Part 1

Epidemiology

INDIAN ADOLESCENT SUICIDE: THE EPIDEMIOLOGIC PICTURE IN NEW MEXICO

PHILIP A. MAY, Ph.D., and NANCY VAN WINKLE, Ph.D.

Introduction

Ever since the earliest contemporary recognition that suicide among some tribes of American Indians was a problem, the high rate among adolescents has been seen as a unique and outstanding feature of Indian suicide. From the first articles appearing on American Indian and Alaska Native suicide problems in both the United States (Curlee, 1972; Dizmang, 1967, 1969; Dizmang, Watson, May, & Bopp, 1974; Frederick, 1973; Levy, 1965; Miller & Shoenfield, 1971; Mindell & Stuart, 1968; Shore, 1972, 1975) and Canada (Trott, Barnes, & Dumoff, 1981; Butler, 1965; Cutler & Morrison, 1971; Jarvis & Boldt, 1982; Kenora Social Planning Council, 1973; Spaulding, 1985–1986; Ross & Davis, 1986; Ward & Fox, 1977), suicide among American Indians and Alaska Natives has consistently been found to be most prevalent among those in the younger age groups. This is particularly true in ages less than 30 years (see May, 1987 and 1990, for a review and bibliographic reference). In more recent studies among both U.S. and Canadian Indians, the trend of adolescent or youth suicide has been found to continue into the 1980s (Garro, 1988; Forbes & Van Der Hyde, 1988; Hislop, Threlfall, Gallagher, & Band, 1987; Jilek-Aall, 1988; Thompson & Walker, 1990).

Another key trait of Indian suicide epidemiology has been the extreme variation of suicide rates from one tribe to the next and from one community to the next. For example, in New Mexico from 1957 to 1979, some tribes recorded no suicides, while others had average rates up to 56.6 per 100,000 population over the entire period (Van Winkle & May, 1986). A third key factor of Indian suicide epidemiology has been the variation of suicide rates overtime. As Thompson and Walker (1990) point out, studies of Indian suicide are too often done in a short time frame and involve numbers so low that trends are difficult to discern. Given these

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extreme tribal and temporal variations (Levy, 1965; Levy & Kunitz, 1987; Shore, 1975; Van Winkle & May, 1986), it is imperative that researchers follow the trends of Indian suicide over time to better understand self-destruction and to examine, in detail, how the trends affect the various age groups. For example, is Indian suicide, and adolescent suicide in particular, continuing to rise in some tribal groups? Is it stabilizing or decreasing, and why? Further, understanding the broader epidemiology over time better enables one to comprehend and pinpoint larger questions of etiology. While new questionnaire-based studies of etiological factors related to youth suicide are appearing in the literature (Lin, 1987; Manson, Beals, Dick, & Duclos, 1989), the need to study and understand the larger and broader trends remains.

One of the most valuable uses of the epidemiological studies on Indian youth suicide has been the planning of prevention and intervention programs (May, 1987; Neligh, 1988). Surveillance of suicide and suicide attempt rates and the delineation of the overall patterns of these behaviors have led to the design of a number of positive programs and responses to problems of self-destruction in many Indian and Native communities. While some initiatives are broadly based public health programs (Claymore, 1988; DeBruyn, Hymbaugh, & Valdez, 1988; Hymbaugh, 1988a; Polk, 1987), others focus more directly on school, small-group, or clinical interventions (Belgarde & LaFromboise, 1988; Berlin, 1987; LaFromboise & Bigfoot, 1988; Main & West, 1987; Neligh, 1988; Thronbrugh & Fox, 1988). Nevertheless, virtually all of these programs have been based on epidemiological knowledge, and in many cases they have resulted from community suicide surveillance and/or a suicide register (Bechtold, 1988; Claymore, 1988; Shore, Bopp, Waller, & Dawes, 1972; Ward, 1984). Recently, Hymbaugh (1988b) has produced guidelines for developing a suicide register for the purpose of setting up prevention programs. These guidelines are being distributed by the Indian Health Service, Office of Mental Health Programs.

Epidemiology, therefore, has been vital to the development and proliferation of prevention programs in Native communities. Continued surveillance in the future also is important. If suicide prevention in particular and mental health programs in general are to continue to serve the needs of a variety of Indian populations, then epidemiological monitoring must continue (see Fox, Manitonabi, & Ward, 1984, and Ward, 1984).

The following are data from 31 years of suicide mortality experience among American Indians in New Mexico. Eight of these years (1980–1987) are being presented for the first time in a published paper. The other years have not been analyzed before in the manner used here, for the explicit focus in this paper is on suicide of individuals under 30 years of age. Previous papers (May, 1987; Van Winkle & May, 1986) can be consulted for further exposure to these data and for the theoretical perspectives underlying this research. The emphasis in this paper is on the

specific suicide trends that may be present in the most recent years, changes from the patterns of earlier years, and more specific analysis of youth age groups from 5 to 9 through 25 to 29 years of age. The detail and the emphasis are on the analysis of various youth age aggregates that have not been explored similarly or in such detail in other literature on Indian suicide.

Methods

The data for this study were obtained from death certificates registered with the New Mexico State Department of Vital Statistics from 1957 through 1987. To be certain that all of the New Mexico American Indian suicides were identified for 1957–1979, the following sources were consulted: (a) summaries of suicides in New Mexico from 1958 through 1974, provided by the New Mexico State Department of Vital Statistics to the Indian Health Service (IHS) Mental Health Program in Albuquerque; (b) computer printouts of suicides from 1969 through 1978 from the Department of Vital Statistics; and (c) a mortality tape of all deaths in New Mexico from 1957 through 1979 generated by Vital Statistics but obtainable through the New Mexico Tumor Registry. Using these three sources of identification ensured the most complete list of cases. Data for 1980–1987 were obtained directly from Vital Statistics. Death certificates were obtained for all of the identified American Indian suicides. As a check for the 1980–1987 deaths, the files of the New Mexico Office of the Medical Investigator were consulted and cross-referenced. To be as consistent as possible, the suicides included in the following analyses are limited to those Apache, Navajo, and Pueblo Indians who were residents of New Mexico at the time of death and who died in New Mexico.

New Mexico mortality data for causes such as suicide have been quite good over the past decade, because the state is organized into one centralized medical investigator system. All suspicious deaths (over 35% of all deaths) are investigated by an agent of the Office of the Medical Investigator. For example, in the 1957–1979 data, 71% of the death certificates of suicides of decedents less than 30 years of age were signed by local physicians and 2% by medical investigators. In the 1980s data, 22% were signed by physicians and 66% by an agent of the Office of the Medical Investigator, indicating some extra degree of investigation into the cause of death. In both time periods, 23% of youth suicides were autopsied, indicating consistency in the investigation of suicide over time.

Population estimates used to calculate rates for all tribes and reservations in the study came from the IHS. During the period 1957–1979, the population estimates increased from 2,400 to 3,705 for the Apache, 32,309 to 60,841 for the Navajo, and 19,550 to 27,824 for the Pueblo. In 1987 the populations of the three cultural groups were 4,697 (Apache), 75,644 (Navajo), and 30,982 (Pueblo). These figures are based on New

Mexico resident population on and adjacent to the reservation (service population) rather than enrolled tribal population. They are estimates calculated from the U.S.Census, which has been suspected of undercounting the actual Indian population.

Therefore, the rates in this study may be slightly exaggerated due to the low population estimates, but the margin of error for the data prior to 1980 would probably be no greater than 7–10% for most tribes (Passel, 1976). For the data after 1980, the rate of error is lower. In a typical western state such as New Mexico, with a high number of resident Indians, the error is likely to be as low as 0.4 to 3.2% (Passel & Berman, l987). The IHS data were used because they represent the most consistent, detailed, and complete estimates of local resident population available. This consistency allows the most accurate measurement of trends over time and the most accurate intertribal comparisons because the direction of error should be similar for all tribes. In addition, rates may be slightly exaggerated due to the use of a resident population base while including suicides by New Mexico Indians who may have lived outside of the IHS-defined service area for the particular tribe.Instances where this occurred, however, were quite infrequent and are corrected to a great extent by including only suicides by state residents that occurred in-state.

Since tribal affiliation was not recorded on New Mexico death certificates prior to 1982, in earlier years it was determined from the information on birthplace, burial place, surname, and place of residence at the time of death. In the rare cases when these indicators showed little agreement, outside referees from appropriate tribes were consulted to make a positive identification of the particular individual's tribal affiliation. The same methods also were used in the rare cases where tribal affiliation was not recorded on the death certificates in 1982 through 1987.

Results

In Table 1–1, the 3-year average suicide rates for all New Mexico Indians younger than 30 years of age are presented for a 31-year time period. Few studies, if any, have aggregated the data in discrete 5-year age groups such as these. From these tabulations, the reader is better able to examine the specific age group patterns that differentiate the various teenage patterns and also those of the early and late 20s.

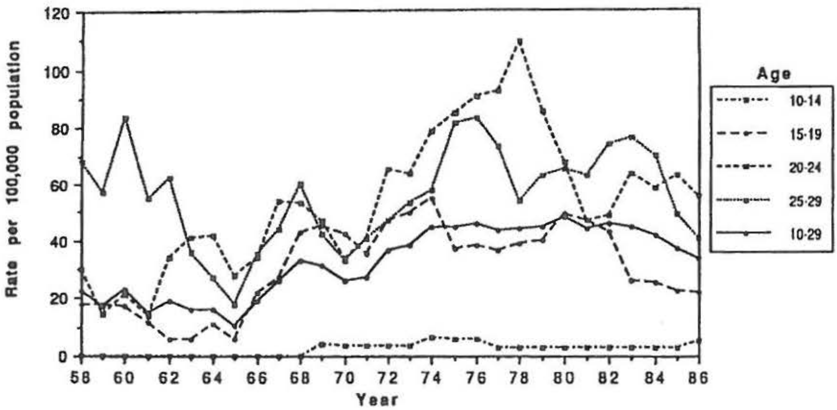
No suicides were recorded among Indians younger than 10 prior to 1983, when there was one hanging death of a Navajo in western New Mexico. Among the 10- to 14-year-olds, suicide began to surface as a problem in 1969. It peaked in 1974, then dropped and stayed low until 1986. Three-year average rates among 15- to 19-year-olds began to rise in the 1960s, stayed high (35–55 per 100,000) from 1968 to 1982, and began to drop with 1983 averages. The very highest rates are currently found among the 20- to 24-year-old age group, and this has been the

Table 1–1
Three-Year Average Age-Specific Suicide Rates (per 100,000) for New Mexico Indian Youth (<30 years of age), 1958–1986

Mid-Point Year	5–9	10–14	15–19	20–24	25–29	10–29 Combined
1958	—	—	18.17	29.91	67.86	22.55
1959	—	—	17.86	14.70	57.19	17.42
1960	—	—	17.46	21.57	83.88	23.22
1961	—	—	11.35	14.02	54.53	15.10
1962	—	—	5.54	34.19	62.05	19.14
1963	—	—	5.56	41.19	35.60	16.26
1964	—	—	11.19	41.46	27.01	16.37
1965	—	—	5.63	27.82	18.04	10.49
1966	—	—	22.15	34.19	35.46	19.14
1967	—	—	27.22	53.79	43.58	26.06
1968	—	—	42.83	52.90	60.00	32.76
1969	—	3.90	45.24	46.90	42.31	31.00
1970	—	3.74	42.19	33.65	33.10	26.24
1971	—	3.57	34.95	40.42	40.31	26.97
1972	—	3.45	46.52	65.20	46.82	36.54
1973	—	3.36	49.40	63.47	53.18	38.11
1974	—	6.37	54.67	78.27	57.65	44.59
1975	—	6.00	36.74	84.97	81.36	44.22
1976	—	5.64	37.99	90.52	82.85	45.83
1977	—	2.71	36.51	92.11	73.49	43.02
1978	—	2.62	38.57	109.02	53.38	43.65
1979	—	2.73	39.58	85.50	62.75	44.71
1980	—	2.86	49.18	67.54	65.37	47.70
1981	—	3.00	46.84	46.71	63.17	44.05
1982*	2.87	2.92	42.93	48.75	73.83	45.65
1983	2.79	2.85	26.13	63.29	75.89	44.45
1984	2.72	2.77	25.44	58.54	70.00	41.54
1985	—	2.70	22.29	62.99	49.21	37.07
1986	—	5.25	21.70	55.47	40.53	33.62

* Represents only one death in 1983.

Figure 1–1
Three-Year Average Suicide Rates for All 3 Native American Cultural Groups Combined 1958–1986



case fairly consistently since the late 1960s. The 25- to 29-year-old age group had the highest rates prior to 1962 but generally had lower rates than the 20- to 24-year-old group from 1963 to 1980 and again in 1985 and 1986.

Therefore, in summary of Table 1–1, the different age groups show variable suicide patterns and rates over the years; this is illustrated in Figure 1–1. The highest rates generally occur in the ages from 20 to 29 rather than in the younger age groups. The rates do not indicate a pattern of a cohort of suicidal people moving through life but instead suggest that differing social and cultural conditions and variables are at play in each age category. An age cohort that is consistently self-destructive does not seem to exist.

Cultural variation in suicide rates has been a hallmark of many studies, and Table 1–2 and Figures 1–2 and 1–3 present cultural-specific data for New Mexico's three main Indian cultural groups. The age-specific rates again indicate variation between the different age groupings in both the early years (1957–1979) and the later period. In the early period, Apaches had the highest age-adjusted rates of youth suicide and extremely high rates in the 15–19 age category. The Apache rate in this category was 29 times higher than the comparable U.S. rate, 12 times higher than the Navajo rate and 3 times higher than the Pueblo rate. In the 20-year-old age groupings, the Apache and Pueblo rates are rather comparable, and the Navajo rates remain consistently lower (one third to one half as high). Figure 1–2 illustrates these patterns.

In the later years studied (1980–1987), the overall age-adjusted rate for Indian youths has remained similar for each of the tribes (see

Table 1–2
Age-Specific and Age-Adjusted* Suicide Rates (per 100,000) for New Mexico Indian Youth by Tribe with U.S. Comparison, 1957–1987

	1957–1979						1980–1987					
Age Group	Apache	Navajo	Pueblo	All 3 Combined	U.S. 1968	Ratio Indian to U.S.	Apache	Navajo	Pueblo	All 3 Combined	U.S. 1984	Ratio Indian to U.S.
5–9	0.0	0.0	0.0	0.0	0.0	—	0.0	1.52	0.0	1.03	0.0	—
10–14	10.17	0.72	4.29	2.29	0.6	3.82	0.0	4.65	0.0	3.15	1.3	2.42
15–19	149.53	12.37	45.56	29.16	5.1	5.72	45.84	11.38	75.86	30.88	9.0	3.43
20–24	96.06	39.49	97.26	60.53	9.6	6.31	138.81	39.64	70.99	52.59	15.6	3.37
25–29	114.97	32.60	84.07	52.78	11.4	4.63	175.19	45.68	79.06	60.47	15.7	3.85
Age-Adjusted* (Ages 5–29)												
	33.06	7.45	20.30	12.71	2.34**	5.43	31.27	8.94	20.12	12.98	3.66**	3.55

* Adjusted to the 1940 U.S. population.

** U.S. rates are for midyears 1968 and 1984, respectively.

Figure 1–2
Age-Specific Suicide Rates for Native American Cultural Groups and U.S. for 1957–1979

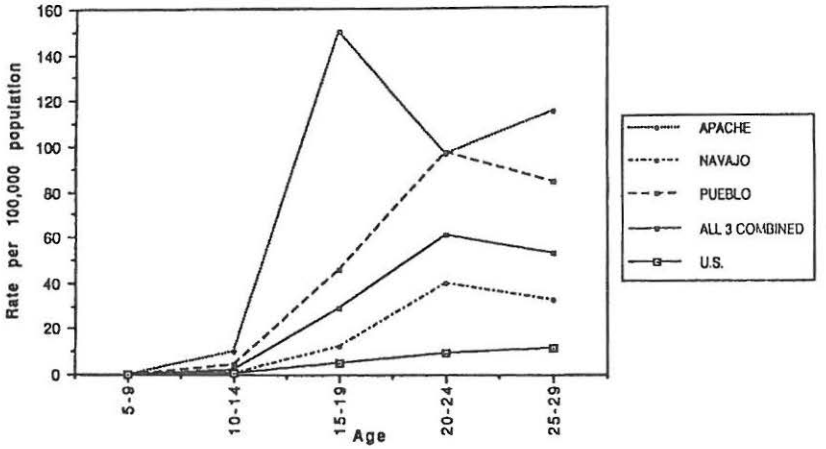
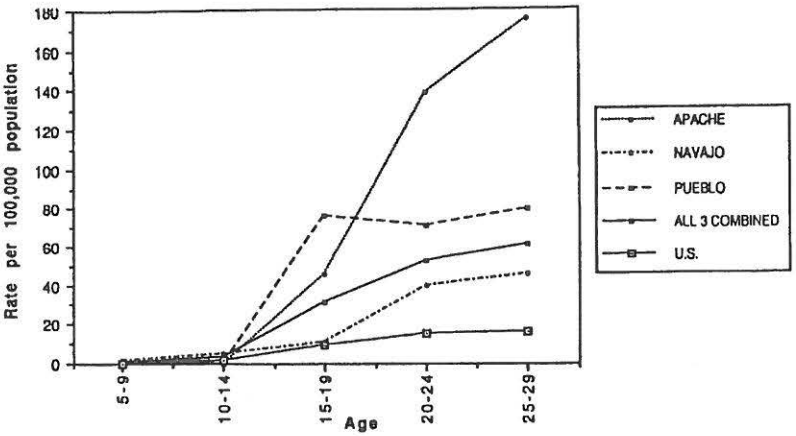


Table 1–2). Within each tribe, however, there is a tendency for the problem to be dispersed more evenly into the various age groups. In other words, the rates are more equal between the ages 15 and 19, 20 and 24, and 25 and 29 within each cultural group, the major exception being the Apaches. The highest rates are currently found in the older youths for the Apache and the Navajo; the Pueblo youth suicide rates are virtually identical in each of the three youth age categories (see Figure 1–3).

The ratio of the age-specific rates for the U.S. and New Mexico Indians has dropped from the early period to the most recent period. In the early years, the ratios of age-specific suicides for U.S. all races and New Mexico Indians was 3.8 to 6.3 higher than U.S. rates; the ratio now is 2.4 to 3.9.Furthermore, the ratio of the age-adjusted youth rate has gone from 5.4 to 3.6. This reflects the rise in youth suicide in the general population of the United States more than any drop in suicide among New Mexico Indian youth.

Tables 1–1 and 1–2 and Figures 1–1, 1–2, and 1–3 raise several interesting points. Youth suicide over the years has been characterized by a number of high and low cycles, especially among particular age groups (Figure 1–1). But examining the 10–29 years of age trend line in Figure 1–1, it appears that the current high rates are more chronic or endemic (with only a small downward trend in recent years). In addition, among some cultural groups such as the Pueblo, youth suicide may be losing some of its epidemic or contagious nature in the youngest age categories, only to become dispersed and diffused across the other age groups (see Figure 1–2 versus Figure 1–3).

Figure 1–3
Age-Specific Suicide Rates for Native American Cultural Groups an U.S. for 1980–1987



Another hallmark of New Mexico Indian suicide, although not shared by all Indian tribes or cultures, has been the use of very certain or lethal methods for committing suicide. Firearms and hanging have been the predominant methods, exceeding their prevalence of use in the general U.S. population. Table 1–3 presents the methods used by New Mexico Indian youth over the past 31 years. There has been some change between the early and later years. Firearms are by far the most common method in both periods, but the firearms total dropped from 71.3% in 1957–1979 to 51.6% in 1980–1987. This 19.7% drop was made up by a 19.7% gain in hanging suicides, therefore leaving the bulk of suicides (92.6%) in these two methods. The remaining suicides have registered little change. Somewhat different from Indians in other states or tribes and the general U.S. data, New Mexico Indians have a very low percentage of overdose deaths.[1] Part of this is due to the fact that most New Mexico Indian youth suicides are male, but even among young New Mexico Indian females, 67% of all suicides ($N = 31$) in 1957–1987 have been by hanging and firearms.

There is not a substantial age differentiation in choice of method. In other words, each age grouping picks its methods in similar proportions. Firearms and hanging account for approximately 90% in each category.

Table 1–4 presents a variety of social and demographic variables for Indian youth suicide victims. Throughout the 31 years of the study, suicide of Indian youths has been predominantly a male phenomenon, with approximately 90% of all suicides being male.

Table 1–3
Method of Suicide for New Mexico Indian Youth by Age Group and Percent, 1957–1987

	1957–1979					1980–1987					Overall U.S.	
Method	<15	15–19	20–24	25-29	Total <29	<15	15–19	20–24	25.29	Total <29	Difference	<30 1984
Overdose/ Poisoning	—	7.7	1.4	2.0	3.4	25.0	6.3	2.2	7.3	5.7	+ 2.3	7.7
Hanging	40.0	19.2	17.1	27.5	21.3	75.0	34.4	44.4	39.0	41.0	+19.7	19.7
Firearms	40.0	73.1	75.7	66.7	71.3	—	59.4	51.1	51.2	51.6	–19.7	58.0
Cutting	—	—	1.4	—	0.6	—	—	2.2	—	0.8	+ 0.2	(see other)
Other (incl. drowning, gas and jumping)	20.0	—	4.3	3.9	3.4	—	—	—	2.4	0.8	–2.6	14.7
TOTAL	100.0	100.0	99.9	100.1	100.0	100.0	100.1	99.9	99.9	100.0	—	100.1
(N)	5	52	70	51	178	4	32	45	41	122	--	8646

In recent years it has become even more male. In 1980–1987 there were 113 male suicides and 9 female suicides under the age of 30. The male/female ratio has gone from 7.5:1 in 1957–1979 to 12.5:1, and this ratio is similar throughout the youthful age groups.

Table 1–4
Various Social and Demographic Indicators for New Mexico Indian Youth Suicides (<30 years of age), 1957–1987

Percent Male	Age Group	1957–1979 (N = 178)	1980–1987 (N = 122)	Difference
	5–9	—	100.0%	—
	10–14	60.0%	66.7%	+ 6.7
	15–19	82.7%	87.5%	+ 4.8
	20–24	90.0%	95.6%	+ 5.6
	25–29	94.1%	95.1%	+ 1.0
Total	5–29	88.2%	92.6%	+ 4.4
	Male/Female Ratio		1957–1979	= 7.5:1
			1980–1987	= 12.5:1
Marital Status	Age Group			
1957–1979	10–19	98.2% single, 1.8% married		
	20–29	56.8% single, 33.9% married		
		9.3% divorced, separated or widowed		
1980–1987	10–19	100.0% single		
	20–29	62.8% single, 31.4% married		
		5.8% divorced, separated or widowed		
Veteran Status				
1957–1979		16.2% were veterans		
1980–1987		5.8% were veterans		
Occupation/Employment				
1957–1979		Occupational status (20–29 yrs.):		
	Unclassifiable		31.7%	
	Laborers/unskilled		34.7%	
	Craftsmen		19.8%	
	Employed (15–29 yrs)		53.4%	
	Unemployed		11.5%	
	Student		31.3%	

Marital status is not a highly differentiating variable in a group so young. Virtually all people who kill themselves (91–94%) are either single or married. There is little time for divorce or widowhood (even in these populations with high rates of accidental death), and separation is probably underestimated by the death certificates.

Table 1–4 (Continued)
Various Social and Demographic Indicators for New Mexico Indian Youth Suicides (<30 years of age), 1957–1987

Occupation/Employment (Continued)			
1980–1987	Occupational status (20–29 yrs.):		
	Unclassifiable		31.7%
	Laborers/unskilled		44.4%
	Craftsmen		18.5%
	Employed (15–29 yrs)		64.6%
	Unemployed		11.5%
	Student		21.2%
Place of Injury			
	1957–1979	1980–87	Difference
Residence	67.2%	78.3%	+11.1%
Rural Area	10.9%	7.8%	–3.1%
Jail	9.5%	6.1%	–3.4%
On-/Off-Reservation Community			
1957–1979:	Death	74.1% died on reservation/Indian community	
	Resident	88.1% living on reservation/Indian community	
	Injury	80.2% injured on reservation/Indian community	
1980–1987:	Death	67.8% died on reservation/Indian community	
	Resident	82.8% living on reservation/Indian community	
	Injury*	81.7% injured on reservation/Indian community	
*21.8–22.4% of all suicides in the 20–29 age group were off reservation.			
Significant Conditions			
1957–1979 (N = 13)			
	Alcoholism/alcohol intoxication		84.6%
	Depression/mental illness		7.6%
1980–1987 (N = 12)			
	Alcoholism/alcohol intoxication		50%
	Depression/mental illness		50%

Similarly, veteran status is low, particularly in the recent study years. Prior to 1980, 16.2% of the youth suicides were veterans, but recently, that figure has dropped to 5.8%. This decrease is probably a reflection of the decrease of military experience in this population, and not specifically in those who commit suicide.

The occupational characteristics of these youth also are not well differentiated. Data for these variables were not as complete (and possibly as valid) as other data in this study. Nevertheless, in both the early and late periods, more than half of the 15- to 29-year-olds were employed. However, the predominant occupation listed for these people was laborer (unskilled) or unclassifiable. Strong economic, educational, and/or occupational needs have been a recurrent theme in Indian youth suicide literature.

Suicides of Indian youths generally occurred in or around a residence (67–78%), in a rural (open) area (8–11%), or in a jail (6–10%). Jail suicides, never as big a problem in New Mexico as they have been in other Indian reservation states, have decreased throughout the 31 years of data. Most suicidal injuries were inflicted on a reservation or in a predominantly Indian residential community (80–82%). Only in the 20–29 age categories were off-reservation suicides slightly more common, accounting for 22% of the events. More than 80% of all people younger than 30 who killed themselves were residing on a reservation at the time of the suicide. However, there has been a downward trend in this variable in recent years (88.1% to 82.8%). Both because of the place where the suicides occurred and the location of major hospitals and clinics, 29% of the suicides died off-reservation. Many were transported off the reservation before dying or being pronounced dead.

A final consideration in Table 1–4 is the listing of "significant conditions" on the death certificate. Plagued by incomplete recording, these data may be of little validity or use. Nevertheless, one might note that the two items most commonly mentioned over the years are alcoholism/alcohol intoxication and depression/mental illness.

The final variables to be considered here are the temporal variables. These may be of use to mental health clinics and practitioners or to other health and law enforcement practitioners who need to anticipate and prevent certain patterns of occurrence.

Table 1–5 presents the month of death of the suicides. A shift is occurring away from spring suicides to late summer and fall.[2] May was the most common month for youthful suicide in the 1957–1979 data, but August is the modal month in the 1980–1987 period. In the 1980–1987 data for the various age groups, the 10–14 age group is most likely to commit suicide in the fall (100%). The 15- to 19-year-olds show the greatest dispersion of occurrence throughout the year, but with the highest incidence in November (15.6%) and March (12.5%). The 20- to 24-year-olds are most likely to kill themselves in June through September (55.6%), while the 25- to 29-year-olds die most frequently in August through December (68.4%)

Day of the month is not too useful a variable, for youth suicides are widely dispersed throughout the month (see Table 1–6). In the recent data, however, days 5–9 and 20–24 account for about half of all deaths.

Table 1–5
Month of Suicide Death for New Mexico Indian Youth (<30 years of age), 1957–1987

Month	1957–1979 N	1957–1979 %	1980–1987 N	1980–1987 %	Difference
January	15	8.4	9	7.4	–1.0
February	15	8.4	6	4.9	–3.5
March	15	8.4	7	5.7	–2.7
April	9	5.1	6	4.9	–0.2
May	23	12.9	8	6.6	–6.3
June	15	8.4	12	9.8	+1.4
July	8	4.5	7	5.7	+1.2
August	11	6.2	18	14.8	+8.6
September	14	7.9	14	11.5	+3.6
October	14	7.9	11	9.0	+1.1
November	20	11.2	10	8.2	–3.0
December	19	10.7	14	11.5	+0.8
Total	178	100.0	122	100.0	

Table 1–6
Day of the Month When New Mexico Indian Youth (<30 years of age) Suicidal Injury Occurred, 1957–1987

Day of the Month	1957–1979 N	1957–1979 %	1980–1987 N	1980–1987 %	Difference
1–4	26	17.8	16	14.4	– 3.4
5–9	16	11.0	23	20.7	+ 9.7
10–14	25	17.1	14	12.6	– 4.5
15–19	17	11.6	16	14.4	– 2.8
20–24	27	18.5	25	22.5	+ 4.0
25–29	22	15.1	11	9.9	– 5.2
30–31	13	8.9	6	5.4	– 3.5
	146	100.0	111	99.9	

Further, the 5th through 9th days have gained in incidence in the most recent years.

Table 1–7
Day of Suicide Injury for New Mexico Indian Youths
(<30 years of age), 1957–1987*

Day of the Week	1957–1979 N	1957–1979 %	1980–1987 N	1980–1987 %	Difference
Monday	19	10.7	14	11.5	+0.8
Tuesday	10	5.6	10	8.2	+2.6
Wednesday	19	10.7	14	11.5	+0.8
Thursday	27	15.2	19	15.6	+0.4
Friday	31	17.4	18	14.8	−2.6
Saturday	38	21.3	21	17.2	−4.1
Sunday	34	19.1	26	21.3	+2.2
	178	100.0	122	100.1	

* Note: Day of injury was only completed in 111 of the 122 (91.0%) suicides in the later period and 146 of 178 (82.0%) in the earlier period. Therefore, day of death was used which would skew the sample slightly as to actual occurrences. In the total suicide samples for years 1957–1979, 71.5% of suicides had the same day of injury and death. In 1980–87, 71.7% had the same. Very few suicides, 13.2% and 18.3%, were different by more than one full day.

Weekend suicides are quite common in all youth age categories; in Table 1–7 this is apparent. Overall, there has been little change over the years. In the 1980–1987 data, about 53% of all suicides occur Friday through Sunday, and 65% occur Friday through Monday. In the different age categories 100%, 78.2%, 57.8%, and 58.6% of the suicides of the 10–14, 15–19, 20–24, and 25–29 age groups, respectively, occur on Friday through Monday. Therefore, the younger age categories are more likely to die on weekends, and the older age categories are more likely to die at various times throughout the week.

In the final temporal variable, hour of injury, there has been only moderate change (see Table 1–8). Most suicides of Indian youth occur in the afternoon, evening, and night (63%). But when broken down to 4-hour intervals, 51.4% now occur between 4 p.m. and midnight. When midnight to 4 a.m. is also added, 64.6% occur from 4 p.m. to 4 a.m.

Discussion

Few studies of Indian suicide have been able to closely examine the social and demographic characteristics of specific age groups of Indian youth in any detail. Limited by short time frames and small num-

Table 1–8
Hour of Suicide Injury for New Mexico Indian Youths (<30 years of age), 1957–1987

Hour (Military)	1957–1979		1980–1987		
	N	%	N	%	Difference
Midnight–399	20	16.3	10	13.2	−3.1
400–799	15	12.2	10	13.2	+1.0
800–1199	10	8.1	8	10.5	+2.4
Noon–1599	21	17.1	9	11.8	−5.3
1600–1999	28	22.8	23	30.3	+7.5
2000–2399	29	23.6	16	21.1	−2.5
	123	100.1	76	100.1	
a.m.	48	37.2	28	36.4	−0.8
p.m.	81	62.8	49	63.6	+0.8
	129	100.0	77	100.0	

bers of cases, most research has relied on case studies and other less statistical approaches (Thompson & Walker, 1990). This paper begins to add to our understanding of youth suicide in particular age categories that heretofore have been hidden. Further, the data set allows us to examine the occurrence for several cultural groups over a long time period. While not too many large or substantial surprises have emerged, some unique insights have been gained.

First, we have been able to examine the trends of Indian youth suicide. The rates of suicide have risen and fallen in the various youth categories over the years. No particular cohort seems to account for the high rates, yet the cohorts of youths appear to be responding to a variety of social and psychological confluences. Reservation-specific trends might shed more light on this, but it is another complete study and paper topic. While the Navajo youth suicide rate has risen over the two periods covered in this paper, the overall rates of youth suicide among the Apache and Pueblo have not. These latter groups have experienced a rise and fall of rates over time and a shifting of occurrences and rates within age groups. Currently, the rates of all Indian cultural groups in New Mexico indicate that they reached a plateau in the late 1970s and/or began a slight decline in 1982–1986.[3] Nevertheless, the rates of all groups remain too high, especially those of the Apache and Pueblo, which are three times the U.S. rate in most age categories less than age 30.

New Mexico Indian youth continue to use extremely lethal methods for committing suicide. In addition, the male/female ratio of suicidal

death has increased to 12.5:1. The social role of Indian males in all tribes in New Mexico must, indeed, be considerably less protective (or more stressful) than that of females, and it does not seem to get better for males over time. An examination of the interaction between traditional and modern techniques of male coping in all three of these cultural groups is needed. Further, this study has again shown that Indian females have substantially lower rates of suicide death than males. Studies are needed that highlight the specifics of the male/female differences in self-destruction and also describe the particular strengths, traits, and roles of Indian females that may protect them.

Most youth suicides continue to be reservation residents who kill themselves in and around their residences. In the 20-year-old age groups, however, more suicides (approximately one fifth) are now occurring off-reservation.

The temporal variables generally have remained constant over the years. Youthful suicides are most common on the weekends, in the first and third weeks of the month, and in the late evening and early morning hours. There is, however, a shift in the month of the year, with fewer suicides in the spring and more in the fall. Taken together, all of these youthful age groups are more likely to commit suicide in the fall, possibly due to factors related to school. It is interesting to note, however, that this fall trend is actually more pronounced in the 20–24 and 25–29 age groups. What broad social pressures/influences are at play in the fall? Is it school or more generalized expectations of achievement? This needs to be examined further. Nevertheless, while 55% of suicidal deaths among Indian youth in 1980–1987 occurred in August through December, the other months should not be ignored, particularly from clinical, social, and psychological perspectives.

Several important questions are raised by this paper, most of which call for further research. The weakest part of this data set (and most death certificate data) is the data called significant conditions. Very little social setting, human interaction, and mental status data are available from death certificates and other demographic sources. Furthermore, the Indian literature is in need of additional studies incorporating human interaction data (May, 1990) to interpret more fully the setting, meaning, and circumstances of the particular suicide cases. More social and psychological autopsies would therefore add greatly to the interpretation of data such as these.

Also, in the significant conditions data, there is a dearth of information on alcohol consumption (chronic or acute) related to the suicide. An accurate and complete study of alcohol involvement, drinking patterns, and specific blood alcohol levels of suicide decedents is essential. Some studies that have estimated and measured alcohol involvement have shown that about 75 to 85% of Indian suicides are alcohol related, with a range of 50 to 100%. But these studies are few, are based to a

great degree on anecdotal information, and are not specific enough as to the exact nature of alcohol involvement (see, for example, Andre & Ghachu, 1978; Berman, 1979; Cutler & Morrison, 1971; Fox et al., 1984; Jarvis & Boldt, 1982; Levy, 1965; Shore et al., 1972; Shuck, Orgel, & Vogel, 1980; Spaulding, 1985–1986; and Trott et al., 1981). A fuller evaluation of alcohol involvement in American Indian suicide is long overdue.

Finally, a study like this raises the issue of suicide contagion, imitation, or clusters; but it is not able to adequately measure contagion without utilizing different techniques of analysis or by breaking the data down to local geographic areas (e.g., reservation, town, county). A number of studies have described clusters as they affect young Indians (Bechtold, 1988; Davis & Hardy, 1986; Long, 1986; Tower, 1989; Ward & Fox, 1977; Watson, 1969), but no statistical study exists to test this assumption for Indians demographically and longitudinally. This paper shows that the rates vary between and within Indian cultural groups over time, rising and falling in the aggregate data. A cluster study could add detail to our knowledge and assist in better understanding the etiology of suicide. Most of those who are experienced in reservation settings are aware that clusters occur, but this pattern needs to be tested using techniques similar to those used on other populations.

In conclusion, some Indian youth of various ages in New Mexico and elsewhere have substantial problems with self-destruction. We must continue to work to obtain more accurate knowledge for solving the puzzle and ultimately to develop better prevention and intervention programs.

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Discussion

Dr. Bromet: I would like to talk for a minute about what epidemiology is and why it should be part of a program like this. I hope what we are talking about today will come up tomorrow, very specifically when we talk about programs. Epidemiology is a science that is designed to understand how something is distributed in the population, what the risk factors are, and, therefore, how we can intervene and reduce the morbidity attached with whatever illness that is under study.

So when epidemiologists do their work, they do it on one of three levels, and this morning you've only heard about one level. I want to make sure that you know about all three. The first level is the descriptive level. The work that Phil May and Nancy Van Winkle presented is a very nice example of descriptive epidemiology. On this level, you work with statistical data and do the best you can with it. Of course, you're limited by the data that you're given. In their case they were trying to do something which on the surface is a very simple task; that is, to establish the suicide prevalence rate in specific age groups living in specific areas with specific tribes while also looking at temporal changes and demographic characteristics that it might relate to.

Now, you think that to do something that straightforward as to establish a rate of suicide would be a really easy task. However, as Phil demonstrated and discussed, this is really difficult. You need two things to establish a rate: a numerator and a denominator. In their case, their numerator was based on death certificates that were ascribed to suicide. Which meant, I think by definition, a probable gross underestimation in the numerator. There is a real problem with single automobile accidental deaths in young Indian youth which involve alcohol. Suicidal adulation might go along with it. If we were in a position to actually investigate all

deaths and all suspicious deaths of youths, and not just those that were called suicide, we would find that our numerator was probably even bigger and even more alarming than the data that you presented.

The other problem that Phil and Nancy described was his denominator. In fact, the tribe with the smallest denominator was really too small to study with any reliability for a low-incident issue like suicide. The rate that fluctuated the most was the highest. I think that probably Phil and Nancy have done a wonderful job with the data. However, it's not clear to me how much more they want to do in terms of descriptive epidemiology, working with this kind of data.

I'd like to suggest that there may be other things that they can do in the future that Phil, I think, even suggested in his own discussion. One is working with the coroner's office very closely —hand in glove, if you will. You could even get deputized so that when these deaths occur, they can collect additional information. This would not necessarily mean gathering very extensive information, as would occur in a psychological autopsy, which can take a great deal of time. At least information on use of alcohol and other drugs and maybe some superficial information on people's mental health histories. Clearly, what we've learned from studies of suicide is that they're not random events. They don't just happen. They happen to people who have very specific vulnerable characteristics. They happen to people who are very troubled and come from troubled families. These are findings that are well known. However, it would be nice to try and incorporate this into descriptive epidemiology. I think we can do that without a huge amount of funds.

My bias, though, as an epidemiologist personally is not to do descriptive epidemiology. I think it is really important. We need to know these things and I am glad there are people who are willing to do it. My own interest is looking at analytic studies. Studies like case control studies where we can begin to isolate what the causal variables are. A lot of that work already has been done in non-Indian populations.

Risk factors for suicide attempts within a Navajo population as described by Grossman et al. are so comparable with any other study (Grossman, Milligan, & Deyo, 1991). When I read his paper, it reminded me of a conference that I attended on the psychological effects of the accident at Chernobyl. The concern was the vast majority of the population that had not received radiation exposure. Not having any health consequences, they were all terrified and ascribing every symptom and every illness to the accident at Chernobyl, including things that could not possibly have anything to do with radiation exposure. Not only was the population convinced that Chernobyl had caused all these health problems, but so, too, were the local physicians.

This was a "déjà vu" experience. We've been doing studies of people living near Three Mile Island. We've been following them over time. Over time people have gotten more and more symptomatic. These

mothers of young children are beginning to do exactly what these women who live in these areas far outside Chernobyl are doing. They are ascribing every health problem that their children or any child that they know of to the Three Mile Island accident. This situation seems to be getting worse instead of better.

If the risk factors for adolescent suicide attempt really are not unique, then maybe what we need to begin to think about is integrating the wider literature on adolescent suicide into our thoughts about what we're going to do about the current problems. From a research point of view, how are we going to design analytic epidemiologic studies that take us a step further? We don't need to reinvent the wheel. We don't need to do exploratory, analytic studies where we again test out whether certain variables are going to be risk factors for adolescent suicide or suicide attempts in Indian populations. I think we can hypothesize that they will be and, therefore, design our studies in such a way that we test focused hypothesis about those variables.

The variables that I am even more concerned about that we haven't talked enough about are the ones that probably are unique to the experiences that American Indian populations are having. Issues like assimilation, intermarriage, employment, and keeping kids in school. I would like to suggest that when we start talking about school-based interventions that we begin long before kids enter school. We're talking about family pathology that begins before kids ever arrive on the scene.

I wanted to make one other comment about analytic epidemiology. We've talked about case control studies, but I don't really know what that means. There can be all sorts of controls. One of the most interesting studies that I know about used a control group of kids who were hospitalized for suicide attempts that were so severe that they should have died. It was just a miracle that these kids were found in time and saved. The other control group were kids who made attempts or were severely depressed but that weren't as severe. It turned out that some of the similarities between the kids who had killed themselves and the kids who should have died were quite remarkable. It suggested that we could learn a lot by doing studies that are focused on these unique kids who really did try to kill themselves in a lethal way and somehow managed to survive.

I'd like to also suggest that a school-based study that was suggested by Dr. Grossman could be an opportunity for a two-stage study. Epidemiologists love to do two-stage studies where the first thing they do is a massive screening. This is done since we're often looking at things that have low prevalence. In this case, we find the prevalence was 15%. We can pick, then, people whom we want to go reinterview for more detailed information. What were these attempts all about? What did people actually do? Did they do it on a number of occasions? Did they do it the same way? Did their attempts get more and more lethal? When did they say that they were having problems with their mental health? Specifically,

what are they talking about? How long have these problems gone on? What kind of coping mechanisms do they say they use that helped them or didn't help them?

Dr. Shaffer is absolutely right when he says anytime you rely on an analysis of a single item, you do have problems with reliability. What you would want to do is a two-stage study in a group twice the size of the group who said they did make an attempt. Then use a group that is maybe twice as big that said they didn't. We can really begin to understand the groups who did and didn't in major analytic ways.

My hope is that all the things we're talking about this morning are going to be relevant tomorrow. If they're not, then we've failed in some way. Epidemiology really is a field that should be going on simultaneously at three levels: the descriptive level, where we understand the rates and how they change over time; the analytic level, where we begin to understand the things that cause it; and the intervention level. If we think we know what the roots are, then we should be designing interventions that tap those roots. Ultimately, we should see rates going down because of it. It really takes a careful, experimental study to prove whether those things we felt were risk factors really were risk factors.

Dr. Van Winkle: One of the things that we are trying to do is to work more closely with the Office of Medical Investigator. You're absolutely right. You are real limited in the reliability of a number of things on the death certificates. I think we'll find out a lot more and a have a much better understanding when we can use the medical investigation records and coordinate those with the death certificates.

Dr. Bromet: My colleague in Pittsburgh, a psychiatrist, became deputized. His relationship with the coroner was close. They are now publishing their papers together. I think the coroner, being a traditional medical model type, learned a lot from my colleague. They both learned tremendously from their relationship.

Dr. Shaffer: This morning, as people were talking about secular changes, it occurred to me that most of the analyses were presented without differentiation of sex. An epidemiologist always likes to find different rates in different populations. That gives you some means with which to try and pry apart what might be important risk factors. One of the main goals in the New York study is to try and explore some of what Evelyn called descriptive epidemiology a bit further in a case control design. I thought the data that we have recently analyzed might be relevant to the work that you are doing now. If you look at changes since 1968 in 15- to 19-year-olds, what is very striking is that the majority are white males. There has been a smaller increase among nonwhite males followed by white and nonwhite females.

The finding is that the secular increase is confined to one sex. This is very exciting. When we embarked upon our descriptive and more

analytic study, we looked for risk factors that might be confined to a single sex. We took 179 consecutive adolescent suicides that occurred in the New York tri-state area over a 2-year period. We had normal controls and attempted suicide controls. They all involved extensive interviewing with survivors. One of the early findings was that if you looked into the history of substance abuse in the male completers, it was just under 40%. This compared to about 5% of the normal controlled males. On the other hand, the rate of substance abuse or alcohol use in the females was no different from the controls. We need to find an element that had been subject to change over this time period, affecting one's sex selectively. That could possibly explain the mystery.

Substance abuse, mainly alcohol use and a little bit of cocaine use, did not seem to be a factor in the young suicides. We found virtually no alcohol or substance use under the age of 15. However, one third of all adolescent suicides are actually 18- and 19-year-olds; two thirds of these had a history of associated substance abuse. The patterns of substance abuse among the suicides were different from the normal controls. They were more likely to be complex patterns of co-morbidity of conduct disorders with substance abuse, disruptive disorder, and/or depression or the three combined, accounting for the majority. Substance abuse in normal controls was more likely to be uncomplicated by another diagnosis.

We were very interested in trying to pry apart some of the competing models for co-morbidity. The very fashionable one says that depression comes first, then you self-medicate, and that gets you into trouble. Most of the data would suggest the reverse. You start off at a very early age with conduct disorder, which predisposes you to substance abuse, which then gets you into a lot of trouble with mood. Interruptions in intake, as you get in binge drinking, can induce dysphoric responses. What we tried to do was to look at the co-morbid cases and see which single diagnosis these resembled. The co-morbid cases were 100% males. The depression only cases were 40% male, substance abuse showed 93% were male, and disruptive behavior were 100% male. That is, the co-morbid group looked very much like the other disruptive conduct disorders and the substance abuses.

Also, we were able to get at onset of first symptom. Among the 11 co-morbid cases where we could date onset, 10 cases either the substance abuse or the disruptive behavior came on first. In only one the depression came first. Our conclusion from this is that it looks most likely that an increased substance intake is the reason for the male-specific secular increase. The traditional pathways probably do apply rather than the secondary medications pathways. Given the high prevalence of this risk factor in the American Indian/Alaska Native population, it may be well worth focusing on this perhaps in an even more detailed way than we were able to do. We probably neglected to do it as thoroughly as we should have.

I also would like to disagree with Evelyn that it is not okay to go ahead and do prevention projects without information. I think there are well-documented prevention projects that have a paradoxical effect. They have been more harmful than beneficial.

Dr. Van Winkle: I would like to make one comment on the data that we've looked at overall for 1980–1987, which is the most recent data that we have. We have looked at male and female age-specific rates. They are very different for males and females. The females are much lower. All the female suicides are for women under 44. For men, it extended out in this period. It went down to 9 years for one young boy and went up to 96 years for an elderly gentleman who walked in front of a train. So what we see are much higher rates for the males for all the different age groups. They peak very high into the 1990s per 100,000 for the 25- to 34-year age groups, and then they drop down substantially after that. But there are suicides for older men and none that we've seen for women.

Dr. Shaffer: Nationwide, the rate for over age 65 is between 40 and 60 per 100,000. The 85-plus tiny age group death-by-suicide rate is 80 per 100,000, so it is way above the national average.

Dr. Van Winkle: What we find is that as you get older in our age categories, the U.S. rates are much higher than the Indian rates. Also, if you figure age-specific rates for the small numbers of elderly Indian suicides, you have this outrageous rate of 60 per 100,000 and it's one suicide. So it is really not representative of what is actually happening in the population.

Dr. Bromet: I think the double issue is alcohol and firearms. If you look at David Brent's data in Pittsburgh showing blood alcohol content levels have been going up steadily in adolescent suicides with the availability of firearms. Loaded guns in people's houses is just phenomenal. These kids can kill themselves.

Dr. May: It doesn't explain it all, because in New Mexico everybody has access to firearms. There is still a tremendously differential rate. The Indian rate is higher. Substance abuse prevalence, particularly for those under 20 in New Mexico Indians of Pueblo and Navajo descent, are actually lower than Hispanics and Anglos. So there is a cultural factor in addition. Those two things are very important — alcohol and firearms — but the cultural factor still is there.

Dr. Neligh: I have heard talk about alcoholism and substance abuse as a generic condition. Joe Bloom reported in working with Alaska Native homicides that alcohol was predominately an agonal event. In other words, it was found on autopsy, but it was a unique precursor to the homicidal event as opposed to being a long-standing condition. What are your thoughts about this? Is it a long-standing condition, or is it just something people do just before they try to kill themselves?

Dr. May: In our studies, 61 to 74% of our suicides from 1980–1987 are alcohol related, as verified by autopsy with BAC, vitreous or muscle samples. The suicides who are intoxicated are intoxicated at outrageously high levels. The average is going to be about .20. The other 25 to 30% are stone sober. There is both a mix of chronic alcoholism, and in some cases the people are coming off a 2- or 3-month binge, but in other cases alcohol is just a very sporadic, acute influence. That's why it is very important that we look at this stuff really carefully.

Dr. Shaffer: In the New York study, we actually had higher reported rates of alcohol use than found in positive toxicology. That was against our expectations, because we thought that the history would be underreported and the toxins would give you the true role. That is compatible with acute dysphoria or mood changes being induced by withdrawal. I was just wondering when you said "coming off of a binge," how many of the kids were in fact dysphoric as a function of forced withdrawal.

Dr. May: I do not know.

Dr. Shore: Addressing Evelyn's comment, it has become clear to me how descriptive and nonanalytic the work in Indian adolescent suicide has been. I think as we move toward the third day of this conference, this group should be charged with developing recommendations. There is a lot of data out there. Do we need to spend another 5 years rediscovering some of those high-risk factors that we know apply equally to American Indian adolescents? If we don't, then that is going to change the wording of our research priorities.

Twenty years ago, after our first study involving a community of 3,000, we returned to the tribal council. We said to them and the health committee, we had the ability in this study to identify through risk factors analysis 20% of your tribe. Certain extended families of the tribe accounted for 80% of the suicides, homicides, and deaths of alcohol cirrhosis. That identified about 600 tribal members. We noted that half of those 600 are kids, which further reduces 3,000 to 300 kids. We proposed that they consider a very specific intervention program for these 300 kids. Half of the group is adolescent, so you now go from 3,000 to 150. Their response was basically, "Are you kidding? We can't politically give anything special to those families. They're the worst families on the reservations." They did not at that point have the political ability to narrow the available resources to target a high-risk group.

From Phil's hypothesis in his follow-up article, they have developed that capacity, or for other reasons the rates have gone down. In my experience there are at least three major hurdles and probably more that we need to consider in taking the facts that we have to communities with specific intervention efforts that are analytically evaluated. We do not need to do this nationally. We've got to do it focused in a few places where we study carefully. That if you do what we suggested, do you

label? Do you stigmatize? How can you minimize the negative effect of either labeling the tribe, labeling the risk families of the tribe, or the high-risk adolescents within the high-risk families?

Another important factor is this very interesting dichotomy that comes out of this morning's discussion that you labeled the descriptive versus the analytic. That's a problem because all of our data on American Indian adolescent suicide is descriptive. Can we then really take the next step? Can we move forward more quickly by using the analytic data from the majority population identifying some of the major risk factors important for Indian adolescents?

Dr. Forbes: I am curious about whether the drinking patterns among the Indians in New Mexico were recognized as being different patterns than the alcohol abuse among the whites. For a long time, for political reasons, this was not recognized. It is pretty widely recognized in Alaska that Alaska Natives use alcohol differently than whites do even though they encounter the same labels and are frequently subjected to the same programs. They are more episodic for one thing, and they are more tied to community events. They are more tied to the need for companionship, and they're also much more public in their lives.

Dr. Shaffer: What about the Type 1, Type 2 distinction? I imagine the males are associated with antisocial behavior and early onset.

Dr. Forbes: I really don't know. In New Mexico do you find that same sort of thing?

Dr. May: In New Mexico we all have drinking problems. As the Director of the Center on Alcohol, Substance Abuse and Addictions, it is more and more obvious to me every day. The differentiators are mainly quantity and frequency, as you are indicating. However, the politically sensitive issue to bring up given our epidemiology is that Hispanic and Indian patterns are almost identical. However, you can't say that in our state. Give you an example: If you look at the BAC of fatal accident victims in our state, Anglos are dying at .12, Hispanics are dying at .18, and Indians are dying at .19. In a number of other types of mortality that we are starting to examine, we are seeing the same, exact pattern.

Yes, the Indian patterns are unique in some sense, but unfortunately, not that unique. There are really two very different patterns in the Indian populations. There is the recreational drinker, which is more your Type 1 drinker. That is the younger, very sporadic, heavy quantity, and binge drinker. The more chronic, anxiety alcoholic is in the minority. These people are ostracized, alienated, and not part of the mainstream Indian society at all. It's important to differentiate those when you come up with prevention programs, because 75 to 80% of all the problems of alcohol-related mortality and morbidity are caused by the recreational drinkers; whereas all the alcohol programs focus on the older, anxiety drinkers.

Eighty percent of the problem is with recreational drinkers, while 20% with the anxiety drinkers. The funds are spent in an inverse manner; 80% are spent for the small problem and 20% for the larger.

Dr. Schoettle: While serving with the Public Health Service in Alaska, I observed two different types of drinking. The town was basically Athapaskan, and about 10% Eskimo population; the other percent, which is minimal, is Caucasian. The adolescents that I saw there drank when it was available and almost exactly like the Caucasian adolescent, college-age adolescent, and young adult drinkers. They'll drink whatever is available. Whereas the chronic alcohol abusers try to make the alcohol available throughout the year. If they were firefighting, we would see a lot of increased violence, increased money, and increased drugs. When that money ran out, there would be slight resumption of some increased stability.

So it really depended on not just the political factors and the sociocultural factors but literally the financial-availability factors. They had a two-bed log cabin jail, which was the drunk tank. The chronics were almost never jailed, while the acute adolescents were jailed. If the chronic people would come in and out of the jail, people would question what were they doing in the jail. The basic focus of the response was to the acute situation.

Dr. Guilmet: I'm troubled by the fact that alcoholism is the etiological factor here. I don't want to disregard that completely, but it seems to me that this assumption is almost always our first hypothesis. We saw, since the 1960s, a huge increase in the suicide population, yet alcohol has always been available. There must be ways in this kind of methodology to integrate what sociology calls the structural-functional perspective. This looks at differences in perceptions and expectations and the individual's perceived ability to achieve those goals or achieve those ends that they're defining. Then you consider, for example, the role of media in terms of changing these perceptions. During the civil rights movement, change in expectations was necessary for individuals to mainstream. Is it possible to talk about these kinds of variables within this methodology?

Dr. May: I think you're exactly right. Alcohol is a necessary condition in a lot of these suicides, but it is not a sufficient condition. Some of my colleagues from Indian Health Service, Lemyra DeBruyn, Carol Lujan, and I did a case control study of child abuse and neglect in northern New Mexico. When we looked at abused kids and their families, we found with neglect it was 97% alcohol-related incidents. With abuse, it was 70% alcohol related. Deciding to pick a control group, we employed a mast stratified control group. We found that supposedly nonproblem families had a few cases of abuse and neglect, but they had a 55 to 60% alcoholism or alcohol abuse problem. Basically, all we could conclude from that was that alcohol was necessary in a lot of these cases but not sufficient

for abuse. Here these 55 to 60% of the families who had similar alcohol abuse problems but had no abuse or neglect. I think the same thing applies to suicide and a number of these other behaviors. What are these other factors — social, cultural, psychological, whatever — that explain another part of this? Alcohol is one part of it and one part only.

Dr. Manson: Back to Evelyn's earlier comments about what is it that is uniquely cultural about this phenomenon among Indian and Native people. George, the arena that you called our attention to holds considerable promise for understanding the unique social and cultural contributions. If Jerry Levy were here today, he would say in his fashion, "Well, it's obvious." He would cite a list of unique cultural factors. I'll never forget one that he described that was not necessarily specific to Hopi, but it was in terms of its manifestation. Those of us who are anthropologists understand this intimately. It has to do with issues of marriage rules, specifically endogamy and exogamy, the rules that govern whom an individual is expected, indeed almost required, to marry. If those rules are broken and the individual doesn't marry a person from that class or category of persons and marries someone else from another category, not necessarily outside of the tribe, then the offsprings of those marriages clearly have the greatest risk. They have the history of most frequent attempts and actual completions of suicide among the Hopi. This suggests in a very pointed fashion a way in which a very unique cultural dynamic can contribute to that increased risk.

I think that there are other examples that we can draw. They underscore, George, your point about the structural-functional arena in terms of what additional risk factors and unique ones we need to consider. We've been talking from my point of view at a macro level. I would like to take this for a moment, to the microanalytic level. David, your comments about the adaptability or the nonadaptability of some of the measures in the adolescent health survey served to remind me that issues of measurement always are part of a necessary task that we have to undertake. This happens especially as we move from a descriptive to an analytic level in our epidemiology. How we operationalize such constructs as family cohesion, alienation, etc., etc., must be informed by the particular cultural context in which we're working. This may vary by tribe or may not, but it seems to me that it is a necessary part of our task.

All too often, in my experience, that when moved to that level the dialogue gets very polarized. We have one camp of individuals who say all of these measures can be used with impunity because the nature of the experiences we're talking about are universally human. On the other side of the camp, we have to go in and develop something unique and specific to every group that we work with. I guess in most things the answer lies somewhere in between those two extremes. So it seems important to me to be careful as we move from descriptive to analytic types of research enterprises. We must do our basic homework about the

properties of the measures that we're using. We must pay attention to and make sure that we are constructively critical in the way in which we operationalize these efforts.

Another area of concern I have probably springs from my background as an anthropologist. Rubbing shoulders with epidemiologists like Evelyn, Phil, and others, I am excited by the importance of having greater numbers of records and data available to us that will allow us to at least think on more generalizable terms to Indian and Native populations. On the other hand, I think it is important that we not lose sight of the value of small studies, especially if they're longitudinal and continuous in design. Studies that we have going on, for example, as far as I know, unfortunately are unique. Just taking one school and looking at it over a 5- to 10-year period, we can examine the cohorts of adolescents coming into that school. Then we can begin to tease out through multiple measurement periods, from cohort effects, from maturational effects, from environmental effects of what impacts these kinds of school contexts have on the students, the impacts of peer association and peer relationships, and what kinds of impacts the process of being socialized into that milieu path. If we don't attend to these contexts in addition to the broader, more epidemiologically, demographically driven studies, it would be an imbalance. That is not to say that epidemiology doesn't do the latter. It can and has a lot to suggest about how to proceed. In fact, such inquiry probably represents that analytic level of epidemiology we're suggesting.

Dr. Bromet: I think some of epidemiology is pretty superficial. At the same time some of the smaller studies tend to be on unrepresentative samples of people, and so generalizing from them is hazardous. There is no reason why it cannot be done simultaneously.

Dr. Shore: Reviewing the mental health literature, there is only one published case study in Indian adolescent suicide. I would challenge us all as to why haven't we done more.

Dr. Shaffer: Surely, it's an empirical question as to whether there is a high rate of human universally applicable risk factors, lower rate of universally applicable protective factors, or whether there are idiosyncratic factors. The major barrier to progress is perhaps the assumption that there are idiosyncratic factors.

Dr. Manson: It is my personal opinion that we experienced an interesting dynamic probably reflected in research with other minority populations. In fact, it was Norm Dinges a decade ago that gave me the pseudospecies argument that American Indians and Alaska Natives are so different from all other populations that the comparative enterprises are much less worthwhile. I think that argument is beginning to fall apart in the beginning of the accumulating empirical evidence. The challenge now is to say, How can these other studies constructively inform our future efforts? We must

not lose sight of that critical edge of attending to what can transfer and might not transfer rather than simply tending to the reverse side of that coin: the wholesale adoption of those other methods of design and measurement techniques. It seems that we need a critical, dialectic process that hasn't really characterized our work.

Dr. Shaffer: It also represents deficiency in design. It means when you do get to do your analytic studies in this population, you either have to link them in some ways with similar measures in non-Native populations or else have a non-Native controller.

Reference

Grossman, D. C., Milligan, B. C., & Deyo, R. A. (1991). Risk factor for suicide attempts among Navajo adolescents. *American Journal of Public Health. 81,7* 870–874.

Notes

1. In the later period, two or three of the overdose deaths among one tribe were from ingestion of battery acid, making these more lethal than most overdose/poisoning methods.

2. Seasons are roughly defined as: winter: January through March; spring: April through June; summer: July through September; and fall: October through December.

3. This plateau trend is even more obvious in comparisons using all ages from this New Mexico data set. Work in progress indicates that overall rates are not rising.