

Comment

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1. INTRODUCTION

In assessing the paper by Freedman and Navidi, we must remind ourselves of what it was that brought so many statisticians to New York City in 1984 to put their views on trial in a United States district court. The problem at hand, then and today, is that the Decennial Census on which so many of this country's decisions depend does not do what it is supposed to do: fairly measure the population. The census falls short of that standard in a particularly intolerable manner, for it is by and large the least advantaged members of our society who are consistently left out. In the face of this, the courts have turned to our profession to ask, "can we do better, and if so, how?" It was that question, one that inherently calls for comparisons, that Ericksen and I sought to address in presenting our regression analysis.

In criticizing the regression and indeed any proposed means of adjusting the census, therefore, one must strive to quantify alleged deficiencies and compare them to the deficiencies in the census itself. Freedman and Navidi, while they enumerate points at which a regression analysis could fall short of truth, fail to make that fundamental comparison. While we welcome insights as to our methodology, what is called for from those who would criticize proposed adjustments is an honest effort to measure their weaknesses against those of the census. Without such an approach, we are given only the sound of one hand clapping and may be saddled with the injustices of the census for decades to come.

I begin by explaining the context of the New York lawsuit, and the rebuttal testimony in which I participated. Next, I discuss Freedman's surrebuttal testimony. I then explain my view of the proper role of assumptions in a statistical analysis and conclude by addressing specific points raised by Freedman and Navidi.

2. CONTEXT

In *Cuomo v. Baldrige*, New York City and State (and a number of New York officials and residents) sued the U. S. Census Bureau and other federal gov-

ernment officials and entities seeking:

- (1) Judgement declaring that New York City and New York State were disproportionately undercounted in the 1980 Census and
- (2) An order requiring the Bureau to adjust the 1980 Census to reduce that disproportionate adjustment to the maximum extent feasible"

(Plaintiffs' memorandum, pp. 90, 91)

The first point, at least with regard to New York City, was essentially conceded by the middle of the trial ("I am prepared to find right now, and based upon the testimony I heard from both the government's witnesses and your witnesses, that New York City was disproportionately undercounted. I don't even think that's an issue anymore" (Judge Sprizzo, transcript at pp. 2099–2100)). Inasmuch as Freedman and Navidi point out that adjustment methodologies are subject to biases, it is important to understand the biases in the *census* that lead to the conclusion that there was a disproportionate undercount. According to demographic analysis of the 1980 Census, blacks were undercounted by 4.8%, while nonblacks were overcounted by 1.1% (Bureau of the Census, 1982). In stating briefly that the Census Bureau made intensive efforts to eliminate the undercount in the 1980 Census, Freedman and Navidi note that demographic analysis "indicated that at the national level, there was an overcount of about $\frac{1}{4}$ of 1% of the legal population, although some of the illegal population was missed." The apparent effort to minimize the extent of the undercount ignores the well recognized fact that it is the *differential* undercount between the rates at which different groups are missed that causes inequity—not the overall national undercount. Places having disproportionate numbers of groups that are highly undercounted receive unfair treatment. The gap between the 1980 Census black undercount rate and the nonblack undercount rate is about the same size as it was in 1970 (when the gap was 6.1%) (Bureau of the Census, 1982). The Bureau's Post Enumeration Program (PEP) estimates of undercount, derived from matching studies, also showed disproportionately high undercounts for blacks as well as the poor, Hispanics, and other disadvantaged groups (Ericksen affidavit, paragraphs 111–121.)

Thus attention focused on the second point, which would require the Census Bureau to adjust, but does

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not seek to impose a particular method upon it. The defendants argued that the plaintiffs are not entitled to relief, and that “there is at present no feasible method for . . . reliably adjusting the official census counts to reflect more accurately the true population distribution of the United States” (Defendants’ memorandum, p. 2). Thus but for their argument that the plaintiffs are not entitled to relief, the government’s case rests on the alleged inability of statisticians to do any better than the raw counts.

The latter proposition is very difficult to support, other than by repeating it many times. The Census Bureau submitted affidavits touching on this point, including those of Bureau employees Bailar, Seigel (at paragraph 21), and Wolter (at paragraph 16), and outside experts Coale (at paragraph 8), Keyfitz (at paragraph 5), Nathan (at paragraph 11), Stoto (at paragraph 6), and Trussel (at paragraph 2). Each gave the opinion, possibly after reviewing some alternatives and the potential weaknesses of those alternatives, that those methods are either not “feasible” or not “reliable.”

There was little discussion by Bureau witnesses of the inaccuracy of the census counts themselves (but see Passel testimony at paragraphs 951, 967, 1041, 1042, and 1080) and consequently not very much analysis of the comparison between an hypothesized adjustment and the census counts themselves. However, the Bureau’s experts were not unanimous in their approaches and views, and one (Keyfitz affidavit, paragraph 11) conceded, “The witnesses for the plaintiff 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.” (At trial, Keyfitz took the position that in statements of this sort he was referring to absolute numbers rather than proportional numbers (Keyfitz testimony, paragraphs 2156 and 2157).)

A central focus of the discussion on adjustment was the Post Enumeration Program, the Bureau’s matching study of the 1980 Census. The Bureau had originally produced some 29 studies of PEP estimates by varying assumptions. After criticism from the American Statistical Association Technical Panel on the Undercount (on which I served), the number of such estimates was cut down to 12. After presenting its views to the Bureau orally, the Technical Panel subsequently published a report in which it stated, “These (PEP) estimates are based on various assumptions, some of which are clearly more reasonable than others. The Bureau should state its views on the relative worth of each estimate” (ASA Technical Panel on the Census Undercount (1985)). To the best of my knowledge, the Bureau has not acted on this suggestion.

Even among these 12, there were estimates that the Bureau conceded were based on extreme assumptions (Cowan testimony at paragraphs 630 and 631 and Stoto testimony at paragraphs 712–714). Nonetheless, witnesses testifying for the Bureau pointed to the differences among the PEP estimates as support for the proposition that they were unreliable.

There are, of course, many ways statisticians might handle such a situation. One could take the mean, or the median, of the 12 estimates for each area as a compromise member. Ericksen and I chose instead to study the way in which the PEP series were derived, and to choose a main series on that basis. For reasons explicated by Ericksen (affidavit, paragraphs 105–108) we chose the series 2/8, 2/9, 3/8, and 3/9 as especially reasonable, and chose to do our calculations based on series 2/9 PEP estimates as the most reasonable estimates of the undercounts for 16 central cities and the 50 states and remainders of states with the central cities excluded.

One could stop with the PEP 2/9 series as the estimate. However, we believe that such a strategy could lead to overadjustment. Sampling theory leads us to think of the place-by-place estimates as sample averages of independent observations, and hence asymptotically normal. The series are intended to estimate the undercount, and having chosen series 2/9, we were willing to suppose it normally distributed around the “true undercount,” with standard deviations as estimated by the Census Bureau. The classic work of Stein (1956) and James and Stein (1961) warns us not to take a high dimensional (here 66-dimensional) average as the estimate of a normal mean vector. The work of Stein suggests shrinking these estimates toward some common origin. If one shrinks them entirely to such an origin, one will have adjusted each place by the same percentage, which is equivalent to not adjusting at all with respect to congressional apportionment and almost all revenue-sharing programs. Thus we need a model that will allow the data themselves to indicate an appropriate amount of smoothing. One simple and tractable model that permits smoothing is to assume that the true undercounts of the 66 areas, themselves, are independent, identically and normally distributed with some mean β and some variance σ^2 . As $\sigma^2 \rightarrow 0$, all the areas are regarded as having been undercounted by β . As $\sigma^2 \rightarrow \infty$, the place-by-place PEP undercount data are smoothed less and less.

Already, of course, such a model has a regression structure in which the second stage of the hierarchical model design matrix is a column of ones. One could stop here, using as estimates of the true undercounts the appropriate weighted average of the sample mean and the overall average. Something would have to be done about the variance σ^2 , either declaring a value of

it, declaring a distribution for it and integrating with respect to that distribution, or estimating it.

Such an account of the undercount is incomplete in the following sense: some places may reasonably be expected to have lower undercount rates than others for systematic reasons. Demographic analysis surely suggests that the extent of racial minorities (blacks and nonblack Hispanics) in an area is likely to be associated with the undercount. In some rural areas, a "conventional" census without an address register was used. Such a difference in collection methodology makes it a natural candidate as an explanatory variable, either to cause, or to be associated with, systematic geographic differences. Thus we come to the model

$$(1) \quad \mathbf{y} \sim N(\boldsymbol{\gamma}, D_r),$$

$$(2) \quad \boldsymbol{\gamma} \sim N(X\boldsymbol{\beta}, \sigma^2 I),$$

where \mathbf{y} is a 66-dimensional vector of PEP-measured undercounts, $\boldsymbol{\gamma}$ is a 66-dimensional vector of true undercounts, D_r is a diagonal matrix with estimated PEP variances on the diagonal, X is a matrix of independent variables, including a constant, percentage minority, percentage conventionally enumerated, and perhaps other variables. See Ericksen and Kadane (1985) for a fuller statement of the model, and Fay and Herriott (1979), Morris (1983), and DuMouchel and Harris (1983) for other uses of it.

The important aspects of this model, from my perspective, are:

1. It permits a wide variety of views on the undercounts to be expressed. For example, if $\boldsymbol{\beta} = (1, 0, 0 \dots 0)$ and $\sigma^2 \rightarrow 0$, there were no differential undercounts and the raw counts should be accepted. If $\sigma^2 \rightarrow \infty$, the PEP data should be used without smoothing. Values of σ^2 in between would be smoothed compromises between these positions. Using such a model and estimating σ^2 allows the data to help to tell us how much smoothing to do.

2. The model is incomplete in that it does not specify all of the independent variables.

3. Since tractability was used as a reason for specifying both normality and linearity in the second stage, the results must be checked for reasonableness, especially against the possibility of overadjustment.

4. This model does not specify how estimates for these 66 large geographic areas (like states) should be carried down to smaller geographic areas (like counties and towns). There are several ways in which this might be done, including the demographic-synthetic method, using the regression at the lower levels, iterative proportional fitting, and imputation. (See Citro and Cohen (1985) for a discussion of these methods.)

The Ericksen-Kadane model had been proposed as a general framework as early as 1981 or 1982. When

the PEP data became available, I programmed the implementation of the model, and we began to explore its behavior for the various PEP series and with various independent variables. In early 1984, faced with Census Bureau claims that no methods were feasible to do what in fact we were doing, Ericksen and I agreed to present our initial calculations to the court. We made no claim that these were definitive, nor that they represented our finished judgment. Thus, in a sense, Freedman and Navidi's paper is a comment on work in progress at the time we testified about it, and that just now is being written up (Ericksen and Kadane, 1985).

3. FREEDMAN'S TESTIMONY AND STATISTICAL PHILOSOPHY

In his surrebuttal testimony, Freedman testified, "If the assumptions underlying the method hold, then you can rely on the computer outputs. If the assumptions behind the method do not hold, then it is hard to rely on the outputs" (transcript at paragraph 2627) and a bit later "... these are difficult assumptions to make . . . , so I am very troubled by this use of regression" (transcript at paragraph 2633). While we may be sure that Judge Sprizzo was sympathetic to Freedman's declaration of his psychological distress, it should be noted that Freedman does not say that the conclusions are wrong. He hints, politely, at catastrophic error without quite saying it.

4. THE PROPER ROLE OF ASSUMPTIONS IN STATISTICAL PRACTICE

I believe that assumptions are useful to state in statistical practice, because they impose a discipline on the user. Once a full set of assumptions is stated, the conclusions should follow. (Actually, only a Bayesian analysis can meet this standard, but that's another topic for another time.) Starting and following the assumptions, then, can be regarded as a way of being sure that one has not introduced an internal inconsistency into one's analysis.

But in applications, only a very naive user would believe in the literal truth of the assumptions. Thus in my view, when I state and use an assumption, I mean that I think something like this is true, but surely I do not mean that exactly this is true. The modern statistical interest in robustness, I believe, stems from an interest in the possible vulnerability of statistical conclusions to imprecision of the assumptions, and a desire for reasonable protection against such likely imprecision. (In the Bayesian context, see my book Kadane (1984) and Box and Tiao (1973) for examples of how various scholars have dealt with this issue.)

Thus the task in statistical modeling, as I see it, is

to model the most important gross effects simply and not to bother with minor effects unless for some reason they are of practical importance. In the Ericksen-Kadane model as we implemented it, the gross effects we are looking for are undercounts in minority populations, especially in central city high crime ghetto areas and in very rural areas.

Casually criticizing assumptions of a model is easy, but unrevealing. To show that too simple a model was used, I would embed the model in a more complex one, and then by estimation show that the data support the more complex view. To simplify a model, I would show that nothing essential is lost by the simplification, either in terms of one's view of the phenomenon or in expected utility terms if a decision is required. Both of these processes are constructive since they lead to replacing one model with another one.

Freedman and Navidi call for empirical validation of the assumptions of our model. To do this, presumably they would have us embed our model in a more general, weaker model, and see the extent to which our model is sustained by the data. This is exactly the process we used in coming to the model stated above. It would have been simple and would have reduced the list of assumptions considerably to have stopped with our equation (1). For the reasons just outlined, we thought that to do so might lead to overadjustment, and might not give the data themselves an opportunity to tell us that our position on the undercount was wrong. Using the full model (1) and (2), the empirical work sustained our view of the process underlying the undercount, and gave us estimates of the undercount we deemed to be reasonable. Any such embedding requires assumptions, which can always be attacked, so in some ultimate sense every scientific analysis of data can be questioned in the same manner. What is at issue, however, is not whether the assumptions are in error, but whether the errors are so large as to lead to worse estimates than those provided by the raw census counts.

Indeed, while they freely criticize the presence of assumptions in the regression methodology, Freedman and Navidi never quantify the error that purportedly results. Without an effort at quantification, we cannot make the necessary comparison to the raw census counts to decide whether the adjusted figures are better than the unadjusted census. The fact that, as Freedman points out, the adjustment methodologies are based on "assumptions" does not advance the ball. The raw census is based on assumptions that ought to stagger Freedman. Specifically, the entire census procedure is critically dependent on whether a person's home is clearly recognizable as a residential structure at an identifiable address; on whether the person who lives there is willing to cooperate with the Government by providing the requisite information; on whether that person is proficient in English; on

whether that person is educated enough to understand the questions put to him; on whether that person is a member of a well defined family unit; and on a host of other similar assumptions that many members of our society, particularly the disadvantaged, cannot satisfy.

Any hope that the assumptions underlying the census are in fact satisfied is belied by all of the studies (referred to above) showing a sharply disproportionate undercount. Potential adjustment methodologies such as PEP and demographic analysis can alleviate the systematic biases in the census by supplementing it with information obtained from alternative sources (the Current Population Survey in one case and birth, death, and migration data in the other). Yes, the adjustment methodologies are based on assumptions; the question is whether the inaccuracies introduced by the adjustments outweigh the benefit they give us in reducing the systematic biases in the census. Freedman and Navidi, content merely to denounce the presence of assumptions in adjustment methodologies, do not give us a clue.

5. THE SCHOLARSHIP OF THE FREEDMAN/NAVIDI PAPER

The position that Freedman and Navidi criticize is in many respects a figment of their own imagination; it does not represent "New York" (for which only its attorneys can speak), and it invents a position of "New York experts" as though we are assumed to agree on each point, although, of course, we do not necessarily do so.

This invention of a position to attack is most egregious in the matter of estimation of the population of subareas. One method mentioned in passing as a possibility for estimating subareas was to carry the regression down. There are, of course, lots of other methods. The focus of the effort I engaged in was the estimation for the 66 areas for which we had PEP data.

A second invention is with respect to my attitude toward the independent variables. I think that percentage minority and percentage conventionally enumerated are rather well established; the former by demographic analysis, the latter because a different data collection process was used (and even if it hadn't been, these are very sparsely populated areas where people are easy to miss). The third variable, crime rate, I have some doubts about. Of course, there are measurement problems with it, since it relies on reported crime, which is widely suspected to be handled differently in different places. The urbanization variable suggested by Freedman and Navidi is another we considered; however, it also has measurement problems, since the Census Bureau regards quite small towns as "urban." We also thought about and used "central city" as a variable, which picks up the 16

largest cities and ignores all the others. My view on this matter, contrary to what Freedman and Navidi may have led the reader to believe, is that any of these choices is not unreasonable, and that all (or almost all) of them lead to an adjustment for the 66 areas that is more accurate than the raw census counts.

A third invention has to do with my attitude toward standard errors, both the reported PEP standard errors and the standard errors estimated using our model. First, with respect to estimates of the parameters K_i (τ_i in the notation above), I agree that sampling error in these estimates is present and is a possible problem. However, it is not so great a problem as Freedman and Navidi would have the reader believe. I have conducted several computer experiments to explore different ways of smoothing these estimates. For example, replacing K_i for states by the median K_i for states, and for central cities by the median for central cities, changes our estimates very little (see Ericksen and Kadane, 1985, for details). Increasing the K s generally (we explored doubling them), merely led to a reduction in our estimate of σ^2 , and hence leads us to put relatively greater reliance on the regression estimate and less on the raw PEP estimate. Second, Freedman and Navidi point to sources of variability excluded from the standard errors in our model. It seems to me that the sequence of steps a problem like this will go through leads to better estimates and larger (more realistic) standard errors as we learn how to take more and more uncertainty into account. I agree that taking into account uncertainty in σ^2 would add to the variances reported (but I doubt it would add much). But the essential point is that we are talking of replacing the census counts with their implicit zero variances. Surely our estimates of the variances are more realistic than that!

Had Freedman and Navidi been more scrupulous about giving references when they made statements about the positions and motives of others, they would have had greater contact with what was actually said, and might not have made these errors. As it stands, they report what *should* have happened by their lights to make a good story, but it's not what actually occurred in many respects.

Whatever their deficiencies as historians, I have no complaints about their mathematics. It is strictly matters of statistical interpretation that are at issue.

6. STATISTICS IN THE FREEDMAN/NAVIDI PAPER

Much of the body of the Freedman/Navidi paper is mere speculation. For example, their demonstration that PEP is biased comes to the single sentence, "Since the great majority of persons were counted in

the census, there is a strong tendency for mistakes to inflate rather than deflate the estimated count." To be counted as a census omission for the PEP series, a person not only had to be on the CPS (April or August, as the case may be), and not be found in a search of census records, but had to be found again by census field personnel. Thus while there may be a bias in PEP, I believe that the PEP estimates of the undercount were generally too low. It is comforting that the PEP series Ericksen and I favor, 2/8, 2/9, 3/8, and 3/9, particularly 2/9, coincide quite well with demographic analysis and do so better than the other series do (Ericksen and Kadane, 1985).

Of the nonspeculative portion of the paper, most arguments rely on PEP series 10/8. This choice of comparison series was made, according to Freedman and Navidi, because it was next to 2/9 on the computer printout they were given. This was a bit of a surprise to us, since 3/8 follows 2/9 on each of the Census Bureau printouts we have. As luck would have it, Freedman and Navidi were thus led to one of the least sensible of the 12 series. It is from the August PEP, and thus suffers from error because of those who moved between April and August. Nonetheless, for this series no imputation was done for movers. Two Census Bureau witnesses, Stoto (see testimony at paragraphs 712–714) and Cowan (see testimony at paragraphs 630 and 631) exclude 10/8 from their lists of reasonable series, as did Ericksen (affidavit, paragraph 106). I think that a choice among these PEP series should be made based on how those series were produced. Whatever the method and motives of Freedman and Navidi, pick an extreme series they did.

The simulation experiment reported in the last few pages of their paper requires special comment. They begin by assuming that the vector of undercounts, γ , is fixed and take the values as estimated by the (highly questionable) 10/8 series. (Whatever one's view of the "randomness" of the raw census counts, the 10/8 series estimates cannot be taken to be correct. If it could, then the optimal adjustment would be clear—use the 10/8 series. Freedman and Navidi do not seem eager to embrace this consequence of their assumption.) Having taken the 10/8 series, with all its infirmities, they then add stochastic error to it (their equation (26)) to generate artificial data sets. On the basis of so slender a reed, then Freedman and Navidi feel justified in concluding "i) The variables which belong in the equation cannot be identified from the data; ii) σ^2 cannot be reliably estimated from the data; (iii) New York's (sic) standard errors are much too optimistic." When it comes to making bold assertions from unsupported assumptions, Freedman and Navidi, by the end of their paper, show themselves to be in a class by themselves.

7. ROBUSTNESS OF THE ERICKSEN-KADANE MODEL

Agnosticism is not a relevant position in this debate. Population estimates must be made, either using the raw census counts as is now done or in some other way. The question is what is the best way in the light of the knowledge that we have or can get.

While Freedman and Navidi do not contribute much to possible answers to this question, their paper does suggest the following sensitivity analyses of the Ericksen-Kadane model:

a. Change the PEP series used.

b. Change Freedman/Navidi's equation (1) to allow bias. How could this be made operational? Should the bias be taken to be a function of known independent variables? Will it make a difference?

c. Assume correlation in both equations (1) and (2). How much correlation is reasonable? Will it matter? How large would those correlations have to be in order to affect seriously the resulting estimates?

d. Add or change independent variables.

To date, Ericksen and I (1985) have done extensive computer work with questions a and d and found generally that our estimates of the 66 areas are unaffected by these changes.

8. CONCLUSION

The 1990 Census is hard upon us. To whatever extent there are real deficiencies in the PEP and any other adjustment methodologies, it is time that we statisticians turned our attention to devising solutions rather than merely pointing to problems. As Barbara Bailar of the Bureau testified:

"We have been doing matching studies off and on over a period of 30 years. . . . I think if we had been working on matching and really devoting all kinds of research necessary to learn how to do matching well, we would probably be a lot further ahead."

(Transcript paragraph 1493)

Let us hope that the statistics profession is soon ready to meet the challenge of the disproportionate undercount.

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