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Adjusting the 1980 Census of Population and Housing

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In 1980, several cities and states sued the U.S. Census Bureau to correct census results. This correction would adjust for the differential undercounting of Blacks and Hispanics, especially in cities. In this article, the authors, each of whom testified for New York City and State in their joint lawsuit against the Census Bureau, describe the likely pattern of the undercount and present a method to adjust for it. We first explain why the undercount is concentrated among minority populations living in large cities. We describe the demographic and survey data available for adjustment from the Census Bureau's Post Enumeration Program. We present adjustment results obtained by two simple methods—synthetic estimation, and sample estimation for a few large subclasses. The Census Bureau used the latter method, known as the National Vacancy Check, to adjust the results of the 1970 census. We also describe our regression-based, composite method for adjustment. This method takes sample estimates of the undercount rate for a set of mutually exclusive geographic areas, and regresses these estimates upon available predictor variables. The composite estimates of the undercount rate are matrix-weighted averages of the original sample and regression estimates. We compute estimates for 66 areas: 16 large cities, the remainders of the 12 states in which those cities are located, and 38 whole states. As expected, we find that the highest undercount rates are in large cities, and the lowest are in states and state remainders with small percentages of Blacks and Hispanics. Next, we analyze how sensitive our estimates are to changes in data and modeling assumptions. We find that these changes do not affect the estimates very much. Our conclusion is that regardless of whether we use one of the simple methods or the composite method and regardless of how we vary the assumptions of the composite method, an adjustment reliably reduces population shares in states with few minorities and increases the shares of large cities.

KEY WORDS: Census adjustment; Differential undercount; Dual systems estimation; Sensitivity analysis.

Nearly the whole of the states have now returned their census. I send you the result, which as far as founded on actual returns is written in black ink, and the numbers not actually returned, yet pretty well known, are written in red ink. Making a very small allowance for omissions, we are upwards of four millions; and we know that the omissions have been very great.

Thomas Jefferson (1791)

1. INTRODUCTION

Although it has spent vast sums of money to improve coverage, the U.S. Census Bureau continues to be plagued by large undercounts in the American census. In 1980, as in 1950, 1960, and 1970, the bureau undercounted Blacks more than non-Blacks. The difference computed by demographic analysis was 5.2% (Blacks 5.9, non-Blacks .7; U.S. Bureau of the Census 1988a). This is close to the corresponding 1970 figure of 5.8% (Blacks 8.0, non-Blacks 2.2). Revenue sharing lent an increasing sense of urgency to reports of census-taking problems in cities and gave greater impact to the calls of many technical experts and policymakers for an adjustment.

Adjusting census counts at the national level is neither difficult nor controversial. The Census Bureau has enough confidence in demographic analysis to adjust the national count for postcensal population estimates (U.S. Bureau of the Census 1978, 1988b). Other researchers have done the same. Having accurate sex ratios for Blacks was important to Goldman, Westoff, and Hammerslough (1984) in their analysis of marriage chances of persons in various age–race–sex categories. Accordingly, they adjusted the 1980 census results for Blacks, correcting deficiencies due to the greater undercounting of Black males.

The problem is to allocate adjustments to local areas. This is what Ericksen and Kadane set out to do as expert witnesses for New York in the undercount litigation, relying on a hierarchical Bayesian model (Ericksen and Kadane 1985). Using this model, they first obtained sample estimates for 66 local areas—16 large cities, 12 remainders of states, and 38 whole states. Next, they regressed these sample estimates on a set of variables related to the undercount. Finally, they calculated composite estimates, which weighted averages of the original sample estimates and the regression estimates they had just calculated. Extrapolating from these 66 states and cities to the many local areas for which census results are needed is an additional problem (see Sec. 6.1).

Freedman criticized the Ericksen and Kadane approach both in his testimony as an expert witness for the Census Bureau and in a later article (Freedman and Navidi 1986). Freedman argued that the model relied on unverified assumptions that in his view were probably wrong.

In his comment on Freedman and Navidi, Kadane (1986) wrote that he and Ericksen had studied their model's sensitivity to changes in important assumptions, such as those used for treating missing values in the underlying sample data. This article reports the work Kadane referred to in his comment. We describe the data available for making an adjustment, present two simple adjustments that im-

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prove upon the census, and finally present the analysis of the Ericksen and Kadane model and evaluate its sensitivity to changed assumptions. Because we show that the two simpler models improve upon the unadjusted census, the question of the Ericksen and Kadane model is not whether it proves that adjustment is feasible, but whether it improves upon the simpler methods.

2. THE CENSUS IS AN ESTIMATE

The unadjusted census count can be regarded as an estimate of the population. As an estimate, it is subject to error. In 1980, the census plan for most of the country was to mail census forms to all households and to have the forms filled out and sent back. Eighty-three percent of households did so, with the head of household typically filling out the census form for the entire family. If the head of household did not know someone's age, he or she guessed it or left it blank; if a relative slept in the house two or three nights a week, he or she may or may not have been listed as a household member.

Many of the 17% of households not returning their forms were hard to count. Census-takers went to their homes, but frequently found no one there. If they could make no contact at an address after three or four attempts, they asked a neighbor or passerby to supply the census information as a "last resort." If last-resort information was inadequate or unavailable, a computer program created the census "data." In 1980 the Census Bureau added about 3 million people (1.4% of the total population) to the count by a computer-generated imputation program. Census-takers "counted" many other people on the flimsiest of evidence, either by last resort or by unauthorized "curbstoning," in which a census-taker simply made up the data.

Many of the "counts" were duplicated, fabricated, or otherwise erroneous. Census Bureau statisticians Cowan and Fay (1984) estimated that there were 6 million erroneous enumerations in 1980. Most were duplications, but as many as 1 million were fabrications, or "curbstone" cases (see also Fay 1988). According to a Census Bureau report (1988a, p. 20), from 1.6 to 2.5 million people live in housing units that were counted twice and many other people were counted in each of two different locations. The bureau's "coverage improvement" programs, which it intended to eliminate the undercount, duplicated an additional 420,000 people. These duplications were 16% of the 2.6 million "people" added by these programs in 1980 (U.S. Bureau of the Census 1987, p. 10).

A recent Census Bureau (1988a) report indicates a *net* undercount of 1.4%, or 3.2 million people, in the 1980 census. This net is the difference between omissions and erroneous enumerations. Adding the net undercount to the 6 million erroneous enumerations gives an estimate of 9.2 million omissions, about 4% of the count. A decision not to correct the census is reasonable only if errors occurred at the same rates everywhere and the net undercount was a constant 1.4% in all areas. This flies in the face of data and common sense.

Research based on the Census Bureau's Post Enumeration Program (PEP) data show that Black and Hispanic undercount rates for the nation were similar. Counting conditions were much worse in central cities than elsewhere, and the PEP data show higher Black and Hispanic undercounts in cities than in suburban or rural areas. Census Bureau studies of erroneous enumerations (Fay 1988; U.S. Bureau of the Census 1985) show that the problem of *overcounting* occurs everywhere, but to a moderately greater extent in rural areas than in cities. For Blacks, the 1980 erroneous enumeration rate was 7.1% in rural areas, and 4.2%–4.9% elsewhere (Woltman, Alberti, and Moriarity 1988). Omissions and erroneous enumeration rates vary across areas and groups, but they do not offset each other. A correction is therefore needed to lessen the bias caused by the differential undercount.

3. DATA FOR ADJUSTMENT

Beginning with the seminal paper of Ansley Coale (1955), the Census Bureau has used demographic analysis to estimate the undercount by age, race, and sex. Relying on birth and death registrations and available migration data, the bureau constructs a total population estimate it considers to be more accurate than the census. The demographic method provides good estimates of the national undercount, but the lack of good migration data at the local level precludes its use for smaller areas.

For local estimates of undercount rates, survey data are needed. In 1980, such data were provided by the PEP (Cowan and Bettin 1982; U.S. Bureau of the Census 1982, 1983a). The PEP had two components, the *P* sample, obtained from the Current Population Survey (CPS) and used to estimate omissions, and the *E* sample, used to estimate erroneous enumerations.

The *E* sample surveyed 110,000 households counted by the census and located in CPS primary sampling units. The undercount rates provided by the PEP are the net of omissions and erroneous enumerations. In areas where erroneous enumerations outnumber omissions, this is negative, indicating an overcount. The samples were large enough that local sample estimates were calculated for the 50 states, 23 metropolitan areas, and 16 central cities.

The Census Bureau used dual-systems estimation (Table 1) to analyze the PEP data. The n_{11} term is the estimated number of people covered by the CPS *and* counted by the census. The n_{12} and n_{21} terms estimate, respectively, the number of people counted by the census and the number covered by the CPS, but not included in both. The Z term, those missed by the census *and* the CPS, is estimated by

$$Z = kn_{12}n_{21}/n_{11}. \tag{1}$$

Table 1. Population Distribution in the Dual-Systems Estimation Model

Census	Covered by Current Population Survey: Yes	No	Total
Yes	n_{11}	n_{12}	$n_{1\cdot}$
No	n_{21}	Z	$n_{21}+Z$
Total	$n_{\cdot 1}$	$n_{12}+Z$	T

NOTE: $Z = kn_{12}n_{21}/n_{11}$. $n_{1\cdot}$ is the census count with imputations and erroneous enumerations removed.

In calculating the PEP estimates, the Census Bureau assumed that $k = 1$, which means that the event of being counted in the census is independent of the event of being covered by the CPS. Under the independence assumption, if the census includes 95% of the population and the CPS covers 90%, then $n_{11} = .855T$, $n_{12} = .095T$, $n_{21} = .045T$, and $n_{22} = .005T$. Because under this assumption the ratios of n_{11} to n_{21} and n_{12} to Z are assumed equal, it does not matter whether n_{12} is greater than n_{21} or that the CPS covers the population less well than the census. The error caused by any lack of independence is known as *correlation bias,* which can be positive ($k > 1.0$) or negative ($k < 1.0$). In practice, since the census and the CPS cover most of the population, this error does not have a large impact. Assuming that $k = 1$ implies that the CPS covers 90% of the uncounted population in our example. If we retain the assumptions that the census covers 95% and the CPS covers 90% of the total population, then setting $k = 2$ implies that the CPS covers 82% of the uncounted population. Setting $k = .5$ implies that the CPS covers 95% of it. It is most likely that the correlation bias is positive (Ericksen and Kadane 1985) and hence that the Census Bureau assumption of $k = 1$ underestimates the undercount.

3.1 Missing Data in the Post Enumeration Survey

The PEP data were beset with missing information, and this affects the dual-systems estimate of total population. The CPS nonresponse rate on the *P* sample was about 4%. For an additional 4% of responding cases the bureau could not determine whether the person was counted or omitted. The main reason for this was that when the bureau could not find a CPS sample member on the census form corresponding to the CPS address, it tried to verify the correctness of the census-day address through field-checking. In cases where a person had moved or could otherwise not be interviewed, decisive verification data could not be obtained.

For the *E* sample, the corresponding rates were each about 2%. When it calculated the PEP undercount estimates, the bureau handled the missing data in five different ways for the *P* sample, and three different ways for the *E* sample. The bureau reported 12 of the 15 resulting possible series of undercount estimates. The Census Bureau assigned the numbers 2, 3, 5, 10, and 14 to the five *P*-sample strategies and the numbers 8, 9, and 20 to the three *E*-sample strategies. The combined PEP series is denoted by one number from each set; for example, PEP Series 2-9 relies on set 2 for omissions and set 9 for erroneous enumerations. Choosing among the 12 different series is necessary, because the geographic distributions of their undercount estimates are different. The missing-data strategies, the numbers to which they correspond, and the national estimates of omissions and erroneous enumeration rates they provided are listed in Tables 2 and 3.

3.1.1 Omissions in the PEP Data. For the *P* sample, the major differences in assumptions were (a) whether the April or August CPS data were used; (b) whether "Type A" non-interviews were included; (c) for the August data, whether movers were included in the analysis or excluded from it; and (d) whether the values for missing data were imputed or treated as simple non-interviews. The *P* sample was actually two samples, those of the April (Ser. 2, 3, and 14) and August (Ser. 5 and 10) waves of the 1980 CPS. The April and August waves were used because they did not overlap (U.S. Bureau of the Census 1978). They provide nearly independent estimates of omission rates.

Type A non-interviews were cases where the household had not been interviewed in the April CPS, but had been interviewed in either or both of the March and May editions. The Census Bureau substituted information from one of these adjacent months for the missing April data in Series 2.

The problem of movers occurred in the August data when field workers could not obtain an accurate address for April 1, census day. For Series 5, the Census Bureau used an imputed address if one was needed, but for Series 10 all movers were excluded from the analysis whether or not an accurate April address was available.

The Census Bureau counted a CPS sample member as an unimputed omission from the census only after he or she was found not listed on the appropriate census form, and field workers had verified the person's census-day address and that he or she had not been counted. Cases where the field check showed that the person was counted were called "matched" and added to the set of cases where the CPS and census data were the same. If the field check gave an ambiguous result or no information, the case was called "unresolved." For all but Series 14, the bureau linked an unresolved case to a similar case that had been sent to the field and resolved, and imputed the disposition of the latter to the former. Cowan and Bettin (1982, pp. 22–24) reported that the bureau imputed 51% (April) and 42% (August) of the originally unresolved cases as omissions, and that about one-third of all omissions were so designated by imputation. In computing Series 14, the bureau eliminated unresolved cases from the analysis, and assumed that its omission rates within broad demographic categories were the same as those obtained for the full sample, whether sent to the field or not. Eliminating the unresolved cases profoundly affected the results. It effectively assumed an omission rate of 5%, rather than 51% or 42%, for unresolved cases. This means that the Series 14 estimates were not very different from assuming that all missing-data cases had actually been counted in the census.

As shown in Table 2, the systematic effects of choosing April or August data or deciding how to treat Type A non-interviews are small. For the August data, though, we see that eliminating movers from the analysis causes a substantial drop in the omissions rate. Because movers generally have a higher omissions rate than nonmovers, we do not agree with eliminating them from the analysis in the manner of Series 10. The Series 14 omission rate is lower than the rates for the other series. In our view it should be taken less seriously because the "unresolved case" assumption is not reasonable.

Table 2. Assumptions Used in PEP Estimates of Omissions

Series	CPS survey	Treatment of unresolved cases	Treatment of April/August movers	Treatment of Type A non-interviews[a]	Omission-rate estimate[b]
2	April	Imputation	Included	Included	5.55%
3	April	Imputation	Included	Excluded	5.40%
5	August	Imputation	Included	Excluded	6.11%
10	August	Imputation	Excluded	Excluded	4.69%
14	April	Excluded	Included	Excluded	3.66%

NOTE: Omission estimates were obtained from the P sample. When cases were excluded from analysis, remaining cases were given higher weights to compensate.

[a] Type A non-interviews were cases where no information was obtained in the April Current Population Survey, but information was obtained from the March or May editions.

[b] Obtained from the U.S. Bureau of the Census (1983a).

3.1.2 Erroneous Enumerations in the PEP Data. The bureau made two key decisions for handling missing data from the E sample: (a) whether they used certain information obtained from the U.S. Postal Service, and (b) whether they imputed values for missing data. The proportion of E-sample cases with missing data was smaller than the corresponding proportions of P-sample cases. Changing assumptions about missing data had less effect (Tables 2 and 3).

3.2 The Pattern of Sample Estimates

To examine local variations in the undercount, we divided the United States into 66 areas: the 16 central cities, the 12 remainders of states in which they were located, and 38 whole states. Many of the standard metropolitan statistical areas (SMSA's) for which PEP estimates were available crossed state lines, and data for the separate state components (e.g., for the New Jersey and Pennsylvania parts of the Philadelphia SMSA) were not available. To keep separate states as separate observations, we did not include any SMSA's in our study.

In Table 4 we present the correlations among the 12 PEP series across 66 areas. Looking at series having the same assumptions for erroneous enumerations, we find very high correlations between Series 2 and 3, and between Series 5 and 10. Correlations between Series 2 and 3 and Series 14 tend to be only moderate, indicating that deciding not to impute missing data blurred geographic differences in the omission rates. Correlations are also low when comparing a series based on the April CPS (2, 3, 14) with one relying on the August (5, 10) data. This is because there is no overlap in the April and August CPS samples, and the low correlations reflect the random nature of sampling error.

When we compare series using the same missing-data strategies for omissions, but different strategies for erroneous enumerations (e.g., 2-8 with 2-9 or 2-20), we find that the correlations are all high. This tells us that choice of a missing-data strategy for erroneous enumerations has little impact.

Table 3. Assumptions Used in PEP Estimates of Erroneous Enumerations

Series	Treatment of post-office information[a]	Treatment of unresolved cases	Erroneous enumeration rate estimate[b]
8	Included	Imputation	3.25%
9	Excluded	Imputation	2.86%
20	Excluded	Excluded	2.55%

NOTE: Erroneous enumeration estimates were obtained from the E sample. When cases were excluded from analysis, remaining cases were given higher weights to compensate.

[a] For certain cases where E-sample interviewers were not able to contact selected respondents, the post office supplied information possibly indicating the correct census-day address.

[b] Obtained from the U.S. Bureau of the Census (1983a).

3.3 Freedman and Navidi's Critique

In seeking to use the PEP data to adjust for the undercount, our approach was to select one PEP series (2-9) and to use regression analysis to smooth out the errors due to sampling. In our view, the PEP omissions Series 2, 3, and 5 were clearly better than Series 10 and 14, but as long as we ruled out the bad ones, it did not matter which series we selected. Freedman and Navidi disagreed with this view. They made three groups of criticisms:

1. They argued that the PEP data were unreliable because of correlation bias and errors made when CPS data were matched to the census.
2. They argued that the results depended on which PEP series is selected.
3. They argued against the use of the regression model. In criticizing the regression model, they made four points:

Table 4. Correlations Among PEP-Sample Estimates of the Undercount

PEP series	2-9	3-8	3-9	2-20	3-20	5-8	5-9	10-8	14-8	14-9	14-20
2-8	.97	.99	.96	.96	.96	.67	.69	.67	.70	.80	.80
2-9		.96	.99	.99	.99	.65	.73	.64	.59	.78	.80
3-8			.97	.96	.96	.68	.69	.68	.71	.81	.82
3-9				.99	.99	.65	.74	.64	.60	.79	.81
2-20					.99	.64	.73	.61	.58	.78	.81
3-20						.65	.73	.61	.59	.78	.81
5-8							.96	.93	.37	.44	.45
5-9								.89	.30	.48	.49
10-8									.41	.46	.44
14-8										.92	.89
14-9											.99

NOTE: Correlations are calculated across 66 areal units with observations weighted equally. The areal units are: 16 cities—Baltimore, Boston, Chicago, Cleveland, Dallas, Detroit, Houston, Indianapolis, Los Angeles, Milwaukee, New York, Philadelphia, Saint Louis, San Diego, San Francisco, and Washington, D.C.; 12 remainders of states—California, Illinois, Indiana, Maryland, Massachusetts, Michigan, Missouri, New York, Ohio, Pennsylvania, Texas, Wisconsin; 38 remaining whole states. Series 5-20, 10-9, and 10-20 were not reported by the Census Bureau.

(a) you cannot reliably decide which independent variables to use in regression; (b) you cannot generalize beyond the 66 sample areas; (c) the standard errors of the model are too small; (d) the sample estimates are not independent from one of the 66 areas to another.

In the remainder of this section we respond to criticisms 1 and 3d. In Sections 4 and 5 we respond to criticism 2 by comparing the adjustments obtained when different PEP series are used in the same model. Finally, in Sections 5 and 6 we respond to criticisms 3a, 3b, and 3c.

3.3.1 Matching Errors. Anyone who reads Appendix C of a 1980 census report will know that data are frequently missing from census forms. For example, 45% of persons had at least one piece of missing information on the long form in 1980 (U.S. Bureau of the Census 1983b, table C-3). The CPS has the same problem. In addition, some of the obtained information is incorrect. This can make it hard to match the CPS to the census, especially for groups and areas where undercount rates are higher (Bailar 1983). When matching problems occur, the same person could be in both the census and the CPS, but would not be identified because of missing or incorrect information, causing omission rates to be overestimated. Fortunately, there are three reasons why the problem may not be serious.

First, for each area, separate PEP estimates were calculated for Blacks, Hispanics, and all others. By controlling to separate national totals for each group, Ericksen and Kadane (1987) adjusted the PEP estimates for the possibility that undercount estimates were inappropriately higher for Blacks and Hispanics than for Whites because of matching error. These adjustments had little effect on the outcomes. Second, PEP procedures were conservative in that no one could be called an omission without a field check, but no provision was made for the possibility that some people were improperly matched to the census, and labeled as "counted" when they were in fact omitted. Third, we can reasonably believe that Series 14 underestimated the omissions rate. There is no particular reason to think that Series 2, 3, and 5 overestimated this rate, but if they did, the high correlations among all series should make us feel relatively comfortable about averaging them.

3.3.2 Correlation Bias. Correlation bias is a well-known problem of the PEP. Comparisons of the CPS to demographic estimates (U.S. Bureau of the Census 1978) indicate that the CPS misses 10% of Blacks counted in the census, but only 3% of Whites. Within each group, the correlation bias is likely to be positive, especially for Blacks. This causes the PEP to underestimate the true racial differential in the undercount. Freedman and Navidi posit the opposite argument, that the correlation bias is negative. They argue that because the census occurred at the same time as the April CPS, some people confused the CPS with the census and were not counted. In other words, being in the CPS made them harder to count.

Freedman and Navidi give no empirical support for this idea, and it seems fanciful for two reasons: (a) seven-eighths of the CPS sample had already been interviewed on the previous waves of the survey and were familiar with it, and (b) the Census Bureau based its count on an address register. If a CPS respondent discarded the census form thinking he or she had already been counted, the bureau would follow up at that address until it got a count, even if it was a last resort or closeout. It is doubtful that correlation bias caused the omissions rate to be too high.

3.3.3 Independence. Freedman and Navidi argued that there was little support for the assumption that the errors among the 66 areas were independent. They pointed out that the Census Bureau had three data-processing centers and 12 regional offices, and that random events like an April snowstorm over the Rockies might cause census errors to be correlated in neighboring states. They provided no empirical support for their suspicions, and we find them groundless for three reasons:

1. The 1980 census was administered by more than 400 district offices, an average of eight per state.
2. To our knowledge no one has suggested that there actually was an April snowstorm or any other event that affected the census in neighboring states.
3. When we correlated PEP estimates for cities with the corresponding estimates in their states (e.g., Detroit with the remainder of Michigan), we found no evidence of a correlation.

In the remainder of this article, we consider the second and third of Freedman and Navidi's arguments—that the choice of a PEP series matters, and that it is inappropriate to use regression. We present two simple adjustments, a synthetic and a PEP-based procedure modeled after the Census Bureau procedure used in the 1970 National Vacancy Check. Next, we present our composite procedure, and we close with an extended sensitivity analysis of it.

4. TWO SIMPLE ESTIMATES

Schirm and Preston (1987) showed that a simple synthetic procedure adjusting the census is "expected to improve the estimated proportionate distribution by state" (p. 975). Their synthetic procedure assumed that Black and non-Black undercount rates were the same in all areas, so the undercount rate for any particular area depended only on its Black population proportion. The procedure also assumed that Whites and Hispanics had the same undercount rates, and it made no provision for differences in counting conditions between areas. That such a simple procedure improves upon the census gives us confidence that we can do even better with the PEP data. We expect this for two reasons: (a) the Hispanic and Black undercount rates are similar, and (b) Black and Hispanic undercount rates are higher in central cities than elsewhere.

We modified the Schirm and Preston synthetic model in two ways. First, we assumed that the Hispanic undercount rate is the same as for Blacks, not Whites. Second, we assumed that there were 3 million undocumented aliens in 1980 and that 9.6% were Black (U.S. Bureau of the Census 1988a, chap. 3). With these assumptions, the national undercount rate is 1.4%. The rate for Blacks and Hispanics is 5.9%, and for the predominantly White "all others" it is .3%.

To see the effect of a synthetic estimate, we divided the 66 PEP areas into three groups:

Group 1—the 16 central cities

Group 2—the 20 states and remainders of states where at least 10% of the population was Black or Hispanic

Group 3—the 30 remaining states and state remainders.

Using our assumptions, the implied undercount rate for Group 1 is 2.9%, for Group 2 it is 1.7%, and for Group 3 it is .7%. Although this result underestimates the between-area differences by assuming that Black, Hispanic, and "all other" undercount rates do not vary across areas, it creates an adjustment that differentiates among areas more than Schirm and Preston's method does. Using Schirm and Preston's method, the implied undercount rate for Group 1 is 2.4%, for Group 2 it is 1.5%, and for Group 3 it is .9%.

A Simple Method Previously Used by the Census Bureau

In the 1970 census, the Census Bureau augmented the count by a sampling procedure known as the National Vacancy Check (U.S. Bureau of the Census 1974), the objective of which was to determine the proportion of housing units classified as vacant that were really occupied. The bureau selected a sample of 13,546 vacant housing units stratified into 12 cells defined by 4 regions and 3 census-taking methods. In each cell, a sample estimate of the proportion of housing incorrectly labeled "vacant" was calculated, and these rates varied from 6.9% to 18.8%. The bureau then applied the cell-specific rates to all housing units classified as vacant, randomly selecting vacant housing units and reclassifying them as "occupied." The bureau added 1.1 million people to the count, increasing the population by .5%. No check was made of housing units originally classified as occupied that might actually have been vacant.

In the 1980 census, Blacks and Hispanics were substantially harder to count in cities than elsewhere. For example, according to PEP Series 2-8, the undercount rate for Blacks in the 16 central cities (Group 1) was 10.1%, and for Hispanics it was 8.5%. In Group 2 areas the rate was 4.9% for Blacks and 3.8% for Hispanics, and in Group 3 areas it was 3.4% for Blacks and 3.0% for Hispanics. For the predominantly White "all others," the variation was much less—1.0% undercount in Group 1 and figures near 0 elsewhere (U.S. Bureau of the Census 1982).

To improve upon the synthetic estimates by taking these between-area variations into account, we designed a procedure in the manner of the National Vacancy Check. We divided the nation into the three groups already defined, and calculated separate sample estimates. For Series 2-8, the estimated undercount for Group 1 was 5.3%, for Group 2 it was 1.2%, and for Group 3 it was .1%.

In Table 5 we have converted these undercount estimates to changes in shares of total population. We show the changes in shares for each of the PEP series and indicate the comparable changes in shares implied by two synthetic estimates. This produces three results. First, whether we rely on the PEP or synthetic estimation, an adjustment shifts population shares from Group 3 to Group 1—that is, from states with few minorities to cities. Second, except for Series 14-8, use of the PEP data instead of synthetic estimation shifts a greater share of population to cities. This reflects the greater undercounting of minorities there. Third, when the August (rather than the April) data are used, there is less of a shift into the central cities. This systematic difference between April and August cannot be accounted for by sampling error, but it does not alter our qualitative conclusion; regardless of the PEP series chosen, the central-city undercount is higher and the undercount in areas outside cities with few Blacks or Hispanics is lower. Furthermore, an adjustment that shifts population shares to the cities would improve the accuracy of the census. Use of the PEP data improves upon synthetic estimation because it takes into account the greater difficulty of counting minorities in cities.

This "improved" adjustment is still not likely to be optimal, because it considers only two factors—whether the area is a city, and the percent minority. With a process as complex as the census, other factors surely matter, as we

Table 5. Changes in National Population Shares Resulting When Counts Are Adjusted by Sample Estimates Pooled Across Areas and Synthetic Estimates

PEP estimate	Group 1[a]	Group 2[a]	Group 3[a]	Estimated national undercount rate
2-20	+.52%	+.09%	−.61%	+1.9%
3-20	+.51%	+.08%	−.59%	+1.7%
2-9	+.50%	+.06%	−.56%	+1.6%
3-9	+.49%	+.04%	−.53%	+1.4%
2-8	+.41%	+.04%	−.45%	+1.1%
3-8	+.39%	+.03%	−.42%	+1.0%
5-9	+.31%	+.25%	−.56%	+2.1%
5-8	+.22%	+.23%	−.45%	+1.7%
14-20	+.21%	+.02%	−.23%	−.2%
10-8	+.19%	+.07%	−.26%	+.3%
14-9	+.19%	−.01%	−.18%	−.5%
14-8	+.10%	−.03%	−.07%	−1.0%
Synthetic A[b]	+.17%	+.14%	−.31%	+1.4%
Synthetic B[c]	+.12%	+.06%	−.18%	+1.4%
Shares of census count	10.76%	44.24%	45.00%	

[a] Group 1 includes the 16 central cities indicated in Table 4. Group 2 includes three state remainders (California, Maryland, and Texas, excluding Group 1 cities) and 17 whole states (Alabama, Arizona, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Louisiana, Mississippi, Nevada, New Jersey, New Mexico, North Carolina, South Carolina, Tennessee, and Virginia). All areas are at least 10% Black or Hispanic. Group 3 includes nine state remainders (Illinois, Indiana, Massachusetts, Michigan, Missouri, New York, Ohio, Pennsylvania, and Wisconsin) and 21 whole states (Alaska, Hawaii, Idaho, Iowa, Kansas, Kentucky, Maine, Minnesota, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, Oregon, Rhode Island, South Dakota, Utah, Vermont, Washington, West Virginia, and Wyoming). All Group 3 areas are less than 10% Black or Hispanic.

[b] The Synthetic A estimates assume that (a) Blacks have the same undercount rates as Hispanics, 5.9% (U.S. Bureau of the Census 1988a); (b) the undercount rate of persons neither Black nor Hispanic is .3%; (c) the undercount rates for Blacks, Hispanics, and all others are invariant across geographic areas; and (d) there are 3 million undocumented aliens, 9.6% of whom are Black.

[c] Following Schirm and Preston (1987), the Synthetic B estimates assume that (a) the Black undercount rate is 5.9%; (b) Hispanics and other non-Blacks have an undercount rate of .7%; (c) the undercount rates for Blacks, Hispanics, and all others are invariant across geographic areas; and (d) there are 3 million undocumented aliens, 9.6% of whom are Black.

demonstrate in Table 6. The "conventional" method is the old-fashioned method of taking the census, used everywhere in 1950, but limited in 1980 to sparsely populated rural areas mainly in the west. Conventional areas typically have few Blacks or Hispanics, and most states where the conventional method was used extensively are in Group 3. There is no mailout–mailback, and no address register is compiled in advance. Instead, enumerators are sent to the field and told to list addresses and count people at the same time. Because the Census Bureau did not compile an address register in advance, it did not create the problem of duplicate housing counts in the conventional areas, as it did elsewhere.

It is not possible to separate those parts of states where the conventional method was used from the remainders. Instead, we compare the five states where more than 75% of the people were counted by the conventional method (Alaska, Montana, North Dakota, South Dakota, and Wyoming) with other states having few minorities. In Table 6 we see that omission rates are similar in conventional and the other Group 3 states, but that rates of erroneous enumeration are lower in the conventional states. This causes the net undercounts to be higher in the conventional states because there are so few erroneous enumerations to subtract from the omissions. Table 6 also shows that even though the omission rates in cities are very high, the rates of erroneous enumeration are near the national average. In sum, concentrations of omissions in cities are not offset by concentrations of erroneous enumerations.

The results of Tables 5 and 6 show that an appropriate adjustment will shift population shares into central cities, but how large should the shifts be? We want a method to consider all available predictors of the undercount and select a parsimonious model with the best predictors. Our choice is the composite method described by Ericksen and Kadane (1985, 1987), and we use this method to compute improved estimates for the 66 areas making up Groups 1, 2, and 3. Study of the method will not "prove" that an adjustment will improve the census. This has already been demonstrated by Schirm and Preston and the results of Tables 5 and 6. Study of the composite method indicates whether we can improve upon the results provided by the simpler methods.

Table 6. Omissions and Erroneous Enumerations by Type of Area

	Group 1	Group 2	Group 3: Conventional*	Group 3: Other	Total
			Omissions		
Series 2	9.96%	6.41%	3.44%	3.63%	5.55%
Series 3	9.71%	6.24%	3.32%	3.54%	5.40%
Series 5	9.26%	7.34%	5.03%	4.15%	6.11%
Series 10	7.75%	5.62%	3.70%	3.09%	4.69%
Series 14	5.71%	4.38%	2.18%	2.50%	3.66%
Average	8.48%	6.00%	3.53%	3.38%	5.08%
			Erroneous enumerations		
Series 8	3.78%	3.71%	1.25%	2.71%	3.25%
Series 9	2.63%	3.28%	.97%	2.55%	2.86%
Series 20	2.15%	2.90%	.82%	2.35%	2.55%
Average	2.85%	3.30%	1.01%	2.54%	2.89%

NOTE: Groups 1, 2, and 3 are the same as in Table 5.
* "Conventional" states are those where 75%–100% of the population was counted by the conventional method: Alaska, Montana, North Dakota, South Dakota, and Wyoming.

5. REGRESSION ANALYSIS

The first step of the regression analysis was to identify the undercount rates provided by PEP Series 2-9 as the dependent variable. The form of the dependent variable was $Y_i = (1 - P_i/D_i)$, where P_i is the population count and D_i is the dual-systems estimate for area i.

We selected Series 2-9 for several reasons. For omissions, we preferred the April to August *P*-sample data because of the movers problem. We preferred Series 2 and 3 to Series 14 because of the way that unresolved cases were handled. Moreover, Series 2 and 3 conform better to the results of demographic analysis. If we assume that there were 3 million undocumented aliens in 1980, the Census Bureau's (1988a) current best estimate, then demographic analysis indicates a net Black undercount of 5.9%. The PEP estimates of Black undercount, when omissions Series 2 or 3 is used, range from 5.6% to 7.2%, but when Series 14 is used the range is from .7% to 2.5%. PEP series with higher omission rates uniformly had higher estimates of Black undercount. Choosing between Series 2 and 3 was a close call, but we preferred Series 2 because it included information from Type A non-interviews (see Tables 2 and 3). For erroneous enumerations, we followed the bureau's lead (Cowan and Bettin 1982, p. 10) in preferring Series 9 to Series 8, rejecting the use of post-office data. We preferred the imputation strategy of Series 9 to the weighting strategy of Series 20 for the same reason that we prefer Series 2 and 3 to Series 14. Some of these choices matter more than others. As suggested by the correlations of Table 4, it matters little how we choose among Series 2-8, 2-9, 2-20, 3-8, 3-9, and 3-20. The choice between April and August makes some difference, and in our view the only four series based on *unreasonable* assumptions are 10-8, 14-8, 14-9, and 14-20.

5.1 Regressors

We had prior empirical reasons to expect larger undercounts in central cities (Ericksen and Kadane 1985). Urban census-taking problems were substantial, mailback rates were low, and studies by the Census Bureau (1987) and the U.S. General Accounting Office (1980) showed that few of the coverage improvement programs intended to reduce the differential undercount worked, especially in cities. Imputation rates for missing data were especially high in cities, and there is evidence that the intended one-in-six long-form sampling rate was not well maintained in large cities (U.S. Bureau of the Census 1983b, tables C-3 and D).

Aware of these problems, we did not know the extent to which they were limited to cities. We sought predictors of the undercount that would be sensitive to variations in both urban and nonurban areas, and we settled on eight variables as potential predictors:

1. the percentage of the population that was Black or Hispanic (minority percentage)

Table 7. Values for Eight Predictor Variables: 66 Areas, PEP Series

Area	Minority[a] (1)	Crime rate[b] (2)	Poverty[c] (3)	Language difficulty[d] (4)	High school graduates[e] (5)	Small multiunit housing[f] (6)	Central city[g] (7)	Conventional[h] (8)
Alabama	26.1%	49	18.9%	.2%	43.5%	7.6%	0	0%
Alaska	5.7%	62	10.7%	1.7%	17.5%	23.6%	0	100%
Arizona	18.9%	81	13.2%	3.2%	27.6%	8.1%	0	18%
Arkansas	16.9%	38	19.0%	.2%	44.5%	7.0%	0	0%
California (R)	24.3%	73	10.4%	5.0%	26.0%	11.8%	0	4%
Colorado	15.2%	73	10.1%	1.2%	21.4%	9.2%	0	19%
Connecticut	10.8%	58	8.0%	2.4%	29.7%	21.0%	0	0%
Delaware	17.5%	68	11.8%	.7%	31.4%	8.9%	0	0%
Florida	22.3%	81	13.4%	3.6%	33.3%	10.1%	0	0%
Georgia	27.6%	55	16.6%	.3%	43.6%	10.2%	0	0%
Hawaii	9.1%	75	9.9%	5.7%	26.2%	17.0%	0	29%
Idaho	4.2%	48	12.6%	1.0%	26.3%	9.1%	0	56%
Illinois (R)	8.1%	48	7.7%	1.0%	29.8%	13.5%	0	0%
Indiana (R)	7.1%	48	9.4%	.5%	33.6%	9.9%	0	0%
Iowa	2.3%	47	10.1%	.3%	28.5%	10.4%	0	0%
Kansas	7.9%	54	10.1%	.5%	26.7%	8.5%	0	14%
Kentucky	7.7%	34	17.6%	.2%	46.9%	10.6%	0	0%
Louisiana	31.4%	54	18.6%	1.1%	42.3%	9.7%	0	0%
Maine	.7%	44	13.0%	1.0%	31.3%	19.5%	0	40%
Maryland (R)	16.7%	58	6.8%	.8%	28.2%	10.5%	0	0%
Massachusetts (R)	3.8%	53	8.5%	2.1%	27.4%	26.9%	0	4%
Michigan (R)	7.0%	61	8.7%	.7%	29.9%	9.4%	0	8%
Minnesota	2.1%	48	9.5%	.5%	26.9%	10.7%	0	11%
Mississippi	35.8%	34	23.9%	.2%	45.2%	7.2%	0	0%
Missouri (R)	7.8%	45	11.2%	.3%	34.9%	9.1%	0	0%
Montana	1.5%	50	12.3%	.4%	25.6%	12.8%	0	75%
Nebraska	4.8%	43	10.7%	.5%	26.6%	9.7%	0	33%
Nevada	13.0%	88	8.7%	1.6%	24.5%	11.7%	0	10%
New Hampshire	1.0%	47	8.5%	.8%	27.7%	20.3%	0	0%
New Jersey	19.0%	64	9.5%	3.6%	32.6%	23.7%	0	0%
New Mexico	38.4%	59	17.6%	4.6%	31.1%	10.7%	0	58%
New York (R)	8.0%	48	8.9%	1.3%	29.3%	21.6%	0	0%
North Carolina	23.1%	46	14.8%	.2%	45.2%	8.2%	0	0%
North Dakota	1.0%	30	12.6%	.5%	33.6%	15.1%	0	70%
Ohio (R)	8.9%	52	9.6%	.5%	32.1%	11.3%	0	0%
Oklahoma	8.6%	50	13.4%	.5%	34.0%	8.0%	0	0%

2. the number of reported serious crimes per 1,000 population (crime rate)
3. the percentage of the population living in poverty
4. the percentage of the population having difficulty speaking and writing English (language difficulty)
5. the percentage of persons age 25 and older who have not graduated from high school
6. the proportion of housing in small, multiunit structures
7. whether the area was one of the 16 PEP central cities (1 if yes, 0 if no)
8. the percentage of households counted by the conventional method.

All variables are based on unadjusted census data, and we list their values for the 66 study areas in Table 7. The first six are associated with census-taking problems in large cities, but they are prominent in certain other areas as well. For example, rates of poverty and educational attainment are similar in the cities and many southern states. Several eastern states have large concentrations of multiunit housing. Several western states have high crime rates and concentrations of persons with language difficulties. Many southern and western states have substantial minority populations. The seventh variable isolates the purely urban aspect of the undercount, and the eighth variable indicates use of the conventional method discussed in Section 4.

Freedman and Navidi (1986) argued that we should have considered the proportion urban as an independent variable, since "There is strong opinion in the bureau that urban and rural areas present very different kinds of enumeration problems" (p. 9). The Census Bureau (1983c) defines the urban population as "all persons living in urbanized areas and in places of 2,500 or more inhabitants outside urbanized areas" (p. A2). Clearly, census-taking problems differ in kind and degree among large central cities, affluent suburbs, and small towns. Yet each of the following areas is, by census definition, 100% urban: New York City; Scarsdale, New York; and Slippery Rock, Pennsylvania (population 3,047). Less than half the nation's urban population lives in central cities, so if census-taking problems were concentrated in large central cities, the "urban percentage" would be at best a blurred predictor of the undercount. In the PEP data, there were 16 metropolitan areas where central cities could be separated from the predominantly urban suburbs. For Series 2-8, the undercount for these 16 cities was 4.8%, for the 16 re-

Table 7 (continued)

Area	Minority[a] (1)	Crime rate[b] (2)	Poverty[c] (3)	Language difficulty[d] (4)	High school graduates[e] (5)	Small multiunit housing[f] (6)	Central city[g] (7)	Conventional[h] (8)
Oregon	3.9%	60	10.7%	.8%	24.4%	7.9%	0	13%
Pennsylvania (R)	4.8%	33	8.8%	.6%	33.6%	13.3%	0	0%
Rhode Island	4.9%	59	10.3%	3.2%	38.9%	29.6%	0	0%
South Carolina	31.0%	53	16.6%	.2%	46.3%	7.9%	0	0%
South Dakota	.9%	32	16.9%	.4%	32.1%	12.0%	0	84%
Tennessee	16.4%	44	16.4%	.2%	43.8%	9.4%	0	0%
Texas (R)	30.6%	55	15.0%	4.7%	38.7%	7.7%	0	1%
Utah	4.7%	58	10.3%	.9%	20.0%	11.3%	0	14%
Vermont	.9%	50	12.1%	.5%	29.0%	20.8%	0	0%
Virginia	20.0%	46	11.8%	.5%	37.6%	10.3%	0	0%
Washington	5.4%	69	9.8%	1.0%	22.4%	9.4%	0	4%
West Virginia	3.9%	25	15.0%	.2%	44.0%	9.0%	0	0%
Wisconsin (R)	1.7%	45	7.9%	.4%	29.5%	12.8%	0	9%
Wyoming	5.9%	49	7.9%	.7%	22.1%	13.2%	0	100%
Baltimore	55.5%	100	22.9%	.7%	51.6%	23.3%	1	0%
Boston	28.4%	135	20.2%	4.4%	31.6%	52.1%	1	0%
Chicago	53.7%	66	20.3%	6.7%	43.8%	51.4%	1	0%
Cleveland	46.7%	101	22.1%	1.6%	49.1%	36.4%	1	0%
Dallas	41.6%	118	14.2%	3.1%	31.5%	12.9%	1	0%
Detroit	65.4%	106	21.9%	1.6%	45.8%	18.6%	1	0%
Houston	45.1%	80	12.7%	5.1%	31.6%	8.9%	1	0%
Indianapolis	22.5%	53	11.5%	.3%	33.3%	13.6%	1	0%
Los Angeles	44.4%	100	16.4%	12.7%	31.4%	15.0%	1	0%
Milwaukee	27.2%	65	13.8%	1.6%	36.4%	27.2%	1	0%
New York City	44.0%	101	20.0%	8.9%	39.8%	32.2%	1	0%
Philadelphia	41.3%	60	20.6%	2.2%	45.7%	21.7%	1	0%
Saint Louis	46.7%	143	21.8%	.5%	51.8%	40.9%	1	0%
San Diego	23.6%	81	12.4%	4.2%	21.1%	11.2%	1	0%
San Francisco	24.8%	107	13.7%	9.2%	26.0%	20.3%	1	0%
Washington, D.C.	72.6%	102	18.6%	1.1%	32.9%	21.0%	1	0%

NOTE: R indicates the remainder of a state after one or more central cities have been removed.
[a] Percentage of the population that was Black or Hispanic.
[b] Ratio of serious crimes per 1,000 population.
[c] Percentage of the population living in poverty.
[d] Percentage of the population having difficulty speaking and writing English.
[e] Percentage of persons age 25 and older who had not graduated from high school.
[f] Proportion of housing in small, multiunit structures.
[g] Whether the area was one of the 16 PEP central cities (1 if yes, 0 if no).
[h] Percentage of households counted by the conventional method.

mainders of SMSA it was 1.1%, and for the remainder of the country it was .6%. We concluded from these results that central cities should be separated from suburbs, and we opted for a dummy variable indicating central-city location, rather than the census definition of urban, as one of our eight predictor variables.

5.2 Choosing a Best Set of Predictors

To select a best set of predictor variables, we regressed Series 2-9 on all subsets of two, three, and four variables. We considered all equations in which *each* regression coefficient was at least twice its standard error, and selected the one that minimized σ^2, the unexplained variance. Best results were obtained with the three-variable subset consisting of the reported crime rate and the minority and conventional percentages. The margin of choice was small, and regressions substituting the language difficulty or central-city variables for the reported crime rate also fit the data well. Omitting any of the three selected variables to produce a two-variable regression led to substantially increased values of σ^2. Finally, when these calculations were made for dependent variables based on the other 11 PEP series, the optimal choice of predictor variables was the same or similar. It was the same for Series 2-8, 2-20, 3-8, 3-9, 3-20, 5-8, and 5-9. For Series 10-8, 14-8, 14-9, and 14-20, the optimal set included only two predictors, the reported crime rate and the percent conventional.

5.3 The Results From the Composite Method

The composite method is based on a hierarchical model we explained in Ericksen and Kadane (1985, 1987). Cressie (1988a,b) explored an interesting variant of our model with a different assumption about variances in the second stage.

Our estimate of the undercount rate is a certain matrix-weighted average of a regression estimate and the initial sample estimates. (See Table 8.) The regression estimate is

$$\hat{Y} = -3.0 + .059 \text{ min} + .026 \text{ conv} + .055 \text{ crime}, \quad (2)$$

where Y is the Series 2-9 undercount rate, *min* is the proportion Black or Hispanic, *conv* is the proportion counted

Table 8. Sample Estimates of Undercount (in percentages): 66 Areas, 12 PEP Series

Area	2-8 (1)	2-9 (2)	3-8 (3)	3-9 (4)	2-20 (5)	3-20 (6)	14-8 (7)	14-9 (8)	14-20 (9)	10-8 (10)	5-8 (11)	5-9 (12)
Alabama	-.37	-.04	-.35	-.02	.59	.60	-2.29	-1.95	-1.33	-1.50	-.37	-.03
Alaska	2.91	3.35	2.79	3.24	3.53	3.42	1.23	1.68	1.86	4.47	7.36	7.80
Arizona	1.69	2.48	2.00	2.79	3.00	3.30	-.09	.72	1.24	2.60	4.87	5.64
Arkansas	-1.00	-.74	-1.06	-.80	-.08	-.13	-2.35	-2.09	-1.41	-.04	1.43	1.69
California (R)	3.09	3.60	2.84	3.35	3.92	3.67	.29	.81	1.14	1.26	2.92	3.34
Colorado	.69	1.34	.32	.99	1.61	1.26	-1.76	-1.10	-.83	1.47	3.58	4.22
Connecticut	-.63	-.26	-1.16	-.78	.22	-.28	-2.18	-1.81	-1.33	-1.48	.43	.81
Delaware	-.38	-.16	-.62	-.40	.23	-.01	-1.85	-1.62	-1.23	.23	1.03	1.27
Florida	1.63	2.20	1.42	1.99	2.63	2.42	-1.03	-.45	.01	1.34	4.18	4.74
Georgia	-.06	.37	-.45	-.02	.70	.32	-2.24	-1.79	-1.45	-.97	1.32	1.75
Hawaii	1.30	1.46	1.09	1.25	2.08	1.87	-.77	-.63	.02	1.25	2.25	2.40
Idaho	1.41	1.53	1.24	1.36	1.84	1.67	.18	.30	.61	2.77	4.45	4.57
Illinois (R)	1.42	1.69	1.21	1.48	1.75	1.54	-.63	-.35	-.30	-.77	.10	.37
Indiana (R)	-.77	-.68	-.71	-.62	-.53	-.47	-1.90	-1.80	-1.65	1.26	1.83	1.92
Iowa	-.59	-.59	-.68	-.68	-.55	-.64	-1.32	-1.32	-1.27	-.34	.73	.73
Kansas	.77	.94	.55	.73	1.29	1.08	-.84	-.66	-.31	.91	1.86	2.04
Kentucky	-1.41	-1.41	-1.58	-1.57	-.85	-1.02	-2.63	-2.62	-2.06	-.75	.19	.19
Louisiana	2.18	2.46	2.28	2.56	3.19	3.29	-.52	-.25	.50	.21	1.87	2.10
Maine	2.02	2.06	1.97	2.00	2.30	2.24	.67	.70	.95	1.13	1.90	1.93
Maryland (R)	1.79	2.03	1.65	1.89	2.14	2.00	.33	.57	.69	1.50	2.38	2.61
Massachusetts (R)	-1.22	-.57	-1.22	-.56	-.27	-.26	-2.13	-1.47	-1.18	-1.62	-.86	-.23
Michigan (R)	.63	.89	.43	.69	1.04	.84	-.65	-.39	-.24	-.25	.81	1.07
Minnesota	1.20	1.57	1.10	1.46	1.68	1.57	.23	.60	.71	.45	1.12	1.49
Mississippi	.98	1.52	.96	1.50	1.69	1.67	-.92	-.36	-.19	.95	2.95	3.47
Missouri (R)	.75	.81	.55	.61	.94	.74	-.16	-.10	.04	-.74	.11	.17
Montana	1.53	1.81	1.42	1.70	2.20	2.09	-.18	.10	.48	2.09	2.44	2.72
Nebraska	.15	.36	.08	.29	.59	.52	-.73	-.52	-.29	.50	1.37	1.58
Nevada	2.93	5.08	2.59	4.75	5.64	5.32	-.27	1.89	2.47	.93	4.76	6.83
New Hampshire	-1.67	-1.49	-1.61	-1.43	-1.03	-.98	-2.40	-2.21	-1.76	-.85	1.08	1.26
New Jersey	1.45	1.44	1.29	1.28	1.83	1.67	-.33	-.34	.05	.04	1.44	1.43
New Mexico	2.43	2.69	2.30	2.56	3.03	2.91	-1.07	-.82	-.46	2.40	3.76	4.04
New York (R)	-1.61	-1.48	-1.60	-1.47	-1.45	-1.44	-2.52	-2.38	-2.34	-.74	.55	.69
North Carolina	1.14	1.36	1.17	1.39	1.74	1.77	-.31	-.09	.29	1.17	2.07	2.29
North Dakota	.06	.35	.05	.33	.37	.36	-.71	-.42	-.40	.34	.60	.88
Ohio (R)	.87	.97	.84	.94	1.17	1.14	-.58	-.49	-.28	-.27	.67	.77
Oklahoma	-.14	-.12	-.23	-.21	.28	.19	-2.61	-2.59	-2.18	-.61	.90	.93
Oregon	.38	.93	.27	.83	1.32	1.21	-1.35	-.85	-.46	.80	2.12	2.63
Pennsylvania (R)	-.98	-.78	-1.17	-.96	-.56	-.75	-1.73	-1.52	-1.31	-1.78	-.81	-.60
Rhode Island	.84	.74	.88	.78	1.13	1.17	-.85	-.96	-.56	-.95	1.17	1.07
South Carolina	5.84	6.19	5.94	6.30	7.22	7.32	3.49	3.85	4.90	2.17	4.07	4.42
South Dakota	.30	.42	.08	.21	.52	.33	-.62	-.49	-.39	.92	2.20	2.32
Tennessee	-2.90	-2.31	-2.91	-2.33	-2.38	-2.40	-4.15	-3.58	-3.64	-3.33	-2.37	-1.78
Texas (R)	-.48	.27	-.70	.05	.74	.51	-3.31	-2.56	-2.08	-.72	2.00	2.72
Utah	.46	1.14	.39	1.08	1.22	1.16	-.17	.52	.60	.33	1.92	2.61
Vermont	-.98	-1.12	-1.13	-1.27	-.41	-.56	-1.48	-1.62	-.91	-1.61	-.22	-.37
Virginia	.68	1.11	.09	.52	1.33	.74	-.91	-.48	-.26	.83	1.70	2.13
Washington	1.54	1.48	1.40	1.35	2.03	1.89	-.63	-.69	-.13	1.59	3.45	3.39
West Virginia	-.66	-.69	-.58	-.61	-.38	-.30	-1.93	-1.96	-1.64	-2.18	-.99	-1.03
Wisconsin (R)	1.45	1.45	1.48	1.48	1.48	1.48	.68	.68	.70	-.24	.11	.11
Wyoming	3.61	4.01	3.48	3.88	4.04	3.91	1.62	2.03	2.06	2.15	4.46	4.86
Baltimore	5.83	6.15	5.35	5.66	6.94	6.47	3.16	3.49	4.32	2.66	4.57	4.90
Boston	-.83	2.27	-1.00	2.11	2.54	2.38	-5.30	-2.00	-1.72	.41	1.44	4.73
Chicago	3.57	5.42	4.36	6.20	5.78	6.56	-1.62	.30	.68	2.06	3.80	5.71
Cleveland	4.71	5.01	4.91	5.22	4.95	5.15	1.69	1.99	1.93	5.26	7.23	7.51
Dallas	7.00	8.18	5.93	7.12	8.50	7.45	-1.64	-.34	.02	3.32	4.75	5.95
Detroit	3.26	4.33	3.07	4.13	5.04	4.84	.40	1.46	2.19	.66	3.60	4.64
Houston	4.76	5.79	4.60	5.63	6.57	6.41	-3.49	-2.34	-1.51	5.46	8.13	9.14
Indianapolis	.31	.31	-.18	-.18	.36	-.12	-2.46	-2.46	-2.41	3.22	5.81	5.81
Los Angeles	5.28	7.52	4.56	6.82	7.72	7.01	.76	3.04	3.25	1.85	3.31	5.51
Milwaukee	3.17	3.17	3.14	3.14	3.22	3.18	1.20	1.20	1.25	1.25	1.95	1.95
New York City	6.43	7.39	6.04	7.00	7.90	7.51	.83	1.81	2.34	2.04	3.23	4.20
Philadelphia	5.91	6.41	4.73	5.24	6.70	5.53	1.51	2.04	2.34	1.50	2.85	3.36
Saint Louis	3.10	3.60	3.13	3.63	4.86	4.89	.34	.82	2.13	1.30	4.72	5.22
San Diego	-.98	.47	-.96	.50	1.42	1.45	-1.99	-.54	.43	-.58	.05	1.49
San Francisco	4.31	5.18	4.63	5.50	5.93	6.25	.02	.93	1.71	-.46	2.30	3.19
Washington, D.C.	3.95	5.93	3.61	5.60	7.06	6.73	1.36	3.38	4.54	-.84	.49	2.50

by the conventional method, and *crime* is the number of reported crimes per 1,000 population. In Table 9 we have aggregated these composite estimates for Groups 1, 2, and 3. We see that the highest undercounts are in cities and the lowest are in Group 3 areas, especially when "conventional" states are removed. There is little overlap between the estimates for Group 1 and Group 3 areas. Estimates for Group 3 areas are usually lower than estimates for Group 2 areas. For any given minority proportion, areas with a higher reported crime rate, and those with more of the population counted by the conventional method, have higher estimates. Outside the cities, the undercount rates are high in western states, where reported crime rates are higher and the conventional method was more likely to be used. Finally, the standard error of the weighted averages was less than 1% in all cases, averaging .8 across the 16 cities and .6 across the 50 states and state remainders, where sample sizes were larger. Corresponding average standard errors for the original sample estimates are 2.0 in cities and 1.0 in the states and state remainders. Thus the indicated reduction in variation was by a factor of about 4.

We consider this analysis to improve upon two alternatives. It improves upon the original sample estimates because the standard errors are smaller, reflecting gains provided by the systematic nature of the relationships between the undercount and its predictors. It improves upon simple synthetic estimation and the use of sample estimates displayed in Table 5 by using the additional information provided by other predictor variables. Note that all three sets of estimates lead to the same general conclusion. Any good adjustment will shift population shares into central cities principally from rural and suburban areas with small minority populations. The composite procedure provides greater precision, but an elaborate model is not necessary to improve upon the raw counts.

5.4 The Effect of Choosing Among the PEP Series

One of the major criticisms of Freedman and Navidi was that use of the regression model did not solve the problem of deciding which PEP series to use. They found no basis of choice between Series 2-9 and 10-8 and, since they appeared to give very different results, said that neither should be used. More generally, Keyfitz (1983) argued:

> witnesses . . . who say that the census count is an estimate, that the estimate is subject to substantial error, and that several methods are available that would bring the numbers closer, *on the average,* to the true population, are entirely right. In fact adjustments could be devised, any one of which might make the figures better overall. And in that lies the principal difficulty: there is no way of choosing among the methods and each would lead to different results. (p. 5)

We disagree with Keyfitz's statement on the grounds of basic statistical policy. That there are many ways to improve an estimate is no reason not to choose one. The census itself is full of subjective judgments—for example, which models to use when imputing either to create person records or to substitute for missing data. We say that it is equally reasonable to use subjective judgment to select a PEP series. If one series, say 2-9, is not clearly better than all of the others, then an appropriate strategy may be to average the best competitors.

Even so, we would feel better about our results if the impact of selecting a PEP series is minor. In this section we analyze variations among regression estimates obtained with different PEP series. We first evaluate the variations among all 12 series, and then restrict our attention to the eight most reasonable ones. When choice of a series does not matter much to the results, then either choosing one or averaging them is sensible. When choice of a series does matter, then a more painstaking analysis of the origin and results of each would be appropriate.

5.4.1 Empirical Results. Our strategy was to compute the composite estimates for each PEP series using as predictor variables the reported crime rate and the minority and conventional percentages. In Tables 5 and 6 we saw that the national uncercount rates depended on which PEP series was used, but cities consistently had the largest undercounts. Much of the difference between the Series 2-9 and 10-8 composite estimates that Freedman and Navidi observed occurred because the national rate for Series 2-9 was 1.3 percentage points higher than the rate for Series 10-8. Because national rates do not affect the population shares, we wanted to compare different estimates for the same areas with the national "effects" subtracted out. In Table 10 we present the composite estimates in

Table 9. A Summary of Composite Estimates of Undercount Based on PEP Series 2-9

Area unit	Median	Range[a]	75% range[b]	50% range[c]
Group 1 (16 areas)	5.48%	1.11%–7.15%	2.46%–6.46%	3.64%–6.05%
Group 2 (20 areas)	1.28%	−.31%–3.50%	.55%–3.18%	.92%–2.28%
Group 3 (30 areas)	.67%	−1.25%–3.32%	−.84%–1.77%	−.28%–1.11%
Group 3, excluding conventional states[d]	.61%	−1.25%–2.14%	−.84%–1.14%	−.39%– .98%

NOTE: Groups 1, 2, and 3 are the same as in Table 5.
[a] Ranges are of results for as many of the 66 areas as fall in the indicated group.
[b] This is the range in which the middle 75% of estimates fall.
[c] This is the range in which the middle 50% of estimates fall.
[d] This group excludes five states where 75%–100% of the population was counted by the conventional method (Alaska, Montana, North Dakota, South Dakota, and Wyoming).

Table 10. Residual Undercount Rates (percentages) Obtained From Composite Estimates: 12 PEP Series

Area	PEP Series 2-8	2-9	3-8	3-9	2-20	3-20	5-8	5-9	10-8	14-8	14-9	14-20	Area effect[a]	RMS residual area[a]
Saint Louis	.32	1.09	.23	1.02	1.53	1.45	.08	1.07	-1.51	-2.48	-1.67	-1.14	4.16	1.31
Washington, D.C.	.35	1.31	.22	1.15	1.72	1.56	-.54	.38	-1.55	-2.43	-1.28	-.90	3.47	1.28
Detroit	.43	1.24	.30	1.09	1.59	1.44	-.32	.53	-1.44	-2.33	-1.45	-1.10	3.30	1.25
Boston	.23	.82	.14	.75	1.11	1.03	.16	.90	-1.14	-1.83	-1.29	-.90	3.17	.98
Dallas	.52	1.22	.40	1.11	1.40	1.27	-.19	.52	-1.27	-2.23	-1.56	-1.19	3.10	1.21
Baltimore	.46	1.04	.30	.87	1.31	1.14	-.41	.24	-1.29	-1.75	-1.06	-.84	2.89	1.00
New York City	.60	1.15	.42	.96	1.31	1.12	-.61	-.05	-1.31	-1.64	-1.07	-.89	2.66	1.02
Los Angeles	.26	1.20	.05	.93	1.25	1.00	-.57	.13	-1.31	-1.71	-.63	-.61	2.63	.94
Cleveland	.33	.85	.27	.79	1.03	.96	-.19	.40	-1.06	-1.55	-1.00	-.84	2.51	.86
San Francisco	.20	.58	.14	.54	.76	.71	-.09	.34	-.94	-1.08	-.71	-.47	1.91	.62
Alaska	-.27	-.27	-.26	-.27	-.44	-.44	.67	.79	.73	-.01	-.02	-.20	1.70	.44
Houston	.39	.84	.31	.75	.99	.90	-.15	.26	-.66	-1.58	-1.18	-.88	1.44	.84
New Mexico	.12	.22	.11	.18	.27	.25	.42	.70	.36	-.99	-.89	-.75	1.39	.53
Wyoming	-.16	-.26	-.19	-.30	-.54	-.57	.44	.44	.67	.34	.22	-.10	1.30	.39
Chicago	.28	.89	.35	.91	.97	.98	-.47	-.07	-.77	-1.49	-.84	-.73	1.30	.82
Nevada	-.13	.44	-.20	.36	.46	.38	-.10	.14	-.58	-.61	-.10	-.07	1.13	.35
Arizona	-.13	.03	-.05	.12	.10	.18	.23	.45	-.17	-.41	-.22	-.14	.97	.22
California (R)	.25	.38	.16	.29	.35	.26	-.38	-.31	-.57	-.20	-.09	-.15	.91	.31
Philadelphia	.33	.56	.17	.39	.54	.38	-.56	-.39	-.67	-.41	-.12	-.22	.73	.43
San Diego	-.04	.13	-.02	.15	.28	.30	.01	.20	-.28	-.44	-.23	-.06	.53	.22
Florida	.02	.13	.00	.12	.22	.21	.21	.36	-.20	-.48	-.36	-.24	.47	.25
Hawaii	-.08	-.17	-.06	-.14	-.11	-.08	.33	.33	.26	-.03	-.18	-.06	.41	.18
Montana	-.24	-.43	-.21	-.40	-.56	-.54	.45	.34	.89	.41	.19	.10	.31	.45
Milwaukee	.22	.20	.19	.19	.16	.14	-.32	-.30	-.37	-.01	.00	-.09	.28	.21
Colorado	-.13	-.05	-.21	-.12	.00	-.07	.52	.60	.30	-.38	-.28	-.18	.16	.30
Idaho	-.25	-.51	-.23	-.50	-.61	-.60	.44	.25	.75	.64	.36	.25	.04	.48
South Carolina	.15	.21	.13	.18	.23	.19	-.42	-.42	-.34	-.02	.08	.04	-.05	.24
Louisiana	.12	.17	.12	.18	.22	.22	-.25	-.25	-.19	-.16	-.10	-.06	-.19	.18
New Jersey	.11	-.10	.10	-.09	-.06	-.06	-.12	-.23	-.12	.35	.10	.11	-.26	.15
Washington	.05	-.24	.04	-.22	-.17	-.17	.13	-.03	.10	.37	.02	.11	-.35	.17
Maine	-.01	-.33	.01	-.31	-.53	-.51	-.05	-.37	.40	.88	.53	.29	-.45	.43
Maryland (R)	-.03	-.13	-.03	-.12	-.21	-.20	-.08	-.20	-.01	.45	.36	.21	-.45	.21
Delaware	-.14	-.33	-.05	-.21	-.25	-.12	.29	.25	.29	.16	.01	.11	-.56	.21
Oregon	-.22	-.29	-.18	-.24	-.29	-.25	.33	.22	.39	.25	.13	.14	-.56	.25
Utah	-.36	-.36	-.30	-.30	-.54	-.48	.06	-.02	.17	.78	.83	.53	-.56	.46

residual form. We took the 66 composite estimates for each of the 12 PEP series, and subtracted out the local area and series average estimates, which we refer to as area and series "effects."

The table should be read as follows. The average among the (66 × 12 =) 792 composite estimates is 1.24%. The Series 2-8 average is 1.43%, so the Series 2-8 effect is (1.43 − 1.24 =) .19. Using Saint Louis as an example, the average among 12 estimates is 5.40%, so the Saint Louis effect is (5.40 − 1.24 =) 4.16. Combining, the fitted value based on Series 2-8 for Saint Louis was (1.24 + .19 + 4.16 =) 5.59%. The actual Series 2-8 composite estimate was 5.81, and (5.81 − 5.59 =) .32.

We ranked the areas by the area effects and separated the eight "preferred" PEP series from the other four. The variations in residuals form a clear pattern. Positive residuals are concentrated in the upper-left and lower-right sections of the table, where both area and series effects are either positive or negative. We find negative residuals in the opposite sections, where the area and series effects have opposite signs. This means that there is more variability among PEP Series 2, 3, and 5 than among PEP Series 10 and 14, and there is more variability among different estimates in areas like Saint Louis and West Virginia, with extreme undercount rates, than in areas like South Carolina and Louisiana, where the undercount is near the national average. In other words, when the "better" series (2, 3, and 5) are used, areas like Saint Louis and West Virginia diverge more from each other and the national average than when Series 10 and 14 are used.

The average residual is not large. For each area we computed the root-mean-squared residual, listed in the last column of Table 10. Corresponding root mean squares for the PEP series are shown on the bottom row of the table. The root mean square among all 792 residuals is .59. In contrast, the root mean square of the 66 area effects is 1.60. The area effect is more than twice the root-mean-squared residual for 47 of the 66 areas. The exceptions occur in areas near the middle of the table, where adjustments are small.

Averaging the residuals among all 12 series would be appropriate if each of the series was equally good and should receive equal weight. In Table 11 we eliminate the four less-preferred series from consideration and average the other eight. The root-mean-squared residual drops from .59 to .33. The area effects become more variable, as the root mean square of the 66 area effects increases from 1.60 to 1.92. The area effect is more than twice the root-mean-squared residual for 59 of the 66 areas.

Table 10 (continued)

Area	PEP Series												Area effect[a]	RMS residual area[a]
	2-8	2-9	3-8	3-9	2-20	3-20	5-8	5-9	10-8	14-8	14-9	14-20		
South Dakota	−.36	−.72	−.38	−.76	−.95	−.96	.78	.52	1.39	.81	.43	.20	−.57	.76
Texas (R)	−.05	.02	−.02	.02	.15	.16	.29	.33	.30	−.51	−.46	−.24	−.65	.27
Minnesota	−.12	−.28	−.06	−.21	−.49	−.43	−.41	−.65	.07	1.08	.88	.60	−.69	.54
Kansas	−.09	−.32	−.09	−.32	−.38	−.37	.12	−.09	.35	.58	.32	.27	−.74	.31
Michigan (R)	−.04	−.24	−.06	−.25	−.37	−.38	−.15	−.33	.15	.75	.54	.37	−.74	.36
North Carolina	.00	−.13	.06	−.06	−.15	−.09	−.22	−.39	.03	.40	.31	.24	−.75	.22
Indianapolis	.08	−.03	.07	−.05	−.05	−.05	.08	−.02	.21	.02	−.14	−.12	−.76	.09
Georgia	−.05	−.10	−.12	−.17	.03	.00	.26	.23	.23	−.14	−.17	.00	−.81	.15
Wisconsin (R)	.12	−.27	.23	−.14	−.61	−.51	−.88	−1.29	−.11	1.57	1.17	.72	−.84	.80
Mississippi	.06	.07	.09	.09	−.03	−.01	−.31	−.43	.02	.16	.23	.06	−.85	.18
North Dakota	−.43	−.65	−.29	−.52	−.96	−.83	.32	.06	1.21	.96	.73	.41	−.86	.69
Illinois (R)	.30	.12	.23	.07	−.17	−.20	−.71	−.95	−.25	.76	.56	.26	−.88	.47
Virginia	−.02	−.11	−.18	−.28	−.21	−.36	−.04	−.20	.24	.48	.40	.26	−.93	.27
Nebraska	−.29	−.56	−.21	−.48	−.69	−.61	.22	−.06	.68	.90	.62	.47	−.94	.54
Ohio (R)	.08	−.22	.09	−.15	−.35	−.29	−.31	−.60	.05	.83	.51	.36	−.96	.39
Rhode Island	.02	−.33	.10	−.23	−.35	−.26	−.01	−.29	.05	.73	.29	.29	−.96	.31
Alabama	−.05	−.18	.06	−.07	.02	.10	−.09	−.21	.21	.10	−.03	.15	−1.01	.12
Connecticut	−.07	−.25	−.12	−.31	−.28	−.36	.15	−.02	.27	.47	.27	.25	−1.09	.26
Missouri (R)	.03	−.29	.00	−.32	−.47	−.49	−.39	−.74	.06	1.17	.85	.60	−1.26	.56
Oklahoma	.05	−.28	.10	−.22	−.30	−.23	.21	−.08	.49	.33	−.09	.03	−1.51	.24
Indiana (R)	−.22	−.57	−.10	−.44	−.67	−.54	.48	.00	.62	.75	.39	.29	−1.59	.48
Arkansas	−.16	−.43	−.09	−.36	−.38	−.32	.05	−.25	.46	.67	.41	.41	−1.63	.37
Massachusetts (R)	−.46	−.49	−.31	−.34	−.51	−.36	.08	−.13	.54	.68	.65	.62	−1.69	.47
Iowa	−.18	−.57	−.11	−.50	−.70	−.63	.13	−.25	.52	1.08	.67	.52	−1.72	.56
Tennessee	−.25	−.45	−.12	−.33	−.48	−.35	.21	−.02	.63	.51	.32	.32	−1.76	.37
Vermont	−.27	−.75	−.26	−.74	−.56	−.54	.19	−.21	.43	1.20	.68	.83	−1.88	.63
New Hampshire	−.30	−.67	−.17	−.53	−.67	−.55	.28	−.10	.63	.96	.57	.57	−1.91	.55
New York (R)	−.39	−.87	−.21	−.68	−.93	−.73	.79	.48	1.11	.78	.37	.29	−2.22	.69
Pennsylvania (R)	−.18	−.51	−.17	−.50	−.65	−.64	−.14	−.57	.48	1.23	.90	.72	−2.23	.63
Kentucky	−.19	−.60	−.13	−.53	−.60	−.54	.17	−.27	.74	.92	.50	.51	−2.25	.53
West Virginia	−.11	−.60	−.02	−.50	−.75	−.66	−.14	−.70	.62	1.36	.85	.67	−2.54	.68
Series effect[a]	.19	.66	.03	.51	1.02	.86	.78	1.23	−.66	−1.97	−1.49	−1.12		
RMS residual series[b]	.24	.57	.20	.50	.69	.62	.36	.44	.69	1.02	.66	.50		

NOTE: R indicates the remainder of a state after one or more central cities have been removed. A vertical rule separates the eight PEP estimates that we prefer from the four that we regard as less trustworthy.

[a] The area, or series, effect is the average for that area, or series, with the grand mean subtracted out.

[b] Residuals are calculated as adjustment − (area average − grand mean) − (series average − grand mean) − grand mean = adjustment − area average − series average + grand mean. Here the grand mean, taken over all 66 × 12 = 792 estimates, is 1.24.

5.4.2 Comparing April and August Results. Differences between April and August data contribute substantially to the residuals, and the root-mean-squared difference between Series 2-9 and 5-9 composite estimates is .82. Yet there is clearly a strong relationship between the two series (Figs. 1 and 2). We looked to see if there was a pattern to the differences, since we suspected a northeast to southwest pattern of migration between April and August. With no data for this time period available, we used 1975–1980 migration rates as a proxy. We find that the Series 2-9/5-9 differences correlate with the pattern of between-state migration. For example, among the 11 areas with the highest rates of in-migration, the average Series 5-9 estimate is .47 percentage points higher than the average Series 2-9 estimate. As migration rates fall, this difference decreases systematically as shown in Table 12. As the last column shows, the results are in agreement with those found by prorating the five-year in-migration percentage over four months. A higher rate of in-migration caused Series 5-9 to increase relative to Series 2-9. This indicates that the August data were contaminated by postcensal moves and supports our choice of the April data.

6. ADDITIONAL CRITICISMS MADE BY FREEDMAN AND NAVIDI

Freedman and Navidi argue (see Sec. 3.3) that we cannot reliably choose the independent regression variables. We have three responses to this. First, our algorithm led us to select the same set of predictor variables—reported crime and the percents minority and conventional—for each of the eight "good" PEP series. For the other four series, the best set of predictor variables was limited to the reported crime rate and the percent conventional. Second, if we amend the results of Table 10 by changing the regression equation for Series 10-8, 14-8, 14-9, and 14-20 to include just the reported crime rate and percent conventional, but retain the "best equation" for the other eight series, the root-mean-squared residual is scarcely affected, rising from .59 to .61. The root mean square among the 66 area effects is reduced slightly, from 1.60 to 1.58, and the area effect is at least twice the root-mean-squared residual for 45 of the 66 areas. Third, we looked to see how results changed when we substituted the census percent urban for the reported crime rate. Freedman and Navidi argued that even though there was no substantive

Table 11. Deviations, From the National Mean, of Average Adjustments, Averaged Over the Best Eight Series, for 66 Areas

Area	Area effect*	Root mean squared residual	Area	Area effect	Root mean squared residual
Saint Louis	5.01	.53	Oregon	−.68	.23
Washington, D.C.	4.24	.73	Maine	−.72	.20
Detroit	4.09	.61	Indianapolis	−.76	.06
Dallas	3.88	.52	Georgia	−.80	.15
Boston	3.81	.38	Utah	−.85	.19
Baltimore	3.50	.54	North Carolina	−.87	.13
New York City	3.27	.62	Mississippi	−.91	.19
Los Angeles	3.16	.61	South Dakota	−.93	.62
Cleveland	3.07	.39	Kansas	−.94	.17
San Francisco	2.31	.28	Michigan (R)	−.97	.12
Houston	1.97	.37	Minnesota	−1.02	.19
Chicago	1.77	.51	Illinois (R)	−1.04	.42
New Mexico	1.67	.18	Alabama	−1.07	.10
Alaska	1.63	.46	Virginia	−1.11	.11
Nevada	1.29	.26	Rhode Island	−1.14	.17
Wyoming	1.16	.36	Ohio (R)	−1.18	.21
Arizona	1.08	.17	Connecticut	−1.25	.16
California (R)	1.03	.28	Wisconsin (R)	−1.26	.48
Philadelphia	.91	.40	North Dakota	−1.27	.41
San Diego	.65	.12	Nebraska	−1.28	.29
Florida	.62	.11	Missouri (R)	−1.60	.24
Hawaii	.40	.19	Oklahoma	−1.61	.18
Milwaukee	.33	.21	Indiana (R)	−1.85	.36
Colorado	.23	.29	Arkansas	−1.88	.15
Montana	.10	.36	Tennessee	−1.99	.22
South Carolina	−.02	.26	Massachusetts (R)	−2.00	.19
Louisiana	−.13	.19	Iowa	−2.07	.27
Idaho	−.22	.37	New Hampshire	−2.25	.31
New Jersey	−.32	.11	Vermont	−2.28	.30
Washington	−.43	.13	New York (R)	−2.55	.60
Texas (R)	−.54	.13	Kentucky	−2.59	.26
Maryland (R)	−.58	.07	Pennsylvania (R)	−2.65	.26
Delaware	−.63	.21	West Virginia	−2.98	.28

NOTE: R indicates the remainder of a state after one or more central cities have been removed.

* The area effect is the average for that area with the grand mean subtracted out. Estimates included in the average are Series 2-8, 2-9, 3-8, 3-9, 2-20, 3-20, 5-8, and 5-9. All are expressed in residual form, having had the grand mean, 1.90, calculated across 66 × 8 = 528 estimates subtracted.

reason for this substitution to matter, the empirical results seemed to show that it did. We found that when we substituted the percent urban for the reported crime rate, σ^2 was larger for each of the 12 PEP series. The average σ^2 when the reported crime rate was used was .38; when the census percent urban was substituted it increased to .56. We also found that the standard errors of the 66 area estimates were consistently smaller when the reported crime rate was used. The average standard error was .55 when the reported crime rate was used, but it rose to .63 when the percent urban was used. This is to be expected, since the standard errors are increasing func-

Table 12. Average Difference of Composite Estimates Based on PEP Series 2-9 and 5-9 for Areas Grouped by Rates of In-Migration From Other States

Group of areas[a]	Range of in-migration percentages	Average values[b] Series 5-9	Series 2-9	Difference	Difference calculated from in-migration[c]
One (1–11)	16.9–31.5	+.94%	+.47%	+.47%	+.71%
Two (12–22)	12.6–16.9	−.33%	−.61%	+.28%	+.21%
Three (23–33)	10.6–12.4	+.25%	+.20%	+.05%	−.01%
Four (34–44)	8.4–10.5	−1.18%	−1.10%	−.08%	−.15%
Five (45–55)	6.5–8.2	−.79%	−.46%	−.33%	−.29%
Six (56–66)	3.2–5.8	+1.10%	+1.49%	−.39%	−.47%

NOTE: In-migration rates are the proportions of the population age 5 and older in 1980 who lived in another state in 1975.

[a] Areas are grouped by their ranking according to in-migration rates.

[b] Undercount estimates are expressed in the form of residuals as shown in Table 10, so the mean difference between series is forced to be 0.

[c] These figures are, except removal of the mean, one-fifteenth (April through August is one-third of a year or one-fifteenth of five years) of the mean in-migration percentage.

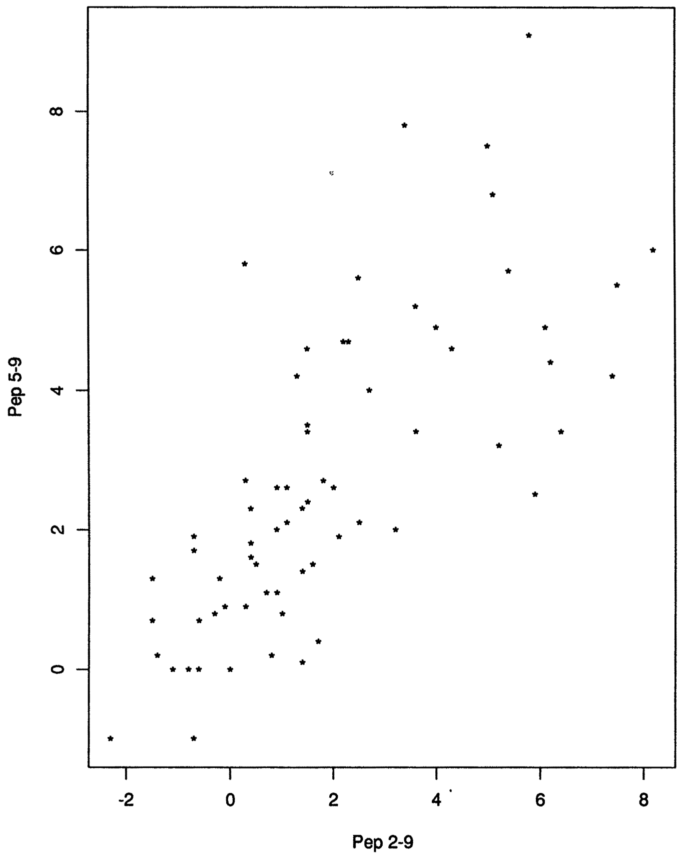

Figure 1. Raw PEP Undercounts.

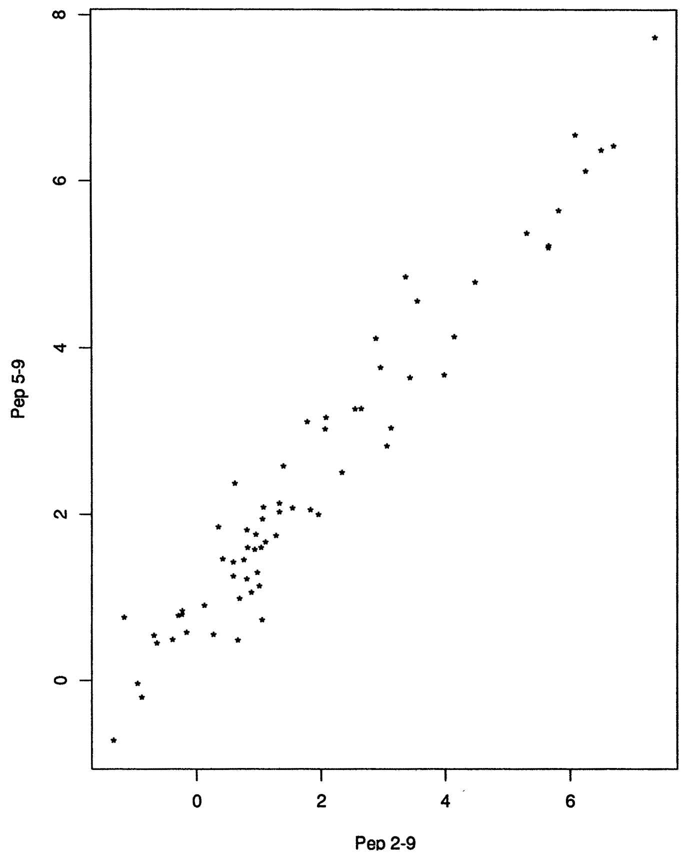

Figure 2. PEP Undercounts After Regression.

tions of σ, which is minimized by the use of the crime rate.

In Table 13 we compare the estimates obtained when we substitute the percent urban for the reported crime rate. Most (55 of 66) of the differences are less than .50 percentage points. Only five of the differences are greater than 1.0, and two are greater than 2.0. All of these occurred in central cities, and large differences occur in cities where reported crime rates are very high, such as Saint Louis (143 per 1,000 population) and Boston (135), or very low, such as Chicago (66), Philadelphia (60), and Indianapolis (53). The root-mean-squared difference is .54.

Reliance on statistical criteria directs us to use the reported crime rate instead of the census percentage urban. The large effects that extremes in the reported crime rates have on the estimates do cause some worry, though. Reported crime rates can be influenced by the size of downtown areas, where few people live but many crimes occur, or by idiosyncrasies of local police departments. Neither of these factors should make it harder to take a census. Ideally, we would prefer not to rely on the reported crime rate as a predictor variable, and we would like to substitute variables more closely related to the census-taking process. Examples other than demographic characteristics of the local populations might include the proportion of the population living in centralized district office areas, the proportion of cases enumerated by closeout or last-resort procedures, or a list of census-taking problems. Such variables were not available to us but would be available to the Census Bureau to carry out a future adjustment.

6.1 Extrapolating to Areas Other Than the 66 Sample Areas

One can take a variety of approaches to make estimates for (smaller) local areas, either singly or in combination (see National Research Council 1985). The simplest thing to do is to apply our best estimate(s) of percentage of undercount for each geographic area to each of the local areas of which it is composed. This can be done once for all, or separately by race and sex. Tukey (1983) showed that if the adjustments at the geographic-area level improve the person-weighted mean squared error, there will be a corresponding improvement at the local-area level. This approach is an improvement.

Another extreme would be to use the regression coefficients found for the 66 areas, applying them separately to each local area. This could cause some large errors for local areas whose reported crime rate or percent minority was extreme, and thus outside the ranges obtained for the 66 areas. There is no guarantee that the best coefficients among the local areas within larger geographic areas are the same as the best coefficients among the larger areas. Indeed, equality would be rather surprising. Using these coefficients within geographic areas guarantees that the total adjustments for the local areas making up a geographic area will give the adjustment previously found for

Table 13. Comparisons of Composite Estimates Obtained When the Percent Urban Is Substituted for the Reported Crime Rate

Area	Set A: crime rate	Set B: percent urban	Difference	Area	Set A: crime rate	Set B: percent urban	Difference
Saint Louis	4.16	2.08	2.08	Oregon	−.56	−.81	.25
Washington, D.C.	3.47	3.37	.10	Utah	−.56	−.26	−.30
Detroit	3.30	2.94	.36	South Dakota	−.57	−.45	−.12
Boston	3.17	1.07	2.10	Texas (R)	−.65	−.38	−.27
Dallas	3.10	2.14	.96	Minnesota	−.69	−.48	−.21
Baltimore	2.89	2.76	.13	Kansas	−.74	−.70	−.04
New York City	2.66	2.47	.19	Michigan (R)	−.74	−.86	.12
Los Angeles	2.63	2.48	.15	North Carolina	−.75	−.82	.07
Cleveland	2.51	2.21	.30	Indianapolis	−.76	.53	−1.29
San Francisco	1.91	1.07	.84	Georgia	−.81	−.88	.07
Alaska	1.70	1.53	.17	Wisconsin (R)	−.84	−.72	−.12
Houston	1.44	1.99	−.55	Mississippi	−.85	−.45	−.40
New Mexico	1.39	1.64	−.25	North Dakota	−.86	−.76	−.10
Wyoming	1.30	1.57	−.27	Illinois (R)	−.88	−.56	−.32
Chicago	1.30	2.45	−1.15	Virginia	−.93	−.63	−.30
Nevada	1.13	.69	.44	Nebraska	−.94	−.76	−.18
Arizona	.97	.71	.26	Ohio (R)	−.96	−.80	−.16
California (R)	.91	1.09	−.18	Rhode Island	−.96	−.65	−.31
Philadelphia	.73	2.03	−1.30	Alabama	−1.01	−1.01	.00
San Diego	.53	.57	−.04	Connecticut	−1.09	−.96	−.13
Florida	.47	.25	.22	Missouri (R)	−1.26	−1.05	−.21
Hawaii	.41	.28	.13	Oklahoma	−1.51	−1.42	−.09
Montana	.31	.13	.18	Indiana (R)	−1.59	−1.62	.03
Milwaukee	.28	1.12	−.84	Arkansas	−1.63	−1.48	−.15
Colorado	.16	−.01	.17	Massachusetts (R)	−1.69	−1.54	−.15
Idaho	.04	.06	−.02	Iowa	−1.72	−1.80	.08
South Carolina	−.05	−.12	.07	Tennessee	−1.76	−2.32	.56
Louisiana	−.19	.13	−.32	Vermont	−1.88	−2.42	.54
New Jersey	−.26	.04	−.30	New Hampshire	−1.91	−2.22	.31
Washington	−.35	−.56	.21	New York (R)	−2.22	−2.09	−.13
Maine	−.45	−.50	.05	Pennsylvania (R)	−2.23	−1.91	−.32
Maryland (R)	−.45	−.18	−.27	Kentucky	−2.25	−1.93	−.32
Delaware	−.56	−.92	.36	West Virginia	−2.54	−2.50	−.04

NOTE: Figures shown are averages of 12 estimates, one for each PEP series, with the national average subtracted out. R indicates the remainder of a state after one or more central cities have been removed. Regression equations also include the percent minority and percent conventional.

that area. It is plausible to anticipate improvement at the local level.

Freedman and Navidi found 11 counties with extreme values on the reported crime rate. They showed that substituting the census percent urban for the reported crime rate in the optimal three-variable regression equation greatly changed the estimated undercount. This does not mean that the estimates based on the reported crime rate are wrong. As an informal test of the consistency of estimates for a sample of 102 cities, remainders of SMSA's, and nonmetropolitan counties, Ericksen and Kadane (1987) compared regression estimates obtained with PEP Series 2-8 and the predictors in Equation (2) with those regression estimates obtained when the census percentage urban replaced the reported crime rate. The estimates were similar, with 63 of the 102 differences less than 1.0% and 93 less than 2.0%. Larger differences occurred for areas with extreme values for the reported crime rate or percent minority.

Part of the problem of extrapolation results from the internal heterogeneity of the 50 states and state remainders. It would be better to define the PEP sample areas so that they would be more homogeneous with respect to census-taking conditions. For example, instead of having Baltimore, Washington, D.C., the remainder of Maryland, and Virginia defined as four sample areas, we might define minority areas of large cities such as Baltimore, Richmond, and the District of Columbia as one sampling unit, the remainders of the cities and suburban areas in Maryland and Virginia as a second sampling unit, small cities and towns as a third sampling unit, and rural areas as a fourth sampling unit. Use of more homogeneous areas would promote the better estimation of the initial regression equation, and make it easier to extrapolate beyond the sample areas. The Census Bureau has proposed such a sampling plan for the 1990 Post Enumeration Survey (Woltman et al. 1988).

6.2 Other Criticisms

Freedman and Navidi argued that the standard errors of the composite estimates for the 66 areas were too small. To demonstrate this, they simulated our estimation process for PEP Series 10-8. They did this by adding random-error terms to the sample estimates and replicating the predictor selection and regression calculation 100 times. They estimated the standard error of the composite estimate to be the root-mean-squared error among the 100 estimates of the same area. When they averaged these root-mean-squared errors over the 66 areas, the combined average was 1.17, whereas our mean standard error of composite estimates was .82. We believe that they exaggerated the increase. They limited themselves to three-variable models, but we found the optimal predictor variable set for Series 10-8 to include only the reported crime rate and the percent minority. Nevertheless, even Freedman and Navidi found that the use of the composite procedure provided a substantial improvement over the use of sample estimates, for which the root mean variance was 1.59.

We use the composite procedure to improve upon the sample estimates. In most cases, the sample and composite estimates are close. For Series 2-9, the sample and composite estimates are within 1% in 51 of the 66 areas and within 2% in 59 of them. Where this does not occur, the area is a city, where sample sizes are smaller and the regression model receives greater weight, or a state like South Carolina or Tennessee, where the sample estimates seem clearly wrong. For example, the Series 2-8 sample estimates for those two states are 5.8% and −2.9%, respectively, whereas the corresponding estimates for the other nine states of the Old Confederacy range between 2.2% and −1.0%. With no substantive reason to believe that counting conditions are greatly different in South Carolina and Tennessee, it is reasonable to use the regression model to smooth out sampling fluctuations.

7. CONCLUSION

Our major substantive finding is that the largest undercounts of the 1980 census occurred in central cities with large minority populations. Above-average undercounts occurred in many western states, especially where the Census Bureau relied on the conventional method. Undercounts were very low in the northeast and north-central regions outside of large cities as well as in states of the "upper south," such as West Virginia and Kentucky. This conclusion does not depend on selecting a particular PEP series or a set of independent variables. Indeed, even if we ignore the PEP data and rely on synthetic estimation, adjustment shifts population to central cities from states and state remainders with fewer than the average shares of minorities.

For the 66 areas included in our study, we are confident of improving upon the raw census count, especially in those areas with large undercounts or overcounts where an adjustment is most needed. Our findings do not permit definitive conclusions for suburban areas, for central cities other than the 16 included in our data set, or for other rural or urban parts of individual states. To compute estimates for such areas, we would prefer not to extrapolate from the regression equations presented in this article. Instead, we would prefer to regroup the CPS sample areas, going from the 16 central cities and 50 states and state remainders to the more homogeneous areas described in Section 6. We would then follow our composite procedure with these redefined units, perhaps with different predictor variables.

We argue forcefully for an adjustment because we are convinced that it would improve the census. The expensive coverage-improvement programs relied on by the Census Bureau in 1980 did not work, and there were 5–6 million erroneous enumerations (Cowan and Fay 1984), a substantial increase over 1970. Moreover, the Census Bureau (1988a, p. 78) reports that the improvements were concentrated in the south, leaving northern cities untouched and actually increasing their differential undercount.

In our view, much of the argument against adjustment is nonstatistical. The Population Association of America (1986) reported that members of its Committee on Population Statistics

> expressed concern about three elements of adjustment: (1) the possibility of opening up the census to political tinkering; (2) the tremendous waste of Census Bureau resources on lawsuits and litigation that will inevitably follow the next census, no matter what happens; and (3) the lack of attention to implications of adjustment for detailed tabulations by social and economic variables and small geographic units. (p. 2)

In considering these points, we should remember that the coincidence of demographic estimates demonstrating the differential undercounting of Blacks with the observed problems of census-taking in areas where Blacks and Hispanics live gave rise to the lawsuits. When the trials took place, it was a simple matter to show that by failing to adjust, the Census Bureau ignored a clear bias, and it was reasonable for plaintiffs at least to have their day in court. We believe that the Census Bureau creates political difficulties for itself when it ignores the undercount. The bureau will put itself in a better position by making its best effort, using available statistical and demographic methods, to adjust for the undercount. Errors will remain, but they will be smaller and we will no longer know in advance who is losing money and power because of the undercounting.

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