Commentary

Another Opinion

Statistical Methods in the Census

Rarely has an application of accepted techniques from the mathematical sciences caused as much discussion among politicians and in the media as the Census Bureau's proposal to use estimation based on statistically designed samples to supplement direct counting in arriving at final counts from the year 2000 Census. Congressional Republicans in particular have strongly opposed this plan, reasoning that increasing the tally of groups undercounted by traditional methods might tilt the reapportionment that follows each census in favor of the Democrats. In response to a suit filed by House Republicans, a federal court panel ruled in August 1998 that the use of statistical sampling for congressional apportionment violates the Census Act.

Here are a statistician's comments on this public controversy. I will ignore the legal and political issues and also will avoid a critique of the specific (and complex) proposal put forward by the Census Bureau. I am mainly concerned about the effects that unrestrained and inaccurate comments made by politicians and commentators during the debate may have on public attitudes toward statistics and toward science in general.

First, estimation based on statistically designed samples is a standard and widely used method for obtaining information about large human populations. The mathematical theory has reached an advanced state, and the practical difficulties that the theory does not deal with are understood, along with methods for at least partially coping with them. Almost all government economic and social data—think of the monthly unemployment rate—are produced by sampling. In fact, most of the data produced by the decennial census come from a "long form" sent to only a statistically selected sample of the population. Even the plaintiffs in the recent court case agree that existing laws "encourage, if not require, the extensive use of sampling" for census responsibilities. The court ruling is based, not on any deficiencies in sampling, but on language in the Census Act that makes a specific exception for its use in apportioning Congress. Broad attacks on sampling as a scientific method are either uninformed or malicious.

In the light of the accepted scientific status of statistical sampling, it is not surprising that two National Research Council panels and a Blue Ribbon Panel of the American Statistical Association concluded that (in the words of the latter) "sampling has the potential to increase the quality and accuracy of the count and to reduce costs" in the 2000 Census. Professionals generally see sampling as another in the long series of innovations in census methods since the first enumerators went out on horseback in 1790.

Second, and perhaps even more important, the nonpartisan professional status of government statistical offices is a national asset that deserves protection. We do not expect to read that, as recently happened in Russia, our chief government statisticians have been arrested for manipulating data to help businesses avoid taxation. We do not expect an unfavorable unemployment report to be withheld because an election is imminent. And we do not expect Census Bureau plans for the 2000 Census to have any motivation other than a desire for the most accurate count possible. I am convinced that the Bureau's plans have no other motivation, and this conviction is reinforced by the fact that planning began on the basis of expert recommendations from the NRC and other groups.

Some respected national columnists have attacked the Census Bureau in language that damages the public interest. William Safire implied that government statisticians are both politicized and lazy. He characterized Census Bureau plans as "having a statistician put a thumb on the scale." George Will took a more subtle approach: this administration, with its "remarkable record of lawlessness," will manipulate the sample selection. Government statisticians are not explicitly accused but are presumably too weak to resist blatant political interference.

Repeated accusations of political motivations in Census Bureau planning, combined with the impressive level of distrust of government that characterizes American public opinion, will lead to distrust of government statistics in general. If the census is politicized, why not the unemployment rate? A conspiracy lurks behind every weighted mean. The distrust of national statistics may grow worse: the continuing debate is hindering the immensely detailed logistical preparation required for a successful census, with or without sampling. The 2000 Census may well be seen by the public as a failure.

Public debate in the age of the sound bite inevitably oversimplifies complex issues. The proposal to use statistical sampling in the 2000 Census combines arcane mathematical science with potentially substantial political implications. It is not surprising that oversimplification has predominated among politicians and commentators and also in how more careful statements, such as those of the NRC and the ASA, are heard. The fallout from oversimplified and sometimes irresponsible claims will damage statistics as a discipline and will damage government statistical offices and public trust in important national data.

—David S. Moore

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Letters to the Editor

Authors Can Publicize History of Papers on Web

Authors submit papers to journals. Journals need authors, but they give little or no opportunity to authors to complain about poor service. Journals publish submission dates (largely for their own protection perhaps), but would hardly publish any history of long delays in publication, of the frequently inane referee's reports, or of rejection of the excellent paper by other journals for idiotic reasons.

Web technology has given authors a tool to deal with the above problems; I suggest using it. Readers interested in background history can be provided with the record of the refereeing process, including referee's comments. On the positive side, this might provide insights into the paper, especially if the refereeing was effective, but it can also provide an often needed outlet for the author's frustrations with the referees and with the real reason for the long gap between submission and publication. Maybe few readers will be interested in such history, but it would make me feel that I have levelled the playing field a little by providing it, and I shall—oh yes, I shall!

—Larry Shepp
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Achievements of Chaos Theory

The commentary "See No Evil, Hear No Evil, Speak No Evil" by S. G. Krantz (October issue of the *Notices*) has just come to my notice. While I sympathize with Krantz's desire to challenge the conformism of the mathematical community, I regret that lack of caution weakens his argument considerably. He claims that "there is not one example of any scientific problem that has been solved (not just described)" using chaos theory. In fact, the only "explanations" that physics provides are through theories that accurately "describe" reality. This has been achieved by relativity, quantum mechanics, and, I believe, chaos theory. (For details on the latter I must refer, for example, to my review "Where can one hope to profitably apply the ideas of chaos", *Physics Today* **47**, no. 7, (1994), 24–30.) The problem is that any successful idea produces an explosion of papers by people who want to invest in the success. This leads to inflated claims, particularly visible at the level of popular science, and then to counterclaims that this is all baloney. (This has happened before with relativity, quantum mechanics, and Gödel's theorem.) Professionals have, of course, a duty to try to set things straight. To return to the general issue of challenging conformism in the scientific community, I think that this has to be done in a careful and responsible way to be effective. As a positive example, I would mention the paper of A. Jaffe and F. Quinn "Theoretical mathematics: Towards a cultural synthesis of mathematics and theoretical physics", *Bull. Amer. Math. Soc. (N.S.)* **29** (1993), 1–13. Even those who did not like this paper will admit that it was useful in prompting the beautiful response of W. Thurston, "On proof and progress in mathematics", *Bull. Amer. Math. Soc. (N.S.)* **30** (1994), 161–177 .

—David Ruelle
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The *Notices* invites letters from readers about mathematics and mathematics-related topics. Electronic submissions are best. Acceptable letters are usually limited to something under one printed page, and shorter letters are preferred. Accepted letters undergo light copyediting before publication. See the masthead for electronic and postal addresses for submissions.

About the Cover

The computer-generated image was provided by Paolo Dominici, who lives in Todi, Italy, and is now elaborating new visualizing styles for rendering mathematical objects and ideas more attractive and understandable.

Dominici writes, "This balanced assemblage of curved tetrahedra and carved cones is the spherical neighborhood, of radius 5/2 around the origin, of the algebraic surface with affine equation

$$4(\tau^2x^2 - y^2)(\tau^2y^2 - z^2)(\tau^2z^2 - x^2) - (1 + 2\tau)(x^2 + y^2 + z^2 - 1)^2 = 0$$

where $\tau = (\sqrt{5} + 1)/2$ is the golden section. It was discovered by Wolf Barth (Erlangen University, Germany) in 1994. The surface has 20 nodes at the vertices of a regular dodecahedron of edge $2/\tau$ and 30 nodes at the midpoints of the edges of a concentric dodecahedron of edge $2/\tau^2$. Its projective completion is of degree 6 and has 15 additional nodes, thus obtaining a total of 65 nodes, the maximum for a sextic surface. Beyond its intrinsic beauty, the surface has the rare gift of enclosing in its symmetric frame many hundred years of geometrical insight and skill."