

TABLE 1—Behavioral Intervention Research Branch (BIRB) Cooperative Agreements and AIDS Cases Reported per 100 000 Population by Major Metropolitan Areas in the United States, 1996^a

Location	BIRB Projects, No.	AIDS/100 000
Northeast		
New York City	5	120.1
Baltimore	1	61.6
Philadelphia	2	33.9
Boston (Newton, Mass)	1	19.0
Pittsburgh	3	8.3
Midwest		
Chicago	2	23.8
Detroit	1	16.3
Minneapolis	1	9.9
Milwaukee	3	9.4
Far West		
San Francisco	4	95.0
Los Angeles	1	40.7
San Diego	1	37.1
Seattle	2	26.1
Portland	1	18.5
South		
Atlanta	2	46.4

are necessary to assure the American people and their elected representatives that resources are being distributed fairly and equitably to the most capable investigators working in the areas of greatest need.

CDC should follow in the footsteps of the National Institutes of Health⁶⁻⁹ by thoroughly reviewing its cooperative agreement and extramural research programs. We encourage Dr Satcher or his successor to call for an independent group of qualified evaluators to examine the grant review, administration, and effectiveness process. Such an independent review would be in keeping with CDC's primary concerns about "the centrality of science," "working ethics," and the formation of "partnerships" with all Americans.¹ □

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Validity of Adolescent Self-Reports of Cigarette Smoking

Wills and Cleary concluded from their research that "self-reports of cigarette smoking by Black and White adolescents were generally valid and that differences in false-negative and false-positive rates across ethnic groups did not seriously qualify results . . . that the ethnic differentials in cigarette smoking indicated by previous epidemiological research are real and not a consequence of reporting artifacts."¹ Their study was an attempt to replicate our research concluding that Black-White differences in self-reports of cigarette smoking and tobacco use were due in part to measurement error.² Wills and Cleary believe their findings differ from ours, whereas we

believe they are strikingly similar. We also consider the conclusions they draw from their data, and the conclusions about our data they attribute to us, to be potentially misleading.

Their findings were consistent across grade in school, and we focus on their subjects as 10th graders because Wills and Cleary consider them most appropriate for this consideration. Using carbon monoxide in alveolar breath as the standard for determining cigarette smoking, they found sensitivity rates (proportions with positive CO who reported smoking) of 0.98 for Whites and .56 for Blacks. That 0.42 difference ($0.98 - 0.56 = 0.42$) is large, consistent with our findings and conclusions, and at variance with the conclusion Wills and Cleary drew from their data and from ours. That 44% of the Black 10th graders who were smokers, according to the CO standard chosen by Wills and Cleary, did not report their smoking ought to be taken seriously by researchers and policymakers who rely on self-reports of smoking by Blacks. Also consistent with our findings, Wills and Cleary found that specificity (proportion with negative CO who report themselves as nonsmokers) was lower for Whites (0.83) than for Blacks (0.98); this difference was more than trivial because most young people are nonsmokers.

The White-to-Black ratio of CO prevalence in our study was smaller than the White-to-Black ratio of self-report prevalence. Wills and Cleary found the same result in their data for 3 of the 4 comparisons they made but concluded there was no difference. They also replicated our analyses that adjusted Black self-reports for measurement error. The adjustments resulted in Black reports being more similar to White reports. These findings are consistent with our study findings and conclusion that invalid reporting appeared to account for part of the Black-White difference in self-reports.

As we did in our paper, Wills and Cleary addressed possible limitations of using CO as the standard to measure smoking. However, they did not mention that we also used cotinine in saliva to assess self-reports of tobacco use. The completely different biochemical method used to assess a second self-report measure produced very similar findings. They also incorrectly described the ages of our 12- to 14-year-old subjects and characterized our standard metropolitan statistical areas with populations of 200 000 to 500 000 as "smaller towns."

A contribution of the Wills and Cleary study that should not be lost here, or in their paper, is that they included Hispanics as a

comparison group and we did not. The differences in reporting error between Hispanics and Whites generally paralleled those between Blacks and Whites.

Wills and Cleary appear to believe that we concluded from our data that the Black-White differences in self-reports were entirely due to measurement error. That would be an incorrect reading of our paper, in which we identified measurement error as a substantial and partial contributor to the differences. Our data, and the data presented by Wills and Cleary, suggest that Black-White differences in measurement error explain part of the Black-White differences in self-reports of tobacco use. That this similarity of findings exists across studies that are different in so many ways (including school vs home data collection, questions asked, data collection procedures, and geographic regions) suggests our findings and theirs are particularly robust and should not be readily dismissed. □

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Wills and Cleary Respond

Bauman and Ennett,¹ using data from a sample of younger adolescents (mean age 13.0 years) in southeastern communities, observed a lower rate of smoking among Black adolescents, as found in a number of other studies.²⁻⁵ They then compared data on self-reported smoking with data from carbon monoxide measurements. Although rates of smoking were low among both Black and White respondents, Bauman and Ennett¹ concluded from their analyses that "much of the [Black-White] difference may be due to measurement error" (p. 394), proposed that future studies "should account for invalid measurement" (p. 397), and suggested, in regard to investigators of Black vs White smoking patterns, that "the differ-

ences they attempt to explain may be due in large part to differential validity" (p. 397).

What message most readers carried away from Bauman and Ennett's report is not known for certain. We read it as proposing that there is no substantial true difference in smoking for Black and White adolescents. Accordingly, we thought it desirable to examine their conclusions in data from an independent study with multiple assessments obtained over a range of ages.⁶

Using the same measures and definitions used by Bauman and Ennett, we were unable to confirm their suggestions. For example, Bauman and Ennett noted that a large difference in the ratio of self-reported to CO-indicated smoking for two ethnic groups is a crucial index of validity, with similar ratios indicating comparable validity. They reported ratios of 11.5 and 3.3 for Blacks and Whites, respectively, and concluded that this large difference provided evidence for invalidity among Blacks. We found ratios such as 2.1 and 1.8 (eighth-grade data) or 2.4 and 4.1 (ninth-grade data). Thus, using Bauman and Ennett's own criteria, the data indicate comparable or greater validity among Blacks. Bauman and Ennett⁷ suggest that readers should view our data as "strikingly similar" to theirs. We find this difficult if not impossible.

Bauman and Ennett⁷ now give great emphasis to sensitivity statistics and selectively focus on sensitivity data for the oldest adolescents (10th graders), although they had previously suggested school dropout to be a biasing factor for such samples.¹ (p³⁹⁷) Their discussion ignores the fact that the sensitivity data for Blacks are based on small numbers at all ages, and both sensitivity and specificity can be influenced by group differences in patterns of smoking (e.g., smoking at home vs school) in addition to factors discussed previously by Bauman and Ennett.¹ (p³⁹⁷) Group differences in sensitivity do not have a straightforward interpretation as implied by Bauman and Ennett,⁷ particularly when there are group differences in prevalence.

Bauman and Ennett⁷ note correctly that adjusted rates (classifying false negatives as smokers) made the Black prevalence rates more similar to the White rates. However, they ignore the fact that even with these corrections, there was a substantial differential in smoking between Black and White adolescents.

Other points made in the letter either are semantic disputations or are erroneous. For example, Bauman and Ennett⁷ suggested that our findings were consistent across the respondent age groups. This is an error; several aspects of our data for older respon-

dents were different from those for seventh graders.

Bauman and Ennett¹ provided a useful contribution by drawing more attention to ethnic differences in the prevalence of cigarette smoking. Our data⁶ show that sweeping generalizations based on small numbers of cases can be misleading. Our data are comparable to findings from a variety of studies indicating that self-reports of smoking are generally valid.⁸ We think our findings demonstrate that research on ethnicity should obtain multiple assessments, should give reasonable attention to validity issues, and should involve further inquiry to clarify why Black adolescents have lower rates of smoking. □

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