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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. □

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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. □

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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 present if respondents gave "old age" as the cause for impairment on any one of seven separate questions on individual activities of daily living. It seems plausible that someone responding to difficulties with multiple activities of daily living would be more likely to include old age as a cause for at least one of them than would someone who was having difficulties with only one. Yet in the study, number of activities named was not included in an otherwise extensive list of control variables in the multivariate analysis.

With no control for the number of activities of daily living involved, attribution to aging could simply be a surrogate for number of activity dependencies present, surely a known risk factor for mortality. ☐

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Reference

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Rakowski and Hickey Respond

Dr Strawbridge has identified a plausible alternative explanation for the results of our investigation,[1] and we have therefore reexamined the data in light of his comments. It is possible that persons who report difficulty with a greater number of activities of daily living might also be more likely to attribute one or more of such problems to old age. We did not include activities of daily living in the original list of control variables because it was evident that the maximum number of activities that could be attributed to old age was limited by the number of activities of daily living that a respondent identified. However, our analyses did include a large number of health-related covariates, and there were only 72 persons who made at least one attribution to aging. Therefore, the large majority of persons who reported several activities of daily living limitations did not provide an aging attribution.

Further review of the data has shown that the respondent's number of activities of daily living was not related to attributing at least one activity problem to old age ($\chi^2 = 7.35$; $df = 6$; $P = .29$). The absolute percentages of attribution to old age for each number of activities of daily living were as follows: 1 activity, 5.5%; 2 activities, 7.0%; 3 activities, 3.7%; 4 activities, 6.3%; 5 activities, 3.5%; 6 activities, 0.0%; 7 activities, 6.3%. The multivariate logistic regression analysis was then repeated, with the number of activities of daily living added to the list of predictors, along with the several other covariates cited in our earlier report. The adjusted odds ratio for attributing one or more activities of daily living to old age as the main cause was 1.76 (95% confidence interval = 1.05, 2.96). These results are extremely consistent with the original report and appear to reflect the absence of a bivariate association between number of activities of daily living and attribution to old age. In effect, number of activities can be added to the list of variables that showed no association with attribution to aging (cited in Table 3 of the original paper).

Dr Strawbridge's observation is important because it draws attention to what might have been an irresolvable confound in the data, had the number of activities of daily living been associated with attribution to aging. Specifically, persons must have health problems before any attribution of health problems to aging is possible, a fact that sets the stage for an interdependence that can be difficult to disentangle. The Longitudinal Study of Aging assessed aging attribution in regard to the specific set of activities of daily living. A more generic scale (i.e., one not tied to specific health problems) was not available. At the same time, there can be no guarantee that a generic scale of aging attribution would have as strong a potential for association with a health outcome as one tied to specific problems. Although our reanalysis supports the original article, Dr Strawbridge's comments are important to consider in any further study of aging attribution and health outcomes. ☐

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