one PEP series (2/9) with another (5/9) as the dependent variable in regression had only a slight effect on the adjustments.

Freedman and Navidi, who have raised some good points, have properly drawn attention to two important issues: the need to incorporate all sources of error into our measure of uncertainty and problems of extrapolating from the set of areas on which the regression equation is calculated to the set of areas where estimates are needed. These points modify, but do not obviate, the use of our method for adjusting the census. They also fail to demonstrate that our adjustments do not improve upon the census-estimated population distribution for 1980.

An ideal composite estimate would incorporate information from demographic analysis, make allowances for other independent variables that could have been included in the regression equation, and give some weight to alternative series of PEP estimates. Use of the additional sources of information would improve the estimates while increasing our measures of uncertainty. This uncertainty would not increase to the point where we would consider the adjusted population for New York City to be less accurate than the census count. Moreover, the uncertainty associated with our adjustment would be less than the uncertainty with which we must currently view the count.

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Comment

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In their provocative article, Freedman and Navidi argue vigorously against the use of "statistical models" for adjustment of 1980 census counts for both large and small regions of the U.S., "even compared to nothing," but indicate that they might allow exceptions if the assumptions were "made explicit" and were "shown to be appropriate." I agree with the authors that explicitness of assumptions is a virtue, but I question whether anyone actually assumes models in so true versus false a form as Freedman and Navidi appear to suggest. Hence, the concept of what is appropriate is considerably more subtle than they allow.

I will discuss below the aspects of modeling which I believe are most critical for regression adjustment of undercount rates derived from the Post Enumeration Program (PEP). I will also take a brief look at the logical foundation of the argument of Freedman and Navidi and I will argue that they have fallen into traps of their own choosing. I agree with them that the frequentist concept of modeling the production of data as "random draws from a box" is only marginally relevant to the applied problem, not only because the methodology is questionable in the specific circumstances, but also and more fundamentally because my attempts to find or construct a satisfying and explicit general account of frequentist logic have all failed. Freedman and Navidi apparently recommend doing "nothing," which I take to be a recommendation to report raw census counts and no more. I prefer a more cheerful outlook. Statistical logic does have merit, and we do have formal tools capable of addressing problems which most professions relegate to guesswork by acknowledged experts. I suggest pushing ahead with a more satisfactory logic. Finally, my comments will conclude with a brief review of the technical development of Freedman and Navidi.

In their zeal to attack certain formal assumptions, Freedman and Navidi risk demolishing statistical principles which lie at the root of our profession's claim to make a scientific contribution to uncertainty assessment. I wish to elaborate on two of these: the principle of randomization and the principle of regression to the mean.

The PEP program does rely on data from formally randomized surveys. The advantage of randomization does not lie primarily in providing a basis for mean square error computations or for randomization tests or confidence intervals, although these may sometimes

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