Relative cohort size is highest for the baby boom cohorts (cohorts

7–10, born between 1945 and 1964). Thus, it is reasonable to surmise that the sharp changes in family structure are most responsible for recent strong increases in youthful suicide, while increases in relative cohort size account for the more modest increases in youth suicide observed in earlier periods, particularly 1960–75. Most important, our results suggest that both of the cohort characteristics related to integration and regulation — relative cohort size and nonmarital births — are needed to adequately model changes in age-specific suicide rates in the U.S. from 1930 through 1995.[24]

Further analyses indicate that the findings in our research are very robust. When we tested for interactions for the youngest groups and the oldest groups, we did not find significant effects for these sets of interactions and the patterns of influence of RCS and percent NB remained virtually unchanged, bolstering our conclusion that the effect of cohort variables persists throughout the life cycle. When we limited the age range and range of periods to eliminate age groups with few cases and periods with few cases, we found substantially the same results.

We also found quite similar and robust lifelong effects on the chances of suicide across the various race-sex combinations. These results are striking, given the rather different age distributions and frequencies of suicides across these different groups and across different periods. For instance, white males have traditionally had higher suicide rates than members of the other race-sex groups, and within this group suicides have, until recent years, been most common among older ages. Suicides are less common in the other groups and also have tended to peak at younger ages. Yet birth cohorts in all four race-sex groups characterized by higher rates of nonmarital births and relatively large cohort size tend to have relatively higher rates of suicides than do others after controlling for age and period.

Despite the similarities in general patterns of results, there were some differences in the strength of the relationships found with each race-sex group. For instance, the $R^2$ value associated with the analyses of suicide rates for nonwhite females was markedly lower (adjusted $R^2 = .895$ in Table 2 and .911 in Table 3) than those for the other analyses. We suspect that this $R^2$ value reflects, at least in part, the lower reliability of the dependent variable for this group. This is consistent with the smaller size of this group and its lower suicide rates. Other scholars have reported similar patterns in analyses of suicide rates of nonwhite women compared to other demographic groups, attributing the results to "the random decreases and increases in the suicide rates of the nonwhite female by age groups" (Gibbs & Martin 1964:71). In addition, however, the coefficients for percent NB were significantly lower for nonwhites (both males and females) than for whites, and that for nonwhite females was significantly lower than that for nonwhite males. The coefficient associated with RCS was significantly lower for nonwhite females than for white males. These results contrast with those obtained in our analysis of homicide deaths (O'Brien

& Stockard 2002), in which the coefficients associated with RCS were significantly larger for both nonwhite males and females than for white males. In that analysis the coefficient associated with percent NB was significantly larger for white males than for the other three groups, but the coefficients for the other groups, as well as the $R^2$ values for all four groups, were very similar.

To summarize, the empirical findings are clear and robust. The percentage of nonmarital births and the relative size of a cohort are positively related to age-period-specific suicide rates after controlling for the effects of age groups and periods. The results hold across the life span, across different race-sex groups, and when subjected to various restrictions and modifications in the analytic procedures. These findings have implications for theory and policy.

## THEORETICAL IMPLICATIONS

Our findings on suicide parallel other findings within the general tradition of cohort research that indicate ways in which formative experiences can produce lifelong effects. When we combine our results on suicide with those for homicide (O'Brien & Stockard 2002; O'Brien, Stockard & Isaacson 1999), we see that similar factors are related to both these forms of lethal violence. We suggest that integration and regulation account for cohort variations in both homicide offending and suicide death, that is, lethal violence directed toward both self and others. While most previous analyses of suicide and homicide have appeared in different venues and scholarly traditions, we suggest that general theoretical understandings of these phenomena might be greatly enhanced if a more unified framework were employed.

In our analyses of homicide we drew on theories of early childhood socialization (including the importance of monitoring and supervision of young children to the development of self-control), social capital/social networks, and social disorganization. Here we build on that framework by noting the connection between those perspectives and the longstanding Durkheimian tradition of work on social integration and social regulation, which has often been used in research on suicide. While the theoretical terms *social integration* and *social regulation* are less often used in the criminological tradition, we posit that these concepts are related, both theoretically and empirically, to the concepts of social networks, social control, and self-control that are found in the criminological literature. Furthermore, we suggest that taking a more unified theoretical approach that uses this broad Durkheimian tradition would advance the study of both suicide and homicide.

Consistent with the strong relationship of nonmarital births to suicide in this research and to homicide in the research of O'Brien, Stockard, and Isaacson (1999) and O'Brien and Stockard (2002), we argue for the key role of family structure in both social integration and regulation. We suggest that

the effect of family structure occurs in two ways: The first involves the lower levels of integration, regulation, support, and social control that young people growing up in single-parent families experience. When the rate of nonmarital births is higher, more children have this experience. The second involves all children within cohorts with higher rates of nonmarital births and reflects the fact that these children are less likely to interact in networks with high degrees of closure or to have consistent levels of support and control from adults. In addition, they will be more likely to associate with peers who come from families with lower levels of family support, integration, and control.

Relative cohort size is also related to rates of suicide and homicide offenses, although this relationship is not as strong as that with nonmarital births. Easterlin suggests that relative cohort size affects well-being through its impact on economic experiences such as unemployment. Our analysis suggests that the negative impact of relative cohort size occurs even before young people have any meaningful contact with the work world by increasing the probability of death from suicide among youth as young as 10–14 years of age. Thus, we suspect that relative cohort size influences social regulation and integration through its effect on family and community resources. When individual families are larger, family resources, both for integration and regulation, are stretched. Similarly, when birth cohorts are large, community resources, in areas such as schools, churches, and community groups, must provide more services and have relatively fewer adults to provide these services, thus again providing less integration and regulation. It must be remembered, however, that the impact of relative cohort size on lethal violence is weaker than that of nonmarital births in both this analysis and in our earlier work involving homicide offenses (O'Brien, Stockard & Issacson 1999) and homicide deaths (O'Brien & Stockard 2002).

Whites have traditionally had higher rates of suicide and lower rates of homicide than nonwhites. Our analyses in this article and in O'Brien and Stockard (2002) indicate that cohort-related variables influence variations in age-period-specific suicide and homicide death rates in similar ways for both whites and nonwhites, but that the influence of these cohort variables tends to be somewhat stronger for whites for suicide and for nonwhites for homicide. These results suggest that cohort-related variables influence lethal violence for all members of the society, but the pattern of this influence may involve interactions between demographic characteristics such as race and sex and the particular form of lethal violence. We hope, in the future, to model the extent and nature of these differences more fully.

Finally, our results stand in sharp contrast to van Poppel and Day's (1996). They suggest that suicide cannot be explained by sociological factors, but instead might be accounted for by "psychological (or even idiosyncratic) explanations." Our results clearly demonstrate the ways in which social factors

can pattern and either promote or constrain the propensity to commit suicide throughout the life course.

POLICY ISSUES

In 1995 more than 54,000 people in this country died from lethal violence (over 31,000 from suicide and almost 23,000 from homicide). Over one-fourth of these deaths involved people under the age of 25 (National Center for Health Statistics 1999). This yearly death toll is larger than the number of deaths suffered by U.S. military personnel in all wars in the twentieth century except World War II.[25] In addition to the heartbreak and anguish suffered by surviving families and friends, such a high loss of life represents a tremendous loss of potential productivity to the society.

This loss is particularly staggering among nonwhites. Even though nonwhite men composed only 10% of the population under age 25 in 1995 (U. S. Bureau of the Census 1997:21), they accounted for 35% of all lethal deaths occurring to this age group: 14% of the suicides and 48% of the homicides (calculated from National Center for Health Statistics 1999). If the current rates of lethal violence continue, we project that, between the ages of 20 to 39, 2.3% of the population of nonwhite males will die from lethal violence.[26] In addition, nonwhite men constitute a large proportion of people arrested for homicide. Such arrests removed more than 9,000 people (most of them young men) from nonwhite communities in 1995 (U.S. Federal Bureau of Investigation 1996:226). Such cumulative losses produce a major impact, especially on communities such as those in deprived inner-city areas, where the local rates are far higher than the averages.[27]

It is difficult to try to predict future events from past trends. Nevertheless, given that the youngest cohort in our analysis (cohort 14), which was born between 1980 and 1984, had an RCS value of 10.42, the lowest in the data set, but a nonmarital birth rate of 19.6, the highest value in the data set, we could expect their suicide rate, controlling for the effects of period and age, to be relatively high throughout their life span (remembering the greater impact of percent NB than RCS on suicide). The suicide rate for 10–14-year-olds in this cohort was 1.70, more than four times the incidence reported for cohort 1, which had a nonmarital birth rate of 2.1%. Cohorts born between 1985 and 1989 and between 1990 and 1994 are similar in relative cohort size to cohort 14 but have substantially more nonmarital births (the percentage for the total population was 28.0 in 1990 and 32.6 in 1994). Our analysis suggests that these younger cohorts should continue to experience rates of suicide and homicide that are high relative to those for other age groups.[28]

A particular advantage of our model is its ability to identify, literally in infancy, cohorts that are likely to be more susceptible to lethal violence. Such

assessments provide the opportunity for policymakers, concerned social scientists, and citizens to develop policies that might ameliorate the factors that lead to the lessening of social integration and regulation that larger cohort sizes and higher rates of nonmarital births can produce.

We have elsewhere expanded the analysis presented in this article by examining the relationship of RCS and percent NB to age-specific suicide rates in other countries, including countries in which the levels of suicide and societal support for families and youth differ from those in the U.S. (Stockard & O'Brien 2002). This research examines the extent to which the effects of RCS and percent NB may be affected by social policies that provide different levels and kinds of family and community support for young people as well as the pace of social change.

## Notes

1. The data in Figure 1 are taken from the Vital Statistics of the U.S. (U.S. Department of Health, Education, and Welfare, National Center for Health Statistics, various years). These are the calculated total rates for each age group for 1930, 1960, and 1995.

2. We recognize that integration and regulation are conceptually distinct (see Thorlindsson & Bjarnason 1998), but they are empirically confounded when analyzing the experiences of birth cohorts at the macro level and often stem from the same causal factors.

3. Some work within sociology has built upon the psychological frustration-aggression tradition to analyze variations in rates of both suicide and homicide (e.g., Henry & Short 1954; Unnithan et al. 1994). While this work has produced interesting and persuasive theoretical arguments, it has had only limited empirical support (e.g., Corzine & Huff-Corzine 1994:152–57). Another tradition, beginning with Merton (1938), has emphasized a version of anomie theory that focuses on the disjuncture between culturally defined ends and the availability of legitimate means for attaining those ends, a formulation that is strikingly similar to the frustration-aggression tradition (see especially Agnew 1997:27). This tradition, termed "strain theory" in the criminological literature, has been applied to many types of criminal behavior. Our own borrowings from Durkheim emphasize the relationship between low levels of social integration and regulation and homicide and suicide, rather than the disjuncture between ends and means. In this respect, we agree with Hirschi (1969:3) when he contrasts Durkheim as interpreted by strain theorists with his own control theory: "Actually, Durkheim's theory is one of the purest examples of control theory: both anomie and egoism are conditions of 'deregulation,' and the 'aberrant' behavior that follows is an automatic consequence of such deregulation."

4. Gibbs and Martin (1964), Johnson (1965), and other authors (e.g., Fernquist & Cutright 1998; Pescosolido & Georgianna 1989; Pope 1976) note that the distinctions Durkheim draws between egoistic, anomic, altruistic, and fatalistic suicides are conceptually confusing and inconsistent. Gibbs and Martin (1964) concentrate their analysis on "social integration, the concept central to Durkheim's general conclusion"

(p. 7), suggesting that Durkheim saw social integration as having "to do, in the final analysis, with the strength of the ties of individuals to society" (p. 16). Similarly Johnson (1965:886) sees social integration as a necessary condition for social regulation and argues that there is only one type of suicide: egoism (anomie). In his own words: "The more integrated (regulated) a society, group, or social condition is, the lower its suicide rate."

More recently, Thorlindsson and Bjarnasson (1998:98) state "On a theoretical level, it is difficult to maintain that the normative force of a social group on its members [regulation] is unrelated to the strength of the bonds between the group and its members [integration]." Further, they note that (p. 98) "Nearly all the indicators used by Durkheim and his successors appear to be tapping both constructs at once. For instance, marriage involves both integration into a relationship, and regulation and self regulation resulting from the relationship." Their individual-level analysis allowed them to disentangle the empirical influence of integration and regulation on suicidality. Unfortunately, such disaggregation would be extremely difficult, if not impossible, to develop with a macro level of analysis.

In addition, in the case of both integration and regulation Durkheim maintains that too little of either of these variables results in increases in suicide (egoistic and anomic suicide respectively) and too much of either of these may result in increases in suicide (altruistic and fatalistic suicides respectively). We maintain that in the U.S. since the 1930s the danger has not been overintegration and overregulation (with its potential for fatalistic or altruistic suicide), but to be underintegrated and underregulated with the potential for egoistic and anomic suicide. As Durkheim ([1897] 1951) put it, "Anomy, therefore, is a regular and specific factor in suicide in our modern societies" (258).

5. Though we have found no large-scale sociological analyses of this hypothesis at either the micro or the macro level of analysis, clinical reports have documented the joint appearance of violence against self and others among individuals (e.g., Cairns & Cairns 1994; Hendin 1969).

6. As is usual in analyses at the macro level, we do not examine the individual manifestations of lowered levels of integration and regulation. However, micro level data provide evidence of the results of these possible effects, including greater psychological stress among larger birth cohorts (Easterlin 1980; Veroff 1978). This greater stress is assumed to be related to a higher probability of lethal violence, including suicide. Literature cited in the text documents the micro-level effects of living in single-parent families and their relationship to higher suicide rates.

7. Our measure of relative cohort size is quite different from Pampel's (1998) and therefore our test is not a replication of his study. His measure shifts from the relative number of the population who are young to the relative number who are old, depending on the age of the cohort.

8. Not until the 1960s does Vital Statistics of the U.S. provide a breakdown of nonmarital births that distinguishes African Americans from others in the nonwhite category. If we insisted on examining the relationship between nonmarital births and suicide for African Americans, we would need to begin our analysis with the 1960–64 birth cohort rather than the 1915–19 birth cohort.

9. National averages of the nonmarital births have not always been based on complete data from all states. For instance, in 1917 nearly half the states did not supply data to

the national registry. In 1933, Texas was the last state to join. Even after that time, however, not all states required statements on illegitimacy in all years. For example, California, Massachusetts, and New York did not require statement on the legitimacy of the birth for any of the years from 1933 to 1946. Texas did not require such a statement for 1938 and 1939, Maryland, Nebraska, New Hampshire, and Wyoming for 1940–46; Colorado, Connecticut, and New Mexico for 1943–46; Arizona, Idaho, and Nevada for 1945 and 1946; South Carolina for 1946 (Vital Statistics, 1946 part 1, p. XXVIII). Other changes in completeness of registration for illegitimate births have occurred since 1946. Readers should be aware of the potential for bias that these changes might create.

10. RCS for the youngest cohort in our analysis (those aged 10–14 in 1995) was estimated by calculating the percentage of the population aged 10–59 that was 10–14 in 1995.

11. The data used to calculate rates are based on the Current Population Surveys (P25 series) for July 1 of each of the years we use. The degree of undercount by race certainly differs in these surveys, and this degree of undercount may change from year to year. The inclusion of dummy variables for period controls for changes in levels of such an undercount in each of the race-specific analyses and in the total population analysis. Only if the undercount affects different ages differently over time would this create a problem for our analysis, given that we concentrate on the effects of cohort characteristics, RCS and percent NB, on the age distribution of suicides.

12. Again, the period dummy variables control for variables with values that change across periods but that are constant across age. This includes variables such as average temperature, residential density, suburbanization, and unemployment — to the extent that their effects are the same across age groups. We do not maintain that these effects are completely constant across age groups, but to the extent they are relatively constant their effects are controlled.

13. 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 $P - 1$ of the time periods, $A - 1$ of the age groupings, and dummy variables for each of the $C - 1$ cohorts, a problem noted by Mason et al. (1973).

14. We could have represented age as a continuous variable with, for example, the mean age of each age group. If we thought the effect of age were curvilinear, we might have included a quadratic age component. This would save degrees of freedom in comparison with our dummy variable representation. Similarly, one can represent periods with linear or higher-order polynomial terms. But the most complete control for the main effects of age and period is obtained by our dummy variable representation. The importance of controlling for the main effects of age and period when examining cohort effects is highlighted in recent exchanges on cohort effects and changes in vocabulary scores over time (Alwin & McCammon 1999; Glenn 1999; Wilson & Gove 1999a, 1999b).

15. Specifically, data were missing for nonwhite women for cohort 1 for ages 70–74 and 75–79; for cohort 2 for ages 70–74; for cohort 3 for ages 10–14 and 60–64; and for cohorts 4, 6, 7, and 14 for ages 10–14. Data were missing for nonwhite males for cohort 4 for ages 10–14. These data are "missing" because there were too few cases for reliable estimation. In Table 1 these are treated as zeros.

16. One reviewer of this article noted that, to the extent a linear effect of cohorts exists, the effect is confounded with the age and period coefficients reported in Table 2. Readers

are cautioned to keep this in mind when interpreting the age and period coefficients in Table 2.

17. Concretely, if the expected age-period-specific suicide rate were 10, the natural log would be 2.303. If the percent NB associated with this age-period-specific rate changed from 3 to 4%, then the expected value of age-period-specific suicide rate would change to 2.535 (= 2.303 + .232). In terms of antilogs, this is an increase from 10 to 12.61 or a percentage change of 26.1% [= 100% ($e^{.232}$ — 1)]. If we used the age-period-specific rate of 15 to begin this example, we would find that a change of .232 in the natural log of 15 would result in a 26.1% increase in the antilogs.

18. We employed the percentage of those 15 to 64 when the cohort was 15 to 19 as our measure of relative cohort size. This is consistent with Easterlin's (1987) emphasis on the size of the cohort when it enters the job market relative to the size of the preceding cohorts (including the parent generation). To help ensure that our results on this measure were valid, we also examined results with two other operationalizations of relative cohort size: one designed to measure the relative size of the birth cohort during childhood and early infancy (a measure based on the size of a birth cohort when it was 0 to 4 relative to those 0–49) and another designed to measure the size of the birth cohort relative to others at points throughout the life cycle (a measure based on the size of a cohort at a given age relative to all others in the population at that time, for example, the number of people 20–24 relative to the total population or the number of people 25–29 relative to the total population). We calculated total and race-specific measures of RCS using these definitions. Results of analyses using these alternative measures are very similar to those reported in Tables 2 and 3. All the coefficients associated with RCS and percent NB are positive and only those for RCS in some analyses for nonwhites fail to reach the .05 level of statistical significance. These results are available upon request.

19. We used the standard $t$-test for the difference between regression coefficients from independent samples that treats the estimates of the standard errors as separate estimates rather than pooling them.

20. Since there was only one observation for those 75 to 79, this case was perfectly predicted by our model without an interaction term. With an interaction term the two observations in the 70 to 74 age group were perfectly predicted. The two interactions for age group by RCS for the two oldest groups do not enter the analyses, since they create complete multicollinearity. Thus, across the five analyses, only the interactions for the age groups 60 to 64 and 65 to 69 enter the analysis.

21. Adding the age by RCS interactions to the full models in Table 2 creates extremely high VIFs for both the interactions and the age groups involved in the interactions. Some of these VIF values exceed 1,000. On the other hand, the highest value for any of the RCS measures is 2.64 for the nonwhite RCS measure in the analysis of nonwhite females. For any of the measures of percent NB, the highest VIF is 8.04 for the analysis involving nonwhite males.

22. This more limited data set eliminates 4 of the 9 cells for nonwhite women with missing data on suicide rates, thus making the analysis of suicide rates potentially more reliable for this group than that given in Table 2.

23. One successful explanation of gender- and race-based differences in suicide rates is the status integration tradition (see Gibbs & Martin, 1964), which points to structural variations in statuses and role conflicts across the four race-sex groups.

24. Based on their analysis of the relationship between relative cohort size and age-specific suicide rates from 1948 to 1976 and knowing that more recent cohorts were relatively smaller in size, Ahlburg and Schapiro (1984:102–3) predicted that suicide rates for cohorts born after 1960 would decline. In fact, just the opposite has occurred. Our results suggest that Ahlburg and Schapiro reached this faulty prediction because they did not include a measure of changes in family structure, such as percent NB, in their analysis. While more recent cohorts have been protected, to some extent, by their smaller relative cohort size, their sharp increase in nonmarital births has influenced a sharp increase in age-specific suicide rates.

25. Casualties during these periods were 47,000 during the Vietnam War (from August 1964 through Jan. 1973), 34,000 during the Korean War (from June 1950 through July 1953), 292,000 during World War II (from December 1941 through December 1946), and 53,000 during World War I (U.S. Bureau of the Census 1997:363).

26. Without conducting a formal demographic analysis, one can get a sense of the loss of life by taking the rate of lethal violence for those 20 to 24 and projecting that over a five-year period by multiplying it by five. The same procedure is used for those 25–29, 30–34, and 35–39. For nonwhite males, this procedure suggests that between the ages of 20 and 39 2.26 % of the population of nonwhite males will die from suicide or homicide.

27. One of the most compelling outcomes in the context of the current study is a dramatic alteration in the sex ratio, making the probability of nonmarital births even higher, and thus, potentially, if our model were to hold in future years, increasing the probability of lethal violence for younger cohorts in those areas.

28. "Relative" in this context means that when a cohort that is particularly susceptible to lethal violence is in a given age group, it will have rates that are higher compared to other age groups in that period than that age group usually has compared to those other age groups.

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