questionnaire that they were HIV positive, yet their serological tests were negative. In our 1990 Sydney data, 210 subjects volunteered for HIV-1 testing and provided elutable blood blots, and 7 (3.3%) were antibody positive according to Western blot. Six subjects indicated in the questionnaire that they were HIV positive yet had negative serological tests. Paper-absorbed fingerstick blood blots have been demonstrated to have accuracy equivalent to serum samples.[4] The number of respondents who reported themselves to be HIV antibody negative but whose serological tests were positive was 2 in the 1989 sample and 0 in the 1990 sample.

In our analysis of the interviews (which contained the question, "What do you think your chance is of catching the AIDS virus [HIV]?") matched with the respective questionnaires (which asked, "Have you ever received a positive test result?," subsequent to "Have you had an AIDS [HIV] test?"), we found discrepancies. Of those with negative serology tests who reported on the questionnaire that they were HIV positive, 13 of the 17 in the 1989 data and 3 of the 6 in 1990 had indicated in the interview that they were not already infected (one of the response options to the question about the chance of contracting HIV).

If those subjects who gave discrepant responses between interview and questionnaire are reclassified on the basis of their interview response, the two data sets give the predictive value of a positive HIV test as 87% (1989) and 73% (1990), lower than the values provided by McCusker et al. However, we had the opportunity to reinterview one of the subjects who reported a positive test result but who had a negative serological result. He indicated that he had understood a "positive" result to mean a good result, that is, no evidence of HIV infection, in the sense of colloquial English rather than serology. The high number of our subjects who indicated that they had a "positive" test result while negative on serology suggests that this misunderstanding may be the source of significant inaccuracy in self-report. Although this is of uncertain relevance to the results of McCusker et al (the wording of their questions was not reported), researchers' checks on the understanding of the meaning of questions (and, in particular, on the use of the term "infected with HIV (the AIDS virus)" rather than "HIV positive") may eliminate some of these apparent false-positive results. Nevertheless from a behavioral and public health perspective, what is important is *perceived* HIV serostatus and its impact on the individual's behavior. □

Michael W. Ross, PhD, MPH
Wendy Loxley, MPsych
Alex Wodak, FRACP
Adrian Buzolic, BA
Benny Monheit, MB, BS, MPH
Aaron Stowe, BSocSci

The authors are with the Australian National AIDS and Injecting Drug Use Study.

Requests for reprints should be sent to Michael W. Ross, PhD, MPH, National Centre in HIV Social Research, University of New South Wales, 345 Crown St, Surry Hills NSW 2010, Australia.

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McCusker and Stoddard Respond

We computed the sensitivity, specificity, and positive predictive value of self-reported human immunodeficiency virus (HIV) status from the data provided by Ross et al., first for the original self-reports and then for the reclassified self-reports (Table 1). (Reclassification increases the predictive value and specificity by reducing the number of false-positives.) The overall positive predictive value of their reclassified self-report was 79.4%, which is lower than the 90% that we reported from injection drug users in Massachusetts.[1] Their overall values for the sensitivity and specificity of the reclassified report are somewhat higher than ours: 93.1% vs 81.8% for sensitivity, and 99.5% vs 98.8% for specificity. The main contributing factor in their data's lower positive predictive value is the lower HIV seroprevalence[2]: 2% vs 12.1% in our study population.

Ross's comments regarding the wording of questions on HIV status are a timely reminder of the need to carefully pretest survey questions, as study subjects may interpret questions differently from the investigator. □

Jane McCusker, MD, DrPH
Anne M. Stoddard, ScD

TABLE 1—Sensitivity, Specificity, and Positive Predictive Value of Original and Reclassified Self-Reported HIV Status

	Western Blot Result			Sensitivity, %	Specificity, %	Positive Predictive Value, %
	+	−	Total			
1989 data set						
No. subjects	22	1242	1264			
Original serostatus self-report				90.9	98.6	54.1
Positive	20	17	37			
Negative	2	1225	1227			
Reclassified serostatus self-report				90.9	99.7	83.3
Positive	20	4	24			
Negative	2	1238	1240			
1990 data set						
No. subjects	7	203	210			
Original serostatus self-report				100	97.0	53.8
Positive	7	6	13			
Negative	0	197	197			
Reclassified serostatus self-report				100	98.5	70.0
Positive	7	3	10			
Negative	0	200	200			
1989 and 1990 data combined						
No. subjects	29	1445	1474			
Reclassified serostatus self-report				93.1	99.5	79.4
Positive	27	7	34			
Negative	2	1438	1440			

Requests for reprints should be sent to Jane McCusker, MD, DrPH, AIDS Research Unit, School of Public Health, University of Massachusetts–Amherst, Arnold House, Amherst, MA 01003.

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Bias in Weighted vs Unweighted Estimates

In their article "Epidemiologic Studies Utilizing Surveys: Accounting for the Sampling Design," Korn and Graubard discuss when it is preferable to use unweighted as opposed to weighted estimates for the analysis of stratified data.[1] Their recommendation is based on the relative inefficiency of weighted estimates.

However, the formula the authors use to calculate inefficiency of one estimate relative to another assumes that both are unbiased. The general definition of the relative efficiency of two estimates is $E((z_1 - z)^2)/E((z_2 - z)^2)$ where z is the true value of the parameter, and z_1 and z_2 are two estimations.[2] It is equivalent to $(D_1^2 + SE_1^2)/(D_2^2 + SE_2^2)$, where D_1 and D_2 are the two biases and SE_1 and SE_2 are the deviations of estimates. It is well known that unweighted estimates are often biased even asymptotically, whereas weighted estimates in many situations are unbiased.[3] However, if, as is usually the case, SE tends to zero when the sample size grows, any asymptotically unbiased estimate is asymptotically more efficient than any asymptotically biased estimate. For a sample of fixed size, even if SE for weighted estimation is bigger than for unweighted, the bias of the unweighted estimate may be so large that the weighted estimate turns out to be more efficient. The authors unwittingly provide an example of this in Table 3: the unweighted SE is 0.79 and the weighted SE is 2.53. The authors' estimation of relative inefficiency in this case is $1 - (0.79/2.53)^2 = 0.9$ (i.e., 90%). However, this calculation does not take into account the bias of the unweighted estimate. Accepting that the weighted (unbiased!) estimation is equal to the population mean difference, we can estimate the bias of unweighted analysis as $D = 3.63 - 0.81 = 2.82$; and relative inefficiency according to the general formula then is $1 - (2.82^2 + 0.79^2)/2.53^2 = -0.34$). The fact that the inefficiency is negative indicates that the weighted estimation is more efficient. This finding explains why, although the SE increases when the weighted as opposed to the unweighted estimation was used, the *P* value decreases from 0.30 to 0.15. (The authors neglect to mention this decrease in *P* value). The bias of the unweighted estimation proves to be more important than the increase in SE with the weighted estimation. Even when the authors use "unweighted regressions with means adjusted for many of the variables used in defining samples weights" (of final note in Table 3), the estimates of the difference in means and SE of differences are very close to the unweighted estimates, and the *P* remains 0.3. □

Ilya D. Novikov, PhD
Paul E. Ruskin, MD

Ilya Novikov is with the Department of Clinical Epidemiology, Chaim Sheba Medical Center, Tel Hashomer, Israel. Paul E. Ruskin is with the University of Maryland, Baltimore.

Requests for reprints should be sent to Ilya Novikov, PhD, Department of Clinical Epidemiology, Chaim Sheba Medical Center, Tel Hashomer 52621, Israel.

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Korn and Graubard Respond

There are many issues involved in deciding how to use the sample weights in an epidemiologic analysis. As we previously described, an important consideration is that weighted estimators are approximately unbiased but more variable than unweighted estimators, which may, or may not, be biased.[1] We suggested calculating an inefficiency of using the sample weights *when the use of the weights was actually unnecessary for bias reduction* as a guide: Whenever this inefficiency is small, we suggested the use of a standard weighted analysis; we suggested other approaches when it is not. Novikov and Ruskin suggest an alternative inefficiency calculation based on the estimated mean square errors of the weighted and unweighted estimators. (Mean square error incorporates both the variability and bias of the estimator.) This appealing idea is not new and has been developed in the survey context in a more sophisticated manner by Potter.[2] The problem with using this approach with applications like the present one is that it is difficult to estimate the bias of the unweighted estimator with sufficient accuracy. Reconsidering the transferrin saturation (%) for women demonstrates the point: The unweighted estimator (mean ± SE) is 0.81 ± 0.79 and the weighted estimator is 3.63 ± 2.53. An estimate of the bias of the unweighted estimator is 2.82; but how good is this estimate? As we noted,[1] trimming one woman's sample weight to the median sample weight changed the weighted estimator to 1.35 ± 1.16, yielding an estimated bias of 0.54. More formally, calculating the standard error of the estimated bias (using a jackknife[3]), we find the estimate is 2.82 ± 2.88. An approximate 90% confidence interval for the bias is −1.92, 7.56; so an approximate 90% confidence interval for the mean square inefficiency suggested by Novikov and Ruskin is from −8.03 to 0.90. Therefore, we do not find their inefficiency calculation useful. We note that there are additional considerations to bias and variance that are relevant to the question of how to utilize sample weights. □

Edward L. Korn, PhD
Barry I. Graubard, PhD

Requests for reprints should be sent to Edward L. Korn, PhD, Biometric Research Branch, EPN-739, National Cancer Institute, Bethesda, MD 20892.

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The Attribution of Health Problems to Aging

Regarding Rakowski and Hickey's paper, "Mortality and the Attribution of Health Problems to Aging among Older Adults,"[1] there is an alternative explanation to the authors' claim that attributing health problems to aging is a risk factor for mortality. Attribution was measured as