Technical Report 537, Department of Statistics, U.C. Berkeley
To appear in *Jurimetrics*

by Lawrence D. Brown, Morris L. Eaton, David A. Freedman, Stephen P. Klein, Richard A. Olshen, Kenneth W. Wachter, Martin T. Wells, and Donald Ylvisaker.

ABSTRACT

This paper is a discussion of Census 2000, focusing on planned use of sampling techniques for adjustment. Past experience with similar adjustment methods suggests that the design for Census 2000 is quite risky. The planned use of sampling to obtain population counts for apportionment was rejected by the Supreme Court as violating the Census Act. Statistical adjustments for the purpose of redistricting may or may not be constitutional.

I. INTRODUCTION

The census has been the subject of high-profile controversy since the 1980s.[1] In 1980 and 1990, the major cases revolved around proposed adjustments to remedy the differential undercount of minorities. For Census 2000, sampling was the focus of the conflict. Sampling came into the Bureau's proposed design in two main ways.

(i) Only a sample of households that do not respond to the census by mail was to be followed up by interviewers; in the past, all non-responding households were followed up.

(ii) A large sample survey after the enumeration was to be used as the basis for adjustment of census counts to compensate for under- and over-enumeration. This use of sampling was called "ICM" or "Integrated Coverage Measurement."

Sample-based non-response followup has been dropped from the plan, since the courts have ruled that this use of sampling—for purposes of apportionment—would violate the Census Act.[2] That is the topic of Section II.

Despite the name, ICM is not an inseparable part of the census operation. Instead, it is a larger version of the Post Enumeration Survey (PES) that was developed for adjusting the census in 1990. In turn, the PES was a modification of the "Post Enumeration Program" for the census of 1980. These proposals for adjustment are all based on a capture-recapture technique called "dual system estimation:" capture is in the census, recapture is in the post enumeration survey. Details are given below. The ICM is still part of the Bureau's plan for Census 2000, although the acronym has been changed to ACE (Accuracy and Coverage Evaluation). The focus of Sections III–X is the plan for Census 2000 that was reviewed by the courts, although most of the statistical considerations apply as well to the current plan for ACE (Section XI).

In the past, there have been census undercounts that differ by race and ethnicity, sex, and age. Such differentials can distort political representation and the allocation of tax funds. Adjustment is meant to correct these inequities by changing the way resources are shared out. However, adjustment creates no new resources. Furthermore, political representation and tax funds are generally allocated to geographical areas rather than national racial or ethnic groups. For such reasons, the population shares of places—such as states, counties, and cities—matter more than counts. Improving the accuracy of population shares for places is more delicate than improving national counts of demographic groups. Unless the adjustment method is quite exact, it can make estimated shares worse by putting people in the wrong places. For 1990, there were major errors in the proposed adjustments, as shown by Census Bureau evaluation data: adjustment could easily have made population shares less accurate rather than more accurate, even for states.

The 1990 census counted 248.7 million people. As of July 1991, the undercount in the census was estimated as 2.1%, or 5.3 million people (net, nationwide). The median change in state population shares that would have been produced by adjustment was 8 parts in 100,000; signs are ignored in this calculation. Share changes are what need to be estimated, and estimating at the level of 1 in 10,000 or 1 in 100,000 is obviously a difficult business. The difficulty is compounded because the census includes both overcounts and undercounts: the net undercount is the difference between two much larger numbers. That kind of subtraction entails a substantial loss in accuracy (Section III).

Sample-based estimates suffer from sampling error and non-sampling error. The latter was by far the more serious problem in 1990 and is likely to be so again in 2000, as discussed below. The salient points are as follows.

(i) *Processing errors in the PES.* The census makes errors of various kinds; so does the post enumeration survey. Some of the estimated undercount must result from census errors, but some is due to processing errors in the PES. In 1990, of the 5.3 million estimated undercount, processing errors in the PES contributed roughly 3 to 4 million. (These figures are net, and national in scope; Section IVA.)

(ii) *Correlation bias.* Some kinds of people are missed both in the original census and in the adjusted census. Such people are unlikely to be randomly distributed over the landscape, and there are enough of them to matter (Section V).

(iii) *Heterogeneity.* Geographical shares are adjusted by assuming that undercount rates are constant within specific demographic groups called "post strata." This assumption is clearly wrong (Section VI).

(iv) *Instability.* Adjustment depends on a host of somewhat arbitrary technical decisions, some of which have a considerable bearing on the results. For example, in 1990, estimates of the undercount were smoothed using a hierarchical Bayes sort of procedure. However, there was a decision to "pre-smooth" the variances going into the algorithm, using yet another procedure. Pre-smoothing more or less doubled the estimated undercount (Section VIIB).

Efforts to demonstrate the validity of adjustment are more problematic than adjustment itself, because they depend even more heavily on assumptions that turn out to be rather fragile (Section VIII). Sampling for non-response will be discussed in Section IX, and some of the arguments in favor of sampling are reviewed in Section X.

In 1980 and 1990, suits were brought by the City of New York among other plaintiffs to compel adjustment, but the courts held that the federal government's decision not to adjust was reasonable, and strict scrutiny was not required.[3] With Census 2000, there were challenges to sampling as a method of determining the population base for apportionment, that is, the distribution of congressional seats among the states. The arguments were more legal than statistical, and prevailed in court—as discussed in the next section.

II. LEGAL BACKGROUND

To resolve a conflict over funding, Congress enacted statutes requiring the Bureau to report its plans for taking the 2000 census, deeming these plans to be final agency action, and authorizing the Speaker of the House to sue the Department of Commerce to preclude use of sampling for apportionment; such cases were to be heard by a three-judge panel, with expedited review by the Supreme Court.[4] The Executive Branch argued that the Congress had no standing to bring the suit; among other things, only a future Congress would have its electoral districts affected by sampling. Furthermore, the matter was not ripe for adjudication because Congress could for instance deny funds for sampling. The district court rejected these arguments in U.S. House of Representatives v. U.S. Department of Commerce, et al.[5]

> Without holding that 105^{th} House is itself either the "next friend" of or a fiduciary for the 107^{th} House, we hold that because of the special relationship between the present House and its successor once removed, the 105^{th} House has standing to litigate on behalf of the 107^{th} House. This permits the current House to vindicate the later House's interest in fulfilling its constitutional duties regarding the census, without giving rise to general legislative standing. . . .[6]

> If this court does not rule on this question now, and thereafter a reviewing court concludes post-census that statistical sampling is statutorily or constitutionally proscribed, it will be impossible at that point to determine what the headcount-only number would have been.[7]

We turn now to the core issue. The Census Act governs the taking of the census. According to the 1957 version of section 195,

> except for the determination of the population for apportionment purposes, the Secretary [of Commerce] may, where he deems it appropriate, authorize the use of the statistical method known as "sampling" in carrying out the provisions of this title.[8]

The Census Act was amended in 1976, and the revised version of Section 195 recommended sampling more strongly:

> except for the determination of the population for purposes of apportionment of Representatives in Congress among the several States, the Secretary shall, if he considers it feasible, authorize the use of the statistical method known as "sampling" in carrying out the provisions of this title.

The Executive Branch, however, relied on Section 141(a) of the 1976 Act:

> The Secretary shall, in the year 1980 and every 10 years thereafter, take a decennial census of population as of the first day of April of such year, which date shall be known as the "decennial census date" in such form and content as he may determine, including the use of sampling procedures and special surveys.[9]

The court held that the grant of authority in Section 141 was qualified by the restriction in Section 195, the latter being more specific and therefore binding.[10] In sum,

> Reading section 141(a) and section 195 together, and considering the plain text, legislative history, and other tools of statutory construction, this court finds that the use of statistical sampling to determine the population for purposes of the apportionment of representatives in Congress among the states violates the Census Act.[11]

The constitution requires an "actual enumeration of the population:" the Speaker argued that the constitutional language precluded adjustment, but the court ruled that "there is no need to reach the constitutional questions presented."[12] In a summary judgment, the court enjoined the Department of Commerce "from using any form of statistical sampling, including their program for nonresponse follow-up and Integrated Coverage Measurement, to determine the population for purposes of congressional apportionment."[13]

A parallel case, Matthew Glavin, et al. v. William J. Clinton, et al.[14] was initiated by the Southeastern Legal Foundation, with similar results. Plaintiffs included several individuals living in different states; there was also a corporate plaintiff, Cobb County (Georgia). The argument on standing was somewhat different: according to the Executive Branch, plaintiffs lacked standing since there was no proof of harm. However, the court held that harm was probable, since results of adjustment in 2000 were likely to be similar to results in 1990. Vote dilution and loss of federal funds were concrete injuries likely to flow from adjustment, and "plaintiffs do not need to prove with mathematical certainty the degree to which they will be injured. . . ."[15] The court found that "the only plausible interpretation of the plain language and structure of the [Census] Act is that Section 195 prohibits sampling for apportionment and Section 141 allows it for all other purposes."[16] The court granted plaintiffs motion for summary judgment, ordering "that the defendants should be permanently enjoined from using any form of statistical sampling, including their program for nonresponse follow-up and Integrated Coverage Measurement, to determine the population for purposes of congressional apportionment."[17]

A divided Supreme Court upheld the decisions of the lower courts.[18] The opinion focused on *Glavin*, summarized as follows.

> The District Court held that the case was ripe for review, that the plaintiffs satisfied the requirements for Article III standing, and that the Census Act prohibited use of the challenged sampling procedures to apportion Representatives. 19 F.Supp. 2d, at 547, 548–550, 553. The District Court concluded that, because the statute was clear on its face, the court did not need to reach the constitutional questions presented. Id., at 553. It thus denied defendants' motion to dismiss, granted plaintiffs' motion for summary judgment, and permanently enjoined the use of the challenged sampling procedures to determine the population for purposes of congressional apportionment. Id., at 545, 553.[19]

With respect to the issues of standing and ripeness, the Supreme Court reasoned as follows.

> . . . . because the record before us amply supports the conclusion that several of the appellees have met their burden of proof regarding their standing to bring this suit, we affirm the District Court's holding.[20]
>
> [One plaintiff was a resident of Indiana, whose] expected loss of a Representative to the United States Congress undoubtedly satisfies the injury-in-fact requirement of Article III standing. In the context of apportionment, we have held that voters have standing to challenge an apportionment statute because "[t]hey are asserting 'a plain, direct and adequate interest in maintaining the effectiveness of their votes.'" Baker v. Carr, 369 U.S. 186, 208 (1962) (quoting Coleman v. Miller, 307 U.S. 433, 438 (1939)). The same distinct interest is at issue here: With one fewer Representative, Indiana residents' votes will be diluted. Moreover, the threat of vote dilution through the use of sampling is "concrete" and "actual or imminent, not 'conjectural' or 'hypothetical.'" Whitmore v. Arkansas, 495 U.S. 149, 155 (1990).[21]

> And it is certainly not necessary for this Court to wait until the census has been conducted to consider the issues presented here, because such a pause would result in extreme—possibly irremediable—hardship . . . . There is undoubtedly a "traceable" connection between the use of sampling in the decennial census and Indiana's expected loss of a Representative, and there is a substantial likelihood that the requested relief—a permanent injunction against the proposed uses of sampling in the census—will redress the alleged injury. . . . Appellees have also established standing on the basis of the expected effects of the use of sampling in the 2000 census on intrastate redistricting.[22]
>
> Thus, the appellees who live in the aforementioned counties have a strong claim that they will be injured by the Bureau's plan because their votes will be diluted vis-à-vis residents of counties with larger "undercount" rates. . . . For the reasons discussed above . . . , this expected intrastate vote dilution satisfies the injury-in-fact, causation, and redressibility requirements. Accordingly, appellees have again carried their burden under Rule 56 and have established standing to pursue this case.[23]

A majority of the Court continued,

> We accordingly arrive at the dispute over the meaning of the relevant provisions of the Census Act. The District Court below examined the plain text and legislative history of the Act and concluded that the proposed use of statistical sampling to determine population for purposes of apportioning congressional seats among the States violates the Act. We agree. [24]
>
> This broad grant of authority given in §141(a) is informed . . . by the narrower and more specific §195, which is revealingly entitled, "Use of Sampling." See Green v. Bock Laundry Machine Co., 490 U.S. 504, 524 (1989). The §141 authorization to use sampling techniques in the decennial census is not necessarily an authorization to use these techniques in collecting all of the information that is gathered during the decennial census. We look to the remainder of the law to determine what portions of the decennial census the authorization covers. When we do, we discover that . . . §195 directly prohibits the use of sampling in the determination of population for purposes of apportionment. [footnote omitted.][25]
>
> As amended, the section now requires the Secretary to use statistical sampling in assembling the myriad demographic data that are collected in connection with the decennial census. But the section maintains its prohibition on the use of statistical sampling in calculating population for purposes of apportionment. Absent any historical context, the language in the amended §195 might reasonably be read as either permissive or prohibitive with regard to the use of sampling for apportionment purposes.[26]
>
> Here, the context is provided by over 200 years during which federal statutes have prohibited the use of statistical sampling where apportionment is concerned. In light of this background, there is only one plausible reading of the amended §195: It prohibits the use of sampling in calculating the population for purposes of apportionment.[27]

The majority concluded,

> For the reasons stated, we conclude that the Census Act prohibits the proposed uses of statistical sampling in calculating the population for purposes of apportionment. Because we so conclude, we find it unnecessary to reach the constitutional question presented. . . . Accordingly, we affirm the judgment of the District Court for the Eastern District of Virginia in Clinton v. Glavin, No. 98–564. As this decision also resolves the substantive issues presented

> by Department of Commerce v. United States House of Representatives, No. 98–404, that case no longer presents a substantial federal question. The appeal in that case is therefore dismissed.[28]

In response to this decision, the Executive Branch now seems to be planning a two-track census, with a headcount for apportionment, while adjusted counts are used for redistricting within state and allocation of federal funds (Section XI below). Sample-based non-response followup had to be dropped: if 100% followup is needed for apportionment, it would not be sensible to do sample-based followup in parallel for other purposes.

The Executive Branch is required by law to release the unadjusted block counts:

> In both the 2000 decennial census, and any dress rehearsal or other simulation made in preparation for the 2000 decennial census, the number of persons enumerated without using statistical methods must be publicly available for all levels of census geography which are being released by the Bureau of the Census for: (1) all data releases before January 1, 2001, (2) the data contained in the 2000 decennial census Public Law 94-171 data file released for use in redistricting, (3) the Summary Tabulation File One (STF-1) for the 2000 decennial census; and (4) the official populations of the States transmitted from the Secretary of Commerce through the President to the Clerk of the House used to reapportion the districts of the House among the States as a result of the 2000 decennial census.[29]

Some states may elect to use the unadjusted counts for redistricting, while others will use the adjusted figures. Litigation seems inevitable. Many plaintiffs are likely to have standing, but the constitutionality of sample-based adjustments remains to be decided. The balance of the present article mainly considers the statistical issues raised by adjustment. These issues were not prominent in the litigation about use of sampling for apportionment, but may become quite salient in litigation prompted by redistricting.

III. INTEGRATED COVERAGE MEASUREMENT

The version of ICM reviewed by the courts was to be based on a PES or post enumeration survey—a cluster sample of about 60,000 blocks, containing 750,000 housing units and 1.7 million people. A "block" is the minimal unit of census geography: there are about 7 million blocks in the U.S., of which about 5 million are inhabited. The discussion that follows applies to the PES of 1990 as well as 2000; some differences of detail will be noted below. A listing is made of the housing units in the sample blocks, and persons in these units are interviewed after the census is complete. PES records are then matched against census records. A match generally validates both the census record and the PES record. A PES record that does not match to the census may correspond to a gross omission, that is, a person who should have been counted in the census but was missed. Conversely, a census record that does not match to the PES may correspond to an erroneous enumeration, that is, a person who was counted in the census in error. For example, someone may be counted twice in the census—perhaps because he sent in two forms, and quality control failed to identify the duplication. Another person may be counted correctly but assigned to the wrong unit of geography. She would be a gross omission in one place and an erroneous enumeration in the other. Census records fabricated by respondents or enumerators would also be erroneous enumerations.

The main purpose of the post enumeration survey is to estimate the number of gross omissions and erroneous enumerations.

July 15, 1991 was a critical date for the 1990 PES because the Secretary of Commerce had then to decide whether or not to adjust the 1990 census. He decided not to adjust, over-ruling in the process a split recommendation from the technical staff at the Bureau of the Census. At the time, results from the post enumeration survey of 1990 suggested there were about 19 million gross omissions and 13 million erroneous enumerations; 2 million persons in the census had insufficient data for matching.[30] A first approximation to the estimated undercount is $19 - 13 - 2 = 4$ million; another million persons were added by statistical modeling. Relatively small errors in estimating the number of gross omissions and erroneous enumerations can lead to an unacceptably large error in the difference of the two—and it is the difference that is relevant. For example, if the gross omissions are overestimated by 10% and the erroneous enumerations are underestimated by 10%, the undercount is overestimated by $1.9 + 1.3 = 3.2$ million. In combination, the two errors of 10% create an error that exceeds 60% of the estimated undercount. The next section provides further detail, and shows that our illustrative calculation is realistic.

"Non-sampling error" is a catch-all term for errors other than sampling error. Respondents give answers that are not wholly correct. There are clerical errors in processing data. There are systematic biases in matching and in models used to impute missing data. There are differential success rates in finding persons omitted from the census. Sampling error can be expected to go down with bigger samples, but non-sampling error is another story. As sample size increases, it becomes more difficult to recruit, train, and manage personnel. Moreover, complexity increases the likelihood of non-sampling error. Adjustment programs like the PES are most vulnerable to non-sampling error, and it is non-sampling error that is hard to control—or even to quantify.

Generally, the post enumeration survey proposed for 2000 is close in design to the one for 1990. However, matching will be done on site during the interview, not afterwards at field offices; movers will be handled differently; and the sample is larger.[31] Erroneous enumerations are likely to be even more numerous in 2000 than in 1990 because in 2000 the Census Bureau will be broadcasting the forms in many different ways in its "Be Counted" program.[32] The Bureau will use computer algorithms to detect and remove duplicates; the reliability of these algorithms is presently unknown. Differences between Census 2000 and 1990 will be discussed in more detail below.

IV. PROCESSING ERROR

IV A. Evaluation Data

Some systematic error is inevitable in any large survey operation. The PES is particularly vulnerable to such error for two reasons mentioned above: (i) the adjustments to state shares that need to be estimated are tiny, and (ii) relatively small errors in estimates of gross omissions and erroneous enumerations translate into relatively large errors for the net undercount. Moreover, the PES will be conducted under extreme time pressure to meet the legal deadline for the transmission of census data to Congress. Processing errors are likely to be even more serious in 2000 than they were in 1990: the larger sample size and the tighter timetable make quality control more difficult.

The plan to conduct initial matching on site could make it easier to resolve cases by questioning the respondents. On the other hand, enumerators may prove less adept than the specialized matchers of 1990. The fieldwork will be done closer to census day, which should reduce the number of bad census day addresses given by respondents; avoiding interactions between census and PES operations will be harder.

Matching people between two large sets of records (the census and the PES) is a complex and error-prone process. Due to concerns about confidentiality, the census cannot collect unique identifiers, like fingerprints or social security numbers. Instead, records are matched using incomplete and sometimes erroneous data provided by the respondents. One of the paradoxes of adjustment is that matching is hardest to do in the geographical areas that are hardest to count.[33] Therefore, errors introduced by adjustment are far from evenly distributed.

In 1990, the Census Bureau conducted extensive evaluations of the adjustment-related operations, including reinterviewing samples of households and rematching samples of records. (The reinterviewing was done in a special survey called "Evaluation Followup," several months after the post enumeration survey.) The evaluation data showed that false non-matches systematically exceeded false matches, creating an upward bias in estimated net undercounts. A computer coding error, discussed in Section IVE, also created an upward bias. As noted earlier, 3.0 to 4.2 million of the estimated 5.3 million undercount was due to measured errors in the post enumeration survey, rather than errors in the census.[34] These figures are net, nation-wide. Correlation bias offsets the measured errors in counts to some unknown degree, but could exacerbate the errors in shares (Section V).

The evaluation data allow disaggregation of measured biases among thirteen "evaluation strata"—broad groups defined partly by geography and partly by demography. The biases proved to be unevenly distributed, affecting state shares as well as counts. After all, shares are adjusted by adding different numbers of people to the different states. If most of the additional people represent the effects of errors in the adjustment process, rather than errors in the census, then the adjustment to shares is likely to be more artifactual than real. Cancellation of biases in the PES is not an assumption to be lightly made; indeed, the evidence runs against it (Sections IV, V, and VIII).

IV B. Balancing Error

Matching records between the post enumeration survey and the census would be difficult unless the search is restricted—the files are simply too large. Restriction is by geography: a PES respondent is picked up in a block, and census records are searched in that block or nearby blocks. For this purpose among others, addresses must be assigned to census blocks, a process called "geocoding." Errors in geocoding are not infrequent. Expanding the search area would generally increase the number of matches, reducing the estimated number of gross omissions. The effort spent in identifying gross omissions must be comparable to the effort in identifying erroneous enumerations; otherwise, there is "balancing error." According to present plans, the search area will be smaller in 2000 than in 1990.[35] That reduces operational problems, but is likely to add an upward bias to the tally of gross omissions. There are indications that in 1990, balancing error was appreciable.[36] If the search area is reduced, the problem for 2000 will be worse.

IV C. Missing Data

After the initial matching done by the PES, additional fieldwork—"production followup"—may be needed to resolve the status of unmatched cases, that is, to decide whether the error should be charged against the census or the PES. Even after fieldwork is complete, however, some cases remain unresolved. Statistical models are then used to impute the missing match status. The number of unresolved cases may be relatively small, but it is likely to be large enough to have an appreciable influence on the final results. Imputation models have many somewhat arbitrary elements, and should therefore be scrutinized with great care. Past experience is not encouraging.

In 1990, there were about 4 million unresolved cases in the P-sample universe and 5 million in the E-sample. (Sample numbers are weighted up so as to estimate national totals; the "P-sample" consisted of persons found by the post enumeration survey in its sample blocks; the "E-sample" consisted of persons found by the census in the same blocks.) The number of unresolved cases is large, relative to a total estimated undercount of 5.3 million. To see the implications, consider what has been-called the "Q-class" (with "Q" for "question marks"). This was a group of P-sample cases with minimal information—so minimal that these cases were not sent to production followup. The Bureau's model imputed match status for the Q-class as if such cases were comparable to cases with full information, after conditioning on certain covariates. The imputed match rate was much too high. If we assign to the Q-class a match rate equal to the average rate for cases with partial information, the estimated undercount would on our reckoning go up by roughly 400,000 people.[37] This is not a trivial change relative to 5.3 million; nor do we think these people were randomly sprinkled across the U.S. There may be similar problems with the imputation of match status for unresolved E-sample cases.

IV D. Movers

Movers—people who change address between the time of the census and the time of the PES—are another complication. Unless persons are correctly identified as movers or non-movers, they cannot be matched. Identification depends on getting accurate information from respondents on where they were living at census time; this is not a trivial problem.[38] More generally, matching records between the PES and the census becomes problematic if respondents give inaccurate information to the census, or the PES, or both.

In 1990, the post enumeration survey identified "inmovers," that is, persons who moved into the sample blocks after census day. There were 19 million inmovers, who contributed about 5 million to the estimated number of gross omissions.[39] (Again, figures are weighted to national totals.) Movers are a large factor in the adjustment equation. In 2000, the PES must identify both inmovers and outmovers; according to current plans, it is the outmovers who will be matched to the census. Inmovers would have to be matched to the census at their census-day addresses, which are likely to be in non-PES blocks where census followup was to be done on a sample basis. Consequently, matching inmovers would have been quite troublesome, and that is why outmovers had to be considered.

Outmovers will be hard to identify, and information about them will be hard to collect. Apparently, such data will be obtained from "proxy interviews," with neighbors, current occupants of the household, and so forth; the alternative is to try and trace the outmovers to their current addresses. Curiously, non-match rates estimated from outmovers will be applied to inmovers. That may be appropriate in neighborhoods where the population is stable, but seems questionable if the population is changing rapidly. At this time, it is not clear whether or how erroneous enumeration rates will be computed for outmovers. In these respects, the 2000 design is more problematic than 1990.

IV E. The Coding Error and the Revised PES

In 1990, the post enumeration survey picked up some number of respondents who moved into sample blocks after census day but matched to the census. (For a specific example, take someone who moves from Boston to San Francisco right after census day, lists a San Francisco address on the census form, is sampled for the PES, and tells the whole story to the PES interviewer.) Such respondents were gross omissions at their old addresses. They were also erroneous enumerations at their new addresses but were often classified by the PES as correct enumerations. This "computer coding error" added a million people to the estimated undercount. After the adjustment decision in 1991, the Bureau corrected the coding error. Rematching the 104 most extreme block clusters eliminated another 300,000 persons from the estimated undercount, which was therefore reduced from 5.3 million to 4.0 million. The Bureau considered using the revised PES to adjust the census as a base for post-censal population estimates, but in the end decided to stay with the unadjusted census.[40]

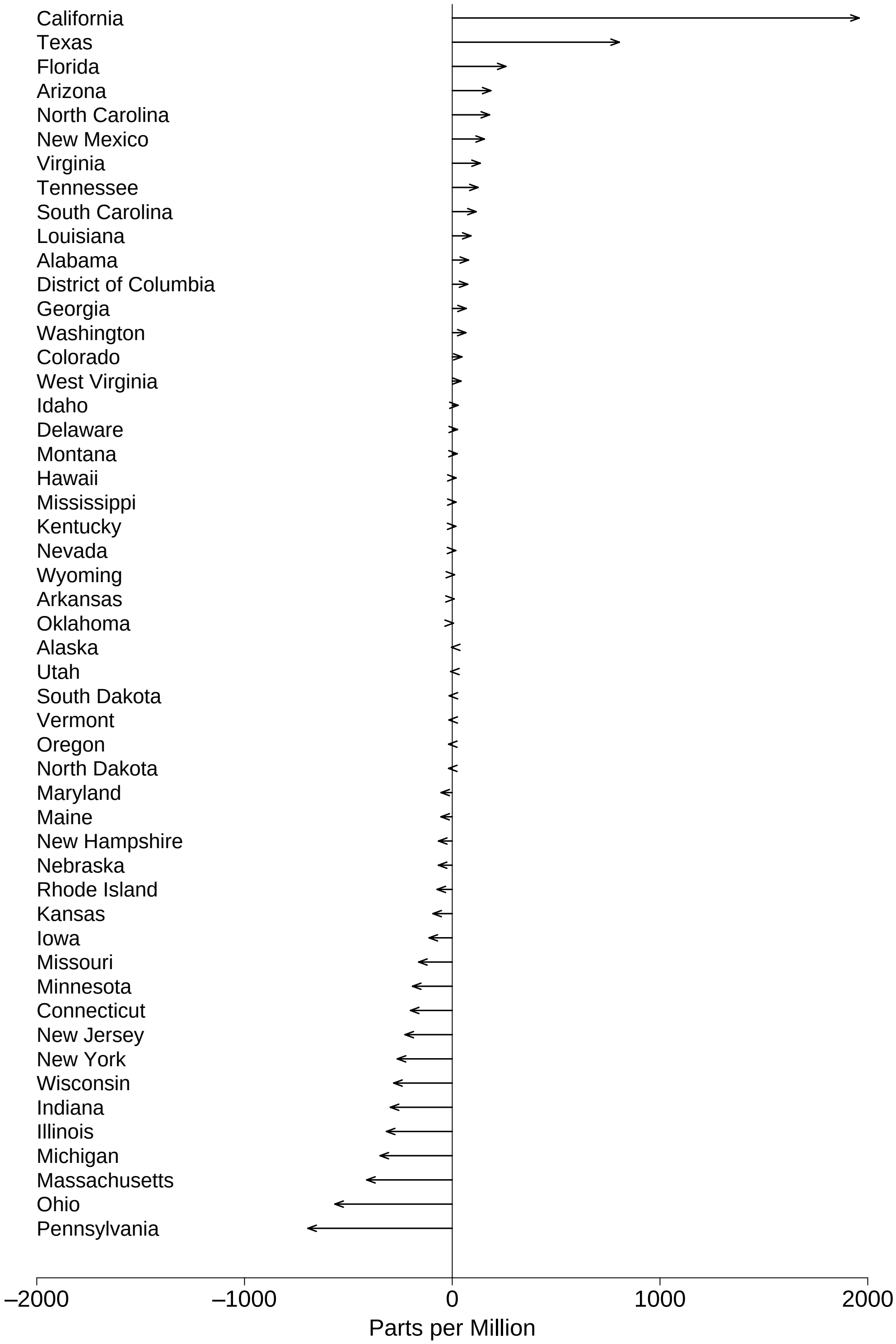
The original adjustment would have shifted two congressional seats. The revised adjustment, with the estimated undercount reduced from 5.3 million to 4.0 million, would only have shifted one seat. Errors in the PES—the coding error being the dominant factor—would have moved a congressional seat from Pennsylvania to Arizona. The original adjustment went too far. The proposed revision also seems problematic, for it still incorporates processing errors of 1.7 to 2.9 million out of the 4 million estimated net undercount.[41]

V. CORRELATION BIAS

Some persons are missed both by the census and by the post enumeration survey. Their number is estimated by capture-recapture methods, under the assumption that within pre-defined demographic groups (the post strata) there is no correlation between being missed by the census and by the PES. When this assumption is not satisfied, there is "correlation bias" in the estimated adjustments. People who are especially hard to reach by any survey—whether the census or the PES—are a prime source of correlation bias. Reachability varies from place to place around the country, and differential levels of correlation bias are a major threat to the accuracy of adjusted population shares.

Figure 1 shows the changes in state shares that would have been produced by census adjustment in 1990, sorted by the size of the change.[42] The adjusted share for a state is its adjusted count, divided by the total adjusted count for the whole country; likewise, the census share is the census count, divided by the census count for the whole country. (The adjustment is the one considered by the Secretary of Commerce as of July 1991.) The share of California increases from .119658 to .121617, that is, by $.121617 - .119658 = .001959$, or 1,959/1,000,000. The length of the arrow to the right of California is 1,959 parts per million. California would have been the biggest gainer from adjustment. Arrows to the left correspond to losing population share. Pennsylvania's share would have decreased from .047773 to .047078; the loss is $.047773 - .047078 = .000695$, or 695 parts per million. That is the biggest loss. (Oddly, Pennsylvania supported adjustment in 1990; so did New York and New Jersey.) For many of the states, the adjustment would have been less than 30 parts per million: the arrowhead overlaps the vertical line marking no change in population share.

FIGURE 1
Share Changes from the Proposed Adjustment to the Census



There are two noteworthy features. First, the share changes are tiny: the median size of the change is 75 parts per million. Second, the Northeastern and Midwestern states with large central-city minority populations—like New York, Illinois, Michigan, Massachusetts, and Pennsylvania—would have lost share from adjustment. If adjustment were accurately correcting for racial differentials in the undercount, these states would have been expected to gain population share. One plausible explanation for the paradox is correlation bias.

Correlation bias is hard to measure at the national level, and almost impossible at the state level. However, a technique called "Demographic Analysis" supplies independent estimates of the total U.S. population by race, sex, and age. These estimates are derived from administrative records, including birth certificates, death certificates, and Medicare registration.[43] Table 1 compares the census undercounts as estimated by the post enumeration survey of 1990 and by demographic analysis. The data suggest there was substantial over-adjustment for females; moreover, a large number of the black males who were missed in the 1990 census were also missed by the proposed adjustment. For the latter group, there is evidence of substantial correlation bias at the national level. It is plausible that the missing black males were concentrated in states with large central-city minority populations—at the bottom of Figure 1. The post enumeration survey may well have been more successful at finding undercounted black males in California and Texas than in New York, Illinois, and Pennsylvania.

TABLE 1
Estimated Net Census Undercounts by Race and Sex
From the Post Enumeration Survey and From Demographic Analysis
As of 15 July 1991. "Other" is Non-Black, Including Whites and Asians

	Post Enumeration Survey	Demographic Analysis	Difference PES − DA
Black Males	804,000	1,338,000	−534,000
Black Females	716,000	498,000	+218,000
Other Males	2,205,000	2,142,000	+63,000
Other Females	1,544,000	706,000	+838,000

Source: U.S. Bureau of the Census, Census Bureau Releases Refined Estimates from Post Enumeration Survey of 1990 Census Coverage, Press Release CB 91-221, Table 4 (June 13, 1991). U.S. Bureau of the Census, Census Bureau Releases Refined 1990 Census Coverage Estimates from Demographic Analysis, Press Release CB 91-222, Table 1 (June 13, 1991).

The discrepancies in Table 1 and Figure 1 are matters of fact. The explanation is more a matter of judgment—necessarily so, due to the absence of solid data on correlation bias at the state level. It is the case that adding and subtracting enough people of various kinds to remove the discrepancies in Table 1 can materially alter the adjustment to state shares, if the relevant scale is parts per million. We conclude that correlation bias is a serious and intractable problem. The bias is serious because it can result in adjustments that make state shares worse rather than better. The bias is intractable because it cannot be measured at subnational levels.[44]

VI. HETEROGENEITY

As noted above, the Bureau divides the population into post strata defined by demographic and geographic characteristics: one post stratum might be Hispanic male renters age 30–49 in California. Sample persons are assigned to post strata on the basis of the fieldwork. The rate of gross omissions, erroneous enumerations, and the net undercount rate are estimated separately for each post stratum (Section VIIA). It is the net undercount rate that matters for adjustment.

To adjust small areas (counties, cities, . . . , blocks), the undercount rate in a post stratum is assumed to be constant across geography. In our example, the number of Hispanic male renters age 30–49 in every single block in California—from the barrios of east Los Angeles to the affluent suburbs of Marin county and beyond—would be scaled up by the same adjustment factor, which is computed from sample data for the whole post stratum. This process is repeated for every post stratum. The choice of post strata may therefore have a substantial influence on the results.[45] Plainly, the choice of post strata is somewhat subjective. The assumption that undercount rates are constant within post strata is the "homogeneity assumption," and failures are termed "heterogeneity." Ordinarily, samples are used to extrapolate upwards, from the part to the whole. Census adjustment extrapolates sideways, from 60,000 sample blocks to each and every one of 5 million inhabited blocks in the U.S. That is why the homogeneity assumption is needed.

In July 1991, the Bureau proposed to use 1,392 post strata. The Bureau later considered adjusting the census as a base for the post-censal estimates (Section IVE), using 357 post strata. With either post stratification, there was considerable residual heterogeneity.[46] This added appreciably to the uncertainties about adjustments for state and sub-state areas, and significantly complicated the task of comparing the accuracy of the census to that of adjustment.[47]

The Bureau has not finalized its post stratification for 2000.[48] In the plan reviewed by the courts, post strata would not have crossed state lines: thus, each state would be adjusted only using data collected within that state. This is an improvement over 1990, because homogeneity of post strata that cross state lines is not assumed. There is a cross-classification within each state (and D.C.) by six race-ethnicity groups, seven age-sex groups, and two "tenure" groups—owners and renters. Post strata can be formed from these $6 \times 7 \times 2 = 84$ categories by collapsing cells with small sample sizes, although that part of the process was not fully defined. These post strata do not take into account area of residence—whether respondents live in major metropolitan areas, suburbs, or rural areas. For that reason, heterogeneity within states may be even more of a problem in 2000 than it was in 1990. In the current plan, post strata will cross state lines (Section XI).

VII. DUAL SYSTEM ESTIMATION

VII A. Weights

We turn now to estimation. Each person in the PES is assigned a "sample weight." If the Bureau sampled 1 person in 100, each sample person would stand for 100 in the population, and have a sample weight of 100. The actual sampling plan is more complex, so different people have different weights. Basically, the weight is the inverse of the selection probability, with adjustments for non-response. Each gross omission or erroneous enumeration detected in the sample is weighted up in the estimation process.

To estimate the total number of gross omissions in a post stratum, one simply adds the weights of all PES respondents who were identified as (i) gross omissions and (ii) in the relevant post stratum. To a first approximation, the estimated undercount in a post stratum is the difference between the estimated numbers of gross omissions and erroneous enumerations. Details are postponed to the Appendix. The "raw adjustment factor" for a post stratum is the ratio of its estimated population to the census count: when multiplied by this factor, the census count for a post stratum equals the estimated true count. Typically, adjustment factors exceed 1: most post strata are estimated to have undercounts. However, many adjustment factors are less than 1. These post strata are estimated to have been overcounted. In 1990, about 70% of the adjustment factors exceeded 1 and 30% were below 1.

Use of sample weights is of course quite standard, but not free of problems in the adjustment context. For one thing, mistakes in fieldwork are magnified by the weights. For another thing, the weights may themselves be adjusted at various stages of the process, for reasons that are not entirely compelling—and the impact of such changes can be striking. Thus, a decision to revise the weights of two block clusters out of the 5,300 in the 1990 post enumeration survey subtracted 654,000 people from the adjusted count.[49]

VII B. Smoothing in 1990

In 1990, the raw adjustment factors had unacceptable levels of sampling error. As noted above, the Bureau tried to reduce sampling error by means of smoothing. This was done separately in four geographical regions—Northeast, Midwest, South, and West. In the Northeast, for instance, there were 300 post strata. The Bureau was using a "hierarchical linear model," with two equations (reported in the Appendix). The first equation states an assumption, that the raw adjustment factor for a post stratum is an unbiased estimate of the true factor. The second equation in the model states another assumption, that the true adjustment factors are linearly related to certain covariates, with additive random errors. The "smoothed adjustment factors" are obtained by averaging the raw factors with values predicted from an auxiliary regression model. The weights in the average depend critically on the variances of the raw factors. These variances are initially estimated from the data,[50] but are then "pre-smoothed," that is, replaced by values predicted from yet another regression model. Finally, the smoothed adjustment factors are "benchmarked"— rescaled so that the smoothing process does not change the adjusted populations of the four census regions.

The detailed structure of the models seems arbitrary, and details had major impacts. For instance, the proposed adjustment in 1990 would have shifted population share from the Northeast and Midwest to the South and West; pre-smoothing and benchmarking account for 1/3 of the shift.[51] Moreover, the estimated variances in the smoothed adjustment factors depend quite strongly on the assumptions in the model. If these assumptions are violated, real variances may be much larger than the estimated variances. When model outputs were taken at face value, smoothing seemed to reduce variances by a factor of about 2. However, simulation studies and sensitivity analysis suggest that estimated variances were too small by a factor of 2 or 3: if anything, smoothing increased variance.[52] Pre-smoothing accounted for nearly half of the 5.3 million estimated undercount.[53]

VII C. Smoothing in 2000

The Bureau planned to smooth the adjustment factors in Census 2000 by means of a log linear model, in order to reduce variance.[54] (Although the ICM would have been 5 times larger than the PES of 1990, there would have been 2 or 3 times as many post strata; thus, sampling error is still a problem.) There are strong similarities between the log linear model for 2000 and the linear model for 1990, although the one for 2000 may be simpler and therefore more robust. The benchmarking will certainly be more extensive. However, the basic issue remains the same: a reduction in variance is likely to be accompanied by some increase in bias, and the tradeoff is extraordinarily hard to assess.[55]

VIII. LOSS FUNCTION ANALYSIS

In brief, "loss function analysis" attempts to make unbiased estimates of the risk in the census and the adjustment. Various loss functions can be considered, and different levels of geography. We focus here on total squared error in population shares for the 50 states and D.C. The object is to estimate "census risk − adjustment risk:" risk is squared error, averaged over possible realizations of the PES and the Evaluation Followup (Section IVA). Using the Bureau's assumptions, this risk difference is estimated as 667, with a standard error of 281. With other assumptions that are perhaps more realistic, the estimate becomes −250, with a standard error of 821; units are parts per 100 million, positive values favoring adjustment and negative values favoring the census. Even at the state level, the case for adjusting the 1990 census was shaky at best: there was a strong likelihood that adjustment would have put in more error than it took out, and conclusions depend heavily on assumptions. A technical discussion is postponed to the Appendix.

IX. SAMPLING FOR NON-RESPONSE

IX A. SNRFU

We now turn to "SNRFU," or Sample-based Non-Response Followup. This was the component of the plan for 2000 ruled out by the courts (Section II). Non-response followup in the census has in the past been done on a 100% basis. In the bulk of the country, forms are mailed out to all identified housing units. If there is no response, interviewers come knocking on the door. In 2000, the Bureau planned to follow up only a sample of non-respondents, within each tract. If, for instance, a tract has 2,000 housing units and 1,200 return their census form by mail, there are 800 non-responding units. The Bureau would then have sampled 600 out of these 800 units, sending interviewers only to the sample units. A separate sample would be drawn from the forms returned by the Post Office as undeliverable. Additional housing units would be imputed into the census using responses from the two samples.

A "tract" is a unit of census geography, containing on average something like 100 blocks, 2,000 housing units, and 5,000 people. The idea is to obtain responses from 90% of the housing units in each tract. In our example, $1{,}200 + 600 = 1{,}800$, which is 90% of the assumed total of 2,000; if the mail-back rate in a tract is above 85%, the sampling rate is held at 1 in 3. Sample data may for instance suggest adding a certain number of households of a given type; some households would be selected from enumerated households of that type and added to the census.

There is some opinion that sampling improves accuracy since interviewers can be better trained and supervised. Given the proposed sampling rates, this advantage cannot be substantial. Sampling seems inherently more complex than a census, and past experience shows that sample surveys have worse coverage than the census. Of course, sampling for non-response in Census 2000 may not be directly comparable to past surveys. Coverage can be explained this way. As noted above, each person in a sample gets a sample weight: this could, for instance, be the inverse of the probability of drawing that person into the sample. Based on sample data, one can then estimate the total number of persons in the population by adding up the weights of the sample persons. "Coverage" is the ratio of this estimated number to the true number. Likewise, the coverage of the census is the ratio of the census population to its (estimated) true value.

The Current Population Survey is a well-established, well-run sample survey; still, it reaches only about 95% of the census population. Relative to the census, the CPS has a 95% coverage ratio. Black and Hispanic sub-populations have noticeably lower coverage ratios.[56] For another example, the post enumeration survey of 1990 had by our reckoning about 98% of the coverage of the census. (The calculation weights the P-sample and E-sample to national totals.) Of course, the census data were in some cases problematic—but so were the post enumeration survey data. It is by no means obvious that SNRFU will reduce differential undercounts.

IX B. How Does Sampling for Non-Response Interact with the ICM?

The essential task of the ICM is to match records against the census. That conflicts with sampling for non-response, because ICM respondents may be in households that did not return a census form and were not selected for followup. To solve this problem, the Bureau proposed to do 100% followup in the ICM sample blocks. The plan for Census 2000 that was rejected by the courts therefore involved at least three kinds of sampling and three kinds of fieldwork.

The three kinds of sampling:

- (i) sampling in the ICM;
- (ii) sample-based followup for census non-response;
- (iii) sampling housing units with undeliverable census forms.

The three kinds of fieldwork:

- (i) the ICM;
- (ii) 100% followup for census non-response in the ICM sample blocks;
- (iii) sample followup for census non-response in the rest of the country.

Other assumptions would be needed here: (i) census coverage is the same whether non-response followup is done on a sample basis or a 100% basis; and, (ii) residents of the ICM sample blocks do not change their behavior as a result of being interviewed more than once. Failure of these assumptions may be termed "contamination error." The magnitude of contamination error is unknown. A test census may provide evidence showing that contamination error is modest. However, power is limited and the real census may be somewhat different.[57] One other feature of the design reviewed by the courts is worth considering here: the ICM could not have detected any non-sampling errors in SNRFU, because the ICM would not—by design—examine any blocks where non-response followup is done on a sample basis.

X. OTHER ARGUMENTS

In this section, we review some additional arguments for sampling and adjustment.[58]

Everything is relative: the census is imperfect, and survey data are of better quality than census data. The imperfection of the census may be granted. However, even if the PES is in some ways better than the census, the central question remains open. Are the PES data good enough for their intended use? Will proposed adjustments to the census take out more error than they put in?

Sampling saves money and improves accuracy. "Without sampling, costs would increase by at least $675 million and the final count would be less accurate than the 1990 census." p. 37. However, sampling 3 households in 4 within each tract cannot have saved very much money. The budget for non-response followup is only about $500 million, and the sampling rate is about 3/4, so the savings may be on the order of $150 to $200 million rather than $675 million. For comparison, the total budget is about $4 billion.[59]

If there was little money to be saved by SNRFU, few gains in accuracy would have resulted from putting those savings into improvements in fieldwork. The case that sampling will improve accuracy must rest therefore on the post enumeration survey and the DSE: "The Census Bureau is confident in the Dual System Estimation methodology based on its experience implementing Dual System Estimation and its expertise explaining Dual System Estimation . . . ." p. 32. In our view, however, the DSE failed to improve accuracy in 1980; and it failed again in 1990.[60] The crucial question is whether the DSE will improve accuracy in 2000.

The argument from authority. The Bureau's plans have been approved by "three separate panels" of the National Academy of Sciences, six Census Advisory Committees, and "public meetings . . . in thirty-one cities across the country." pp. 7, 9. The National Academy reports provide little analytic detail supporting adjustment. Nor are they quite as supportive of sampling as the Bureau makes out: "If sampling for NRFU frees resources for taking steps to reduce other sources of error in the final results, it may produce a more accurate census by some measures." Steffey and Bradburn, supra note 57, at 101. This is hardly a ringing endorsement.

Sampling is scientific. "Scientists understand that sampling has known, objective properties that are preferable to the certainty of missing several million individuals using traditional enumeration methods alone. . . . the issue is not whether to 'sample' but whether to sample scientifically." pp. ii, 23. Presumably, the census is an unscientific "sample" because it is not an exact count of the population; the "known, objective properties" of a scientific sample must refer to the possibility of quantifying sampling error. However, the argument is diversionary as well as murky. The real problem is non-sampling error. The PES will suffer from non-sampling error, which will be as hard to quantify as the errors in the census.

Adjustment will correct the differential undercount for national race and ethnic groups, and may improve the accuracy of state and local shares. We think this was the best argument for adjustment in 1990, although it was seldom made explicit. However, it is not an argument for the post enumeration survey and the DSE, because there is a much simpler way to correct the national undercount. Census figures could be scaled up to match the demographic analysis totals for subgroups of the national population defined by age, sex and race (Section V). The people in a demographic group who are thought to be missing from the census would be added back, in proportion to the ones who are counted—state by state, block by block. Currently, demographic analysis does not account for ethnicity, but the method could be adapted for that purpose. Scaling is transparent, so there is little opportunity for major error. Changes in state and local-area population shares may go in the right direction and will in any event be small. Despite the flaws in demographic analysis and the heterogeneity of undercount rates, we do not see much likelihood of demonstrating that the DSE will be more accurate than scaling. On the contrary, the DSE may do worse because of some hidden breakdown in the operation.

XI. THE MODIFIED PLAN FOR CENSUS 2000

In response to the Supreme Court's decision, the Executive Branch is planning a two-track census. The apportionment numbers will be based on a headcount. For other purposes, census counts will be adjusted using a revised version of the ICM, to be called ACE (Accuracy and Coverage Evaluation Survey). The sample size will be about 40% of the planned size for ICM, that is, about double the size of the 1990 PES. Post strata are not yet defined, but will almost certainly cross state lines.[61] Plans for sample-based non-response followup have been dropped, so the complicated interactions with ICM fieldwork will no longer be a problem (Section IX). In other respects, the new proposal seems very like the one described here, and very like the 1990 plan. The major differences we see between plans for 2000 and 1990 are now as follows:

- (i) Counts will be adjusted using ACE, for purposes other than apportionment of congressional seats to states. In 1990, there was no adjustment.
- (ii) ACE will be almost double the size of the 1990 PES.
- (iii) Outmovers identified by ACE will be matched to the census; the PES attempted to identify and match inmovers (Section IVD).
- (iv) Census fieldwork will be curtailed to make time for ACE fieldwork.
- (v) Census forms will be widely distributed through the "Be Counted" program, as well as being mailed or delivered to all addresses in the Master Address File. This increases the risk of duplicate enumerations.

XII. SUMMARY AND CONCLUSION

One of the oft-stated goals for Census 2000 is "Keep It Simple."[62] However, adjustment adds layer upon layer of complexity to an already complex census. Consequently, the results are highly dependent on many somewhat arbitrary technical decisions. Mistakes are almost inevitable, very hard to detect, and have profound consequences. Examples from 1990 are sobering:

- (i) A computer coding error added a million people to the adjusted count (Section IVE).
- (ii) In total, about 3.0 to 4.2 million out of the estimated 5.3 million undercount resulted from errors in the PES rather than census errors (Section IVA).
- (iii) The treatment of the Q-class by the imputation model subtracted 400,000 to 900,000 people from the adjusted count (Section IVC).
- (iv) A decision to revise the weights of two block clusters out of the 5,300 in the 1990 PES subtracted 654,000 people from the adjusted count (Section VIIA).
- (v) The decision to pre-smooth estimated variances added 2.5 million people to the adjusted count (Section VIIB).

The specific sources of instability discovered in 1990 may well be avoided in 2000. But the lesson is that the kinds of methods proposed to fix the census are quite dependent on many somewhat arbitrary technical decisions, and inherently vulnerable to error. Furthermore, the statistical assumptions behind the adjustment methodology are rather shaky. The homogeneity assumption is one example (Section V); the imputation and smoothing models can also be mentioned (Sections IVC, VIIC); and the list does not end there. If the PES of 2000—like the PES of 1990 before it—puts in more error than it takes out, Census 2000 will be at considerable risk.

APPENDIX

The Dual System Estimator

The formula for the raw dual system estimator (DSE) is

$$\text{DSE} = \frac{\text{Cen} - II}{M/N_p} \times \left[1 - \frac{EE}{N_e}\right].$$

In this formula, DSE is the dual system estimate of the population in a post stratum; Cen is the census count; II is the number of persons imputed into the census count; M is the estimated total number of matches obtained by weighting up sample matches; N_p is the estimated population obtained by weighting up P-sample counts; EE is the estimated number of erroneous enumerations; and N_e is the estimated population obtained by weighting up E-sample counts. The formula is applied separately to each post stratum.

The "match rate" M/N_p appears in the denominator of the DSE. Intuitively, the complement of the match rate estimates the gross omissions rate in the census. Likewise, EE/N_e estimates the rate of erroneous enumerations in the census. The object of the post enumeration survey is to estimate these fractions. Cen and II come from census records. Some persons besides the II's are counted in the census without enough detail for matching; such persons are classified as erroneous enumerations, as are persons who have been counted more than once.[63]

The Smoothing Model

We use the Northeast as an example. There were 300 post strata, which we index by i. Let γ_i be the "true" adjustment factor for post stratum i, and Y_i the "raw" adjustment factor derived from the DSE. The Bureau was using a hierarchical linear model. The first equation in the model states an assumption, that the raw adjustment factor is an unbiased estimate of the true factor:

$$Y_i = \gamma_i + \delta_i,$$

where δ_i is a random error term.

The second equation in the model states another assumption, that the true adjustment factors are linearly related to certain covariates, with additive random errors:

$$\gamma = X\alpha + \epsilon.$$

Here, α is a vector of unknown parameters and X is a matrix of covariates computed from census data; the ith row of X describes post stratum i. Again, ϵ is a vector of random errors, assumed independent of δ, with $\mathrm{E}(\epsilon) = 0$ and $\mathrm{cov}(\epsilon) = \sigma^2 I$, where σ^2 is another parameter to be estimated. Random errors are assumed to be multivariate normal.

Let δ be the vector whose ith component is δ_i, and let $K = \mathrm{cov}(\delta)$, where K is a 300×300 matrix that is estimated from the data by the jackknife. After estimation, variances are "pre-smoothed," that is, replaced by fitted values computed from some auxiliary regression model. Call the resulting estimate $\hat{K}$. The "smoothed" adjustment factors $\hat{\gamma}$ are obtained by projecting the raw adjustment factors Y toward a hyper-plane defined by X; direction and distance are determined by $\hat{K}$. Thus, the algorithm depends strongly on estimated variances.

To implement the smoothing model, the Bureau drew up a list of admissible covariates. From this list, some initial version X_0 was chosen for X. Then σ^2 and α are estimated by maximum likelihood, and a new version of X is chosen by a "best subsets" routine from the list of admissible covariates, and there is iteration to convergence.[64] In the Northeast, for instance, the algorithm chose 18 covariates out of a possible 32.

Write X for the "final" version of the design matrix. Denote the MLE for σ^2 by $\hat{\sigma}^2$. Let P_X be the OLS projection matrix, $P_X = X(X'X)^{-1}X'$. Define the 300×300 matrix $\hat{\Gamma}$ as follows: $\hat{\Gamma}^{-1} = \hat{K}^{-1} + \hat{\sigma}^{-2}(I - P_X)$. The vector of smoothed adjustment factors could be computed as $\hat{\gamma} = \hat{\Gamma}\hat{K}^{-1}Y$, with covariances estimated as $\widehat{\mathrm{cov}}(\hat{\gamma} - \gamma) = \hat{\Gamma}$. In 1990, however, the Bureau "benchmarked" $\hat{\gamma}$ so the total population in each of the four regions was unaffected by smoothing. This necessitated a more complex formula for the covariances of $\hat{\gamma}$. The formula allowed for uncertainty in $\hat{\sigma}^2$. No allowance was made for uncertainty in $\hat{K}$, or in the choice of X. No allowance was made for specification error.[65]

Loss Function Analysis

Index the areas by $k = 1, \ldots, 51$, corresponding to the 50 states and D.C. Let μ_k be the error in the census population share for area k. Let X_k be the production dual system estimate for μ_k, derived from the post enumeration survey and the smoothing model. The bias in X_k is denoted β_k; this is estimated from evaluation followup data as $\hat{\beta}_k$. The Bureau's model can be stated as follows: $X \sim N(\mu + \beta, G)$, $\hat{\beta} \sim N(\beta, H)$, and X is independent of $\hat{\beta}$. Here, X, β, and $\hat{\beta}$ are 51-vectors of share changes; G and H are 51×51 matrices of rank 50. Shares add to unity, so share changes add to 0, and one degree of freedom is lost. The smoothing model provided an estimator $\hat{G}$ for G, and the Bureau had an estimator $\hat{H}$ for H; these estimators were assumed to be nearly correct. Likewise, the model assumed $\hat{\beta}$ to be an unbiased estimator for β. We find these assumptions to be extremely questionable: for example, $\hat{\beta}$ is severely biased (Section IV above).

The estimated risk from the census for area k is $(X_k - \hat{\beta}_k)^2 - \hat{G}_{kk} - \hat{H}_{kk}$, while the estimated risk from adjustment is $\hat{\beta}_k^2 + \hat{G}_{kk} - \hat{H}_{kk}$. The estimated risk difference is

$$\hat{R}_k = (X_k - \hat{\beta}_k)^2 - \hat{\beta}_k^2 - 2\hat{G}_{kk}.$$

If we ignore the variability in $\hat{G}$,

$$\mathrm{cov}(\hat{R}_i, \hat{R}_j) = 4\mu_i\mu_j G_{ij} + 2G_{ij}^2 + 4E(X_i X_j)H_{ij}.$$

The displayed covariance can be estimated from sample data as

$$\hat{C}_{ij} = 4(X_i - \hat{\beta}_i)(X_j - \hat{\beta}_j)\hat{G}_{ij} - 2\hat{G}_{ij}^2 - 4\hat{G}_{ij}\hat{H}_{ij} + 4X_i X_j \hat{H}_{ij}.$$

Thus, we have unbiased estimates—given the model, and given that

$$\mathrm{E}(\hat{G}) = G, \quad \mathrm{E}(\hat{H}) = H.$$

Let ERD be the estimated risk difference, summed over all 51 areas: $\text{ERD} = \sum_k \hat{R}_k$. Now var(ERD) can be estimated as $\sum_{ij} \hat{C}_{ij}$. Table 2 shows ERD and var(ERD), computed using the formulas above, under various sets of assumptions. Bias was measured for 13 evaluation post strata, and allocated by the Bureau to 1,392 post strata. The Bureau's allocation assumed (i) bias was proportional to some covariate for post strata within evaluation post strata, and (ii) bias was constant across states within post strata. Bias was allocated down, then reaggregated to the 51 areas. The evaluation followup sample was about 7% of the size of the post enumeration survey; there was no smoothing (factor of 2 reduction in apparent variance). Trace($\hat{H}$) should therefore have been around

$$\frac{1}{.07} \times 2 \times \mathrm{trace}(\hat{G}).$$

Instead, on the Bureau's reckoning, trace($\hat{H}$) turned out to be around $.33 \times \mathrm{trace}(\hat{G})$. We conclude that $\hat{H}$ is off by a factor of around

$$\frac{1}{.07} \times 2 \times \frac{1}{.33} = 84.$$

The explanation: the Bureau's allocation scheme converted variance in $\hat{\beta}$ to bias, and their model assumed that bias in $\hat{\beta}$ vanishes.

TABLE 2
Impact of Allocation Schemes for State-Level Biases
Correction of Final Variances and Estimated Variances of Bias Estimates
ERD = Census Risk − Adjustment Risk
The "SE" Column Gives the Estimated Standard Error of ERD
in the Various Scenarios. Units are parts per 100 million

Allocation	$\hat{G}$	$\hat{H}$	ERD	SE
PRODSE	1	1	667	281
PRODSE	1	50	667	890
PRODSE	2	1	542	371
PRODSE	2	50	542	885
.25 × undercount	1	1	193	199
.50 × undercount	1	1	−125	156
.50 × undercount	1	50	−125	859
.50 × undercount	2	1	−250	169
.50 × undercount	2	50	−250	821

The matrix $\hat{G}$ is too small as well, by a factor of 2 to 3, as discussed in Section VIIB. Furthermore, measured biases in the post enumeration survey—response errors, matching errors, errors in the imputations for missing data, the coding error, and so forth—amount to well over half the estimated undercount, as reported in Section IIIA. The first line in Table 2 does the loss function analysis on the Bureau's assumptions. The other lines show what happens when the variance estimates are corrected, and the bias allocations are revised.[66]

Table 2 may in some respects understate the problems with loss function analysis. For instance, heterogeneity is probably larger than all the other errors put together, and heterogeneity is not taken into account in Table 2; there is no allowance for bias in the imputations; and the Bureau's scheme for allocating biases to states violates the assumption that $\hat{\beta}$ should be independent of X. (Ours does too.) We think the last line in the table is the most plausible.[67] Taken as a whole, the table shows that loss function analysis depends very strongly on the assumptions going into the evaluation: the available data do not decide the issue. We suspect that methods like loss function analysis will be used to justify the ICM in Census 2000.

AFFILIATIONS

L.D. Brown, Department of Statistics, University of Pennsylvania, Philadelphia; M.L. Eaton, Department of Theoretical Statistics, University of Minnesota, Minneapolis; D.A. Freedman, K.W. Wachter, Department of Statistics, U.C. Berkeley; R.A. Olshen, Division of Biostatistics, Stanford University; S.P. Klein, RAND Corporation, Santa Monica, M.T. Wells, Department of Social Statistics, Cornell University, D. Ylvisaker, Statistics Department, UCLA.

ACKNOWLEDGMENTS

We thank the Donner Foundation for its support (DAF, KWW, MTW, and DY). We also thank our editor, David Kaye, for his help, along with Mike Finkelstein and Philip Stark. DAF and KWW testified for defendants in the census litigation of 1980 and 1990; more recently, they have consulted for the Freshpond Research Institute on census adjustment.

FOOTNOTES

1. For conflicting views on proposed adjustments to the 1990 census, see the exchanges of papers at 9 Stat. Sci. 458 (1994), 18 Survey Methodology No. 1 (1992), 88 J. Am. Stat. Ass'n 1044 (1993), and 34 Jurimetrics J. 65 (1993).

2. Title 13 of the U.S. Code.

3. In Wisconsin v. City of New York, 116 S.Ct. 1096, the Supreme Court resolved the conflict among the circuits over the legal standard governing claims that adjustment is compelled by statute or constitution. Compare City of New York v. United States Dep't of Commerce, 34 F.3d 1114 (2d Cir. 1994) (equal protection clause requires government to show compelling interest that could justify Secretary of Commerce's refusal to adjust 1990 census), rev'd sub nom. Wisconsin v. City of New York with City of Detroit v. Franklin, 4 F.3d 1367 (6th Cir. 1993) (neither the statutes nor the constitution requires adjustment), cert. denied sub nom. City of Detroit v. Brown, 510 U.S. 1176 (1994); Tucker v. United States Dep't of Commerce, 958 F.2d 1411 (7th Cir.) (issue is not justiciable), cert. denied, 506 U.S. 953 (1992). In Wisconsin, the Supreme Court unanimously determined that the exacting requirements of the equal protection clause, as explicated in congressional redistricting

and state reapportionment cases, do not "translate into a requirement that the Federal Government conduct a census that is as accurate as possible. . . ." 116 S.Ct. at 1099–1100. The Court therefore applied a much less demanding standard to the Secretary's decision.

"In 1990, the Census Bureau made an extraordinary effort to conduct an accurate enumeration, and was successful in counting 98.4% of the population. . . . The Secretary then had to consider whether to adjust the census using statistical data derived from the PES. He based his decision not to adjust the census upon three determinations. First, he held that in light of the constitutional purpose of the census, its distributive accuracy was more important than its numerical accuracy. Second, he determined that the unadjusted census data would be considered the most distributively accurate absent a showing to the contrary. And finally, after reviewing the results of the PES in the light of extensive research and the recommendations of his advisers, the Secretary found that the PES-based adjustment would not improve distributive accuracy. Each of these three determinations is well within the bounds of the Secretary's constitutional discretion. . . . Moreover, even those who recommended in favor of adjustment recognized that their conclusion was not compelled by the evidence. . . . Therefore, and because we find the Secretary's . . . prior determinations . . . to be entirely reasonable, we conclude that his decision not to adjust the 1990 census was 'consonant with . . . the text and history of the Constitution. . . .'" 116 S.Ct. at 1101, 1103.

Concluding that the government had shown "a reasonable relationship" between the decision not to make post hoc adjustments and "the accomplishment of an actual enumeration of the population, keeping in mind the constitutional purpose of the census . . . . to determine the apportionment of the Representatives among the States," the Court held that the decision satisfied the Constitution. Indeed, having rejected the argument that the constitution compelled statistical adjustment, the Court noted that the constitution might prohibit such adjustment. 116 S.Ct. at 1109 & n.9. This footnote is adapted from D.H. Kaye and D.A. Freedman, Statistical Proof, in Modern Scientific Evidence, Vol. I, 83, 92 (D.L. Faigman, D.H. Kaye, M.J. Saks, J. Saunders, eds., 1997). There is further discussion in D.A. Freedman and K.W. Wachter, Planning for the Census in the Year 2000, 20 Eval. Rev. 355 (1996).

4. Pub.L. 105-18, tit. VIII, 111 Stat. 158, 217 (1997). Sections 209 and 210 of the Departments of Commerce, Justice, and State, the Judiciary, and Related Agencies Appropriations Act, 1998, Pub.L. 105-119, 111 Stat. 2440, 2480-87 (1997). For the Bureau's report, see infra note 32.

5. 11 F.Supp.2d 76 (D.D.C. 1998) at 87, 90–91.

6. Id. at 89.

7. Id. at 94.

8. 13 U.S.C. §195.

9. 13 U.S.C. §141(a).

10. 11 F.Supp.2d at 103.

11. Id. at 104.

12. 11 F.Supp.2d at 104.

13. Order and Judgment at 2.

14. 19 F.Supp.2d 543 (E.D.Va. 1998).

15. Id. at 548.

16. Id. at 552–53.

17. Id. at 553.

18. Dep't. of Commerce et al. v. U.S. House of Representatives et al. and Clinton et al. v. Glavin et al. Slip Op. January 25, 1999.

19. Id. at 9.

20. Id. at 11.

21. Id. at 13.

22. Id. at 14.

23. Id. at 16.

24. Id. at 16.

25. Id. at 20.

26. Id. at 21.

27. Id. at 22.

28. Id. at 26.

29. Pub.L. 105-119 §209(j).

30. We computed the figures from the Bureau's Advisory Use File. There is substantial overlap between the 19 million and the 13 million, namely, persons whose addresses were geocoded to the wrong block (Section IVB). Other observers quote different numbers; see, for instance, S.E. Fienberg, The New York City Census Adjustment Trial: Witness for the Plaintiffs, 34 Jurimetrics J. 65, 70 (1993). According to the GAO, "The Bureau of the Census estimated that about 6 million persons were counted twice in the 1990 Census, while 10 million were missed." U.S. General Accounting Office, 2000 Census: Progress Made on Design, But Risks Remain, Washington, D.C., at 7, 56 (1997). These figures are widely quoted. We believe they apply to the revised PES (Section IVE), but cannot verify them; indeed, they are somewhat inconsistent with other data published by the Bureau.

31. The ICM would have had a sample of about 1.7 million people; ACE is likely to have a sample of about 700,000; the 1990 PES had a sample of about 380,000.

32. U.S. Bureau of the Census, Report to Congress—The Plan for Census 2000, Bureau of the Census, Washington, D.C. at 21–22, 36 (1997).

33. K. Wolter, Comment, 1 Stat. Sci. 24, 26 (1986)

34. The estimate of 3.0 million can be derived from data in U.S. Bureau of the Census, Decision of the Director of the Bureau of the Census on Whether to Use Information from the 1990 Post-Enumeration Survey (PES) to Adjust the Base for the Intercensal Population Estimates Produced by the Bureau of the Census, 58 Federal Register 69, 75 (January 4, 1993). The figure of 4.2 million is reported by L. Breiman, The 1991 Census Adjustment: Undercount or Bad Data? 9 Stat. Sci. 458, 471 (1994). There is further discussion in D.A. Freedman and K.W. Wachter, Rejoinder, 9 Stat. Sci. 527, 531, 535–36 (1994).

35. J. E. Farber, R. E. Fay, and E. J. Schindler, The statistical methodology of Census 2000, Technical report, Bureau of the Census, Washington, D.C. (1998) at 15–16.

36. Breiman, supra note 34, at 473–4.

37. The Bureau seemed initially to believe that the imputation models used in 1990 were robust and unbiased. Undercount Steering Committee, Technical Assessment of the Accuracy of Unadjusted versus Adjusted 1990 Census Counts, at 10; reprinted in U.S. Department of Commerce, Office of the Secretary, Decision on Whether or Not a Statistical Adjustment of the 1990 Decennial Census of Population Should Be Made for Coverage Deficiencies Resulting in an Overcount or Undercount of the Population, Explanation, three volumes, Washington, D.C., Appendix 4 (1991). Reprinted in part in 56 Federal Register 33582 (July 22, 1991). M. Mulry and B. Spencer, Accuracy of the 1990 Census and Undercount Adjustments, 88 J. Am. Stat. Ass'n 1080, Table 1 (1993). On the other hand, senior Bureau technical staff later acknowledged the "subjectivity" of model specification, and the difficulty of quantifying the resulting uncertainties; they also agreed that the missing data problem is quite intractable because the missingness mechanism is non-ignorable. R.E. Fay and J.H. Thompson, The 1990 Post Enumeration Survey: Statistical Lessons in Hindsight, Proceedings, Bureau of the Census Annual Research Conference, Bureau of the Census, Washington, D.C. 71, 76 (1993). There is discussion of the 1990 imputation model by T.R. Belin, G.J. Diffendal, S. Mack, D.R. Rubin, J.L. Schafer, and A.M. Zaslavksy, Hierarchical Logistic Regression Models for Imputation of Unresolved Enumeration Status in Undercount Estimation, 88 J. Am. Stat. Ass'n 1149. Also see T.R. Belin and J.E. Rolph, Can We Reach Consensus on Census Adjustment, 9 Stat. Sci. 486, 489. But see K.W. Wachter, Comment: Ignoring Nonignorable Effects, 88 J. Am. Stat. Ass'n 1161 (1993), Breiman, supra note 34, Freedman and Wachter, supra note 34, at 531 (which introduces the Q-class terminology), 535–36.

38. Breiman, supra note 34, at 471–2.

39. We computed these figures from the Bureau's Advisory Use File.

40. On the revised adjustment, see U.S. Bureau of the Census, supra note 34. H. Hogan, The 1990 Post-Enumeration Survey: Operations and Results, 88 J. Am. Stat. Ass'n 1047, 1053–54 (1993). Fay and Thompson, supra note 37, at 74. Belin and Rolph, supra note 37, at 498. D.A. Freedman and K.W. Wachter, Heterogeneity and Census Adjustment for the Intercensal Base, 9 Stat. Sci. 476 (1994). (In Table 2 of that paper, entries 3 and 4 should be interchanged.) Breiman, supra note 34, at 471.

41. Processing errors in the PES of about 1.7 million are reported by M. Mulry, 1990 Post Enumeration Survey Evaluation Project P16: Total Error in PES Estimates for Evaluation Post Strata, Bureau of the Census, Washington, D.C., Table 19 (1991). The coding error and the results from the extreme blocks add 1.3 million to this estimate, for a total of $1.7 + 1.3 = 3$ million, in agreement with U.S. Bureau of the Census, supra note 34, at 75. On the Bureau's figures, the processing errors left in the revised PES amount to 1.7 million. Breiman, supra note 34, adds 1.2 million to this estimate.

42. Shares were computed from data supplied by the Bureau, and may differ slightly from other published results due to rounding.

43. R.E. Fay, J.S. Passel, J.G. Robinson, and C.D. Cowan, The Coverage of the Population in the 1980 Census, U.S. Department of Commerce, Government Printing Office (1988). D.A. Freedman, Adjusting the 1990 Census, 252 Science 1233 (1991).

44. Efforts to model the bias at the state level in 1990 were quite unsuccessful, as acknowledged by senior Bureau technical staff. Fay and Thompson, supra note 37, at 76. For another perspective, see Fienberg, supra note 30, at 75. Also see E.P. Ericksen, S.E. Fienberg and J.B. Kadane, Comment, 9

Stat. Sci. 511, 514 (1994). But see K.W. Wachter, The Census Adjustment Trial: An Exchange, 34 Jurimetrics J. 107, 110–13 (1993), or Freedman and Wachter, supra note 34, at 533–34. For more comparisons between the PES and Demographic Analysis, see K. Darga, *Sampling and the Census*, The AEI Press, Washington, D.C. (1999). For a formal model of correlation bias and heterogeneity, see D.A. Freedman, P.B. Stark, and K.W. Wachter, A model for correlation bias, heterogeneity, and ratio estimator bias in census adjustment, Technical Report 557, Department of Statistics, U.C. Berkeley (1999).

45. H. Woltman, Estimated State Level Adjusted Counts Based on Revised State Groups, Bureau of the Census, Washington, D.C. (1991).

46. Freedman and Wachter, supra note 40.

47. Initially, residual heterogeneity was deemed trivial by the Bureau. The Undercount Steering Committee, supra note 37 at 7, was "convinced" that "block parts are homogeneous within post strata"; they saw "no evidence to indicate there are any serious flaws" in the homogeneity assumption at the state level; they relied on J. Kim, 1990 PES Evaluation Project P12: Evaluation of Synthetic Assumption," Bureau of the Census, Washington, D.C. (1991). Later, senior Bureau technical staff acknowledged that residual heterogeneity was appreciable. Fay and Thompson, supra note 37, at 81. J. Thompson, ICM Stratification and Post Stratification, Bureau of the Census, Washington, D.C., at 7 (1998).

48. P.J. Waite and H. Hogan, Statistical Methodologies for Census 2000—Decisions, Issues, and Preliminary Results, Prepared for Joint Statistical Meetings, August 13, 1998. Technical Report, Bureau of the Census, Washington, D.C., at 8–9 (1998).

49. H. Hogan, Downweighting Outlier Small Blocks, Memorandum from Howard Hogan to John Thompson, dated 18 June 1991, Bureau of the Census, Washington, D.C. Hogan, supra note 40, at 1050.

50. Variances and covariances were estimated using a statistical technique called the "jackknife." R.E. Fay, VPLX: Variance Estimates for Complex Samples, Bureau of the Census, Washington, D.C. (1990).

51. D.A. Freedman, K.W. Wachter, D. Coster, R. Cutler, and S. Klein, Adjusting the Census of 1990: The Smoothing Model, 17 Eval. Rev. 371 (1993).

52. See R.E. Fay, Inferences for Small Domain Estimates from the 1990 Post Enumeration Survey, Bureau of the Census, Washington, D.C. (1992). Also see Freedman et al. supra note 51. But see J.E. Rolph, The Census Adjustment Trial: Reflections of a Witness for the Plaintiffs, 34 Jurimetrics J. 85 (1993).

53. E.P. Ericksen and J.W. Tukey, Letter dated 11 July 1991, reprinted in U.S. Department of Commerce, supra note 37, appendix 16. Freedman and Wachter, supra note 3, has further discussion; also see Fay and Thompson, supra note 37, at 83, Freedman et al. supra note 51.

54. Waite and Hogan, supra note 48.

55. Thompson, supra note 47, at 7.

56. U.S. Bureau of the Census, Money Income of Households, Families and Persons in the United States: 1992, Series P-60, Washington, D.C., Table D-2 (1993).

57. Freedman and Wachter, supra note 3. Also see D.L. Steffey and N.M. Bradburn, editors, Counting People in the Information Age, National Academy Press, Washington, D.C., at 115 (1994).

58. Unless otherwise noted, quotations are from U.S. Bureau of the Census, supra note 32.

59. U.S. General Accounting Office, 2000 Census: Preparations for Dress Rehearsal Leave Many Unanswered Questions, Washington, D.C., at 18, 38 (1998). U.S. Bureau of the Census, The Plan for Census 2000, Revised and Reissued February 28, 1996, Washington, D.C. at V-6 (1996). The arithmetic behind our estimate: if the cost of non-response followup without sampling is \$667 million, the cost with sampling at the rate of 3 in 4 is roughly $0.75 \times \$667 \doteq \$500$ million; and

$$\$667 - \$500 = \$167 \text{ million.}$$

It should further be noted that the ICM would cost in the neighborhood of \$500 million. In the census context, sampling may not be cost effective.

60. On the 1980 census, see Fay et al. supra note 43; also see D.A. Freedman and W.C. Navidi, Regression Models for Adjusting the 1980 Census, 1 Stat. Sci. 1 (1986). D.A. Freedman and W.C. Navidi, Should We Have Adjusted the U.S. Census of 1980? 18 Survey Methodology 3 (1992).

61. U.S. Bureau of the Census, Updated Summary: Census 2000 Operational Plan, Bureau of the Census, Washington, D.C. (1999). D. Kostanich, R. Griffin, D. Fenstermaker, Accuracy and coverage evaluation survey, Prepared for the March 19, 1999 meeting of the National Academy of Science panel to review the 2000 census, Bureau of the Census, Washington, D.C. (1999). Post strata may be defined in part by 9 "census divisions:" for instance, the Pacific Division comprises California, Oregon, Washington, Hawaii, and Alaska. A typical post stratum might then consist of Hispanic male renters age 30–49 living in the Pacific Division.

62. See, for instance, U.S. Bureau of the Census, Census 2000 Operational Plan, Bureau of the Census, Washington, D.C., at I-3 (1998).

63. The formula discussed in the text is the one used in 1990. Hogan, supra note 40, at 1050. For the 1980 formula, see Fay et al., supra note 43, or Freedman, supra note 43. Apparently, the formula for 2000 remains to be determined. The logic behind the formula is discussed in Freedman, Stark, and Wachter, supra note 44.

64. For a detailed description of the algorithm, see Hogan, supra note 40, or Freedman et al. supra note 51.

65. This section is adapted from Freedman and Wachter, supra note 3.

66. "PRODSE" is one of the two schemes used by the Bureau to allocate bias from evaluation post strata to individual post strata: bias was allocated in proportion to the production dual system estimate of population. Mulry and Spencer, supra note 37, at 1089. This procedure allocates errors to evaluation post strata, then to the post strata themselves, with reaggregation to states. Our allocation operates directly on share changes for states. β is probably smaller than μ, which offsets our factor of 84 to some degree.

67. For additional details on loss function analysis and the underlying estimates of error, see Breiman, supra note 34, Freedman and Wachter, supra note 34, Freedman et al. supra note 51, D.A. Freedman, K.W. Wachter, R. Cutler, S. Klein, Adjusting the Census of 1990: Loss Functions, 18 Eval. Rev. 243 (1994). The latter papers discuss simulation studies to calibrate the numbers in Table 2. Among other problems, ERD/SE may have a skewed distribution under the null hypothesis, even if $\hat{G}$ and $\hat{H}$ are unbiased: much depends on the nuisance parameters in the model. For a view from the

architects of the Bureau's loss function analysis, see Mulry and Spencer, supra note 37; that paper does not respond to the issues raised here. Responses are given by Belin and Rolph, supra note 37, or Ericksen, Fienberg and Kadane, supra note 44; but also see Fay and Thompson, supra note 37, at 77–83, Breiman, supra note 34, at 521–27, Freedman and Wachter, supra note 34, at 527–37.

Proponents of adjustment often cite A.M. Zaslavsky, Combining Census, Dual System, and Evaluation Study Data to Estimate Population Shares, 88 J. Amer. Statist. Assoc. 1092 (1993). However, Zaslavsky's calculations depend on statistical assumptions that turn out to be wrong. For instance, he assumes that after the census and the PES are done, the Bureau has unbiased estimates for the biases in the DSE, with a known covariance matrix. In fact, the bias estimates and their estimated covariances were themselves severely biased (Section IV). Also see Freedman and Wachter, supra note 34, at 530, 532, showing that loss function analysis does not measure the social costs of errors, but merely provides a statistical summary, which may or may not be useful. This section is adapted from Freedman and Wachter, supra note 3, which has further discussion.

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