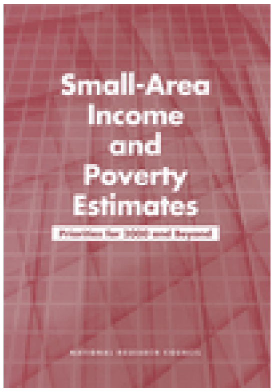

Small-Area Income and Poverty Estimates: Priorities for 2000 and Beyond

Constance F. Citro and Graham Kalton; Editors, Panel on Estimates of Poverty for Small Geographic Area, Committee on National Statistics, National Research Council

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Small-Area Income and Poverty Estimates

Priorities for 2000 and Beyond

Panel on Estimates of Poverty for Small Geographic Areas

Constance F. Citro and Graham Kalton, *Editors*

Committee on National Statistics

Commission on Behavioral and Social Sciences and Education

National Research Council

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PANEL ON ESTIMATES OF POVERTY FOR SMALL GEOGRAPHIC AREAS

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Acknowledgments

The Panel on Estimates of Poverty for Small Geographic Areas wishes to thank the many people who contributed to the preparation of this, the panel's final report, and to the panel's work over the past 4 years. The panel's three interim reports (updated and combined into a single volume, National Research Council, 2000c) evaluated the Census Bureau's estimates of poor school-age children for use in allocating Title I funds; this report outlines an agenda for research and development of the Bureau's income and poverty estimates for small areas and discusses issues about the use of such estimates.

We thank, first, current and former staff of the Census Bureau who prepared the poverty estimates for states, counties, and school districts that we reviewed, many of whom also worked on the evaluations of those estimates: David Aultman, William Bell, Patrick Cardiff, John Coder, Robert Fay, Robin Fisher, Matthew Kramer, Esther Miller, Mark Otto, Ronald Prevost, Douglas Sater, Paul Siegel, Cotty Armstrong Smith, Alexander Strand, Jess Thompson, George Train, David Waddington, and Signe Wetrogan. We also thank the Census Bureau staff who facilitated the arrangements for the work: Cynthia Clark, Nancy Gordon, Charles Nelson, and Daniel Weinberg. We thank Charles Alexander, Pat Doyle, and Joseph Knott, respectively, for useful information about the planned American Community Survey, the Survey of Income and Program Participation, and the Bureau's Master Address File and geocoding capabilities.

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I thank my panel colleagues for their sustained commitment to the work of the panel. For contributions to the final report, I particularly thank Nancy Dunton, who supervised the collection of information about state uses of small-area income and poverty estimates for fund allocation (see Chapter 2); Tom Louis, who contributed importantly to the research and development agenda for the current Census Bureau income and poverty models (see Chapter 3); and Alan Zaslavsky, Allen Schirm, and David Betson, who analyzed the interactions of provisions of fund allocation formulas and uncertainty in estimates of poverty (see Chapter 6 and the Appendix).

The panel was assisted by a very able staff. The panel is enormously indebted to Connie Citro, who has done a superlative job as the project's study director. Her exceptional writing skills and depth of understanding of the many complex matters involved were irreplaceable input to our efforts, enabling the panel to produce four reports under very demanding deadlines. Connie had prime responsibility for drafting all the reports. Her skills in doing so, her good humor in crafting revisions, and her ability to keep the panel on track contributed greatly to the enjoyment of participating in this project. Michael Cohen made important contributions throughout the entire project and assisted in drafting the section of the final report on the current income and poverty models. Michele Ver Ploeg assembled the information on federal program uses of small-area income and poverty estimates and drafted the section on the needs for such estimates. Meyer Zitter contributed to the section on uses of administrative records for improved estimates. Telissia Thompson and Jamie Casey provided excellent administrative support for the study and for the preparation of the report. Eugenia Grohman, associate director for reports of the Commission on Behavioral and Social Sciences and Education, improved the report through her fine technical editing. To all we are grateful.

This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the Report Review Committee of the National Research Council. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making the published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process.

We thank the following individuals for their participation in the review of this report: John Adams, RAND, Santa Monica, California; John F. Geweke, Department of Statistics, Henry B. Tippie School of Business, University of Iowa; Malay Ghosh, Department of Statistics, University of Florida; Roderick J. A. Little, Department of Biostatistics, School of Public Health, University of Michigan; Lee Rainwater, Department of Sociology, Harvard University; Edward Spar, Council of Professional Associations on Federal Statistics, Alexandria, Virginia; Bruce Spencer, Department of Statistics, Northwestern University; and Harold Watts, Greenport, New York.

Although the individuals listed above provided constructive comments and suggestions, it must be emphasized that responsibility for the final content of this report rests entirely with the authoring panel and the institution.

Graham Kalton, *Chair*
Panel on Estimates of Poverty for
Small Geographic Areas

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Small-Area Income and Poverty Estimates

Executive Summary

Recent trends in federal policy for social and economic programs have increased the demand for regularly updated small-area estimates of income and poverty. More than $130 billion of federal funds are allocated each year to states and localities by means of formulas that include such estimates, and the estimates are used for program evaluation and other purposes as well. States also use small-area income and poverty estimates to allocate their own and federal funds to substate areas. The funds support a wide range of activities and services, including child care, community development, education, job training, nutrition, public health, and others.

The newest source of small-area income and poverty estimates is the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program, which was begun in the early 1990s to provide estimates that would be more timely than those from the decennial census. The 1994 "Improving America's Schools Act" called for the use of the SAIPE estimates of poor school-age children for counties and school districts to allocate more than $7 billion annually for programs for educationally disadvantaged children under Title I of the Elementary and Secondary Education Act. The 1994 act also required a panel of the Committee on National Statistics at the National Research Council to determine if the estimates were sufficiently reliable for Title I allocations and to make recommendations for their use and future development.

The first state and county SAIPE estimates were issued in early 1997 (for income year 1993); they included estimates of median household income, numbers of poor people, poor children under age 5 (states only), poor children aged 5-17 in families, and poor people under age 18. The estimates released in early 1999 (for income year 1995) also included the numbers of poor school-age children in families for more than 14,000 school districts. The U.S. Department of Education has used the SAIPE estimates for Title I allocations since 1997, and some other programs use them as well.

Because there is no one data source that can provide the SAIPE estimates, the Census Bureau develops them by using statistical modeling techniques that combine data from household surveys, the decennial census, and administrative records. The SAIPE estimates, consequently, are "indirect," and, as such, their quality depends on the choice of a suitable statistical model.

CONCLUSION

In the coming decade, it should be possible to develop more accurate and timely income and poverty estimates for small areas by using new and improved sources of data from household surveys and administrative records. However, none of the existing or planned surveys or administrative records sources can, by itself, provide direct estimates of sufficient reliability, timeliness, and quality of responses for all of the SAIPE income and poverty estimates. Therefore, the panel concludes that the SAIPE program must continue to rely primarily on models that combine data from more than one source to produce indirect estimates.

USING ESTIMATES IN PROGRAMS

The use of small-area income and poverty estimates for allocating funds or related program purposes imposes significant requirements if the estimates are to satisfy the intentions of program legislation. Such requirements include the desired concept or definition of poverty or income measured, the level of geographic detail, the level of population or demographic detail, the timeliness of production and updating, and the accuracy of measurement. The selection of a set of estimates to use in a given program will generally involve tradeoffs among competing goals. For fund allocation, it is important to consider features of the specific allocation formula, some of which may be sensitive to the level of accuracy in the estimates. For example, if a formula has a threshold for eligibility for funding, an area that is erroneously estimated to be below the threshold will not receive any funds, even if the degree of underestimation is small.

For program use, policy makers should consider the advantages and disadvantages of alternative sources of income and poverty estimates and choose estimates that are most in accord with program goals. Data from the decennial census have the advantage of providing small-area poverty estimates on the basis of a very large household survey (the sample of households that receives the census long form), but the census estimates are only available every 10 years. For very small areas, they also have considerable error due to sampling variability. In contrast, administrative records sources of poverty estimates, such as school district counts of children approved for free or reduced-price lunches under the National School Lunch Program, are timely and not subject to sampling error. However, they may not relate in a consistent manner to poverty across areas because income eligibility guidelines for programs often differ from the poverty thresholds, and program participation may vary substantially across areas.

Evaluations of the SAIPE poverty estimates found them to be a marked improvement over outdated census estimates for states and counties and at least as good as, if not better than, other estimates that were being used for school district allocations. However, the evaluations also found that the level of inaccuracy in the estimates could be sizable, particularly for small school districts.

RECOMMENDATIONS FOR PRODUCERS AND USERS

The SAIPE Program indirect estimates of income and poverty, which use official concepts and are updated on a regular basis, are likely to become more widely used for fund allocation and other program purposes in the future. We recommend practices that we believe are critically important for the SAIPE Program in the production of estimates and that are important for users to follow in applying estimates for program use.

Producers

The producing agency for a program of model-dependent estimates, such as SAIPE, should, first of all, have adequate staff and other resources for all the component operations. The producing agency should also:

- maintain regular contact with key users, so that the estimation program produces those estimates that are most needed and appropriate for important program uses within the constraints of available resources;
- as a matter of routine practice every time a new round of estimates is prepared, check the input data for errors and assess each data source for its continued suitability for use in estimation models;
- search for possible new data sources whose use might lead to im-

proved estimates and consider pilot efforts as appropriate to establish their value for use in models;

- pursue efforts, such as reducing the lag in availability of key data sources, to reduce the time between release of estimates and the year to which they refer;
- carry out research and development on methods that may improve the estimates in terms of their variability, bias, and timeliness;
- thoroughly evaluate the estimates every time they are produced, by conducting internal evaluations of the model outputs and, to the extent possible, external evaluations with other data sources; and
- document the evaluations and results in detail, make the documentation available to users, and provide research access to the input data and models to permit independent replication and evaluation, taking care to address confidentiality concerns.

Users

Agencies that use estimates for fund allocation or other program purposes should:

- carefully review the documentation provided by the producing agency to understand the properties of the estimates;
- periodically obtain independent reviews of the estimates and alternatives to them; and
- regularly study the effects of using the estimates for the allocations made by the agency and, where appropriate, for suballocations made by others.

Policy makers need to have information about the effects of alternative estimates and formula provisions to consider in making decisions about program uses of estimates. For this purpose, we urge that policy makers:

- commission assessments of formulas and the estimates used in them to identify key issues and develop detailed alternatives for consideration in the early stages of crafting new or modified program legislation.

RECOMMENDATIONS FOR SAIPE

Internal and external evaluations of the 1993 and 1995 estimates of poor school-age children for small areas from the SAIPE program found that the state and county models are working reasonably well but identified areas for further research and development for both the models and the data sources used in them. For school districts, the Census Bureau was constrained to use relatively crude estimation procedures because of the lack of suitable data at the school district level with which to develop a more effective statistical model. Marked improvements in the estimates for school districts and other subcounty areas will require new sources of data for use in models.

Research and Development for Current Models

The Census Bureau's SAIPE Program estimates poverty and income for states and counties by combining the estimates from statistical regression models that are based on the March Current Population Survey (CPS) with the direct CPS estimates (where available). The procedure for combining the regression predictions and the direct estimates weights them by their relative accuracy.

The use of regression models is necessary because of the high sampling variability of CPS estimates for all but the largest states and counties and the lack of any sample households for two-thirds of the counties. In the state regression models, the state's direct CPS estimate of poverty (or income) for the reference year is the dependent variable, and the predictor variables are obtained from such sources as Internal Revenue Service (IRS) tax returns, Food Stamp Program records, population estimates from the Census Bureau's demographic estimates program, and the previous census. The county regression models use the same general approach. One difference is that the dependent variable in the county models is a 3-year average CPS estimate, centered on the reference year, rather than a single-year estimate. Another difference is that the county regression models are estimated from data for only the counties that have some households in the CPS, whereas the state regression models are estimated from data for all states. The formulation of the county poverty models also requires that a county have at least one poor household in the CPS sample with a member in the relevant age group in order to be included in the model. For poverty models, the county models estimate numbers of poor, while the state models estimate the proportions poor. As a last step in developing county poverty estimates, each of the county estimates in a state is multiplied by a state raking factor so that the sum of the adjusted county estimates equals the state estimate from the state model.

From its review of the state and county models for poor school-age children, the panel identified the following areas for research and development by the Census Bureau in the near term. The Bureau has already begun work in these areas, which would likely benefit the models for other age groups as well.

- The state and county models, while similar in broad outline, differ in many important details that raise questions about possible inconsistencies in their estimates. A goal for the future should be closer integration of the state and county models. In the interim, work should be conducted to determine the usefulness of including state effects in the county models, for example, by developing a random state-effects model.
- The current formulation of the county model has the disadvantage of excluding counties from the estimation that have households in the CPS sample but no sampled households with poor school-age children. Work should proceed on estimation techniques, such as generalized linear mixed models, that would include all counties with households in the CPS sample.
- Both the state and county models have problems in estimating the relative weights that are used to combine the regression predictions and the direct CPS estimates. Procedures that the Census Bureau is developing to address these problems in the short term should be evaluated and implemented, as appropriate, while awaiting the results of longer term research and development.
- Looking to the future, as more data become available from such sources as the American Community Survey and the 2000 census, the use of time-series and multivariate modeling techniques that make use of multiple years of data from the same survey, separate surveys, or both, could be advantageous. Work on such models should proceed, building on the Census Bureau's previous efforts along these lines.
- SAIPE model estimates are currently produced with a 3- to 4-year lag between the release date and the income reference year. Work should proceed to find ways to reduce the time lag. For example, for the county model, the estimates might be raked to the state model estimates for the latest of the 3 years of CPS data used in the county model instead of to the state model estimates for the middle year.

The current school district estimation procedure uses 1990 census data to estimate each school district's share or proportion of its county's total number of poor school-age children. These estimates of shares, which are then applied to updated estimates from the county model, have considerable error due to sampling variability for many small school districts. Work should proceed on ways to reduce the sampling variability in the census estimates beyond what has already been achieved by using a simple ratio-estimation technique.

Role of Survey Data

New sources of household survey data may support significant improvements in SAIPE program estimates in the next decade and beyond. These sources include the 2000 census long-form survey, which will provide income and poverty estimates for 1999 from a sample of about 18 million housing units, and the planned American Community Survey (ACS), which when fully implemented in 2003 will provide income and poverty estimates on a continuous basis from a large sample of more than 2 million responding housing units each year. In addition, two smaller ongoing surveys, the March CPS and the Survey of Income and Program Participation (SIPP), will continue to provide income and poverty estimates.

The panel reviewed these surveys and the possible ways in which estimates from them might be used in the SAIPE Program in light of such considerations as error due to sampling variability, timely availability of updated estimates, and likely quality of responses and comparability with the current CPS-based estimates and reached several conclusions and recommendations.

General

To inform decisions about the use of the 2000 census long form, ACS, March CPS, and SIPP for SAIPE, the Census Bureau should conduct research to understand and document the differences in their measurement of income and poverty. For this purpose, the Census Bureau should conduct a series of exact matches and analyses of each survey with the 2000 census data and also with data from IRS tax returns for income year 1999.

American Community Survey

Research and development by the Census Bureau should begin now to explore two possible uses for the ACS in SAIPE models for counties. One use is for ACS estimates to form one of the predictor variables in regression models for which the official source of income and poverty estimates, the March CPS, continues to provide the dependent variable. Another use is for ACS estimates to serve as the dependent variable in county models, which could thereby include all, or nearly all, counties in the estimation. The Census Bureau should also conduct research on using ACS estimates for school districts and other subcounty areas to form within-county shares or proportions to apply to updated county model poverty estimates.

If the American Community Survey is to fulfill its potential to play a major role in the SAIPE program, it is important that the survey have sufficient funding for planned sample sizes over the next decade. Reductions in funding could jeopardize the usefulness of the ACS for SAIPE and, more generally, make it difficult to properly assess the potential uses of ACS data in small-area estimation.

2000 Census

The Census Bureau should plan to use 2000 census long-form estimates to form one of the predictor variables in the SAIPE state and county models. For SAIPE estimates for income year 1999, it could be possible to use the direct estimates from the 2000 census long form, but whether this will be feasible (the data may not be available in time) or desirable is not clear. The Census Bureau should consider the available options and discuss them fully with users.

Role of Administrative Data

SAIPE estimates for school districts and other subcounty areas cannot currently be produced by using regression models similar to the state and county models, although such models would likely improve upon the current shares procedure. No administrative records data sources currently exist that can provide consistently measured predictor variables for a subcounty model, in the way that tax return and food stamp data are used in the state and county models.

The panel reviewed the advantages and problems of developing IRS tax return and food stamp data for use in subcounty models. Such use would require improvement of the Census Bureau's capabilities for geocoding addresses from administrative records to small geographic areas. The Bureau should give high priority to the continued development of its Topologically Integrated Geographic Encoding and Referencing/ Master Address File (TIGER/MAF) system, and, as soon as possible after the 2000 census is completed, it should study the extent to which TIGER can be used to geocode addresses on tax returns to school districts.

Use of administrative records data requires regular reviews of their quality and consistency in terms of how they relate to income or poverty across geographic areas and over time. The review should include identifying possible changes to administrative records systems that would benefit estimation without undue cost to the data collection agency or burden on respondents. For the SAIPE poverty models, it is particularly important to review the interarea comparability of food stamp data in light of the changes in eligibility provisions and participation rates for food stamps that have occurred as a consequence of the changes in welfare programs beginning in 1996.

The panel also considered the advantages and problems of using data from the National School Lunch Program for improved poverty estimates, specifically for school districts. School lunch data might be used, alone or combined in some manner with census and ACS data, to form within-county shares to apply to updated county model estimates. Alternatively, school lunch data might be used as a predictor variable in a regression model for school district poverty estimates. Although issues of comparability across areas and the current lack of a centralized source for the data present problems in using school lunch counts to estimate poverty, the panel concludes that further evaluation may be warranted to determine the usefulness of those data for the SAIPE school district estimates.

Estimates of total population and population by age are required for many uses of small-area income and poverty estimates from SAIPE; postcensal population estimates are developed by using administrative records such as tax returns. The panel recommends several areas for research and development to improve the estimates, including: ways to improve population coverage in tax return files, use tax returns for estimating population by age, and geocode tax returns to subcounty areas; reassessment of the usefulness of school enrollment data for county and school district estimates of school-age children; and ways to use the MAF and, perhaps, the ACS to improve population estimates.

1

Introduction

In the early 1990s the Census Bureau began the Small Area Income and Poverty Estimates (SAIPE) Program to meet growing demands for regularly updated estimates of key income and poverty measures for subnational areas, such as states, counties, and school districts. SAIPE is not the first program for providing more frequent estimates than are provided by the decennial census of population, but it is the most ambitious effort of its type to date (see below, "Background").

The first SAIPE estimates were issued in early 1997 for states and counties for income year 1993. Estimates for states and counties for income year 1995 were issued in early 1999; also issued at that time were estimates of the numbers of poor school-age children for school districts in 1995. The SAIPE estimates were developed by using a variety of survey, census, and administrative records data sources with statistical modeling techniques.

An important application of intercensal small-area estimates of poverty is for the allocation of over $7 billion of funds annually for programs for educationally disadvantaged children under Title I of the Elementary and Secondary Education Act. Reauthorization of that act in 1994 provided that updated estimates of poor school-age children for counties and school districts, produced every 2 years by the Census Bureau, should be used for Title I allocations in place of estimates from the most recent census, unless the Secretaries of Commerce and Education found that they were "inappropriate or unreliable" on the basis of a review by a panel of the Committee on National Statistics at the National Research Council.

The 1994 act also provided for the Panel on Estimates of Poverty for Small Geographic Areas to carry out that task. The panel's findings, which supported use of the estimates, were published in three interim reports (National Research Council, 1997, 1998, 1999). The panel subsequently combined the three interim reports into a single technical volume (National Research Council, 2000c), which documents the current methods for producing SAIPE estimates of poor school-age children for states, counties, and school districts, and the evaluations of them that have been conducted to date. The technical volume is designed to complement this report.

In this, its final report, the panel addresses its broader charge to review the SAIPE Program as a whole. The report offers recommendations to the Census Bureau for future research and development that can lead to improved SAIPE estimates for use in programs, such as Title I and others, that require updated small-area income or poverty estimates for such purposes as fund allocation. The report also identifies issues that user agencies need to consider in deciding to adopt small-area income and poverty estimates from SAIPE or other sources for program purposes.

BACKGROUND

A large and growing number of federal programs use small-area income and poverty estimates for allocating funds to states and localities, providing matching funds for state expenditures, performance monitoring, and program evaluation. This trend reflects a shift in policy away from individual entitlement programs to formula block grant programs. An example is the 1996 Personal Responsibility and Work Opportunity Reconciliation Act, which replaced the Aid to Families with Dependent Children entitlement with the Temporary Assistance to Needy Families (TANF) block grant. Data sources used for fund allocation and related federal program purposes include the decennial census, SAIPE estimates, and small-area personal income estimates from the Bureau of Economic Analysis (BEA).

States also allocate both federal and state funds to localities under formulas that target less well-off areas or populations. Data sources used by the states include the decennial census, the new SAIPE estimates, and, for many programs, state and local administrative records that relate to poverty or income, including TANF caseloads, numbers of children approved for free or free and reduced-price meals under the National School Lunch Program, and, in a few instances, income estimates from state tax records.

Historically, decennial census estimates have been the most frequently used data source for federal programs that require small-area estimates of income and poverty, and they have been used by many state programs as well. However, for most areas, census estimates decline in accuracy over the years between census enumerations and, consequently, increasingly misrepresent the distribution of income or poverty as the period since the last census lengthens. For example, from 1989 to 1993, not only did the United States as a whole experience a 21 percent increase in the number of poor people because of economic recession, but the increase in the poor population was not uniform across the nation. Some states experienced greater than average increases in their poor populations (e.g., 52% in Florida, 44% in California), while other states experienced smaller than average increases (e.g., 4% in Texas, 7% in Illinois).[1]

The Census Bureau originally began the SAIPE Program in response to inquiries about updated estimates of per capita income for local governmental jurisdictions. Such estimates had been regularly produced by the Census Bureau every 2 years from 1971 to 1987, using changes in income reported on tax returns and BEA personal income estimates to update census income estimates (see National Research Council, 1980, for a review of the methodology). The estimates were used in allocating funds to 39,000 local governments—states, counties, cities, towns, and other units—under the General Revenue Sharing Program; but when that program expired, the Bureau discontinued its per capita income series.

Efforts to build a consortium of federal agencies to fund a broader set of SAIPE estimates got under way in early 1993. By August, five federal agencies had agreed to provide sufficient funding to initiate the project: the U.S. Department of Agriculture, Food and Nutrition Service; the U.S. Department of Education, National Center for Education Statistics; the U.S. Department of Health and Human Services, Head Start Program; the U.S. Department of Housing and Urban Development, Office of Policy Development and Research; and the U.S. Department of Labor, Employment and Training Administration. The Statistics of Income Division (SOI) of the Internal Revenue Service (IRS) agreed to become a partner in the project, and its data were essential for several anticipated estimation methodologies.

About the same time, Congressman Thomas C. Sawyer (D-Ohio), who then chaired the House of Representatives Subcommittee on Census, Statistics, and Postal Personnel, organized a hearing on the need for

[1]From tabulations of the March Current Population Survey (CPS) (see the Census Bureau's web site: www.census.gov/hhes/www/saipe.html). The United States subsequently experienced a 12 percent decrease in the number of poor people from 1993 to 1998; however, state differences in the rate of decline were not large enough to be reliably distinguished by the March CPS.

more current measures of poverty for small areas and how those measures might be developed. Following this hearing, Congressman Sawyer introduced authorizing legislation requiring the Secretary of Commerce to develop methods to produce "intercensal data relating to the incidence of poverty for each State, county, and local jurisdiction." The legislation further called for estimates of the numbers of poor children aged 5-17 for school districts and of the numbers of poor people aged 65 and over for states and counties. The legislation was passed by the House of Representatives in November 1993, but the Senate did not act on it. One year later, in September 1994, Congress passed the "Improving America's Schools Act," which called for the use of updated Census Bureau estimates of poor school-age children for allocation of Title I funds, if they were found sufficiently reliable by a panel of the National Research Council.

SAIPE IN BRIEF

The main objective of the SAIPE program is to produce updated income and poverty estimates for the administration of federal programs, including the allocation of federal funds to local jurisdictions. At present, SAIPE provides the following estimates:

For states, biennially beginning in early 1997 (for income year 1993) and annually beginning in 1999 (for income year 1996):

–median household income;
–number of poor people;
–number of poor children under age 5;
–number of poor related children aged 5-17;[2] and
–number of poor people under age 18.

For counties, biennially beginning in early 1997 (for income year 1993):[3]

–median household income;
–number of poor people;
–number of poor related children aged 5-17; and
–number of poor people under age 18.

[2]Related children include family members in a household under age 18, except married sons, daughters, or spouse of the householder and foster children (see the Census Bureau's web site for a detailed definition and the reason for using it: www.census.gov/hhes/www/saipe.html).

[3]The county estimates (for income year 1993) were revised and reissued in early 1998.

For school districts, biennially beginning in early 1999 (for income year 1995):

–number of poor related children aged 5-17.

In addition, the Census Bureau produces small-area population estimates of the numbers of persons in relevant age groups, which can be used to construct estimates of poverty rates. Population estimates by age are developed annually for states and counties from a demographic estimates program that has been active for many years. For school districts, the Census Bureau is now producing biennial estimates of the numbers of school-age children and the total population; the first estimates were issued in 1999 for July 1996.

The SAIPE estimates are developed by using a variety of data sources with statistical estimation methods. There is at present no single data source—either a sample survey, such as the March CPS, or an administrative records system—that can be used to produce reliable direct estimates between decennial censuses.[4] Instead, multiple sources must be combined in statistical models to produce reliable indirect estimates.

Model-dependent indirect estimators use data from other areas and, possibly, other time periods that are obtained from several sources to "borrow strength" and improve the precision of estimates for small areas.[5] The basic methodology was first developed several decades ago, and the Census Bureau has used this strategy for several types of estimates. Specifically, it used model-dependent methods in the 1970s to improve 1970 census small-area income estimates for use in developing updated per capita income estimates for governmental jurisdictions (Fay and Herriot, 1979) and, in part, to develop population estimates for states and counties (see National Research Council, 1980:App. A). More recently, it used model-dependent methods to estimate median family income for states (Fay, Nelson, and Litow, 1993).

The SAIPE estimates for states incorporate predicted values from statistical regression models that predict state poverty or income in the March

[4]Census income and poverty estimates, which are based on the long-form sample, themselves exhibit a considerable degree of error due to sampling variability for many small areas, in addition to other kinds of error, such as misreporting.

[5]By "model-dependent" we mean that the accuracy of the estimates depends on the validity of the assumptions of the estimation model: see Marker (1999) and Rao (1999) for overviews of small-area estimation methods; see U.S. Office of Management and Budget (1993) for definitions of direct and indirect estimators and other terms used in the research literature.

CPS on the basis of variables derived from such sources as IRS tax returns, food stamp records, the 1990 census, and population estimates. The regression predictions are combined with the corresponding direct state estimates from the March CPS by using a procedure in which the weights given to the predicted values and the direct estimates depend on their relative precision. Similar procedures are used for county estimates, although the state and county models differ in several respects. For poverty measures (but not median household income), each set of county estimates is adjusted by state to sum to the applicable SAIPE state estimates.

For school districts, a different procedure is used to develop school-age poverty estimates because of the lack of district-level data from administrative records that could be used to form predictor variables in a school district model similar to the SAIPE county model. Instead, school district estimates are produced by using 1990 census data to calculate the percentage shares or proportions of poor school-age children for the school districts (or parts of school districts) in each county, using more recent district boundaries from a special survey that is conducted every 2 years. These shares are then applied to updated SAIPE county estimates of poor school-age children.

PLAN OF THE REPORT

This report has two goals: to identify and discuss key issues for federal agencies and other users when applying small-area income and poverty estimates for such purposes as fund allocation; and to outline priority areas for research and development for the Census Bureau that can lead to improved small-area income and poverty estimates from the SAIPE Program.

Chapter 2 focuses on uses and users. It describes the growing needs for small-area income and poverty estimates for federal and state program purposes, important criteria for such estimates (e.g., timeliness, appropriate concept of poverty or income), and the extent to which available data sources satisfy the criteria.

Chapters 3-5 focus on the SAIPE estimates, which the panel believes will be more widely used in the future. Chapter 3 provides a technical discussion of the SAIPE estimation models, focusing on the models for poor school-age children for states, counties, and school districts. It summarizes earlier evaluations of the models and lists priorities for model research and development, previously identified by the panel, that the Census Bureau should pursue in the short to medium term (see also National Research Council, 2000c). Chapters 4 and 5 discuss, respectively, new survey data sources, such as the 2000 census and the planned American Community Survey, and improvements in administrative records that, if implemented, could support major improvements in the SAIPE estimates in the longer term, particularly for such subcounty areas as school districts.

Chapter 6 returns to the user perspective, considering interactions between the properties of small-area income and poverty estimates and the properties of allocation formulas. Estimates will always have some degree of error or uncertainty, and users must be aware of the unintended effects that errors in estimates, in conjunction with formula features, may have on allocations. Chapter 7 provides general recommendations to users and producers of small-area income and poverty estimates from SAIPE (and other sources) in the following areas: practices that are important to follow in the production, evaluation, and documentation of estimates; assessments that users should conduct of estimates; and the need for policy makers to consider carefully the use of estimates for fund allocation and other program purposes in light of their uncertainty. The appendix presents some results of simulating fund allocations with varying levels of uncertainty of estimates and different rules for allocating funds.

Chapters 2, 6, and 7 are addressed primarily to administrators, analysts, and policy makers in federal agencies who are considering the use of estimates of income and poverty for programs. Chapters 3, 4, and 5, which are more technical, are addressed primarily to researchers in small-area estimation, including the staff at the Census Bureau. However, each of the technical chapters includes an overview of the key points for non-technical readers.

2

Needs for Small-Area Income and Poverty Estimates

Regularly updated small-area estimates of income and poverty are increasingly in demand for federal and state programs. Formulas that include such estimates are used to allocate billions of dollars each year to states and localities (U.S. General Accounting Office, 1999), and the estimates have other program uses as well. These uses place significant requirements on estimates, including requirements for geographic and population detail, timeliness of production, and accuracy of measurement. No estimates will perfectly meet all requirements. Users, including government agencies that administer programs by using estimates and policy makers that legislate program uses of estimates, need to be aware of the strengths and weaknesses of estimates for their purposes.

In this chapter we describe the growing demand for small-area income and poverty estimates, identify key requirements for estimates from the perspective of program uses, and assess in general terms the ability of alternative data sources to satisfy these requirements. The chapter is addressed primarily to users, but also to suppliers of estimates who should be knowledgeable of the need for their product and the implications for estimation methodology.

PROGRAM TRENDS

The use of small-area statistics for such purposes as allocation of federal funds to states and localities has a long history (see Anderson, 1988:178-179, 203-205). Financial grants-in-aid were developed in the late nineteenth century as an alternative to land grants, which Congress had historically used to encourage state and even private development. The first grant-in-aid was adopted in 1887 in the Hatch Act, which provided a small amount of funds to each state for agricultural experiment stations. By the early 1920s, grants covered highways, vocational education, agricultural extension work, conservation programs, and public health. Generally, the formulas were simple, using such measures as total population, area, or road mileage.

Beginning with the New Deal in the 1930s, grant-in-aid programs were more and more seen as a way to help equalize the national impact of programs by accounting for differences among states in their wealth and fiscal capacity. Also, an increasing number of programs specifically targeted low-income areas as a way to redistribute national wealth to address social problems. In response, allocation formulas were written to require estimates for such factors as per capita income, poverty rate, unemployment rate, or proportion of substandard housing; all of these are more difficult to measure than total population. In addition, some programs provided allocations directly to cities and other local areas, which necessitated estimates for areas smaller than states. Formulas also became more complex, not only by including multiple factors, but also by incorporating such provisions as thresholds for eligibility, minimum allocation amounts, and maintaining a percentage of prior year grant amounts for areas that would otherwise see a decrease in funding ("hold harmless"). These kinds of provisions generally required more accurate estimates.

By the 1990s, an estimated $180 billion of federal funds were allocated each year to states and localities on the basis of formulas that included one or more factors requiring estimates for population groups (e.g., total population, elderly, children; see U.S. Census Bureau, 1999d; see also U.S. General Accounting Office, 1999). Although no precise dollar figure is available, more than $130 billion of these funds were allocated on the basis of formulas that specifically included small-area income or poverty estimates as a factor. Estimates were obtained from such sources as the decennial census long-form sample income information; administrative record counts of participants in particular social programs; the per capita income estimates developed from administrative records, censuses, and surveys by the Bureau of Economic Analysis (BEA); and, more recently, the estimates from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program developed by applying statistical methods to data from several sources.

The largest federal program that uses small-area income or poverty estimates for fund allocation is Medicaid, which has a matching formula for reimbursing state expenditures, the Federal Medical Assistance Percentage (FMAP). Other programs, including Adoption Assistance and Foster Care, also use FMAP. To determine what percentage of state expenditures will be reimbursed (the percentage is constrained to be no less than 50% and no more than 83%), the FMAP formula takes account of the ratio of state per capita income to total U.S. per capita income–the higher the ratio, the smaller the amount reimbursed. The per capita income estimates for each state are 3-year averages from BEA.

Some federal programs also use small-area income or poverty estimates to determine eligibility to apply for project grants or other benefits. For example, to receive tax benefits from the U.S. Department of Commerce Empowerment Zones Program, rural and urban areas that are designated for the program must demonstrate a poverty rate of not less than 20 percent in each census tract in the area and, for at least 90 percent of the census tracts in the area, the poverty rate must not be less than 25 percent.[1]

Looking to the future, it seems likely that an increasing number of federal programs will provide funding on the basis of allocation formulas that include small-area poverty or income estimates because of the trend in social welfare policy of replacing individual entitlement programs with block grants to states and localities. For example, the new block grant Welfare to Work Program, funded beginning in fiscal year 1998 at $1.5 billion, requires that 75 percent of the funds be allocated to states according to the state share of the national number of poor persons and the state share of the national number of adult recipients of Temporary Assistance to Needy Families (TANF), with each factor equally weighted. In turn, states must suballocate 85 percent of the federal funds they receive to service delivery areas, which can be a county, city, consortium of counties or cities, or, in some cases, part of a large city.[2] Half of the suballocations must be made according to the number of persons in poverty in excess of 7.5 percent of the population in each service delivery area; the other half of the suballocations can be made according to the number of adult TANF recipients and the number of unemployed people in the area.

The Welfare to Work Program was enacted after welfare reform, in which the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) abolished the Aid to Families with Dependent Chil-

[1]Poverty rate estimates for census tracts are from the census long-form sample. Empowerment zone areas must also meet several other criteria, such as how much of the area is zoned for commercial and industrial use. States and localities nominate empowerment zone areas; the final designation is made by the U.S. Secretary of Housing and Urban Development (for urban areas) and the U.S. Secretary of Agriculture (for rural areas).

[2]States must match every $2 of federal funding with $1 of state funding.

dren (AFDC) entitlement program and replaced it with the block grant TANF program. The TANF formula does not include poverty or income as a factor (it essentially allocates to states the amounts they had received in federal AFDC matching funds at an earlier point), but PRWORA requires state-level child poverty rates for administering TANF. States must measure year-to-year changes in their child poverty rates and develop an action plan if the child poverty rate can be determined to have increased by more than 5 percent due to the operation of the TANF program.

For federal formula allocation programs in education, two recent trends have added to the requirements for small-area poverty estimates. First is a trend–evident in other programs as well–toward requiring estimates of child poverty more frequently than can be provided by the decennial census. Second is a trend toward requiring estimates for very small areas, namely, school districts, to permit direct allocation of federal funds to those areas. Developing estimates for school districts is particularly difficult because of the small population size of most districts and because school district boundaries in many instances cross county lines and can and often do change over time.

The Improving America's School Act of 1994 reflected both of these trends in mandating changes for Title I of the Elementary and Secondary Education Act, which provides more than $7 billion each year for programs to help educationally disadvantaged children. Under the act's provisions, the U.S. Department of Education must allocate funds directly to school districts by using estimates of poor school-age children that are updated every 2 years, provided the estimates are found to be sufficiently reliable for this purpose. (Previously, the department allocated funds to counties on the basis of estimates from the most recent decennial census, and states then suballocated the county funds to school districts.) A new program in fiscal 1999 for class size reduction will allocate $1.2 billion to school districts in 2000 according to the same school district estimates of poor school-age children that are used for Title I allocations. These estimates are from the SAIPE Program.

In addition to federal program uses of small-area income and poverty estimates, state governments increasingly use such estimates for allocating state funds to local areas (see Midwest Research Institute, 1999). These uses are in addition to the requirements in some federal programs for states to suballocate federal funds to localities by means of a formula that includes income or poverty estimates. Many state allocation formulas that include income or poverty are education programs that are primarily targeted at school districts. Some states also use income or poverty-based allocation formulas for allocating social service and health program funds to counties or other areas. Data sources that states use to provide estimates for state allocation formulas include the decennial census and state administrative records (e.g., the number of children approved for free or reduced-price school lunches or state income tax data).

Table 2-1 summarizes key features of selected federal and state allocation formulas that use small-area income or poverty estimates as a factor, chosen to illustrate the variety of program areas, funding levels, and provisions of current allocation formulas. As the table shows, a wide variety of programs allocate funds on the basis of small-area income and poverty estimates, including child care, community development, education, job training, nutrition, and public health.

REQUIREMENTS FOR ESTIMATES

The use of small-area income and poverty estimates for allocating funds or related program purposes imposes significant requirements for the estimates if they are to satisfy the intentions of the framers of program legislation.[3] Ideally, requirements for estimates include the desired concept or definition of poverty or income measured, level of geographic detail, level of population or demographic detail, timeliness of production and updating, and accuracy of measurement. The cost of producing estimates is also a consideration. In practice, it is rare that any one data source or even a combination of data sources can provide estimates that satisfy all requirements: for example, one source may provide an outdated measure of the specified poverty concept, while another source may provide an updated measure for a concept that is only partly related to poverty.

This situation does not mean that users should decline to target programs on the basis of small-area income or poverty estimates. Rather, they should select sources of estimates with as much knowledge as possible of the strengths and weaknesses of each source and the implications for the resulting fund allocations or other program uses of the estimates. Users should also consider interactions of estimates with formula features. It may be that altering a formula provision would result in more appropriate use of available estimates (see Chapter 6). In the section below we briefly discuss each type of requirement for estimates and indicate some of the tradeoffs involved.

[3]For discussion purposes, we take as given that the elements of allocation formulas and their intended purposes are clear, although this may be far from true in many cases. For example, the concept underlying a formula factor may not be well specified.

Concept of Poverty or Income

Many programs that use small-area estimates of poverty for fund allocation or other purposes specifically target those in poverty according to the official poverty measure (i.e., those in families with before-tax money incomes below the official poverty level). Alternatively, some programs target those in families with incomes that are a multiple of the official poverty line (e.g., 125% or 185%). Other programs use a different standard of need, such as those with incomes below 70 percent of the lower living standard income level defined by the U.S. Department of Labor.[4] Other programs have a less specific definition, targeting those with "low incomes" or those who are "needy." Income concepts used for fund allocation often specify per capita income or, sometimes, median income.

Given a specific definition of poverty (or income) in an allocation formula, the small-area estimates used for allocation should measure that concept. To the extent that the estimates measure a somewhat different concept, there may be a bias that results in a persistent misallocation of the funds.[5] As an example, when Title I education funds were allocated by a two-stage process, many states obtained approval from the U.S. Department of Education to suballocate the county amounts to school districts on the basis of the number of children approved for free or free and reduced-price school lunches in each district. However, school lunch counts include children in families with incomes as high as 130 percent of poverty (free lunch) or as high as 185 percent of poverty (reduced-price lunch).[6] Hence, some school districts that received Title I concentration grant funds because they had sufficient proportions (or numbers) of children approved for free or free and reduced-price school lunches would not have received funds on the basis of an estimate of children in families

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[4]Lower living standard income levels are published by the Employment and Training Administration for 25 metropolitan areas and for metropolitan and nonmetropolitan components of the four census regions, Alaska, and Hawaii. The levels represent the Bureau of Labor Statistics lower level family budgets, developed for 1967 on the basis of 1960-1961 Consumer Expenditure Survey data and last published for 1981, updated for price changes. Seventy percent of these levels for a family of four ranges from about 100 to 166 percent of the official poverty level (which does not vary by area).

[5]See "Accuracy of Measurement" below for definitions of bias and other types of errors in estimates.

[6]Almost twice as many children are in families with incomes below 185 percent of the poverty threshold (38%) as are in families with incomes below 100 percent of the poverty threshold (20%). About 26 percent of children are in families with incomes below 130 percent of the poverty threshold. (Data from panel tabulations of the March Current Population Survey [CPS] for income years 1994-1996.)

TABLE 2-1 Key Features of Selected Federal and State Allocation Formulas that Use Small-Area Income and Poverty Estimates

FEDERAL FORMULAS

Agency and Program Name (Amount Allocated per Year)	Areas to Which Funds Allocated
U.S. Department of Agriculture	
Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) ($3 billion in 1999)	States
Rural Housing Preservation Grants ($9 million in 1999)	States
U.S. Department of Education	
Title I of Elementary and Secondary Education Act ($7.6 billion for 1999-2000 school year)[a]	School districts (prior to 1999, the Department of Education allocated to counties; states suballocated to school districts)
U.S. Department of Health and Human Services	
Child Care and Development Block Grant ($1 billion in 1999)	States
Title V Maternal and Child Health Services Block Grant ($576 million in 1999)	States
Medicaid (reimbursement of state expenditures) ($200 billion, federal and state, in 1999)	States

Income or Poverty Estimates Required (Other Factors if Known)	Data Source for Estimates
States receive allotments equal to previous year; up to 80 percent of remaining funds are allocated as inflation adjustments; remaining funds are allocated based on state share of national number of children in families below 185 percent of poverty	Model-based estimates using March CPS, decennial census, and administrative records (prior to fiscal 1995, used decennial census)
Formula assigns one-third weight to state share of: (1) total rural population, (2) total rural occupied substandard housing units, (3) total rural families with income below poverty	Decennial census
Basic grants allocated to school districts with at least 10 formula-eligible children and more than 2% formula-eligible children; concentration grants allocated to school districts with more than 15% or more than 6,500 formula-eligible children (principally school-age children in poor families)	Model-based SAIPE estimates (prior to 1997-1998 school year, used decennial census estimates)
Formula considers number of children under age 5, number of children receiving assistance, through the National School Lunch Program, and state per capita income	Population estimates, administrative records, BEA income estimates
Formula includes number of poor children under age 18; some states also use poverty measures to suballocate funds to counties	Decennial census
Formula includes the ratio of state per capita income to U.S. per capita income (the higher the ratio, the smaller the amount reimbursed; between 50% and 83% of state expenditures)	BEA income estimates

Table continued on next page

TABLE 2-1 *Continued*

FEDERAL FORMULAS

Agency and Program Name (Amount Allocated per Year)	Areas to Which Funds Allocated
U.S. Department of Housing and Urban Development	
Community Development Block Grants ($1.2 billion in 1999 to states to use for non-entitlement communities; $2.9 billion in 1999 directly to entitlement communities, which are metropolitan cities and counties)	Cities with 50,000+ population, metropolitan counties with 200,000+ population, some nonmetropolitan areas
HOME Investment Partnership Program ($1.5 billion in 1999)	States, cities, urban counties, and consortia of local governments
U.S. Department of Labor	
Job Training Partnership Act Title II-A (adult) ($955 million in 1999)	Service delivery areas (one or more counties or cities of 200,000+ population)
Job Training Partnership Act Title II-B (summer youth) and Title II-C (youth training) ($871 million and $130 million, respectively, in 1999)	Service delivery areas (as defined above)

Income or Poverty Estimates Required (Other Factors if Known)	Data Source for Estimates
States receive funds equaling the greater of two formulas. First formula includes factors for: (1) population, (2) poor households, (3) overcrowded housing units (1.01+ persons per room) in the balance of the state outside entitlement communities (factors weighted at 0.25, 0.50, 0.25, respectively). Second formula includes factors for: (1) population, (2) poor households, (3) number of housing units built before 1940 (factors weighted at 0.2, 0.3, and 0.5, respectively). The formulas for entitlement communities include poor households and several other factors.	Decennial census
Formula includes six factors and weights: (1) 10% weight on vacancy-adjusted rental units with household head in poverty; (2) 20% weight on occupied rental units with defined housing problems; (3) 20% weight on rental units built before 1950 occupied by poor families; (5) 20% weight on number of poor families; (6) 10% weight on population adjusted by the ratio of net per capita income to U.S. net per capita income.	Decennial census
Formula includes number of persons aged 22-72 in families with income not more than higher of OMB poverty line or 70% of lower living standard income level.	Decennial census (special tabulation)
Formula includes number of persons aged 16-21 in families with income not more than higher of OMB poverty line or 70% of lower living standard income level.	Decennial census (special tabulation)

Table continued on next page

TABLE 2-1 *Continued*

SOME STATE FORMULAS

State and Program Name (Amount Allocated per Year)	Areas to Which Funds Allocated
Arizona	
2000 Early Childhood Block Grant ($19.5 million in 1999)	School districts
Colorado	
Child Welfare Block Grant ($230 million in 1999)	Counties
Florida	
Intensive Crisis Counseling ($4.6 million in 1999)	Counties
Massachusetts	
State Education Aid ($2.5 billion in 1999)	School districts
Minnesota	
County Employment and Training Services Block Grant ($38.4 million in 1999)	Counties
New Mexico	
State education foundation formula ($1.3 billion in 1999)	School districts
New York	
State Education Aid ($11.9 billion in 1999)	School districts

[a]Several other education programs use the Title I allocation amounts as the basis of allocations.

Income or Poverty Estimates Required (Other Factors if Known)	Data Source for Estimates
Formula includes number of children in grades K-3 approved for free lunch	School lunch data
Formula includes number of children below 200 percent of the poverty line	Decennial census
Formula includes number of poor female-headed households with dependent children	Decennial census
Formula includes number of students approved for free or reduced-price lunch	School lunch data
Formula includes number of persons receiving Minnesota Family Investment Program benefits	State administrative records
Formula includes Title I allocation allocation amounts (see above, U.S. Department of Education, for Title I formula)	See U.S. Department of Education, Title I
Formula includes number of children approved for free or reduced-price lunch; per capita income in district	School lunch data; state income tax records

SOURCES: Federal programs: U.S. General Services Administration (1999). State programs: Midwest Research Institute (1999), which describes allocation programs for California, Delaware, Indiana, Missouri, Oregon, South Carolina, Texas, and Wyoming in addition to the states shown above. Many details of formulas are omitted.

with incomes below 100 percent of poverty.[7] Given a fixed total amount, allocating funds to these districts meant less money for other, possibly poorer districts.

Geographic Specificity

Geographic areas for which estimates are currently needed for federal fund allocation formulas include states, counties, school districts, and service delivery areas. Service delivery areas may be a single county or city, a group of counties or cities, or, in some instances, part of a large city.

Subcounty areas that have ill-defined or shifting boundaries present particular problems for developing income or poverty estimates. School districts are a case in point: in 1990, 27 percent of school district had boundaries that crossed county lines, and 7 percent had boundaries that changed between 1980 and 1990. Also, 26 percent of school districts served specific grades, such as high school or elementary grades, and not all grades. Moreover, the growing number of charter schools often do not serve areas with clearly defined boundaries. These kinds of problems make it difficult to develop estimates with census or survey data that accurately reflect current boundaries. Moreover, administrative records that are used for model-based estimates for states and counties, such as income tax returns, cannot currently be used in models for school districts or other subcounty areas because a significant proportion of the addresses in the records cannot readily be assigned (geocoded) to these areas.

Another problem, given that estimates are based in whole or in part on survey data, is that estimates for smaller areas will be statistically less reliable than estimates for larger areas–that is, they will have greater variability due to sampling error. Indeed, the problem of sampling error has resulted in heavy reliance on the decennial census to provide estimates not only for substate areas, but also for states, because the long-form sample size is so much larger than that of more frequently conducted household surveys. However, even long-form estimates exhibit high sampling error for very small areas. Moreover, reliance on the long form means that estimates can be updated only once every 10 years.

Model-dependent techniques, such as those used in SAIPE, are designed to provide regularly updated estimates on a frequent basis that have much lower levels of error for small areas than would be possible if estimates were produced directly from surveys. However, estimates

[7] Title I concentration grants allocate funds only to school districts with large numbers or proportions of poor school-age children, in contrast to Title I basic grants, for which the thresholds for eligibility to receive funds are low (see Table 2-1).

produced by such techniques require careful evaluation to determine that the modeling has not introduced biases over time by systematically overpredicting or underpredicting poverty or income in specific types of areas.

Population Specificity

Allocation formulas often target those in poverty in specific age groups. Examples include the elderly, all children, school-age children, pre-school-age children, and working-age adults. Other demographic characteristics that are used in some formulas include gender, family type, and urban or rural classification. Formulas may also include total population or the population of a particular group as a separate factor not linked to poverty or income.

Not all data sources for estimating poverty or income provide information for developing estimates for a particular demographic group. In many cases, the only reliable estimates may be from outdated census information. The Perkins III Program provides an example of this problem. Beginning in fiscal 2000, states are required to allocate Perkins III funds for secondary school vocational and technical education programs to school districts on the basis of (1) estimates of people aged 15-19 in the district (30% of funds) and (2) estimates of poor people aged 15-19 in the district (70% of funds). However, updated estimates are not available for poor people aged 15-19 by school district. To use newer estimates than the census, the U.S. Department of Education advised states to use the SAIPE estimates of poor children aged 5-17 instead (Midwest Research Institute, 1999:App.B). The department's analysis found that poverty correlates highly for children in the two age groups (5-17 and 15-19); however, the use of estimates for a somewhat different target group may introduce some biases, and it is not clear whether such biases are offset by the increased timeliness of the 5-17 age group estimates.

Timeliness

Fund allocations are typically made annually and, presumably, one goal of poverty-based programs is for allocations to keep up with changes in need–providing more funds to areas that experience a rise in poverty. In practice, there may be competing goals, such as a desire not to sharply cut back funds for jurisdictions that have come to rely on the funding even if their need is reduced (see Chapter 6).

Considering timeliness, estimates of poverty and income from the decennial census are clearly problematic. They are not usually available until 2-3 years after the census, and they must then be used for another 10 years until estimates from the next census are available. Use of estimates for such long periods will inevitably introduce biases in the allocations. The extent of the bias will depend on such factors as how greatly areas of the country differ in their rates of change in income and poverty. For example, if all areas but the Midwest experienced an increase in poverty from one census to the next, then the Midwest would receive relatively more poverty-targeted funds over the decade (than other areas) by using the previous census's poverty estimates throughout the period than if up-to-date estimates were available. The effects of such biases will also depend on other provisions of the allocation formula. To continue the example, if a formula has a 100 percent hold-harmless provision so that no area may receive less than its prior-year allocation, then the Midwest would continue to receive relatively more funds even after the results from the next census were available. Although estimates from regularly conducted surveys or administrative records may be more up to date than the census, they may have other problems, such as higher variability or measuring a somewhat different poverty or income concept from that specified in the formula.

An aspect of timeliness concerns the lag between the reference year for the data and the year in which estimates are released. Census data, as noted, are not usually available for 2-3 years after the census, so that they are out of date even when they are first used. Survey and model-based estimates may also require 1 or more years to prepare: SAIPE estimates currently are released about 3 years after the reference year for income data. Administrative records data are usually available on a more timely basis for the agencies (usually at the state level) that collect them, but they may not be available in a timely manner for use by a federal statistical agency such as the Census Bureau.

Accuracy of Measurement

Accuracy has two components–bias and variance. Bias is a systematic difference (high or low) between an estimate and the true value that persists over repeated measurements. An example of how bias can occur is when respondents to a survey persistently underreport their income. Variability, or variance, is random variation that occurs due to sampling error or other sources of error. Because the variation is random, an estimate that is high (low) in one measurement is equally likely to be high or low on another measurement. Administrative records and the census short form, which are not surveys, do not have variability from sampling error, but they can have variability from other sources, such as random errors in keying or recording data onto forms.

Bias and variance are often combined (estimated bias squared plus estimated variance) into a single accuracy measure, mean square error. As its name indicates, mean square error measures the average (squared) difference between an estimated value and the true value. To understand the effects of estimates on fund allocation and other program uses, it is important to assess bias and variance separately to the extent possible. Not only are their effects likely to differ (discussed below), but so also are ways to reduce those effects. As an example, expanding sample size is a way to reduce sampling error in a survey, although at likely considerable expense. Another way to reduce survey sampling error is to average estimates for several years, but this procedure may introduce bias. Reduction of bias requires different strategies–for example, rewording of questions in a survey or adjustment of survey estimates by using other data sources.

Persistent bias is of serious concern for small-area estimates of poverty or income. Such bias means that certain areas may consistently receive more or less funding than what they would receive with unbiased estimates. Bias can occur because of some of the characteristics of estimates discussed above, for example, if the estimates measure a concept of poverty or income that is not the same as the concept specified in the formula, or if the estimates are very out of date and do not reflect changes since the reference year for the data.

But bias can also occur even if the data used for estimates are otherwise timely and measuring the appropriate concept. Regularly conducted household surveys (such as the March CPS) that are designed to collect information with which to estimate the official concept of poverty can result in biased estimates due to measurement problems (see Chapter 4). For example, two sources of downward bias (i.e., underestimation) in poverty rates from household surveys are that they tend not to cover lower income groups as completely as middle- and higher-income groups, and that they have disproportionately more nonrespondents among lower income people. A source of upward bias (i.e., overestimation) in survey poverty rates is that respondents tend to underreport their income. For administrative records that are otherwise appropriate to use for income and poverty estimates, there can be biases across areas due to differences in participation rates and other factors of program design and administration (see Chapter 5).

If the resulting overall bias in estimates, up or down, is the same for all areas, then the effects on allocations may not be great if the formula does not also contain such provisions as thresholds for eligibility. Much more likely, however, is that biases will affect some types of areas more than others, affecting allocations even in formulas that simply distribute shares of a fixed amount to all areas.

Variance is generally of less concern than bias because it is expected that errors will balance out over time–for example, one state or county may have its poverty rate overestimated in one year and underestimated the next year and vice versa for another state or county. However, formula provisions, such as thresholds, can interact with the variability in estimates in ways that disproportionately favor or disfavor particular areas. Moreover, the greater the variability, the greater these kinds of effects (see Chapter 6).

DATA SOURCES

Current and planned sources of data for developing small-area income and poverty estimates include: the decennial census; household surveys, including the March CPS, the Survey of Income and Program Participation (SIPP), and the planned American Community Survey (ACS); administrative data, ranging from school lunch counts to federal and state income tax records; and programs for deriving estimates from multiple sources, including the BEA program for estimating total and per capita personal income for states and counties and SAIPE. The strengths and weaknesses of these data sources are briefly described below in terms of the requirements for income and poverty estimates, further illustrating the tradeoffs involved in using particular sources of estimates for program purposes. Chapters 4 and 5 provide more detailed discussion of household surveys and administrative records, respectively, and the role they can play in improving SAIPE estimates.

Decennial Census

The decennial census long form, which was sent to 15-18 million households in the 1990 and 2000 censuses, contains the small number of questions that are asked of all households on the short form and other questions that are unique to the long form. The additional information collected includes annual income amounts for about seven sources and other characteristics that permit estimating income and poverty for a wide range of population groups and geographic areas. Census long-form income and poverty estimates, classified by age and other characteristics, are routinely provided for the income reference year (1989 for the 1990 census and 1999 for the 2000 census) for states, counties, towns and townships, places, census tracts, and block groups. Estimates are also often prepared for other kinds of small areas, such as school districts, by aggregating the estimates for individual census blocks. As noted above, these estimates are usually released 2 to 3 years after the census is completed.

The long-form census survey has major strengths as a source of income and poverty estimates: it measures official concepts of household poverty and income;[8] it collects a range of characteristics for developing estimates for specific population groups; and it provides estimates with low sampling error for many subnational areas. The long-form survey also has important drawbacks: it is conducted only once every 10 years; it is believed to measure income and poverty less well than the March CPS and SIPP (although more research is needed to compare measurement error in the census with household surveys–see Chapter 4); and long-form estimates for very small areas can have high sampling variability. Generally speaking, long-form estimates for areas with fewer than about 20,000 people have relatively large sampling errors, and there are many areas smaller than this size for which estimates are needed: 47 percent of counties and 82 percent of school districts are below 20,000 population (although these areas account for small proportions of people–see Chapter 4). If the American Community Survey is implemented as planned, there may be no long-form survey in the 2010 or subsequent censuses.

Household Surveys

March Current Population Survey

The March income supplement is an annual addition to the monthly CPS labor force survey that currently has a sample size of about 50,000 households. (The sample size may increase in the future.) The March supplement obtains detailed responses on sources of income in the preceding calendar year (about 30 separate questions) and on many other characteristics that permit estimating a range of income and poverty statistics. The March supplement is the source of official income and poverty estimates for the preceding year that are published each fall for the United States as a whole and certain population groups.

The March CPS has potential advantages for small-area income and poverty estimates because it is conducted annually and obtains extensive income data and other characteristics. However, at present, the survey's relatively small sample size rules out its use to produce reliable direct estimates for subnational areas, except for the largest 10 or 12 states and a handful of very large counties (see Chapter 4). Indeed, the survey includes no households in the sample from which to develop direct esti-

[8]That is, the census uses the official poverty thresholds for different size and type families and compares them to the official income definition, which is before-tax money income. However, it does not provide precisely the same estimates as the March CPS, which is the official source of income and poverty statistics, because of differences in questionnaires, data collection procedures, and other features of the two surveys.

mates for about two-thirds of the country's counties. The only way to use the March CPS data for small-area income and poverty estimates is in statistical models, such as the SAIPE state and county models discussed below, that combine information from multiple sources.

Survey of Income and Program Participation

The SIPP survey began in 1983 as a series of panels, each of which followed a sample of household members for about 32 months, with interviews every 4 months. The 1996 SIPP panel followed the members of about 37,000 originally sampled households over a 4-year period from 1996 to early 2000. A new SIPP panel of about the same size is expected to follow sample members for a 3-year period beginning in 2001. Design changes are being considered for SIPP that could make it possible for the survey to provide official income and poverty statistics in place of the March CPS.

SIPP obtains even more detailed information about income and population characteristics than the March CPS, asking about 60 questions on sources of income. The survey obtains more complete reporting of many income sources than the March CPS, and two Committee on National Statistics reports have recommended that SIPP become the basis of official income and poverty statistics (National Research Council, 1993, 1995a). To date, however, such a role for SIPP has not proved practicable. One reason is the time to process the data, which has typically delayed release of data files for several years after the income reference period. Another reason relates to the longitudinal nature of the survey. Sizable proportions of households drop out of each panel before it is completed, and research shows that the dropouts differ importantly from full-panel respondents in their income and poverty characteristics. Funding is being sought for a design that would introduce a new 3-year SIPP panel every year, so that several panels would be in progress at the same time. This design would make it possible to develop annual poverty and income statistics that do not have an increasing level of error due to dropouts over the course of a single panel.

Like the March CPS, SIPP cannot provide reliable direct income and poverty estimates for subnational areas because of its relatively small sample size (smaller than in the CPS), and, unlike the CPS, it is not currently designed to provide reliable estimates at the state level even for the largest states. There is a potential to use SIPP in models if these problems are resolved.

American Community Survey

The ACS is intended to be a large-scale, monthly sample survey of U.S. households, similar to the census long-form survey in content and administration but operating continuously. The ACS is now (1996-2002) in a design and testing phase. If funds are appropriated, it will become operational in 2003, sampling 250,000 households each month, spread across the nation, so that every county, school district, and other small area will have sample households. The annual sample will be about 3 million households; over a 5-year period, the ACS sample size will cumulate to about 15 million households. This sample size is only somewhat smaller than the expected 2000 census long-form sample size, although the ACS sample size will be reduced for analysis because only one-third of households that do not respond to a mail questionnaire or telephone follow-up will be followed up in person.[9]

If it is implemented as planned, the ACS will have important advantages for small-area income and poverty estimates. It will measure current official concepts of income and poverty, collect a range of population characteristics permitting estimates for particular groups, provide data at frequent intervals, and have much larger sample sizes (when cumulated to 1 or more years) than any existing household survey. Also, its design will provide sample households in every state and county each year.

However, the ACS may have important disadvantages as well. Although research will be needed to evaluate income measurements across surveys, it is likely that the ACS will prove to be a relatively crude instrument for measuring income and poverty in comparison with the March CPS and SIPP. One reason is that the ACS questionnaire, like the long form, contains a small number of questions on income. Also, the "rolling" nature of the ACS may create measurement problems. Thus, the questionnaire will ask about income in the past 12 months and not the more natural reference period of the past calendar year (see Chapter 4).

In addition, although the ACS will have a much larger sample size than other household surveys, direct estimates of income and poverty will still not be reliable for many small areas, such as school districts, even if the data are cumulated for as many as 5 years. Moreover, cumulating data for multiple years could lead to biases that would affect program uses. For example, an allocation formula that targeted poor areas might, using 5-year poverty estimates, give the same allocation to an area that had experienced a pronounced rise in poverty over those 5 years as to an

[9]For the census, the goal is 100 percent folllow-up for nonrespondents.

area that had experienced a pronounced decline if the two areas had the same average poverty estimate over the 5-year period. Moving averages, in which 5-year estimates are produced annually that dropped the earliest year and added the most recent year, would gradually direct funds toward areas with increasing need and away from areas with declining need. However, the adjustment might be more gradual than intended for some programs, unless some other form of weighting were used (e.g., weighting recent years more heavily than earlier years).

The potential of the ACS for small-area income and poverty estimates warrants careful consideration by the users and suppliers of estimates. Such assessments should include both its role for direct estimates and its use with other data for model-based estimates.

Administrative Records

Many federal and state programs include data from administrative records as factors in formulas for allocating funds to states and local areas. Examples include the number of children approved for free or reduced-price school lunches; participants in the TANF program; post-secondary students who receive Pell Grants or other assistance; people who receive food stamps; children enrolled in Head Start; and people in families with low income, based on their tax returns (see Midwest Research Institute, 1999). Administrative records vary in how much information they provide on the characteristics of people, and the information recorded may change over time in response to program administration needs.

For fund allocation purposes, administrative data may be included in a formula because of an intent to target funds to particular groups of people receiving related benefits. Often, however, administrative data are used in formulas as a proxy for poverty estimates that are not available or that are perceived to have drawbacks in comparison with the administrative data (e.g., lack of timeliness). The use of administrative data as a proxy for poverty is particularly common in states for suballocating federal funds or allocating their own funds to localities.

As a proxy for poverty, administrative counts of program beneficiaries (e.g., food stamp or school lunch recipients) have advantages, particularly for use by states: they are often readily available at little added expense for such areas as counties and school districts; they are regularly updated; and they are not subject to variability from sampling error, although they may have other sources of random error (e.g., errors in data entry and updating). They often have "face validity": comments from state agencies suggest that school lunch counts are viewed as good proxies for school district estimates of poor children and are preferred to out-of-date census estimates and to SAIPE estimates, when those estimates do not match the school lunch counts (Midwest Research Institute, 1999).

Administrative counts of program beneficiaries may not be a good proxy for differences in poverty across areas because of differences in program administration and participation. For food stamps, for example, eligibility requirements are similar to official poverty concepts–generally, eligible households must have gross income below 130 percent of the poverty level and net income after certain deductions below 100 percent of the poverty level. However, data for states and counties are counts of people actually receiving food stamps, not of people who are eligible to apply for them, and research has shown that the proportion of the eligible population enrolling in the program varies across areas. Reasons for such variations include differences in program outreach and other features of program administration, as well as differences in the willingness of eligible people to sign up for benefits. Whatever the reasons, differences in participation rates mean that food stamp recipient populations may not be a consistent indicator of poor populations across areas. Moreover, changes in program features may affect how consistently recipient populations relate to poor populations over time. For example, the 1996 welfare reform legislation restricted food stamp eligibility for certain groups, such as recent immigrants, who are distributed unevenly across geographic areas, and may have had other effects on both interarea and intertemporal comparability as well (see Chapter 5).

Other administrative programs do not relate as closely in their eligibility requirements to official poverty concepts as the Food Stamp Program. For example, the eligibility standards for the National School Lunch Program are considerably higher than the poverty threshold (130% of the poverty threshold for free lunch and 185% of the poverty threshold for reduced-price lunch). Consequently, it is likely that using school lunch data will overestimate the number of poor children, and the extent of overestimation across areas will vary. Reasons for such variation include: differences across areas in the income distribution–one area may have fewer near-poor children relative to poor children than another area; in program administration–some school districts may be more aggressive in encouraging families to participate than other districts; and in participation–some families may not enroll their children because of perceived stigma.

For allocation programs for poor children that have significant thresholds to receive funding, such as Title I concentration grants, the use of school lunch counts as a proxy measure would likely provide funds to districts that would not be eligible if a poverty measure were available. For programs that have no or very low thresholds for eligibility, the use of school lunch counts to apportion shares of the total amount to school districts would not necessarily be problematic if there were no variations across areas in the extent to which school lunch counts overestimated poverty. However, such variations are likely. Analysis by the panel found no advantage of school lunch counts over SAIPE school district estimates of poor school-age children for Title I allocations in two states (see Chapter 3; see also National Research Council, 2000c).

Generally, careful consideration needs to be given to the use of administrative data as proxy measures of poverty in an allocation formula. However, such data can be very useful in another role, namely, to provide predictor variables for developing small-area poverty (and income) estimates from models, as is done in the SAIPE program. For this use, it is not necessary that the administrative data measure the official poverty concept, but only that the data are a good predictor of poverty and be available at the required geographic level of detail. Yet this use still requires that the administrative data be consistently measured across areas, such as states and counties, so that biases favoring some areas over other areas are not introduced in the prediction models.

BEA Income Estimates

BEA produces state and county estimates of personal income and per capita personal income as part of the national income and product accounts. The data used to produce the income estimates are primarily from the decennial census and administrative records from federal and state government programs (e.g., records for unemployment insurance, Social Security, Medicare, Medicaid, other social welfare programs, and tax records); surveys also provide some data inputs. The per capita income estimates for state and counties are the total income estimates divided by population estimates, which are obtained from the Census Bureau's population estimates program. The BEA estimates are produced quarterly for states and annually for counties, with a 2-year lag between the time of release and the reference year for the income data.

The advantages of the BEA income estimates for use in fund allocation formulas and other program purposes are that they are regularly updated and measure an income concept that distinguishes more well-off from less well-off areas. However, the BEA program does not provide estimates for subcounty areas or for population groups, and it does not provide estimates of poverty or other types of income measures, such as median or average family income. Also, while the BEA personal income measures are viewed as more complete than household income reports from surveys, the BEA personal income concept is not quite the same as the household income concept that is measured in surveys. The BEA concept is broader than household income, including income of quasipersons (e.g., nonprofit institutions that serve individuals and private trust funds) and treating some sources of income differently. The sampling variability of the BEA estimates for states and counties is not known.

SAIPE Estimates

The SAIPE Program is a new source of regularly updated small-area estimates of income and poverty. As noted in Chapter 1, SAIPE currently produces the following estimates for states and counties: all poor persons, poor children under the age of 5 (states only), poor children under the age of 18, poor related children aged 5-17, and median income of households. For school districts, SAIPE produces estimates of poor related school-age children. SAIPE state estimates are available for 1993, 1995, and 1996, and will be released annually hereafter. County and school district estimates are available for 1993 and 1995 and will continue to be released on a biennial schedule, with about a 3-year lag from the income reference year.

The SAIPE estimates for states and counties are developed from regression models that predict poverty (or income) in the March CPS on the basis of data from administrative records and the previous census. Predictions from the regression model are then combined, when possible, with the direct estimates from the March CPS to form model-based estimates (see Chapter 3 for a description of the estimation procedure).

The income tax and food stamp administrative data that are used in the state and county regression models are not currently available for school districts, so a simpler model is used to estimate poor school-age children at the district level. That model applies each school district's share or proportion of the county total of poor school-age children, as measured in the 1990 census, to the updated county estimate from the SAIPE county model. The school district model captures changes in poverty across counties, but necessarily assumes that, within each county, the poorer (less poor) districts at the time of the census remain just as poor (less poor) in later years.

The SAIPE model-based estimates have several advantages for use in fund allocation formulas and other program purposes: they are updated annually or biennially; they reflect official concepts of income and poverty with the survey that is currently the source of official income statistics; and they are available for school districts as well as states and counties. Validation work conducted to date indicates that the SAIPE estimates are preferable to continuing to use outdated census estimates: the differences between SAIPE model-based estimates for income year 1989 and 1990 census estimates are substantially smaller than the differences between 1980 census and 1990 census estimates (see Chapter 3).

However, the SAIPE estimates have some disadvantages for program use. Although more up to date than census estimates, they lag the income reference year by 3 or more years. Also, they are currently limited in scope (e.g., no estimates are available of poor elderly or poor single-parent families). In the future, it would be possible to reduce the time lag somewhat and to develop estimates for other groups.

Although considerable evaluation work has been done, more needs to be learned about the properties of the models and data inputs to assess whether any persistent biases are present in the estimates. Random error will always be present in model-based estimates (which is also true for estimates from any other source). Also, model estimates will generally be less accurate for areas that are at the extremes of the variable being predicted in comparison with areas that have less extreme values. In this regard, evaluation showed that the SAIPE county model overpredicted (underpredicted) the number of poor school-age children in 1989 in areas that experienced a marked decline (increase) in child poverty from 1979 to 1989, but the SAIPE estimates performed substantially better for these areas than the 1980 census estimates. Evaluation to date has not identified significant biases in the SAIPE estimates for other characteristics, but further work is needed on this issue.

The production of model-based estimates, such as the SAIPE estimates, requires a significant, continuing investment in model and data validation, research and development, and related activities to ensure that estimates are as accurate as possible. A model-based estimates program should provide full documentation to inform users about the properties of the estimates and their advantages and drawbacks for program use (see Chapter 7).[10]

CONCLUSION

Different data sources for estimates of poverty and income for small areas each have strengths and weaknesses for use in fund allocation formulas and program administration. For example, while the decennial census provides estimates of poverty and income with low sampling variability for many small areas, the estimates are only available once every 10 years. In contrast, administrative records may be available on a timely basis and have other advantages, but they do not always measure the

[10]Users should also require documentation and evaluation for estimates that are not developed on the basis of an explicit model: for example, evaluation of the effects on fund allocations of using census estimates over a decade or more or of using administrative data as a proxy for poverty.

concept of income or poverty targeted in the program for which they are needed or consistently relate to that concept across geographic areas or over time. In considering particular sources of small-area income and poverty estimates, it is important for agencies and policy makers to understand their properties and how bias and variability in the estimates may affect their intended program use.

We were charged with reviewing the SAIPE Program of small-area income and poverty estimates. We believe that these estimates will be increasingly used for such purposes as fund allocation and program evaluation, as users come to understand their properties and as the SAIPE Program responds to user needs. For example, state estimates are now produced annually instead of every 2 years, and it may be possible to develop estimates for other population groups.

In Chapter 3 we identify short-term priorities for research and development for the current SAIPE models. In Chapters 4 and 5 we consider the possible role of new or modified sources of survey data and administrative data to further improve the estimates, particularly for subcounty areas. These three chapters are aimed primarily at the Census Bureau and other researchers in the field, but they have overview sections that highlight key points of interest to users.

In Chapter 6 we return to a user perspective, considering how errors in estimates, which always will be present, may affect formula fund allocations. We illustrate the possibly unintended consequences that can result from interactions between the properties of estimates and provisions of formulas. It is particularly important for users to consider such interactions when deciding to change from one source of estimates to another–as occurred when the Title I program shifted from using decennial census estimates for allocations to using SAIPE estimates–or when developing new funding formulas and deciding which source of estimates to use for them.

3

Current SAIPE Models

USER OVERVIEW

The Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program produces income and poverty estimates for states and counties, including estimates of median household income, total poor, poor under age 5 (states only), poor aged 5-17 in families, and poor under age 18. These estimates, which are updated every year for states and every 2 years for counties, are termed "indirect estimates." They are indirect because they are developed from statistical models that use data from other areas and time periods, unlike "direct estimates," which are based solely on a survey's sample cases in the given area and period.[1] The use of indirect estimation for producing updated state and county income and poverty estimates is necessary because there is currently no survey or administrative record data source that can provide the required estimates with sufficient reliability for intercensal years. Indirect estimates of poor school-age children for school districts are derived by using decennial census data to allocate the updated county estimates among districts.

The March Current Population Survey (CPS) collects the detailed in-

[1]Other terms are also used in the research literature for these concepts: for example, direct estimates are sometimes called "sample-based" estimates, and indirect estimates are sometimes called "synthetic," "model-based," or "model-dependent" estimates (see U.S. Office of Management and Budget, 1993).

formation on income needed to produce the required income and poverty estimates. However, the sample is too small to produce sufficiently reliable direct estimates for states, let alone counties. Indeed, most counties have no CPS sample. Therefore, state and county income and poverty estimates are obtained from statistical regression models, and the SAIPE estimates are produced by using weighted averages of the regression predictions and the direct CPS estimates, when the latter are available. The weighted average approach for combining the model predictions and the direct estimates is advantageous in that it strikes an effective tradeoff of the model error of the model predictions and the sampling error of the direct estimates.

The state-level model predictions are obtained from regression models in which a state's direct CPS estimate for the reference year is the dependent variable and the predictor variables are obtained from such sources as Internal Revenue Service (IRS) tax returns, food stamp records, population estimates from the Census Bureau's demographic estimates program, and the previous census. The SAIPE estimate for a state is then a weighted average of the model prediction and the direct estimate for the state.

The same general approach is used for the SAIPE county estimates, with the same sources of data for the predictor variables in the regression models. One difference is that 3 years of March CPS information are combined to form the dependent variables in the regression models and to calculate the direct estimates. For the poverty models, another difference is that the county models estimate numbers of poor (in logarithms), while the state models estimate the proportions of poor. For the one-third of counties that have households in the CPS sample, the model predictions are combined with the direct estimates, as is done for the state models. For the other two-thirds of counties, the model predictions are taken to be the estimates. As a last step in developing the SAIPE county poverty estimates, each of the county estimates in a state is multiplied by a constant factor that makes the sum of the adjusted county estimates equal the SAIPE state estimate.

For school districts, no administrative data are currently available from which to form predictor variables for use in poverty models. IRS and food stamp data are not available at the school district level. Counts of students approved to receive free school lunches are a potential source for all districts, but they are not now nationally available, and there are serious concerns about the comparability of the counts across all districts. Hence, the Census Bureau produces estimates for districts using a "shares" approach. This approach assumes that each school district in a county has the same proportion (share) of that county's poor school-age children in the estimation, or reference, year as it did in the 1990 census.

Then the 1990 census shares of poor school-age children for school districts within counties are applied to the updated SAIPE county estimates to produce the SAIPE school district estimates for the reference year.

The production of indirect estimates like those from the SAIPE program is a complex operation that needs to be fully evaluated. The evaluation should check on the input data from the multiple sources, it should examine the adequacy of the models used to produce the model predictions, and it should carefully assess the resulting estimates. Since flaws in any aspect of the estimation process can distort indirect estimates, an evaluation scheme of this form should be a standard component of a small-area estimation program. Moreover, the evaluation should be done every time that estimates are produced.

The panel and the Census Bureau performed detailed evaluations of the SAIPE state and county estimates of poor school-age children, which are described in the companion volume to this report (National Research Council, 2000c). These evaluations include internal assessment of the structure and functioning of the regression models, external comparisons with census data, and, for counties, external comparisons with aggregate CPS estimates. Census and CPS aggregate data are not ideal for evaluation purposes. Yet they can help answer the key question of whether the model estimates show any strong, persistent biases for areas with specific attributes (e.g., areas with large or small populations, high or low poverty rates, rapid or slow changes in poverty rates) that could have adverse consequences when the estimates are used for fund allocation or other program purposes.

SAIPE county estimates of poor school-age children have also been evaluated by consulting state demographers and others with local knowledge. Since estimates are always subject to error, whether they are produced by a model or from local (or other) information sources, one should not be overly concerned by discrepancies between individual estimates and local sources. However, local assessment may indicate persistent patterns of marked discrepancies for areas with common attributes that should be investigated.

The internal and external evaluations of the 1993 and 1995 state and county estimates led the panel to conclude that the models are working reasonably well and that these estimates are preferable to 1990 census estimates as a basis for Title I allocations (National Research Council, 1998, 1999). According to Census Bureau calculations, the SAIPE estimates, on average, have more variability due to sampling error and prediction error than the census estimates. However, the out-of-date census estimates have considerably more bias. For example, estimates produced for 1989 using the modeling approach differed much less from the 1990 census estimates than did estimates from the 1980 census.

Although the evaluations of the SAIPE state and county estimates have supported their use for fund allocation, they have identified aspects of the models that require additional research and development. Some priorities for SAIPE model development are presented later in this chapter (see also National Research Council, 2000c). In addition to research to improve the existing models, research is needed to examine how data from new sources, such as the 2000 census and the proposed American Community Survey, may contribute to the production of the SAIPE estimates. (The potential uses of these sources in the SAIPE program are discussed in Chapter 4.)

As noted above, the lack of administrative data at the school district level led the Census Bureau to use a simple shares approach based on 1990 census data for allocating the updated SAIPE county estimates of poor school-age children among school districts. Only limited evaluations of the school district estimates are possible, but it is clear that the estimates are not very reliable for most school districts. Nevertheless, the evaluations led the panel to conclude that the 1995 school district estimates were the best available for Title I allocations–for example, as good as or superior to 1990 census estimates or estimates based on school lunch counts. Marked improvement of the SAIPE poverty estimates for school districts and other subcounty areas will require investment in new or modified data sources that can provide the basis for improved models for these areas. (Chapter 5 identifies possible new administrative data sources that would likely improve SAIPE subcounty estimates.)

The next few sections of this chapter present a technical overview of the SAIPE models for estimates of poor school-age children for states, counties, and school districts, including a description of the Census Bureau's methods for estimating variability in the state and county estimates, and a summary of the evaluations conducted to date. The chapter then briefly summarizes the other SAIPE models (e.g., median household income and poverty for other age groups) and the Census Bureau's methods for producing small-area population estimates and their evaluation. (Population estimates are used both in the SAIPE poverty models and in Title I and other fund allocation programs.) The last section of the chapter provides recommendations to the Census Bureau for research and development to improve the current SAIPE models.

MODELS FOR POOR SCHOOL-AGE CHILDREN

State and County Models

The Census Bureau constructs separate regression models for estimating the numbers of poor school-age children at the state and county levels.[2] In the state model, the dependent variable is an estimate of the proportion of school-age children who are poor; in the county model, it is the logarithm of the number of poor school-age children. In both cases, the dependent variable is constructed from CPS data. For both models, the deviations from the regression are assumed to follow a variance components model with two components. One component represents *sampling error* in the dependent variable. The other component represents the deviations in the model predictions from the true values that would occur in a model in which the dependent variable is not subject to sampling error; the Census Bureau, as is commonly done, refers to this component as *model error*. The state and county estimates are weighted averages of the direct CPS estimates (where available) and the regression predictions, where the weights are functions of the variance components. School district estimates are derived from county estimates under the assumption that the relative proportion (share) of the poor school-age children in a county who are in a particular school district in the reference year is the same as it was in the 1990 census.

Input Data

Both the state and county models of poor school-age children use input data from five sources: the March CPS; the previous census; the Census Bureau's population estimates program; food stamp administrative records; and IRS individual income tax returns. The dependent variable in the state regression model is formed from data from the March CPS for the reference year. The dependent variable in the county model is created as a weighted average of estimates calculated from 3 years of March CPS data, centered on the reference year, in order to improve the precision of the CPS estimates. The other four sources are used to form predictor variables for the regression models.

After examining a variety of administrative records, the Census Bureau chose food stamp and tax return data as sources of predictor variables. These sources were chosen because they contain data from which variables related to poverty can be constructed, because they are available for all states and counties, and because they are, as far as possible, constructed using the same definitions and procedures nationwide (see National Research Council, 2000c, for details of how these data are obtained). The Census Bureau receives an extract of information on tax returns each fall that were filed in April for the preceding year (the extract omits some

[2] More precisely, the Census Bureau's estimates pertain to related children aged 5-17 in poor families, termed "poor school-age children" in this report; see Chapter 1:fn 2.

returns, such as those filed late). The Census Bureau receives monthly counts of food stamp recipients from the U.S. Department of Agriculture for states. For most counties, the Bureau receives food stamp counts that pertain to July 1 of the reference year; for some counties the counts are an average of the monthly counts for the year. A concern with using food stamp recipient data in the state and county models is that participation rates (recipients as a proportion of people who are eligible to apply) differ across areas. These differences may have become larger due to the effects of the 1996 legislation that changed several social welfare programs (see Chapter 5).

State Model

As noted above, the state model for the proportion of school-age children who are poor is estimated for the year of interest—the reference year—using CPS data for that year (the year subscript is suppressed below). The state model is

$$y_j = \alpha_0 + \alpha_1 x_{1j} + \alpha_2 x_{2j} + \alpha_3 x_{3j} + \alpha_4 x_{4j} + u_j + e_j, \tag{3.1}$$

where:

- y_j = estimated proportion of school-age children in state j who are in poverty based on the March CPS that collects income data pertaining to the reference year,
- x_{1j} = proportion of child exemptions reported by families in poverty on tax returns in state j,
- x_{2j} = proportion of people receiving food stamps in state j,
- x_{3j} = proportion of people under age 65 not included on an income tax return in state j,
- x_{4j} = residual for state j from a regression of the proportion of poor school-age children estimated from the prior decennial census on the three predictor variables, (x_{1j}, x_{2j}, x_{3j}), for the census reporting period,
- u_j = model error for state j, and
- e_j = sampling error of the dependent variable for state j.

The u_j are independent of e_j for all j and i. Also, it is assumed that $u_j \sim \mathrm{NI}(0, \sigma_u^2)$ and that $e_j \sim \mathrm{NI}(0, \sigma_{e_j}^2)$, where $\sim\mathrm{NI}(\mu, \sigma^2)$ is read "distributed normally and independently with mean μ and variance σ^2." The $\sigma_{e_j}^2$ are estimated from CPS data using a generalized variance function (GVF) procedure documented in Otto and Bell (1995).

The coefficients for model (3.1) and the model error variance (σ_u^2) are estimated by maximum likelihood, treating the estimated $\sigma^2_{e_j}$ as known. The SAIPE estimate of the proportion of school-age children living in poverty in a state is a weighted average of the model-based estimate ($\hat{y}_j$) and the CPS-based direct estimate for the state (y_j), where the weights are proportional to the estimated precision of the two components. The SAIPE estimate for the proportion of school-age children in poverty in state j is

$$\tilde{y}_j = \gamma_{sj} y_j + (1 - \gamma_{sj}) \hat{y}_j, \tag{3.2}$$

where:

$$\gamma_{sj} = \hat{\sigma}^2_u \left(\hat{\sigma}^2_u + \hat{\sigma}^2_{e_j} \right)^{-1},$$

$$\hat{y}_j = \hat{\alpha}_0 + \hat{\alpha}_1 x_{1j} + \hat{\alpha}_2 x_{2j} + \hat{\alpha}_3 x_{3j} + \hat{\alpha}_4 x_{4j},$$

$\hat{\sigma}^2_u$ is the maximum likelihood estimate of σ^2_u, ($\hat{\alpha}_0$, $\hat{\alpha}_1$, $\hat{\alpha}_2$, $\hat{\alpha}_3$, $\hat{\alpha}_4$) is the maximum likelihood estimate of (α_0, α_1, α_2, α_3, α_4) and $\hat{\sigma}^2_{e_j}$ is the estimate of the variance of the CPS estimate y_j, based on CPS data. (Both "estimator" and "predictor" are used in the literature to describe $\tilde{y}_j$.)

An initial estimate of the number of poor school-age children for a state is obtained by multiplying the estimated proportion poor ($\tilde{y}_j$) by the estimated total number of noninstitutionalized school-age children in the state, which is obtained from the Census Bureau's program of population estimates.

The initial state-level estimates of the number of poor school-age children are then ratio adjusted to sum to the CPS national estimate of poor school-age children. Thus, the final estimate of the number of poor school-age children in state is

$$\tilde{T}_{j\bullet} = \left(\sum_k T_{k\bullet} \right) \left(\sum_k \tilde{y}_k \hat{N}_k \right)^{-1} \tilde{y}_j \hat{N}_j, \tag{3.3}$$

where $T_{j\bullet}$ is the CPS estimate of the number of poor school-age children in state j, $\hat{N}_j$ is the estimated number of noninstitutionalized school-age children in state j from the Census Bureau population estimates, and the summation is over all states. Historically, the ratio adjustment in (3.3) has changed the estimates by less than 1 percent.

County Model

The state model uses proportion poor as the dependent variable and proportions as explanatory variables. The county model is slightly different in that it uses the logarithm of number poor as the dependent variable and is a model linear in logarithms. The county model is

$$z_{ji} = \beta_0 + \beta_1 w_{1ji} + \beta_2 w_{2ji} + \beta_3 w_{3ji} + \beta_4 w_{4ji} + \beta_5 w_{5ji} + v_{ji} + a_{ji}, \qquad (3.4)$$

where:

- z_{ji} = log (3-year weighted average of number of poor school-age children in county i of state j based on 3 years of March CPS data),[3]
- w_{1ji} = log (number of child exemptions reported by families in poverty on tax returns in county i of state j),
- w_{2ji} = log (number of people receiving food stamps in county i of state j),
- w_{3ji} = log (estimated population under age 18 in county i of state j),
- w_{4ji} = log (number of child exemptions on tax returns in county i of state j),
- w_{5ji} = log (number of poor school-age children in county i of state j in the previous census),
- v_{ji} = model error for county i of state j, and
- a_{ji} = sampling error of the dependent variable for county i of state j.

It is assumed that $v_{ji} \sim \text{NI}(0, \sigma_v^2)$, that v_{ji} is independent of v_{km} for all ji and km, and that $a_{ji} \sim \text{NI}(0, n_{ji}^{-1}\sigma_a^2)$, where n_{ji} is the CPS sample size for county i of state j.[4] Although the variables carry a state identification, there are no state effects in the model.

The between-county variance component, σ_v^2, is estimated using data from the 1990 census. A model, analogous to (3.4), is constructed in which the dependent variable is obtained from the 1990 census long form and the predictor variables are for the census reporting year. In this model, the census sampling variance (corresponding to $n_{ji}^{-1}\sigma_a^2$) is estimated using a generalized variance function and is then treated as fixed

[3]The number of poor school-age children is the product of the weighted 3-year average CPS poverty rate for related children aged 5-17 and the weighted 3-year average CPS number of related children aged 5-17; see National Research Council (2000c:Ch.4) for derivation of the weights.

[4]The assumption that the variance of a_{ji} is simply inversely proportional to sample size is only an approximation, given the clustered CPS sample design. A different formulation may be preferable; see the discussion below of improved estimation of variance components.

in fitting the model by maximum likelihood. The maximum likelihood parameter estimates obtained from the census data are estimated census regression coefficients and the estimated model error variance, $\hat{\sigma}_v^2$. The assumption is made that the model error variance in the census regression and the county model regression (3.4) are the same. Documentation of the estimation approach is provided by Fisher (1997); see also National Research Council (2000c:Ch.4).

Data from the CPS and $\hat{\sigma}_v^2$ from the census regression are used to estimate σ_a^2 and the vector $(\beta_0, \beta_1, \beta_2, \beta_3, \beta_4)$ of equation (3.4). The estimate $\hat{\sigma}_v^2$ is treated as fixed in the final estimation. Counties that are in the CPS sample and that have one or more poor school-age sampled children are included in the estimation data set for the county model, and those with no poor school-age sampled children are omitted.

The predictor of the logarithm of the number of poor school-age children in county i of state j is

$$\tilde{z}_{jt} = \gamma_{cji} z_{ji} + (1 - \gamma_{cji})\hat{z}_{ji}, \tag{3.5}$$

where

$$\gamma_{cji} = \begin{cases} \hat{\sigma}_v^2\left(\hat{\sigma}_v^2 + n_{ji}^{-1}\hat{\sigma}_a^2\right)^{-1} & \text{if county } ji \text{ is in the estimation data set;} \\ 0 & \text{otherwise;} \end{cases}$$

$$\hat{z}_{ji} = \hat{\beta}_0 + \hat{\beta}_1 w_{1ji} + \hat{\beta}_2 w_{2ji} + \hat{\beta}_3 w_{3ji} + \hat{\beta}_4 w_{4ji} + \hat{\beta}_5 w_{5ji},$$

and $(\hat{\beta}_0, \hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3, \hat{\beta}_4, \hat{\beta}_5)$ is the maximum likelihood estimator of the regression vector. An initial predictor of the number of poor school-age children for county ji is obtained by transforming back to the initial scale:

$$\hat{T}_{ji} = \exp\left\{\tilde{z}_{ji} + \tilde{b}_{ji}\right\}, \tag{3.6}$$

where $\tilde{b}_{ji}$ adjusts for the bias introduced by exponentiation, which is a nonlinear transformation. This bias adjustment is derived from the expression for the mean of the lognormal distribution (see Fisher, 1997).[5]

The final county estimates for a state are ratio adjusted so that the sum of the county estimates in a state is equal to the estimated state total obtained from the state model. Thus, the estimate for county ji is

[5]Another possibility would be to use the procedure in Duan (1983).

$$\tilde{T}_{ji} = \tilde{T}_{j\bullet}\left(\sum_k \hat{T}_{jk}\right)^{-1}\hat{T}_{ji}, \tag{3.7}$$

where the summation is over the counties in state j, and $\tilde{T}_{j\bullet}$ is the state estimate defined in equation (3.3). Unlike the ratio adjustment for the state estimates, these adjustments are often large and highly variable across states. For the final county estimates of poor school-age children in 1993, the average state ratio adjustment–the SAIPE state estimate divided by the sum of the initial county estimates, known as the state raking factor–was 1.07; two-thirds of the factors were between 0.98 and 1.16. For 1995, the average state raking factor was 0.97; two-thirds of the factors were between 0.88 and 1.06. The correlation between raking factors for states in 1993 and 1995 is low, which implies that there was little systematic variation by state across these years.

School District Procedure

Because of the lack of administrative data at the school district level for constructing predictor variables, the school district estimates of poor school-age children are produced by a shares approach rather than by regression modeling. This shares approach allocates the updated county estimates among school districts in the same proportions that poor school-age children were distributed across the districts in the 1990 census. Although the general approach is simple, a number of complications arise in its application (see National Research Council, 2000c:Ch.7, for further details).

First, school district boundaries change over time. To address this problem, the Census Bureau conducts a survey every 2 years in which officials in every state are asked to update the boundaries for the districts in their state. Using these boundaries, the 1990 census blocks are allocated to school districts, and the census counts of poor school-age children are summed for the blocks in each district. When school district boundaries cut through blocks, the block counts are proportionately allocated.

Second, some school districts cross county boundaries. These districts are divided into parts by county, and the shares approach is applied to school district parts within each county. The estimate for a school district is then obtained by adding together the estimates for its parts.

Third, some school districts cover only selected grades (e.g., kindergarten through grade 8), with the result that some blocks are in more than one school district. This problem is addressed by allocating the poor children in the appropriate age range to each district.

Fourth, for many districts the census estimates of poor school-age children are subject to substantial levels of sampling error because they are derived from data collected from the census long-form sample. To reduce this sampling error, the estimates for the district parts are ratio adjusted to make the total number of school-age children from the long-form sample conform to the number of school-age children from the complete census.

The estimated number of poor school-age children in school district part d in county i in state j for the reference year is given by

$$\tilde{N}_{jid} = R_{jid}\tilde{T}_{ji},$$

where R_{jid} is the ratio-adjusted estimate of the proportion of poor school-age children in that district part in the 1990 census, and $\tilde{T}_{ji}$ is the updated county estimate given by (3.7). The ratio-adjusted estimate R_{jid} is given by $R_{jid} = C_{jid} A'_{jid} A_{jid}^{-1}$, where in district part d in county i in state j, C_{jid} is the estimated number of poor school-age children from the long-form sample, A'_{jid} is the number of school-age children from the complete census, and A_{jid} is the estimated number of school-age children from the long-form sample.

Evaluations

As recommended by the National Research Council panel, the Census Bureau conducted an extensive set of evaluations of the SAIPE estimates of poor school-age children for states and counties. Due to data constraints, more limited evaluations were conducted of the estimates of poor school-age children for school districts. The companion technical documentation volume to this report describes the methods and results of the state, county, and school district evaluations in detail (National Research Council, 2000c:Chs.6,7). Below we summarize the principal evaluation methods under two headings—internal evaluation and external evaluation—and highlight key results.

Internal Evaluations of State and County Models

For each year for which the state and county models were estimated, an internal evaluation was conducted of the underlying assumptions and features of the models. Internal evaluations were also conducted of alternative forms of the county model. Such evaluations, which principally involved examination of the residuals from the regression before taking the weighted average of the regression estimates with the direct estimates or raking to control totals, are necessary to establish that a model is performing well in terms of its assumptions.[6]

Six assumptions were examined for the state and county models, most often by reviewing a variety of graphical plots:

- linearity of the relationships between the dependent variable and the predictor variables;
- constancy of the assumed linear relationship over time (evaluated by comparing the regression coefficients across years);
- absence of systematic patterns in the standardized residuals across categories of states or counties (e.g., counties categorized by population size), where nonrandom patterns could indicate bias and the need for additional predictor variables in the regression model;[7]
- normality (primarily, symmetry and moderate tail length) of the distribution of the standardized residuals;
- homogeneity of the variances of the standardized residuals (typically examined with respect to the values of the predictor variables); and
- absence of outliers for the dependent and predictor variables.

The evaluation also examined *t*-statistics to determine the significance of the predictor variables and whether one or more of them should be excluded from a model.

State Model Results of the internal evaluations of the state model (estimated for each of the years 1989 to 1993, 1995, and 1996) largely supported the model's assumptions.[8] There was no evidence of nonlinearity in the relation between the dependent variable and each predictor variable; the regression coefficients were generally similar across years; only one regression coefficient was not statistically significant (at the 5% level), and it failed to achieve significance in only 1 of the 7 years; there was no evidence of outliers or heterogenous variance; and there was only a small degree of skewness of the standardized residuals. The only evidence of possible bias is that the state model fairly consistently underpredicted the proportion of school-age children who were poor in some Western states and fairly consistently overpredicted this proportion in other Western states.

[6]Such evaluations are often referred to as "regression diagnostics."

[7]See National Research Council (2000c:Ch.6) for the calculation of the standardized residuals and the categories of states and counties examined.

[8]The state model was not estimated for 1994 because a redesign of the CPS sample after the 1990 census was partly but not completely phased in for the March 1995 CPS.

A review of the estimated model error variances in the state model turned up an anomalous result in that the variances were estimated to be zero in every year but 1993. This outcome implies (absent sampling variability) that the model predicts state poverty rates for school-age children perfectly. As a consequence, the direct estimates receive zero weight in the weighted averages of the model estimates and the direct estimates, even when they are quite precise. While differences between the model estimates and the direct estimates are neither unusually large nor strongly persistent, it is not plausible to assume that the model has perfect predictive power. The problem may be that the procedure used by the Census Bureau tends to overestimate the sampling variances. These variances are estimated from the CPS data using a generalized variance function. They are then used in the maximum likelihood procedure that estimates the model error variance in the state model regression. With this procedure, if the estimates of sampling error variances are too large, the estimate of model error variance will be too small.

County Model Internal evaluations were conducted for alternative county models, which were estimated for 1989 and 1993,[9] and for the current county model, which was estimated for 1989, 1993, and 1995. Analysis of the alternative county models largely supported the model assumptions, the analysis did not strongly support one model over another. Some problems were identified: most models tended to overpredict the number of poor school-age children in larger urban counties, especially those with large percentages of Hispanics; all models showed some variance heterogeneity, particularly with respect to CPS sample size and often with respect to the predicted value (number or proportion poor of school-age children); and some models exhibited more problems with outliers and skewness than others. None of the other models was clearly superior to the current SAIPE county model.

Analysis of the current model for 1989, 1993, and 1995 found fairly similar regression coefficients for the predictor variables w_1, w_2, and w_5 in equation (3.4) for all 3 estimation years. The sum of the coefficients for w_3 and w_4 within the regression equation was similar and close to zero in each year. The sum of all coefficients in the regression model was close to

[9]The 13 alternative models varied on three dimensions: treatment of information from the previous census (whether the model included a census-based predictor variable in a single equation or estimated both census and CPS numbers of poor school-age children in a bivariate system of equations); the form of the variables (whether poverty rates or numbers, transformed or not transformed to logarithms); and whether the model included fixed state effects (see National Research Council, 2000c:Ch.5).

1 for all 3 estimation years. (If this sum were 1, the model is expressible as a model with the poverty rate as the dependent variable and rates as predictor variables.) The current model consistently slightly overpredicted the number of poor school-age children in counties with smaller population sizes and in counties in metropolitan areas that are not the central county of the areas.[10] It also exhibited variance heterogeneity with respect to CPS sample size and the predicted value of the number of poor school-age children. The variance heterogeneity with respect to CPS sample size could be a result of a problem in the procedure used to estimate sampling error variances, a problem in the procedure used to estimate model error variance, or, possibly, heterogeneity in the model error variance.

External Evaluations of State and County Models

External evaluations involve comparisons of the estimates from a model with target or "true" values that were not used to develop the model. Such evaluations are important but difficult to carry out. Two sources of comparison values have been used for external evaluations of the SAIPE state and county models for poor school-age children–the previous census and weighted aggregates of CPS direct estimates–but neither source is ideal for this purpose. The census estimates can provide an evaluation for only one year, 1989. Also, they are not true values: they are affected by sampling variability and population undercount. Furthermore, the census measurement of poverty differs from the CPS measurement in ways that are not fully understood (see Chapter 4). The weighted CPS direct estimates can be produced for multiple years, but the sample sizes for CPS estimates, even when the sample is aggregated for 3 years for the county model evaluations, are small for many categories of counties, thus making comparisons with them much less reliable than comparisons with census estimates. Nonetheless, both sources can indicate patterns of differences that suggest possible persistent biases in the model estimates.

In addition to the comparisons with census and CPS estimates, reviewed below, another external evaluation of the 1993 county model estimates of poor school-age children was based on local knowledge. The analysis for this evaluation first identified groups of counties (e.g., large central city counties) for which the 1993 estimates seemed unusually high or low in relation to prior levels and trends (e.g., from 1980 to 1990) in the

[10] A central county is the county in a metropolitan area that contains the central city of the area.

number and proportion poor of school-age children and known socioeconomic trends. Then knowledgeable local people, such as state demographers and state data center staff, were contacted about the counties in these groups. These people questioned the statistical reliability of the 1993 estimates in general and the estimates for specific counties, but they did not identify categories of counties for which the apparent trends in school-age poverty seemed unreasonable.

State Model Comparisons of 1990 census estimates of poor school-age children in 1989 with state model estimates for 1989, 1980 census estimates, and March 1990 CPS direct estimates supported the use of the model estimates. Differences between the 1989 state model estimates and 1990 census estimates were much smaller than the differences between the March 1990 CPS direct estimates and the 1990 census estimates and considerably smaller than the differences between the 1980 census and 1990 census estimates. (Comparable evaluations were not performed for alternative models or for categories of states.)

County Model Estimates of poor school-age children in 1989 from the SAIPE county model and several alternative models and four simpler procedures were compared to 1990 census estimates for all counties and for categories of counties (see National Research Council, 2000c:Ch.6). Overall, the SAIPE model and alternative models performed better than the simpler procedures.[11] For example, the average absolute difference between the 1989 estimates from the SAIPE county model and the 1990 census estimates was 11 percent of the average number of poor school-age children. In contrast, the average absolute difference was 23 percent for the simplest procedure–the stable shares procedure, which assumed no change from 1979 to 1989 in county shares of the national number of poor school-age children.[12]

The SAIPE and alternative models also performed better than the simpler procedures in terms of algebraic differences from census esti-

[11]The four simpler procedures assumed (1) no change from 1979 to 1989 in the county shares of the national number of poor school-age children; (2) no change in the county shares within each state; (3) no change in the county proportions poor of school-age children within each state; and (4) that the 1989 values could be estimated by an average of 1980 census estimates and estimates from one of the county models.

[12]The formula for the average absolute difference, where there are n counties (i), and Y is the estimated number of poor school-age children from a model or the census, is

$$\Sigma[(\,|\,Y_{\text{model } i} - Y_{\text{census } i}\,|\,)\ /\ n]\ /\ [\,\Sigma\,(\,Y_{\text{census } i}\,)\ /\ n\,]\,.$$

[13]The formula for the category algebraic difference for counties (i) in each category (j) is

$$\Sigma_i\,(Y_{\text{model } ij} - Y_{\text{census } ij})\ /\ \Sigma_i Y_{\text{census } ij}\,.$$

mates for categories of counties.[13] A large algebraic difference for a particular category of counties suggests that the estimation procedure is producing biased estimates for the counties in that category. Analysis showed that for most of the categories of counties investigated, the model estimates had smaller algebraic differences and fewer obvious patterns of differences across categories than did the estimates from the simpler procedures. On balance, the current SAIPE county model performed somewhat better than the other models that were evaluated, including a model that was initially selected to serve as the basis for the county estimates.[14] The only potential biases evident with the current model were that it tended to overpredict (underpredict) the number of poor school-age children in counties with the greatest decreases (increases) in school-age poverty rates from 1980 to 1990 and to overpredict the number of poor school-age children in counties with large percentages of Hispanics and counties in the Mountain and Pacific divisions. The problem in the Mountain and Pacific divisions must be attributable to the state model since the county model is raked to the state model, and census divisions are combinations of states. In general, no model can be expected to perform well in predicting for counties that experience very large changes in poverty rates.

Comparisons of algebraic differences for categories of counties between estimates from the county model and weighted 3-year CPS direct estimates centered on 1989, 1993, and 1995 found large model-CPS differences, due mainly to the small sample sizes of the CPS direct estimates. A few differences were both large and in the same direction (plus or minus) for all 3 years, suggesting a possible bias. The model tended to underpredict the number of poor school-age children in counties with large percentages of Hispanics and, to a lesser extent, in counties with large percentages of blacks. The model estimates also differed consistently from weighted CPS estimates for some categories of rural counties classified by economic type.

Evaluations of the School District Model

Evaluations of the school district estimates of poor school-age children in 1995 were constrained by lack of comparison data. An internal evaluation assessed the sampling variability of the 1990 census estimates,

[14]The current SAIPE model uses the population under age 18 as predictor variable w_3; the previous candidate model used the population under age 21. The revised formulation of this predictor variable improved the performance of the model for estimates of poor school-age children for counties categorized by percentage of group-quarters residents and population size.

used to form within-county shares of poor school-age children to apply to estimates from the county model for 1995 (see National Research Council, 2000c:Ch.7). For the census long-form estimates, the average coefficient of variation (the standard error of the estimate divided by the estimate) was 32 percent for all school districts, ranging from 64 percent for the one-sixth of districts with the smallest populations to 14 percent for the one-sixth of districts with the largest populations. For ratio-adjusted estimates, in which the long-form estimates of the proportions poor of school-age children were applied to short-form estimates of total school-age children, the average coefficient of variation was 30 percent for all school districts, a modest reduction from that for the long-form estimates. Even after ratio adjustment, the very high level of sampling variability in the census estimates for many small districts introduces a potentially high degree of error in the updated estimates for these districts. However, it is important to remember that small districts account for a small proportion of the nation's poor school-age children.

An external evaluation compared estimates of poor school-age children in 1989 from several shares models with 1990 census estimates.[15] All of the methods evaluated exhibited large differences from the census estimates–much larger than the differences of the SAIPE county model estimates from the 1990 census estimates (see National Research Council, 2000c:Ch.7). However, the shares method that was analogous to the Census Bureau's procedure for the 1995 school district estimates (which applied 1980 census school district shares of poor school-age children within counties to 1989 county model estimates) performed better than a method that assumed no change from 1980 to 1990 in the nationwide relative shares for school districts. The average absolute difference, relative to the average number of poor school-age children per district, was 22 percent for the school district estimates from the SAIPE county shares method, compared with 29 percent for the estimates from the stable shares method. The SAIPE county shares method also performed better than a shares method based on states instead of counties.

By population size, the SAIPE shares method performed reasonably well for districts with 40,000 or more people in 1990, which were 8 percent of districts and included 55 percent of poor children aged 5-17. It performed poorly for districts with 5,000 or fewer people in 1990, which were 47 percent of districts and included 8 percent of poor children aged 5-17. The greater sampling error in the 1990 census estimates for smaller dis-

[15]The evaluation file was restricted to school districts that were not coterminous with a county, that covered all grades, and that were the same between 1980 and 1990: 9,243 of the 15,226 districts in the 1990 census.

tricts accounted in part for the larger differences between the SAIPE shares method and the 1990 census estimates for small districts relative to large districts.

For New York and Indiana, a similar evaluation was conducted with the addition of two methods that formed within-county shares of poor school-age children for school districts in 1989 from (1) counts of students approved to receive free school lunches in 1990 and (2) counts of students approved to receive free or reduced-price school lunches in 1990. By comparison with 1990 census estimates for these states, the two methods that used contemporaneous school lunch data as the basis for within-county shares performed about the same as a method that used 1980 census within-county shares, with the shares in each case applied to 1990 census county estimates (see National Research Council, 2000c:App. D; Betson, 1999b).

Variance Estimation

The Census Bureau produces variance estimates for the numbers of poor school-age children for states and counties that are estimated from the state and county models. Essentially the same variance estimation procedure is used for the two sets of estimates. Table 3-1 shows illustrative state and county estimates (for Maryland) of poor school-age children, the associated 90 percent confidence intervals that are derived from the variance estimates, and the coefficients of variation. Note that the coefficients of variation for county estimates are similar across counties of all population sizes.

Both the state and county numbers of poor school-age children are estimated from weighted averages of model predictions and direct estimates (see equations (3.2) and (3.5)). For the state estimates, the weighted average is an estimate of the proportion poor of school-age children; for the county estimates, it is an estimate of the logarithm of the number of poor school age-children. In both cases the variance of the model prediction component of the weighted average is estimated from the regression model using maximum likelihood estimation. The variances of the state direct estimates are estimated from a generalized variance function that reflects the CPS sample design. The variances of the county direct estimates (for counties in the estimation data set) are estimated from the partition of the sampling variance estimated from the regression analysis (as described above, "County Model").

The estimated variances of the state and county weighted averages are then computed as a weighted combination of the estimated variances of the model predictions and of the direct estimates, where the weights are the squared values of the weights used in forming the averages. Since

TABLE 3-1 Illustrative SAIPE Estimates of Poor School-Age Children: 1995 State and County Estimates for the State of Maryland

Area	Estimate	90 Percent Confidence Interval	Coefficient of Variation (in percent)
Maryland	107,724	97,793-117,655	5.6
Baltimore City	40,170	31,489- 48,851	13.1
Prince George's County	12,735	9,978- 15,492	13.1
Baltimore County	9,657	7,600- 11,714	12.9
Montgomery County	9,249	7,263- 11,235	13.1
Anne Arundel County	5,571	4,363- 6,779	13.2
Harford County	2,984	2,328- 3,640	13.4
Washington County	2,916	2,288- 3,544	13.1
Allegany County	2,788	2,165- 3,411	13.6
Frederick County	2,303	1,794- 2,812	13.4
Wicomico County	2,456	1,923- 2,989	13.2
St. Mary's County	2,091	1,616- 2,566	13.8
Charles County	2,025	1,556- 2,494	14.1
Howard County	1,894	1,460- 2,328	13.9
Cecil County	1,743	1,355- 2,131	13.6
Carroll County	1,360	1,050- 1,670	13.3
Garrett County	1,256	970- 1,542	13.9
Dorchester County	1,096	850- 1,342	13.6
Worcester County	1,071	833- 1,309	13.5
Calvert County	1,025	795- 1,255	13.6
Somerset County	839	636- 1,042	14.7
Caroline County	810	630- 990	13.5
Queen Anne's County	697	533- 861	14.3
Talbot County	639	496- 782	13.6
Kent County	349	267- 431	14.3

NOTE: The 90 percent confidence interval is derived from the variance estimates developed by the Census Bureau as described in the text. It is the estimate of poor school-age children plus or minus 1.645 times the standard error (the square root of the variance estimate). The coefficient of variation is the standard error as a percent of the estimate.
SOURCE: Census Bureau's web site: www.census.gov/hhes/www/saipe.html.

the weights used in forming the averages are themselves sample estimates, the variances of the state and county weighted averages should also reflect the effect of the sampling error in the estimated weights. The methodology of Prasad and Rao (1990) could be applied for this purpose. However, in practice the weights for the state direct estimates are zero for all but one of the estimation years because the model error variance was estimated to be zero, and they are mostly zero for the county direct estimates because most counties had no CPS sample. The Census Bureau found the effect on the variance estimates resulting from the sampling error of the estimated weights to be negligible for the state averages and judged it to be also negligible for the county averages. Thus, no allowance was made for the sampling error in the estimated weights in estimating the variances of the state and county weighted averages.

The number of poor school-age children in a state is obtained by multiplying the weighted average of the proportion poor of school-age children in the state by the population estimate of the number of school-age children in the state. The state population estimates are subject to error, but this fact is ignored in calculating variance estimates for the state estimates of numbers of poor school-age children. Also, the state estimates of the numbers of poor school-age children are controlled to the national direct estimate of the number of poor school-age children from the CPS. The effect on the variances of the state estimates due to this adjustment was also determined to be negligible, and so was ignored.

The county weighted averages are logarithms of the numbers of poor school-age children. They are then transformed to estimated numbers using equation (3.6). The variances of the estimated numbers are obtained by assuming that the estimated logarithms of the numbers are normally distributed and then using the known relationship between the variance of the logarithms and the variance of the original observations in this situation. After the transformation to the numbers scale, the county estimates are controlled to state estimates of poor school-age children. The effect of this final step on the variance is complicated by the correlations between the county estimates and the state estimates. The linearization (Taylor-Series) method used to account for the effect of these state-level controls on the variances of the county estimates currently incorporates the state variances but ignores the correlation between a county estimate and the corresponding state estimate.

Estimation of the variances of the state and county estimates of poor school-age children depends heavily on the estimates of the model and sampling error variance components in the regression models. As discussed elsewhere in this chapter, these variance components are currently not well estimated for either the state or county model. Improvement in the estimation of these variance components is needed to improve the variance estimates of the state and county estimates.

OTHER SAIPE MODELS

This section describes other models in the SAIPE Program. However, unlike the models described above, the panel did not review these other models.

State-Level Models

In addition to the Title I estimates for poor children aged 5-17 who are related to and living in families (referred to as "poor school-age children" in this report), the Census Bureau develops state-level estimates for four population groups: (1) poor children under age 5; (2) all poor children aged 5-17 (a slightly larger population than poor related children aged 5-17);[16] (3) poor people aged 18-64; and (4) poor people aged 65 and over. The estimates for these four population groups are produced by using models that are similar to the state model for poor related children aged 5-17. The Census Bureau publishes state-level estimates for poor children under age 5; poor related children aged 5-17; poor people under age 18 (the sum of the estimates for groups (1) and (2) above); and total poor people (the sum of the estimates for groups (1)-(4) above). Estimates for poor people aged 18-64 are not published because users have not expressed a need for them. There is interest in state estimates for poor people aged 65 and over, but the SAIPE estimates are not published because Census Bureau evaluations showed that they were not markedly better than census estimates.

All of the state-level poverty models are of the same form as that described above for poor related children aged 5-17. In each case, the dependent variable is a poverty rate for the specified age range, and the regression model is of the form displayed in equation (3.1). As can be seen in Table 3-2, predictor variables for the models for poor under age 5, poor aged 5-17, and poor aged 18-64 are broadly similar, differing only in the age ranges included, but the model for poor aged 65 and over has some different predictors. The models are used to produce model predictions ($\hat{y}_j$) for each of the states, and these predictions are then combined with the state direct estimates (y_j) by means of a weighted average as given in equation (3.2). The resulting weighted estimates are then converted from poverty rates to numbers of poor and ratio adjusted to national CPS estimates by applying equation (3.3) for the specified age group.

The Census Bureau also produces indirect state estimates of median household income. In this case, the regression model uses the state's March CPS median household income for the reference year as the dependent variable and has two predictor variables: median household income from the most recent decennial census and an estimate of median house-

[16]The models for poor related children aged 5-17 and all poor children aged 5-17 differ only in the dependent variable. The reason for the model for poor related children aged 5-17 is to satisfy the requirements of the Title I legislation: it is this model that is described above and that the panel has reviewed (see Ch. 1:fn.2 for a definition of related children).

hold income for the reference year derived from census and tax return data. The estimate of median household income for the reference year is obtained by computing the ratio of a state's median household income in the reference year to that in the census year from tax return data, then applying this ratio to the state's median household income estimate from the census. The regression equation is used to produce a regression prediction of median household income for each state; the final state estimate is produced as a weighted average of its regression prediction and its direct estimate.[17]

County-Level Models

The SAIPE Program produces indirect county-level estimates for poor people under age 18, poor (related) children aged 5-17, and total poor. The methodology for producing the estimates for poor people under age 18 and total poor is essentially the same as that described earlier for poor children aged 5-17. In each case the dependent variable is the logarithm of a 3-year average of county-level observations, and the predictor variables are obtained from census, food stamp, and IRS data and also placed on the logarithmic scale. The predictor variables for the three models differ only in the age ranges covered, as displayed in Table 3-3. The regression model for each age range, given by equation (3.4), is fitted by maximum likelihood estimation. A regression prediction of the logarithm of the number poor is produced from the regression equation, and a weighted average of this prediction and the direct estimate of the logarithm of the number poor (if available) is computed with weights given by equation (3.5). Finally, the logarithms are transformed back to the numbers of poor using equation (3.6), and the county estimates of numbers of poor are ratio adjusted to sum to the state estimates using equation (3.7).

The SAIPE Program also produces estimates of median household income at the county level. The regression model uses the 3-year average of median household income from the March CPS (not transformed to logarithms) as the dependent variable and six predictor variables: median adjusted gross income from tax returns; the ratio of the number of dependent tax returns to the total number of returns; the logarithm of the proportion of the Bureau of Economic Analysis (BEA) estimate of total personal income derived from government transfers; the previous census estimate of median household income; the ratio of the BEA estimated per

[17]See the Census Bureau's web site for information on the state poverty and median household income models: http://www.census.gov/hhes/www/saipe.html.

TABLE 3-2 Predictor Variables for SAIPE State Models of Poor People of Various Ages

	Dependent Variable (from 1 year of March CPS)	
Predictor Variable	Poor Under Age 5	Poor Aged 5-17
x_1	Proportion of exemptions under age 65 reported by families in poverty on tax returns	Proportion of child exemptions reported by families in poverty on tax returns
x_2	Proportion of people receiving food stamps	Same as under age 5
x_3	Proportion of people under age 65 who were not included on an income tax return	Same as under age 5
x_4	Residual from a regression of the proportion of poor children under age 5 from the most recent decennial census on the other three predictor variables for the census income year	Residual from a regression of the proportion of poor children aged 5-17 from the most recent decennial census on the other three predictor variables for the census income year

NOTE: All variables are at the state level.

capita total personal income for the reference year to the BEA estimate corresponding to the time period covered by the previous census; and the product of the two previous predictor variables (census-based median household income and the BEA ratio). The final county estimate of median household income is produced as a weighted average of the regression prediction and the direct estimate.[18]

POPULATION ESTIMATES

The SAIPE Program uses total population estimates and estimates for particular age groups as predictor variables in the state and county models. Such estimates are also needed to accompany the SAIPE poverty

[18]See the Census Bureau's web site for information on the county poverty and median household income models: http://www.census.gov/hhes/www/saipe.html.

Poor Aged 18-64	Poor Aged 65 and Over
Same as under age 5	Proportion of exemptions aged 65 and over reported by families in poverty on tax returns
Same as under age 5	Proportion of people receiving Supplemental Security Income benefits
Same as under age 5	Proportion of people aged 65 and over who were not included on an income tax return
Residual from a regression of the proportion of poor people aged 18-64 from the most recent decennial census on the other three predictor variables for the census income year	Proportion poor of people aged 65 and over from the most recent decennial census

estimates for use in fund allocation programs. For example, Title I requires estimates of the total number of school-age children to convert SAIPE estimates of the numbers of poor school-age children for counties and school districts to poverty rates.

The Census Bureau has an extensive and long-standing program to produce small-area population estimates by using the previous census updated with administrative records. The extent of geographic and demographic detail provided by the estimates program has expanded since it first began producing U.S. population estimates in the early 1900s and state population estimates in the 1940s. The Bureau currently produces estimates of total population by single years of age, sex, race, and Hispanic origin, monthly for the United States and annually for states and counties. Every 2 years, the Bureau also produces estimates of total population for incorporated places and, in selected states, county subdivisions.

TABLE 3-3 Predictor Variables for SAIPE County Models of Poor People of Various Ages

Predictor Variables	Dependent Variable (from 3-Year Average of March CPS): Poor Under Age 18	Poor Aged 5-17	All Poor People
w_1	Log number of child exemptions reported by families in poverty on tax returns	Same as poor under age 18	Log number of exemptions of all ages reported by families in poverty on tax returns
w_2	Log number of people receiving food stamps	Same as poor under age 18	Same as poor under age 18
w_3	Log estimated population under age 18	Same as poor under age 18	Log estimated total population
w_4	Log number of child exemptions on tax returns	Same as poor under age 18	Log number of exemptions of all ages on tax returns
w_5	Log number of poor under age 18 in previous census	Log number of poor related children aged 5-17 in previous census	Log total number of poor in previous census

The Bureau also recently began producing biennial estimates of total population and children aged 5-17 for school districts.

Over the years, the Census Bureau has made advances in estimation methods and in gaining access to and incorporating new sources of administrative records data that relate to population change. The currently used methods for estimating total population and population by age are briefly summarized below (for more detail, see National Research Council, 2000c:Ch. 8; see also U.S. Census Bureau, 1995; Long, 1993; Sink, 1996).

Methodology

Total Population

Total population estimates for the United States are developed by the component method of demographic analysis, in which the population from the previous census is updated by adding births and international immigration and subtracting deaths and emigration.[19] State estimates of total population are the sum of independently developed county estimates that are constrained to sum to the national estimate.

The county estimates of total population are also developed by the component method: the numbers of births and deaths are based on reported birth and death statistics for each county; reports of the Immigration and Naturalization Service are used to estimate net legal immigration from abroad; reports of the Department of Defense and Office of Personnel Management are used to estimate net movement of federal personnel in and out of the country; and administrative records are used to estimate net migration among counties. Net migration of people under age 65 is estimated for each county from a year-to-year match of IRS federal income tax returns; for people aged 65 and over, net migration is estimated for each county from the change in Medicare enrollment. Estimates are developed separately for household and group quarters populations. Each of the various administrative record sources used for county population estimates requires processing and editing, often based on assumptions, to allocate the data to counties as accurately as possible.

For school districts, total population estimates are currently developed by a shares method. In this approach, 1990 census within-county

[19]The methodology for national-level population estimates includes an "inflation-deflation" procedure in which census estimates for age groups are adjusted for net undercount as estimated from demographic analysis. The adjusted estimates are then updated for births, deaths, immigration, and emigration. As a last step, the estimates are readjusted to match the census-based age distribution.

shares of the county population for school districts (or component parts) are applied to the updated county total population estimates. The shares method necessarily assumes that each school district in a county added (or lost) population following the census in the same proportion as the county as a whole.

Population by Age

State estimates for single years of age, controlled to state total population estimates, are developed by a cohort-component method in which migration rates for the school-age population are derived from school enrollment data. In turn, these rates are used to estimate migration rates for other age groups under age 65.

Recently, the Census Bureau developed experimental state estimates of the population by age, sex, race, and Hispanic origin by a cohort-component method in which federal income tax return data are used to estimate net migration on the basis of estimates of gross inmigration and gross outmigration.[20] This procedure for estimating migration is applied to taxfilers and their dependents when the primary taxfiler's social security number matches to a 20-percent sample of the Social Security Administration's Numident file. The demographic characteristics of the primary taxfiler are obtained from the Numident file, the spouse and dependents are assigned the same race and Hispanic origin as the primary taxfiler, and age is assigned by a set of rules (e.g., all child dependents are assumed to be under age 20). For this experimental method, the resulting state age-sex-race-Hispanic origin estimates are controlled to the state age-sex population estimates developed as first described.

County estimates for single years of age are developed from a raking-ratio adjustment of the estimates from the previous census. The initial matrix of counts for each county by age, sex, race, and Hispanic origin from the previous census is adjusted to match simultaneously the postcensal estimate of the total county population and the postcensal estimates for the applicable state by age, sex, race, and Hispanic origin. This ratio-raking procedure is applied separately for people in group quarters and people not in group quarters under the assumption that the age distribution of each county within a state changes in the same manner as that state's age distribution.

School district estimates for children aged 5-17 are developed from a shares approach, similar to that described for total population estimates

[20]See the Census Bureau's web site: http://www.census.gov/population/estimates/state.html.

for school districts. Because school district boundaries change, it is necessary in estimating numbers of school-age children (and total population) for school districts to obtain updated boundaries for the reference year and to retabulate the 1990 census within-county shares according to the new boundaries.

Evaluations

Repeated evaluations of the accuracy of the population estimates, conducted by comparing estimates developed from the previous census to counts from the current census, show several patterns. The proportional differences of the estimates in comparison with the census are larger on average for small areas than for large ones; the proportional differences tend to be larger for areas in which the population is changing rapidly than for areas that are more stable; and the proportional differences for age groups tend to be higher than those for the total population. Furthermore, estimates produced by using components of population change are usually more accurate than those produced by such methods as the raking-ratio adjustment (used for county age estimates) or the shares method (used to produce school district estimates).

Evaluations of 1990 population estimates for counties and school districts show that, for the total population, the average absolute difference between the 1990 population estimates based on updating the 1980 census values and the 1990 census counts was 2.3 percent of the average population for counties and 9.6 percent of the average population for school districts. For all children aged 5-17, the average absolute difference between the 1990 population estimates and the 1990 census counts was 4.9 percent of the average number of school-age children for counties and 12.0 percent of the average number of school-age children for school districts. These differences are much smaller than the average absolute difference for poor children aged 5-17, which was 10.7 percent of the average number of poor school-age children for counties and 22.2 percent of the average number of poor school-age children for school districts (National Research Council, 2000c:Ch.7; see fn. 12 above for the average absolute difference formula).[21] It will be important to repeat these evaluations using 2000 census data.

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[21]A difference between the comparisons of population estimates and those of poverty estimates is that the census comparison estimates for poor school-age children are from the long-form sample and, hence, are subject to error from sampling variability. This error results in an overestimate of the difference between the SAIPE poverty estimates and the census poverty numbers that would be obtained from a complete enumeration.

An additional evaluation found that use of population estimates instead of census counts had only a modest effect on the accuracy of the estimated numbers of poor school-age children for counties. The analysis compared 1990 census estimates of poor school-age children in 1989 with 1989 estimates from two variants of the SAIPE county model. Each variant predicted the log poverty rate for school-age children; one variant converted estimated poverty rates to estimated numbers of poor school-age children by using 1980 census-based population estimates for school-age children for 1990; the other variant converted rates to numbers by using 1990 census population counts. The average absolute difference between the model-based estimates of poor school-age children and the 1990 census estimates was only slightly higher for the first variant than for the second variant (see National Research Council, 2000c:App.C).

PRIORITIES FOR SAIPE MODEL DEVELOPMENT

Evaluations of the SAIPE estimates indicate that, although the estimates are generally better than the available alternatives for states and counties and at least as good as the available alternatives for school districts, they are subject to appreciable levels of error, particularly for small counties and school districts. Thus, efforts to improve the accuracy of the estimates for such purposes as fund allocations are well warranted. In addition, since there is currently a 3- to 4-year lag between the production of the estimates and the year to which they relate, it is highly desirable to seek ways to improve the timeliness of the estimates. This section describes some research priorities for improving the accuracy and timeliness of the state, county, and school district estimates, which the panel believes could be implemented in the next estimation cycle.

Research and development for the population estimates is heavily dependent on enhancements to administrative records. Possible improvements to these estimates are discussed in Chapter 5, which deals with such enhancements.

Research Priorities for the State and County Models

The focus of this discussion is on research activities that should be undertaken in an attempt to improve the SAIPE state and county estimates in the near term. The following areas for research and development are discussed below: the incorporation of state random effects in the county model; the incorporation of counties with CPS households but with no sampled poor school-age children in the county modeling; the possible use of time-series and multivariate models; and improved estimation of the components of variance in both the state and county models.

However, before turning to those activities, the panel offers a broader perspective on the SAIPE Program. The program produces a variety of different estimates (e.g., numbers in poverty in different age bands) at different levels (states, counties, and school districts). Currently, these estimates are produced somewhat independently of one another, and the state and county models are formulated differently in a number of respects. From a theoretical perspective, a preferred approach would be to use a single integrated hierarchical model that would produce all the estimates at both the state and county levels. This approach would not only ensure consistency for the estimates, but it would also likely improve their precision, in part because the estimates for one age band would be able to "borrow strength" from the data available for another age band through the use of a multivariate model.

A further extension of this approach would be to incorporate data for other time periods in the model. For example, sample data are available from the March CPS every year, and data from prior years can provide valuable information in predicting the values for the current year. The same will also be true for the American Community Survey after 2003, if it is implemented as currently planned.

Although such an overarching model may be attractive from a theoretical perspective, its full implementation is almost certainly impracticable, at least in the near term. Nonetheless, the panel considers that it would be useful for the Census Bureau to keep such a model in mind as it develops its longer term plans for the SAIPE program. Even if the single overall model cannot be achieved, model enhancements that move the estimation procedures closer to the ideal may be possible and should be pursued.

Incorporation of State Random Effects in the County Model

State estimates obtained from the county model by aggregating the county estimates within each state are made to conform to the state estimates from the state model by a ratio adjustment, the state raking factor. As noted above, these raking factors vary considerably across states. Several sources could contribute to this variability, including the different measurement scales used in the state and county models (proportions for the former, logarithms of numbers for the latter), the use of 3-year averages of CPS estimates as the dependent variable in the county model versus single-year estimates in the state model, sampling variability, and, possibly, individual state effects that are not captured in the county model. Preliminary work by the panel suggests that a sizable proportion of the variation in the state raking factors is due to sampling variability. Further investigation should be carried out to better understand the causes of this variation.

In an effort to determine whether the state raking factors could reflect state effects that are missing from the county model, the Census Bureau examined a county regression model that included fixed state effects. The use of this model did not reduce the spread of the raking factors; rather, it increased it. Also, while the addition of fixed state effects reduced some nonrandom residual patterns in the regression output, a fixed state effects model estimated for 1989 did not perform better than other models in comparison with 1990 census estimates.

An alternative approach for incorporating state effects in the county model is to treat them as random rather than fixed effects. This formulation leads to a nested model in which the model error is the sum of a county-within-state random effect and a state random effect. Fuller and Goyeneche (1998) describe the model and report on a preliminary evaluation of it. Their evaluation suggests the presence of a small state random effect. The Census Bureau should conduct a thorough evaluation of this model to examine all of its properties.

Including Counties with No Poor Sampled School-Age Children

As described above, the current county model is expressed in terms of logarithmic transformations of the 3-year average numbers of poor school-age children (the dependent variable) and the values of the predictor variables. Although this form of transformation makes the distributions of the variables more symmetric, possibly makes the functional relationship between the dependent variable and the predictor variables more linear, and provides reasonably homogeneous error variances, it has the disadvantage of not accommodating zero input values. Thus, counties with some CPS-sampled households but no CPS school-age children living in poverty in the 3-year average are excluded from the estimation of the regression coefficients in the county model. A large number of CPS counties are excluded from the regression data set for this reason: 304 of 1,488 counties for the 1993 model and 262 of 1,247 counties for the 1995 model.[22] Although the model estimates the numbers of poor school-age children in these excluded counties relatively well (see National Research Council, 2000c:Ch.6), dropping such a large fraction of counties dimin-

[22]In addition, a small number of counties with CPS sampled households (41 for the 1993 model and 27 for the 1995 model) are excluded from the regression data set because the sampled households lacked any school-age children.

ishes the model's face validity and produces estimates with higher variability than if these counties were included.

One solution to this problem is to shift the starting point of the logarithmic transformation (i.e., using log $(z + c)$, $c > 0$) to allow inclusion of all counties that have sampled households in the CPS or to use some other form of transformation. A preferable, but less straightforward, solution is to use generalized linear modeling (see McCullagh and Nelder, 1989), an approach that has been developed to provide models for variables with a wide variety of distributional forms. In this particular case, the Poisson distribution is a natural one to consider, since data on counts–for which zero is a natural observation–are typically modeled well using this distribution. Applying the generalized linear modeling framework, all counties included in the CPS can be used to estimate the regression coefficients, and best linear unbiased predictors (BLUPs) can be used to combine the model and direct estimates.

While the application of generalized linear modeling is fairly routine in many applications, the complex sample design of the CPS must be taken into account in the estimation of the regression coefficients and in estimating the variances of the model predictions. Recent developments in generalized linear mixed models (e.g., Robinson, 1991; Zeger and Karim, 1991) provide the basis for developing approaches that can reflect the sampling design.

The Census Bureau has recently conducted research on a hierarchical Bayesian modeling approach that makes it possible to include counties in the model that have some sampled CPS households but none with poor school-age children (see Fisher and Asher, 1999b). This work should continue.

Time-Series and Multivariate Modeling

As noted above, a unified overall model that provides all the SAIPE estimates and that incorporates data from other time periods is theoretically attractive, but not practical, at least in the immediate future. However, there are possibilities for using multivariate and time-series approaches in more limited ways. The panel recommends that the Census Bureau continue and expand its research in these areas.

Fay (1987) provides an early example of a multivariate approach, applied to the estimation of median income in four-person families by state. The dependent variables in his trivariate model were the state median incomes of four-person, three-person, and five-person families. In estimating the median income for four-person families, the model borrows strength from the regressions for the other two dependent variables by allowing for a correlation of the model errors in the regressions. This kind of approach could, for instance, be applied in SAIPE in an attempt to improve the estimates of poor children aged 5-17 by incorporating estimates for other age ranges in the state and county models.

Bell (1997a) applied a bivariate model for the county estimates of poor school-age children in which the two dependent variables were the 3-year average of CPS data for the reference year (described above) and the 1990 census estimate. The purpose of this model was to make more complete use of census data, through a correlation of the model errors for the two regressions. The panel evaluated several versions of the bivariate model for 1993 estimates, and the results were promising (National Research Council, 2000c:App.B). These models were not pursued for use at that time, primarily because it was not possible to conduct external evaluations of them. However, they have the potential to improve the county estimates, and further research on their application in SAIPE should be conducted.

The above approach could also be generalized to a time-series structure. Census Bureau staff have begun work on assessing the potential benefits of using multiple years of CPS data in the state model but have not yet completed their analyses.

Multivariate and time-series approaches will become increasingly important as data from new sources–such as data from several years of the American Community Survey–become available. The Census Bureau should pursue work on these types of models, which will need extensive development and evaluation to see if they have advantages and to ensure that they do not introduce unanticipated problems. In the longer term, it may be possible to adapt time-series approaches to develop forecasts of income and poverty in order to make the estimates more timely for program use (see "Improving Timeliness" below for approaches to improve timeliness in the near term).

Improved Estimation of Variance Components

Both the state and county models have two variance components, model error and sampling error. Model error is assumed to be independently and identically distributed across areas (states or counties). Sampling error depends on the CPS sample size and poverty rate in the area, as well as the complex stratified multistage CPS sample design. Estimates of these variance components are needed for three purposes: they are used in the maximum likelihood estimation of the regression coefficients in the models; they are used in computing the standard errors of the state and county estimates; and they are used to determine the weights for forming the weighted averages of the model estimates and direct estimates in equations (3.2) and (3.5). The last purpose is most important for the state estimates since, unlike most counties, all states have CPS samples of sufficient size to produce direct estimates that can usefully contribute to the weighted average.

Different approaches are used to estimate the two variance components in the state and county models. In the state model, sampling error variance is estimated by using a generalized variance function (GVF) that reflects the effects of the CPS sample design, and the model error variance is then obtained through maximum likelihood estimation, essentially subtracting the total sampling error variance from the total variance. In the county model, the model error variance is equated to the model error variance in a corresponding regression model for 1990 census data; that model error variance is estimated in the manner described for the state model error variance, with the census sampling error being estimated with a GVF for the census long-form sample. The total sampling error variance in the county model is then obtained by maximum-likelihood estimation and partitioned among counties in inverse proportion to CPS sample size. Both of these approaches are problematic, and further research is needed for both models.

In the case of the state model, the maximum-likelihood estimation has led to zero estimates of model error variance in 6 of the 7 years for which the state model was estimated, with the consequence that the direct estimates are assigned zero weight in the weighted averages. The untenable result of a zero model error variance likely derives from a misspecification of the GVF for the CPS that results in overestimation of the sampling error variance.

Research is needed to improve the estimation of the sampling error variance for the state model. The use of a Bayesian model to account for the uncertainty in the estimates of the model error variance is another approach that should be pursued. Bell (1999) has explored such a model, which yields positive estimates of model error variance that could be useful for producing the state model estimates. Pending the outcome of these two areas of research, some simple adjustments should be examined and applied as appropriate. For example, minimum weights that are a function of the CPS sample size in each state could be assigned to the direct estimates for each state.

For the estimation of the variance components in the county model, reliance on the assumption that the model error variance for the CPS equation is the same as that for the 1990 census equation is questionable. An alternative approach is that used with the state model, that is, estimating the sampling error variance from a GVF and obtaining the model error variance by maximum likelihood estimation. The Census Bureau has examined an empirically based GVF in which sampling error variance of the county direct estimates is inversely proportional to the square root of CPS sample size. This approach improves upon the current method (see Fisher and Asher, 1999a), but more research is needed. An alternative approach that should also be explored is to estimate a within-county design effect based on counties with reasonable numbers of CPS sample segments. This design effect could then be used to develop a GVF from which sampling errors could be estimated for all counties with some CPS sample.

A complication that arises in modeling GVFs for the direct county estimates is that the sampling errors of these estimates are affected not only by the clustered CPS sample within counties, but also by the poverty rates in those counties, rates that can be estimated only imprecisely. Future research should consider alternative methods of estimating county poverty rates for use in the GVFs, including smoothing the estimates in some manner.

Reducing the Variability in the 1990 Census School District Estimates

Essentially, the school district model distributes the updated county estimate of the number of poor school-age children between the school districts (or parts of school districts) in the county in proportion to the estimated shares that the districts (or parts) had of the county's poor school-age children at the last census (see "School District Procedure" above). The census numbers of poor school-age children in the school districts are estimated from the census long form. Since these estimated numbers are based on small long-form sample sizes for many school districts, they are subject to substantial sampling error (see National Research Council, 2000c:Ch.7).

To improve the precision of census long-form estimates, the Census Bureau builds in adjustments as part of regular census data processing to make long-form totals conform to short-form totals for key short-form items for weighting areas (subcounty areas or sometimes entire counties that have a specified minimum number of sample persons). For the purpose of estimating school district shares, the Census Bureau extended this approach by forcing the long-form estimate of the number of school-age children in each school district to conform to the short-form number of such children. In essence, the procedure estimated the proportion poor of school-age children in a district from the long form and then applied that proportion to the short-form number of school-age children in the district.

This adjustment improved the precision of the school district census estimates of poor school-age children by a small, but important, amount. Further improvements might be obtained by extending the adjustment to forcing long- and short-form totals to agree on characteristics that are related to poverty, such as race, ethnicity, home tenure (owner, renter), family type, and type of residential area (central city, urban, rural), at the school district level. Although only a modest improvement in the school district census estimates may be achieved with these further adjustments, any improvement would be helpful.

Another approach for improving the census school district estimates is to use a smoothing procedure to reduce the sampling errors in the long-form estimates of the proportions poor of school-age children. These smoothed proportions would then be multiplied by the short-form numbers of school-age children to produce the census estimates of numbers of poor school-age children. Thus, for example, a school district's proportion poor could be estimated by a weighted average of its estimated proportion poor from the long form and the overall proportion poor for the county in which it is located, with the weight given to the long-form estimate depending on the school district's long-form sample size. This procedure, which reduces sampling error at the cost of potentially introducing some bias, is likely to be effective for school districts (or parts of districts) that have small long-form samples.

Improving Timeliness

The Census Bureau currently produces income and poverty estimates from the SAIPE Program with a lag of about 3 years. So the school district estimates of school-age children in 1996 who were in poverty in 1995 were released in early 1999 for use in Title I allocations for the 1999-2000 and 2000-2001 school years. Although these estimates are considerably more current than estimates based on the 1990 census, they are still out of date by 3 or 4 years. Since there can be substantial changes in income and poverty in short time periods (see National Research Council, 2000c:Ch.3), it is important to explore methods for reducing this time lag.

One reason for the time lag for SAIPE poverty estimates is the length of time it takes to obtain population estimates for use in the state and county models. The population estimates are not available until more than 2 years after the income reference year.[23] A different approach would be to use the population estimates for July of the income reference year rather than the population estimates for July of the following year. This approach would have the advantage of reducing the time lag of the poverty estimates. Alternatively, population estimates could perhaps be developed for January of the year following the income reference year, which would be more timely than the estimates for July of the following

[23]Preliminary estimates are available a year earlier (e.g., spring 1999 for July 1998 estimates), but evaluation has shown that they may differ from the second round of estimates by as much as 3 percent for state estimates and more than 5 percent for county estimates.

year and yet would reflect the CPS concept of measuring poverty for the previous calendar year.

Another source of delay for the SAIPE poverty estimates is the lag in obtaining the food stamp data used in the county model. Monthly food stamp counts for states are available with little delay from the U.S. Department of Agriculture, so the state model uses a 12-month average of food stamp data, centered on January 1 following the income reference year, as a predictor variable. The delay results from the construction of the food stamp predictor for the county model. That predictor makes use of county-level food stamp counts for July of the income reference year (for some counties, the data are the average of the monthly counts for the year), which take much longer to obtain than the state totals. In some instances, the counts must be collected from individual states, and the complete data set is not usually available until 2 years after its reference date. The food stamp predictor in the county model is then formed by raking the county counts to the slightly more current state numbers used in the state model.

In the interest of timeliness, a study should be carried out to investigate the effects of basing the food stamp predictor in the county model on counts from an earlier period, such as data for July of the year prior to the income reference year. Even though the county-level data for July are raked to the state food stamp numbers for the reference year, the use of earlier data for counties may affect the performance of the food stamp predictor variable in the county model. The recommended study should evaluate the extent of any such effects.

Yet another issue that should be examined is the year of the state estimates to which the county estimates are raked. The current practice is to rake the county estimates to state estimates for the middle year of the 3 years of CPS data that are used for the dependent variable in the county model. An alternative approach would be to rake the county estimates to state estimates for the most recent of the 3 years. In effect, such raking would update the county, and hence the school district, estimates by 1 year under a modeling assumption about the uniformity of the distribution of the temporal changes in poverty across counties within states. This assumption only has to be approximately correct for this procedure to provide a benefit. Another possible approach–that could be combined with raking the state estimates to the latest year–would be to construct the dependent variable in the county model as a weighted average of the 3-year CPS estimates that gives more weight to the most recent year.

CONCLUSION

The panel commends the Census Bureau for investigating several of the research topics the panel identified for the current SAIPE state and county models. Work on technical aspects of the models and on the timeliness of the estimates is important in the near term. Also important is work on the role that new data sources could play in improving the state and county income and poverty estimates and the estimates of poor school-age children for school districts. We discuss data sources in the next two chapters.

4

Future Model Development: The Role of Surveys

USER OVERVIEW

Evaluation studies of the Census Bureau's estimates of poor school-age children, produced as part of its Small Area Income and Poverty Estimates (SAIPE) Program, have established that the updated estimates are more accurate than outdated estimates from the decennial census (see Chapter 3). However, these same studies have also highlighted a need for further improvement in the estimates, particularly for subcounty areas. Research and development of the state and county models, as recommended by the panel, can help. However, marked improvement in the SAIPE estimates, particularly for school districts or other very small areas, will require new data sources. Possible new sources of household survey data, discussed in this chapter, may support significant improvements in the quality of the estimates in the next decade and beyond. (Improved administrative records data that may also play an important role are discussed in Chapter 5.)

Estimates from the SAIPE Program now reflect the income and poverty measurements in the Current Population Survey (CPS) March Income Supplement, which asks each March about the previous year's income for a sample of about 50,000 households. The state and county models are tied to the CPS in that the dependent variable in the regressions–the variable being predicted–is from 1-year CPS estimates in the state model and from 3-year average CPS estimates in the county model. Other data sources, including the 1990 census and administrative records, provide predictor variables in the models, but the goal is to predict CPS-measured income and poverty. The school district model is tied to the CPS as well: 1990 census shares or proportions of poor school-age children for school districts within counties are applied to updated estimates from the CPS-based county model.

The use of the CPS as the dependent variable in the SAIPE models reflects a shift from the previous standard of measurement for many uses of small-area income and poverty estimates (e.g., allocating Title I funds), which was the decennial census long-form survey. The definitions of income and poverty are the same in the census and CPS, in that both use the official concept of income (before-tax money income for a calendar year), the official poverty thresholds for different size and type families, and the official unit of measurement (families and unrelated individuals as defined by the Census Bureau). However, differences in data collection procedures and other aspects of the two surveys result in somewhat different measurements. For example, the 1990 census estimate of U.S. median household income (for 1989) was 4 percent higher than the corresponding estimate from the March 1990 CPS, continuing a pattern from previous censuses (see Citro, 1996). Similarly, the 1990 census estimate of the proportion of U.S. poor school-age children was 6 percent lower than the corresponding March 1990 CPS estimate (National Research Council, 2000c:Ch.3).

The CPS is currently the source of official annual income and poverty statistics, and it has several advantages over the decennial census for that purpose. It is conducted more frequently than the census and so permits more regular updating of estimates. Also, the CPS is believed to provide more accurate measures of poverty and income than the census, primarily because it asks more questions about income and is conducted by personal and telephone interviewing instead of mailout/mailback techniques.[1] A main drawback of the CPS, which the regression modeling procedure is intended to address, is the small size of the sample compared to the census long-form sample. This small sample size, together with the clustering of the CPS sample design, results in sizable sampling variability of the CPS state estimates and a lack of any sample in most counties and school districts.

Looking to the future, several household surveys could contribute to improved estimates from the SAIPE program, and, in addition, the sample size of the March CPS itself may increase. These surveys are:

[1]In the evaluations of the SAIPE estimates of poor school-age children, the 1990 census was used as a standard of comparison for SAIPE estimates produced for 1989 because of a lack of other sources for external evaluation (see Chapter 3). However, this use does not make the census a "better" standard of measurement than the CPS.

- The 2000 census long form, which will provide small-area estimates of income and poverty for 1999 from a sample of about 18 million housing units (about one-sixth of total housing units, similar to the 1990 census long-form sample size);
- The American Community Survey (ACS), which is currently under development and contains content similar to the census long form (see Chapter 2); and
- The Survey of Income and Program Participation (SIPP), which plans to start a new panel in 2001 (see Chapter 2).

In the remainder of this chapter, we first compare the major features of the 2000 census long-form survey, ACS, March CPS, and SIPP. We then consider alternative uses for these surveys in the SAIPE Program, including: direct estimates for some areas; estimates to use as dependent variables in models; estimates to use as predictor variables in models; estimates for smaller areas of their shares or proportions of the poor population in larger areas; and estimates for controlling or calibrating other estimates on selected characteristics. The chapter ends with a summary of the panel's conclusions and recommendations on these uses.

To evaluate which uses would be feasible and desirable for one or more of the surveys, we focus on the reliability of survey estimates in terms of their error due to sampling variability; how frequently survey data are available and on what time schedule; and the quality of survey income measurements and how they compare with CPS measurements. Comparability is particularly important if another survey (e.g., the ACS) is to provide the basis for the dependent variables in the SAIPE models in place of the CPS. Depending on the extent of comparability, such a change could alter the standard of measurement and have unintended consequences for the use of estimates in formula allocations (see Chapter 6). However, using another survey for this purpose would be warranted if the change is judged likely to significantly improve the estimates.

Because no survey can provide direct estimates of sufficient reliability, timeliness, and quality to replace all of the SAIPE estimates, the panel concludes that SAIPE must continue to rely primarily on models for updated estimates for small areas. To determine how SAIPE models can best use the income and poverty data from surveys, the Census Bureau will need to learn more about measurement differences among them. To this end, the panel recommends exact matches and other comparisons of the CPS, ACS, and SIPP with the 2000 census records.

If it is implemented as planned, the ACS will provide subnational estimates that are available as frequently as estimates from the March CPS and are more reliable than those estimates. For states, the ACS estimates, averaged over a year, will be sufficiently reliable that they could be used directly for SAIPE. For most smaller areas, the ACS estimates will not be sufficiently reliable to be used directly, even when averaged over several years, but they could be used in models. For the SAIPE county models, the panel recommends that the Census Bureau begin research and development now to explore the use of ACS estimates either to provide one of the predictor variables in CPS-based models or to serve as the dependent variable in the models.

The Census Bureau should also conduct research on using ACS estimates, in place of or possibly combined with estimates from the previous census, to form within-county shares or proportions for school districts and other subcounty areas to apply to updated county model poverty estimates. The shares approach for subcounty estimates is necessary until such time as appropriate administrative data are developed for subcounty areas that can support a statistical model similar to the state and county models.

If the ACS is to play a major role in the SAIPE Program along the lines suggested by the panel, the survey needs to have consistent levels of funding over the next decade that are sufficient for the planned sample sizes. Insufficient funding would likely lead to reduced sample sizes and other discontinuities in the data that could jeopardize the usefulness of the ACS for SAIPE and, more generally, make it difficult to assess the potential of ACS data for small-area estimation.

The panel sees a continuing role for indirect use of the census long-form estimates in the SAIPE Program. The Census Bureau should plan to use 2000 census estimates as predictor variables in the current SAIPE state and county models. The role of the 2000 census direct estimates is less clear. These estimates will be quite reliable for states, many counties, and some smaller areas and will have face validity with users. However, to use these estimates as the SAIPE estimates (for 1999) could result in inconsistencies in the time series of estimates. Also, 2000 census long-form estimates will be unreliable for many school districts and other small areas, and the estimates may not be available in time to meet the Census Bureau's current production schedule, which calls for 1999 SAIPE estimates to be released in fall 2002. The panel recommends that the Census Bureau review alternative approaches for the 1999 SAIPE estimates with key users, so that the Bureau's decisions about whether and how to use the 2000 census direct estimates for 1999 are well understood.

Finally, work is under way at the Census Bureau on experimental measures of poverty, based on the report of a National Research Council panel (1995a), which recommended revising the poverty threshold concept and the definition of family income and using income estimates from SIPP, which is believed to obtain better measures of income and poverty than the CPS. Should the Census Bureau decide to use SIPP for official poverty statistics based on a revised concept (changes in the SIPP design and more timely data processing would be needed to make this feasible), then it would be important to consider how to adjust SAIPE estimates to agree with SIPP totals for selected characteristics, such as age, race, and geographic region.

The panel has outlined an ambitious program of research and development for the Census Bureau to determine the best uses of household survey data for SAIPE models. Such a program may be quite costly, and the Census Bureau will need to monitor progress carefully to try to identify the most promising approaches on which to focus scarce resources. Offsetting the costs is that many of the activities recommended–such as exact matches of survey and census records–will be helpful for many other uses of household survey data, in addition to SAIPE.

SURVEY FEATURES

This section describes the main features of the 2000 census long-form sample, ACS, March CPS, and SIPP, including content, sample size and design, data collection schedule and procedures, residence rules, response rates and other quality measures, and data processing and release. Table 4-1 summarizes the key features of each survey.

2000 Census Long Form

The 2000 census, like every census since 1960, included a long-form questionnaire that was administered to a sample of households. The long form contains the short-form questions that are asked of all households and additional questions. The added questions include total income and income by type from seven different sources (e.g., wages, Social Security) for the previous calendar year for each household member aged 15 or older. Both the short-form and long-form census questions are mandatory.

Design

The sample design for the 2000 census long form was somewhat modified from that used in the 1990 census. In 1990 the overall sampling rate was about 1 in 6, producing a sample of about 15.7 million occupied housing units. Variable sampling rates were used to provide somewhat more reliable estimates for small areas and to decrease respondent burden in more densely populated areas. Specifically, the sampling rate was 1 in 2 housing units for governmental areas with an estimated 1988 population of fewer than 2,500 people. For other areas, the sampling rate was

1 in 6 housing units in census tracts and block numbering areas with a precensus housing count of fewer than 2,000 housing units (fewer than about 5,200 people) and 1 in 8 housing units in larger census tracts and block numbering areas. The definition of areas for the 1-in-2 sampling rate included counties, towns, and townships, but not school districts (unless they happened to be coterminous with another governmental area).

In 2000 the overall sampling rate was also about 1 in 6, producing a sample of about 18 million housing units, but the variable rates were somewhat different from 1990. In 2000 the sampling rate was 1 in 2 for governmental areas with fewer than 800 housing units (fewer than about 2,100 people); 1 in 4 for governmental areas with 800-1,200 housing units (about 2,100-3,100 people); 1 in 6 for census tracts with fewer than 2,000 housing units (fewer than about 5,200 people); and 1 in 8 in larger census tracts. This design adds one more sampling rate, so that governmental areas with populations only slightly larger than areas with a 1-in-2 sampling rate will have a smaller increase in the proportional sampling error of their estimates compared with the 1990 sample design. For determining sampling rates in 2000, governmental areas were defined to include school districts in addition to counties, towns, and townships.

Data Collection

Data collection in the census is mainly by self-enumeration: a respondent for each household fills out a questionnaire received in the mail. Enumerators follow up those households that fail to return a questionnaire and collect the information through direct interviews. The follow-up enumerators are usually temporary workers who are given limited training.

Residence Rules

Residence rules for reporting household members in the census are that people who "usually" live at a residence should be reported and that people who are temporarily visiting should be excluded, unless they have no other permanent home. The usual residence for college students is their college residence and not their home residence; similarly, the usual residence for people who work away from home is their workplace residence if they live there most of the time. The usual residence for people with two homes is their permanent residence and not their vacation home.

TABLE 4-1 Key Features of Major Household Surveys

Feature	2000 Census Long Form	American Community Survey
Type of Survey, Frequency	Mandatory survey, part of census every 10 years since 1960	Mandatory monthly survey, tested in 4 sites in 1996, 8 sites 1997-1998, 31 sites 1999-2001, national survey 2000-2002, full implementation planned beginning in 2003
Income Data	Total income; income from seven sources for previous calendar year	Total income; income from seven sources for previous 12 months
Sample Size and Design	Systematic sample of household addresses and residents of group quarters: average sampling rate of 1-in-6; rates of 1-in-2 or 1-in-4 for small governmental units and 1-in 8 for large census tracts; total sample size about 18 million housing units	Similar design to 2000 census long form; planned sample size before nonresponse of 3 million housing units (including vacant units) per year; design alternatives being considered that would oversample rural and hard-to-enumerate areas
Data Collection Mode	Mail survey, personal follow-up for nonresponse	Mail survey, telephone follow-up, and then personal follow-up for one-third of mail and phone nonrespondents
Residence Rules	Usual residence; college students in dorms counted at college location	"Current" or 2-month residence rule
Response Rates	1990 mail response rate 74% for occupied households; net undercount of 1.8% after follow-up; 19% of aggregate income imputed	Mail response rate 61% in 4 test sites, plus 8% from phone follow-up, plus 9% from one-third follow-up of remaining nonrespondents, for weighted response rate of more than 95%; item response may be better than census, but not coverage

March Current Population Survey	Survey of Income and Program Participation
Voluntary monthly labor force participation survey, begun in 1940s; income supplement every March	Voluntary panel survey: each of 1984-1993 panels covered about 2.5 years; 1996 panel covered 4 years; 2000 panel to cover 1 year; 2001 panel to cover 3 years
Detailed questions on about 28 sources for previous calendar year	Detailed questions on about 65 sources for each month or for 4-month period preceding interview
Clustered sample of household addresses with state-representative design: addresses are in the sample for 4 months, out for 8 months, and in again for 4 months; total sample size of 50,000 occupied households plus 2,500 Hispanic households interviewed in previous November	Clustered sample of household addresses: original sample of occupied households was 12,500-23,500 for 1984-1993 panels; 37,000 for 1996 panel, with over-sampling of low-income households; 11,000 for 2000 panel; 37,000 planned for 2001 panel
1st and 5th interviews in person; other six interviews by phone	1st, 2nd, and one interview in each subsequent year of a panel in person; other interviews by phone
Usual residence; college students in dorms counted at parents' address	Similar to CPS; members of originally sampled households followed for life of panel
94-95% households respond, but some do not respond to income supplement or for all household members; coverage estimated at 92% of census; 20% of aggregate income imputed	91-95% households respond to 1st wave, but sample attrition occurs; cumulative response only 69% by wave 8 of 1996 panel; coverage similar to CPS; 11% of aggregate income imputed

Table continued on next page

TABLE 4-1 *Continued*

Feature	2000 Census Long Form	American Community Survey
Publication	Long-form data planned to be released in 2002; planned to be controlled to short-form data adjusted for undercount; data are published for such small areas as census tracts	Annual reports planned of 12-month averages for areas with 65,000 or more people, 5-year averages for areas with fewer than 20,000 people; goal is to publish 6 months after data collection
Proposed Changes	Long form may not be included in 2010 or later censuses	May replace census long form

Response Rates

Household response rates to the census mailout have declined between 1970, when mailout-mailback techniques were first used, and 1990. In 1990 approximately 74 percent of U.S. households returned their questionnaires with some or all of the requested information; the response rate for households receiving long forms was somewhat lower (70%) than that for households receiving short forms (75%). Data from the balance of the population were obtained by personal interviews (National Research Council, 1995b:189-190).

As in all censuses, some people were uncounted in 1990, and there were also duplications and other erroneous enumerations. The net undercount in 1990 (gross undercount minus gross overcount) was estimated at 1.8 percent for the total population, but there were substantial differences among population groups. For example, the net undercount was estimated at 5.7 percent for blacks and 1.3 percent for nonblacks. The net undercount also varied significantly by age: almost two-thirds of the estimated omitted population consisted of children under age 10 and men aged 25-39 (Robinson et al., 1993:13). The undercount was higher in large cities than in other areas, and it was disproportionately concentrated in the inner areas of those cities. It is likely that undercount rates were higher for lower income groups.

Item nonresponse rates in 1990 were generally higher for income than for most other items. When household income information is missing, the Census Bureau uses statistical techniques to impute it on the basis of

March Current Population Survey	Survey of Income and Program Participation
Income and poverty data published for nation and population groups 6 months after data collection; limited data published for states on basis of 3-year averages	No regular publication series; special reports published for nation and population groups; historically 1-2 year (or more) lag from data collection to publication
Recently received funding to expand sample size for state estimates of low-income children not covered by health insurance	Funding being requested to expand sample size and number of panels and for state-representative design

nearby households with similar characteristics. On average, 19 percent of aggregate household income was imputed for 1990 (National Research Council, 1995b:387).

Publication

Processing and release of the long-form sample data occur later than for the short-form, and long-form estimates on such characteristics as age, race, and sex are controlled to match the corresponding estimates from the short form for various levels of geography. For 2000, the long-form data are planned to be controlled to short-form data that have been corrected for measured population undercount.

Long-form data, including income and poverty estimates, are provided for areas as small as census tracts, school districts, and block groups. Typically, long-form data products are released beginning in year 2 and continuing through year 3 after the census year.

American Community Survey

The American Community Survey is planned to be a large-scale, continuing monthly sample survey of housing units in the United States, conducted primarily by mail. Its content will be similar to that of the decennial census long-form sample, including questions that permit constructing income and poverty estimates for households in small areas. The income questions ask about total income and income from seven different sources for the 12 months preceding the interview month. It is planned that the ACS will be mandatory, like the census, rather than a voluntary survey (although some or all of the ACS questions could be made voluntary in the future). If the ACS is successfully implemented, there will likely be no long form in the 2010 and subsequent censuses.

Development and Design

The ACS was tested in four sites in 1996 and in eight sites in 1997-1998. Beginning in 1999 and extending through 2001, the ACS will be conducted in 31 sites, chosen to facilitate comparison with the 2000 census long-form data for census tracts and other areas. In 25 of the 31 sites, about 0.4 percent of housing units are being sampled each month, which will generate a sample of about 5 percent of housing units for each of the 3 years, or 15 percent for the 3-year period. In the other 6 sites, for budgetary reasons, the 3-year sample will be about 9 percent in 5 of the sites and 3 percent in 1 site. For each year from 2000 to 2002, a nationwide survey, using the ACS questionnaire, will sample about 700,000 housing units, using a clustered sample design.

Beginning in 2003, the full ACS sample will be 250,000 housing units each month throughout the decade, for an annual sample size of about 3 million housing units spread across all counties in the nation. Over a 5-year period, the addresses selected for the ACS sample will cumulate to about 15 million housing units, similar to but somewhat smaller than the expected 2000 census long-form sample size of about 18 million housing units. Some of the ACS sample housing units will be vacant, and the sample size that is available for analysis will be further reduced by the ACS data collection procedures (see below).

Each month's ACS sample will be drawn from the Census Bureau's Master Address File (MAF) for the entire nation. The MAF is a comprehensive residential address list developed for the 2000 census that the Census Bureau intends to update on a continual basis following the census (see Chapter 5). The current design calls for the ACS to use a sample design similar to that of the 2000 census long form, with higher sampling rates for small governmental units (including school districts) and lower sampling rates for large census tracts. The sampling rates would be applied by systematic sampling from the MAF.

Some alternative sampling rates are being considered for the ACS. One scheme would make sampling rates decline as a smooth function of population size rather than vary by population size categories, until reaching a maximum sampling rate for very small areas. The maximum rate, cumulated over 5 years, could be higher than the highest long-form sampling rate in order to provide more reliable data for rural communities.

Another scheme would sample hard-to-enumerate areas at a higher rate than other areas.

Data Collection

The ACS will be conducted by a mail questionnaire, similar to the census long form, to all households in the sample. A replacement questionnaire will be mailed to nonresponding households about 3 weeks later. After about another 3 weeks, nonresponding households will be contacted to the extent possible by the use of computer-assisted telephone interviewing (CATI). In the final stage of follow-up, a one-third sample of the remaining nonrespondent households will be drawn, and field representatives will be sent to interview these households in person, using computer-assisted personal interviewing (CAPI) techniques.

Residence Rules

Residence rules for the ACS are somewhat different from the census because of the ACS's design as a continuing survey. The ACS uses a "current" or "2-month" residence rule: if a person in a sample unit at the time of survey contact is staying there for more than 2 months, he or she is a current resident of that unit whether or not the unit is also the person's usual residence under census rules. If a person who usually lives in the unit is away for more than 2 months at the time of contact, he or she is not a current resident of that unit. Anyone staying in the unit at the time of contact who has no other place where they usually stay is considered a resident of the unit.

Response Rates

Responses were obtained from about 78 percent of the originally designated sample for the four initial ACS test sites: 61 percent of households responded by mail, 8 percent responded to the telephone follow-up, and 9 percent responded to the personal follow-up. That last 9 percent were most of the 11 percent of households that were designated for personal follow-up (one-third of those that did not respond by mail or telephone; see Love and Diffendal, 1998). Because of subsampling at the final stage of follow-up, the weighted response rate in the four initial ACS test sites was more than 95 percent.

Preliminary results from the 1996 ACS test sites showed lower item nonresponse rates than in the 1990 census, at least for some items (Salvo and Lobo, 1998; Tersine, 1998). But the ACS, like other household surveys, may cover the population less well than the census, based on one analysis that found more small households and fewer large households in the 1996 ACS than in the 1990 census (Ferrari, 1998). This result could indicate that the ACS is missing a larger proportion of people in interviewed households than are missed in the census.[2]

Publication

Once it is fully implemented, publication plans for the ACS call for the Census Bureau to issue annual reports containing yearly averages of the monthly data for areas with 65,000 or more people. Such areas include all states, about 25 percent of counties, and about 4 percent of school districts. For smaller areas, the Census Bureau also plans annual publication of multiyear averages: 3-year averages for areas with 20,000-65,000 people and 5-year averages for areas with fewer than 20,000 people.[3] On this basis, 5-year averages will be required for about 47 percent of counties and about 82 percent of school districts.

Although delivery schedules are not known with certainty, yearly averages from the full ACS should be available within a year after the ACS is fully implemented in 2003 (i.e., in 2004). However, 3-year averages will not be available until 2006 at the earliest, and 5-year averages will not be available until 2008 at the earliest. Once sufficient years of data are cumulated to provide the planned 1-, 3-, or 5-year averages, as appropriate, each set will be updated yearly and published within 6 months after the close of a calendar year.

CPS March Income Supplement

The Current Population Survey is a voluntary monthly labor force participation survey, begun in the 1940s, that includes supplemental questions in many months. For the annual March Income Supplement, the

[2]In addition to within-household undercoverage, which occurs when some but not all household members are listed in the interview, there is undercoverage due to whole household misses, which this study did not address. Smith (1999) compared ACS estimates for the counties in the 1996 and 1997 test sites before adjustment to population controls with the population estimates for those counties and found some degree of undercoverage for most of the counties relative to the population estimates. These comparisons include both within-household and whole-household misses.

[3]The population cutoffs for requiring averages of 1 to 5 years correspond to about a 12 percent coefficient of variation for a 10 percent estimate with a typical design effect (Alexander, 1998). Estimates of poor school-age children more typically represent 3 percent of the total population, and, thus, an estimate for them will have a coefficient of variation nearly double that for a group with a similar design effect that is estimated to be 10 percent of the population.

CPS asks household respondents about income received during the previous calendar year, using a detailed set of questions for identifying about 28 different sources.

Design

The monthly CPS sample, beginning in 1996, includes about 50,000 households, or 1 in 2,000–a reduction in sample size of about 17 percent from the early 1990s. Part of the CPS sample is changed each month in a rotation plan: each sampled address is in the survey for 4 months, out of the survey for 8 months, and in the survey for another 4 months, so that three-fourths of the sample addresses are common from one month to the next, and one-half are common for the same month a year earlier. Each March, to obtain more reliable income data for the Hispanic-origin population, all November CPS households with one or more Hispanic persons are reinterviewed if they still include a Hispanic person. This procedure adds about 2,500 Hispanic households to the sample in March.

The CPS uses a multistage probability sample design, which is revised about once every 10 years on the basis of the results of the latest census. A design based on the 1990 census was phased in between April 1994 and July 1995: it included 792 sample areas consisting of about 1,300 counties, chosen to represent all 3,143 counties and independent cities in the 50 states and the District of Columbia.[4]

The CPS has a state-representative design, which generally results in larger CPS sample sizes for larger states, but with the largest states having CPS sample sizes that are smaller than their proportionate share of the U.S. population and the smallest states having proportionately larger sample sizes. For example, California, with 12.2 percent of the U.S. population, has 9.9 percent of the CPS sample; Wyoming, with 0.18 percent of the U.S. population, has 1.3 percent of the CPS sample. This sample design means that income and poverty estimates in large states are generally more precise than those in smaller states. The largest states, however, have larger relative errors due to sampling variability than would be expected if the CPS sample were allocated to the states in proportion to their population; the reverse is true for smaller states.[5]

[4]In January 1996 the number of sample areas was reduced from 792 to 754.

[5]To meet national-level reliability criteria for the unemployment rate, the sample size in a few large states (e.g., California, Florida, New York, Texas) is somewhat greater than what would be required by a state-based design. A full description of the CPS design is provided by U.S. Census Bureau and Bureau of Labor Statistics (2000); see also the joint Bureau of Labor Statistics and Bureau of the Census CPS web site: www.bls.census.gov/cps/mdocmain.html.

In fall 1999 the Census Bureau received an appropriation to adjust the March CPS sample size and design to provide reliable annual estimates at the state level of the numbers of low-income children lacking health insurance coverage by family income, age, and race or ethnicity. The Bureau has not yet decided what changes to make to the CPS for this purpose.

Data Collection

Data collection for the CPS is carried out by permanent, experienced interviewers. The first interview and fifth interviews at an address are usually conducted in person; the other six interviews at an address are usually conducted by telephone; CAPI and CATI are used. One household member who is aged 15 or older is allowed to respond for other members.

Residence Rules

Residence rules in the CPS are similar to the census, except that the "usual" residence concept is applied more broadly. For example, college students who are counted at the location of their college residence in the census are included in the CPS household of their family.

Response Rates

Response rates in the CPS are high: typically, about 94-95 percent of households respond to each month's CPS.[6] However, some interviewed households do not provide information for all members, so there is little data beyond basic demographic characteristics for about 9 percent of members of interviewed households. In addition, some people who respond to the basic CPS labor force questionnaire do not respond to the March Income Supplement. To adjust for whole household nonresponse to the basic CPS, the Census Bureau increases the weights of similar responding households. To adjust for person nonresponse to the basic CPS, it imputes a complete data record for another person with similar demographic characteristics.

Like other household surveys, the CPS exhibits population undercoverage at higher rates than the census itself. For March 1994, the ratio of the CPS-estimated population to the census-based population control total (all ages) was 92 percent; for black men aged 30-44 years, the cover-

[6]Household response rates declined 1-2 percentage points beginning in 1997.

age ratio was as low as 68 percent (U.S. Census Bureau, 1996:Table D-2). It is estimated that about two-thirds of CPS undercoverage is due to missed people in otherwise interviewed households (i.e., people whose existence is not known to the interviewer); the remainder is due to missed housing units. CPS undercoverage is corrected by ratio adjustments to the survey weights that bring the CPS estimates of population in line with updated national population controls by age, race, sex, and Hispanic origin. Beginning with the March 1994 CPS, the population controls for survey weights reflect an adjustment for the undercount in the census. However, the ratio adjustments do not correct for other characteristics, such as income, on which the undercovered population might be expected to differ from the covered population in each adjustment cell.

There is substantial item nonresponse in the March Income Supplement. About 20 percent of aggregate household income is imputed (about the same percentage as in the census; see National Research Council, 1993:Table 3-6). Imputation techniques are used to provide values for people who fail to respond to the income supplement entirely, as well as for people who fail to answer one or more questions on the supplement.

Publication

Publication of detailed official income and poverty estimates from the CPS for the nation as a whole, geographic regions, and population groups occurs each year about 6 months after data collection in March. Limited statistics are also published for states on the basis of 3-year averages.

Survey of Income and Program Participation

Design

The Survey of Income and Program Participation is a continuing voluntary panel survey. The first panel of households (the 1984 panel) began in October 1983. From 1985 to 1993, a new sample (panel) was introduced each February. Adult members of originally sampled households in each panel were followed and interviewed every 4 months for 32 months, although some panels had fewer than 8 interview "waves" because of budget restrictions and some panels had 9 or 10 waves. The 1996 panel, begun in April, followed both adult and child members of originally sampled households every 4 months for 4 years. A new 1-year panel, which may be extended to 3 years if funding is obtained, began in February 2000, and a new 3-year panel will begin in February 2001.

SIPP is focused on income measurement. The core questionnaire, administered at each interview wave, obtains detailed information for each month of the reference period on sources and amounts of income from earnings and public and private transfer payments and information for the 4-month period on income from assets. In total, about 65 separate sources of cash income are identified, together with benefits from 7 in-kind programs. Additional detail on program participation and related topics (e.g., child care, health) is collected in various supplements (topical modules). Four waves of a SIPP panel are required to calculate annual income and poverty statistics for a calendar year.

The SIPP sample covers the U.S. civilian noninstitutionalized population and members of the armed forces living off-post or with their families on-post. Sample size for the 1984-1993 panels varied from 12,500 to 23,500 households in the initial wave of interviewing. The sample size for the initial wave of the 1996 panel was 37,000 households: it included households in all states but was not designed to provide reliable estimates at the state level. The 1996 sample included an oversample of addresses in which the residents had family incomes below 150 percent of the poverty level in 1989, based on information from the 1990 census. Proxy characteristics, such as housing tenure and family type, were used for over-sampling addresses for which only short-form census information was available. In rural areas, some addresses were oversampled on the basis of 1990 census poverty-related characteristics for the census block in which they were located.

The sample size for the 1-year 2000 panel is 11,000 households, while the planned sample size for the 3-year 2001 panel is 37,000 (or possibly more) households; another large-size panel will begin in 2004. If funding is obtained to enable SIPP to become the basis of official income and poverty statistics in place of the March CPS, then the smaller 2000 panel will be extended for 3 years, and overlapping 3-year panels about the size of the 2000 panel will begin in 2002 and 2003. In addition, the design of the sample will be modified to represent all states and provide estimates for the largest states that have about the same level of error due to sampling variability as the current March CPS. Such a sample redesign, however, cannot be made until after the 2000 census results have been analyzed and used to redesign the samples for all major household surveys, which could take several years.

Data Collection

Data collection for SIPP is carried out by permanent, experienced interviewers. The first and second interviews and one interview in each subsequent year of a panel are conducted in person, using CAPI techniques. Other interviews are conducted by telephone from interviewers' homes. Household members age 15 or older are supposed to respond for themselves, but proxy responses are accepted. About 35 percent of interviews for adults in each wave are by proxy; for panels conducted before 1996, 60-65 percent of adult sample members had at least one proxy interview (U.S. Census Bureau, 1998c).

Residence Rules

With regard to residence, SIPP follows members of originally sampled households. Sample members, including adults and children, who move to new households at subsequent waves are followed, and information is obtained about the coresidents in their new households according to the CPS "usual residence" rules. Sample members who become institutionalized are tracked and interviewed subsequently if they return to a household setting.

Response Rates

Response rates to the first wave of a SIPP panel are somewhat lower than CPS response rates: about 5-8 percent of eligible households in the 1984-1991 SIPP panels did not respond to the first interview wave and were dropped from the sample; the household nonresponse rate for the first wave in the 1992 and 1993 panels was 9 percent; for the 1996 panel it was 8 percent. By wave 8, the cumulative household nonresponse rate in the 1984-1991 panels was 21-22 percent; in the 1992 and 1993 panels it was 25 percent. By wave 8 of the 1996 panel, the cumulative nonresponse rate was 31 percent. About three-quarters of household nonresponse is due to refusals, and one-quarter is due to losing track of sample household members who move (U.S. Census Bureau, 1998c:Ch.5).

People who drop out of SIPP tend to differ from those who stay in the survey: attrition is more likely to occur among young adults, males, minority groups, never-married people, poor people, and people with low educational attainment (see, e.g., Lamas, Tin, and Eargle, 1994). There is also evidence that the current noninterview weighting adjustments do not fully compensate for differential attrition across population groups (see, e.g., King et al., 1990).

Like the CPS and other household surveys, SIPP covers the population less well than the census. Coverage ratios (survey population estimates divided by census-based population estimates) are similar for the CPS and SIPP.

SIPP has lower item nonresponse rates than the March CPS: overall, only 11 percent of total regular money income obtained for calendar year 1984 from the first four waves of the 1984 SIPP panel was imputed, compared with 20 percent in the March 1985 CPS. The SIPP and March CPS imputation rates for 1984 for earnings were 10 percent and 19 percent, respectively; for public and private transfers, 12 percent and 21 percent, respectively; and for property income, 24 percent and 32 percent, respectively (Jabine, King, and Petroni, 1990:Table 10.8; see also National Research Council, 1993:Tables 3-4, 3-5).

Publication

Data processing for SIPP involves complex operations, particularly to produce calendar-year and longitudinal panel files. Historically, this has often resulted in delays of 1, 2, or more years between collection of data from an interview wave or all waves in a panel and release of data files and publications. There is no regular publication series for SIPP; publications are released on topics of interest, such as program participation, and include estimates for population groups and the total population by region and metropolitan or nonmetropolitan residence.

USES OF SURVEYS FOR SAIPE

This section notes five different ways in which household surveys could be used for updated SAIPE income and poverty estimates.

First, a survey could provide direct estimates for some or all small areas. For this use, the survey estimates should be available on a frequent, timely basis, at least every 2 years given the current SAIPE production schedule. They should also have acceptably low levels of error due to sampling variability. To reduce sampling variability, survey estimates could be averaged for more than 1 year.

Second, survey estimates for a single year, or averaged over more than 1 year, could be used to form the dependent variable in SAIPE models. If another survey were used for this purpose in place of the March CPS, comparability of the survey income and poverty measurements with the CPS measurements would be desirable, to reduce the likelihood of anomalies in the time series of estimates. For this use, survey estimates must be available on a frequent, timely basis.

Third, survey estimates for a single year or averaged over more than 1 year could be used to provide predictor variables in models. Indeed, models could be developed that include predictor variables from estimates for more than one survey for more than 1 year, using time-series or multivariate modeling techniques (see Chapter 3). As an example, a hypothetical county-level model could include as predictor variables estimates from the 1990 and 2000 censuses and multiple years of the ACS.

For use in prediction, the survey estimates must be available for all areas for which model-based estimates are required. Also, the survey estimates should have low or moderate levels of sampling variability. If the estimates are highly unreliable, they will have weak predictive power, although the predictions will not necessarily be biased. Comparability of income and poverty measurements with the survey used to form the dependent variable is not critical for this use: a predictor variable need not measure exactly what is measured by the dependent variable to be a good predictor. Availability of estimates on a frequent, timely basis is desirable but not critical, as somewhat outdated estimates may nonetheless be reasonably good predictors of current income or poverty levels.

Fourth, survey estimates for smaller areas of their shares or proportions of populations of larger areas could be used to develop small-area income and poverty estimates by applying the proportions to updated model-based (or direct) estimates for larger areas. This approach is similar to the way in which 1990 census within-county shares of poor school-age children for school districts were applied to 1995 county model estimates to produce the 1995 school district estimates.

For this use, survey estimates must be available for all areas for which estimates of shares are required. Ideally, they should be available on a frequent, timely basis and have relatively low levels of sampling variability, which could be facilitated by averaging estimates across more than 1 year or by some type of smoothing procedure (see Chapter 3). Comparability of income and poverty measurements with the survey used to form the dependent variable for the model estimates to which the shares will be applied is desirable but not critical.

Fifth, survey estimates could be used to control or calibrate estimates from other sources on selected characteristics. For example, model-based estimates for states and counties, produced by using one survey to form the dependent variable, could be adjusted to agree with key national or large-area estimates from another survey (e.g., estimates by region, metropolitan versus nonmetropolitan status, minority status). This approach could be followed when the estimates from the survey used for calibration are believed to be of exceptionally good quality but are not reliable for states or smaller areas.

EVALUATING ALTERNATIVE USES

This section discusses several critical considerations for determining feasible and desirable roles for the 2000 census long form, ACS, March CPS, and SIPP in the SAIPE Program. These considerations are: sampling variability, timeliness, and comparability and quality of income and poverty measurements.

Sampling Variability

A key consideration in using survey data for small-area estimates is sample size and the resulting level of error due to sampling variability. The census long-form sample is very large by comparison with the CPS and other existing household surveys–about 1 in 6 households in the census compared with about 1 in 2,200 households in the March CPS and about 1 in 3,000 households in the 1996 SIPP panel. Moreover, the census long form provides for oversampling of small governmental units. Consequently, it provides estimates with much smaller sampling variability than do other surveys for areas of all population sizes. In fact, direct estimates are rarely published from such surveys as the CPS and SIPP for subnational areas, even states. Yet despite the large sample size for the census long form, sample estimates from it exhibit a large degree of variability due to sampling error for many school districts and other very small areas.

The ACS will be a far larger household survey than has ever been fielded by a federal statistical agency on a continuing basis–250,000 residential addresses each month, with each month's sample drawn independently. (Over a 5-year period, no address can be in the sample more than once.) By comparison, the CPS sample includes 50,000 households each month, of which a large fraction were in the sample of the previous month (75%) or of the same month in the previous year (50%).[7] SIPP is even smaller than the CPS, and it also experiences considerable attrition, which not only reduces the sample size over the life of each panel, but also introduces bias into income and poverty estimates.

Nonetheless, the ACS estimates, when averaged over a year to provide a sample size of 3 million or about 1 in 36 housing units, will exhibit considerably higher sampling variability than estimates from the 2000 census long-form sample. Even when cumulated for 5 years, the ACS estimates will be more variable than the long-form estimates–not only is the 5-year ACS sample size somewhat smaller than the long-form sample size (about 1 in 7 compared to 1 in 6 households), but also, by design, the ACS follows up in the field only one-third of the households that do not respond by mail or telephone. As noted above, in the four initial ACS test

[7]The rotation design of the CPS, in which households are in the sample for 4 months, out of the sample for 8 months, and in the sample again for another 4 months, is advantageous in reliability terms for measuring changes in such statistics as the monthly unemployment rate from month to month or year to year. However, the design reduces the effective sample size for estimates that are based on averaging multiple months of data relative to a design in which each month's sample is independent of other months.

sites, the total number of households with completed interviews was about 78 percent of the originally designated sample, due largely to the procedure whereby only one-third of mail and telephone nonrespondents are followed up in person. Moreover, this procedure results in a variation in survey weights (3 to 1, other things equal), which further reduces the effective sample size to about 62 percent of the originally designated sample (78% reduced by a factor to take account of the loss of precision from variable weights; see Kish, 1992).

Below we compare the sampling variability of direct estimates of poor school-age children for states from the 2000 census long-form sample, ACS, and CPS and for counties and school districts of different population sizes from the census and ACS. SIPP estimates would be more variable than CPS estimates.

State Estimates

Table 4-2 shows median coefficients of variation (in percent) for the March 1996 CPS estimates of the proportion poor of school-age children in 1995 for states classified by 1996 population size. Also shown are the estimated coefficients of variation for the proportion poor of school-age children in 1995 based on the 1990 census long-form sample design and on a 1-year average of a fully implemented ACS sample.

From the CPS, the median coefficient of variation for the school-age poverty rate is 7 percent for the largest three states, with total population of 18 million or more; it is 24 percent for the eight smallest states, with total population less than 1 million. For comparison, a common design goal for published survey estimates is a coefficient of variation of 10 percent or less.

From the census long form, the median coefficient of variation for the largest three states is 0.3 percent; it is 2.5 percent for the eight smallest states. The difference between the coefficients of variation for the largest and smallest states is greater for the census than for the CPS because the CPS sample is designed to be state representative. (As noted above, there are proportionately more sample households in smaller states and proportionately fewer sample households in larger states than would occur in an equal probability sample design.) Nonetheless, the census estimates for states are clearly superior to the CPS estimates in terms of error due to sampling variability.

The levels of sampling variability for 1-year ACS state estimates meet commonly accepted standards as well. Thus, the median coefficient of variation for 1-year ACS estimates is just under 1 percent for the largest three states, and it is 7 percent for the eight smallest states.

TABLE 4-2 Coefficients of Variation (CV) for Estimates of the Percentage Poor of School-Age Children in 1995 for States Categorized by Population Size

State Population Size, 1996[a]	Number of States[a]	Median Percent Poor School-Age Children 1995	Median Coefficient of Variation for States in Population Size Category (in percent)		
			March 1996 CPS	1990 Census Long Form	1-Year Average ACS
Less than 1 million	8	18.3	23.7	2.5	6.8
1 million to less than 3 million	14	14.6	21.0	1.6	4.4
3 million to less than 6 million	16	14.3	21.3	0.8	2.2
6 million to less than 10 million	6	14.5	15.1	0.7	2.0
10 million to less than 18 million	4	17.8	11.8	0.5	1.3
18 million or more	3	22.5	7.1	0.3	0.9

NOTES: Percentages poor of children aged 5-17 for which coefficients of variation (the standard error of an estimated divided by the estimate) are calculated are those for 1995, as estimated from the March 1996 CPS. These percentages are used together with generalized variance functions to estimate the CVs that would be obtained with the 1990 census long-form sample design and a 1-year average of a fully implemented ACS sample.

[a]Includes the District of Columbia.

SOURCE: The CVs for the March 1996 CPS are calculated from National Research Council (2000c:Table 6-8, using one-half the difference between cols. 3 and 1, divided by col. 1); 1990 census long-form CVs are calculated by using a formula adapted from Siegel and Fisher (1998:1), assuming a design factor of 1.5; ACS CVs are calculated by using a formula from U.S. Census Bureau (1999a), assuming a 3 percent annual sample and a design factor of 1.6.

County and School District Estimates

Table 4-3 shows illustrative coefficients of variation for estimates of a school-age poverty rate of 18 percent from a sample size equivalent to that of the 1990 long form and from sample sizes equivalent to those for the ACS, averaged over 1, 3, and 5 years of full implementation, for areas of different population sizes. These coefficients of variation incorporate approximate design effects for the long-form census sample and the ACS. The calculations assume average sampling rates and do not allow for differences in sampling rates across areas. Given the oversampling of very small governmental jurisdictions in the census and ACS, the coefficients of variation from the census and ACS will be lower for these smaller areas than those in the table, and, conversely, they will be somewhat higher for larger areas.

For areas with 50,000 or more total population (27% of counties in 1990, but only 6% of school districts), the estimates of poor school-age poverty rates from the census long form have fairly low levels of error due to sampling variability, with coefficients of variation of less than 10 percent. Estimates from the census long form also have reasonably small coefficients of variation for areas with 20,000 or more population (53% of counties in 1990, 18% of school districts)–the coefficient of variation for these areas is 12 percent or less. For areas with 25,000 or more population, estimates from the ACS, when they are averaged over 5 years, have coefficients of variation that are reasonably small (13% or less), but the coefficients of variation for 1-year estimates from the ACS are more than twice as high as those for 5-year estimates, and the coefficients of variation for 3-year ACS estimates are about 29 percent higher than those for 5-year estimates.

For estimates for smaller areas, error due to sampling variability increases for both surveys. The coefficient of variation for estimates from the 1990 census long-form sample is 14 percent for areas with 15,000 population and 35 percent for areas with 2,500 population.[8] The coefficient of variation for estimates from the ACS averaged over 5 years is 15 percent for areas with 20,000 population and 42 percent for areas with 2,500 population. Almost one-half of counties (47%) and four-fifths of school districts (82%) have 20,000 or fewer people; 31 percent of school districts have 2,500 or fewer people, although such districts account for relatively small proportions of the population. Thus, the 82 percent of districts with 20,000 or fewer people include only 31 percent of total popu-

[8]The coefficient of variation would be smaller for areas with 2,500 population if the area were oversampled; see discussion below.

TABLE 4-3 Illustrative Coefficients of Variation for Estimates of an 18 Percent Poverty Rate for School-Age Children for Counties and School Districts by Population Size

County or School District Population Size	Cumulative Percentage Distribution			
	Counties		School Districts	
	Number of Areas	Number of People	Number of Areas	Number of People
250,000	6.6	56.6	N.A.	N.A.
100,000	14.6	72.5	1.9	26.8
50,000	26.8	83.4	5.7	44.4
25,000	46.5	92.1	15.7[a]	63.4
20,000	53.4	94.1	17.7	68.9
15,000	63.2	96.3	24.2	76.4
7,500	83.3	99.1	41.9	89.2
5,000	90.5	99.6	52.7	93.6
2,500	96.2	99.9	69.1	97.4

NOTES: The coefficients of variation are calculated in each instance by assuming that 17 percent of the total population are school-age children and the poverty rate for school-age children is 18 percent. The calculations assume average sampling rates and do not allow for differences in sampling rates across geographic areas. County population size percentages are from Census Bureau data for 3,141 counties in 1990; school district population size percentages are from Census Bureau data for 9,243 school districts defined for 1990 in the Bureau's 1980-1990 evaluation file. The evaluation file excludes 5,983 districts that existed in 1990–districts that were coterminous with a county, districts that did not cover both elementary and secondary grades, and districts for which all or part had no counterpart in 1980. The subset of school districts in the evaluation file closely resembles the entire set of 1990 school districts in terms of the distribution of total population.

Coefficient of Variation for Estimate from a Sample of the Size of the American Community Survey (percent)			Coefficient of Variation for Estimate from a Sample of the Size of the 1990 Census Long Form (percent)
1-Year Average	3-Year Average	5-Year Average	
9.4	5.4	4.2	3.5
14.9	8.6	6.7	5.5
21.1	12.2	9.4	7.8
29.8	17.2	13.3	11.0
33.3	19.3	14.9	12.3
38.5	22.2	17.2	14.2
54.4	31.4	24.3	20.0
66.6	38.5	29.8	24.6
94.2	54.4	42.1	34.7

[a]Interpolated.
N.A. Not available.

SOURCE: ACS 1-year average CVs are calculated using the formula in U.S. Census Bureau (1999a) for a 3 percent annual sample and a design factor of 1.6. ACS 3-year average and 5-year average CVs are calculated by applying a factor of 0.578 and 0.447, respectively, to the 1-year average CVs. The 1990 census long-form CVs are calculated from the formula for the standard error of a proportion adapted from Siegel and Fisher (1998:1, which gives the formula for the standard error of a number), assuming a design factor of 1.5.

lation; the 31 percent of districts with fewer than 2,500 people include only 3 percent of total population (see Table 4-3).

The decision to treat school districts as governmental units for purposes of oversampling in the 2000 census and ACS will reduce the sampling variability of estimates of income and poverty for small school districts. For a small school district of fewer than 2,000 people that was not in an oversampled governmental unit in 1990 and hence was sampled at a rate of 1 in 6, if that district is sampled at a rate of 1 in 2 in 2000, the coefficient of variation will be reduced by a factor of 0.45. However, the coefficient of variation will be still be high for such a small area; it will change from about 39 percent to about 18 percent. In the ACS, oversampling of a small school district will reduce the coefficient of variation for 5-year averages from about 47 percent to about 21 percent.[9]

Timeliness

The second key consideration in using survey data to produce small-area income and poverty estimates for such purposes as annual fund allocation is how regularly data can be provided and on what time schedule. The SAIPE Program is currently on a production cycle of releasing estimates every year for states and every 2 years for counties and school districts. The current lag between the release date and the income reference year is 3-4 years (e.g., 1997 estimates are scheduled to be released in fall 2000). We recommend (see Chapter 3) research and development to reduce the extent of the lag.

The 2000 census long form will provide only one observation, for the 1999 income reference year. The long-form data will likely not be available until 2002, so that the 1999 estimates may not be available in time to use, either directly or indirectly, for the SAIPE estimates for 1999, which are scheduled for release in late 2002. However, the data will be available subsequently to use in models.

The March CPS is conducted annually and data processing is completed within 6 months of data collection. The reason for the lag in producing estimates from the current CPS-based SAIPE models is that other data needed for the models, such as food stamp data, are not available to the Census Bureau on a timely basis.

SIPP is conducted yet more frequently than the March CPS (each panel is interviewed every 4 months); however, 4 waves of SIPP data are

[9]For areas of 2,000 to 3,000 people that are sampled at a rate of 1 in 4, the coefficient of variation would be reduced by a factor of 0.6.

required to produce calendar-year income and poverty estimates. Also, SIPP has not yet been able to meet a regular production schedule, and data products are often provided 1, 2, or more years after data collection.

The ACS will be conducted monthly. It remains to be seen how soon it will achieve a regular, timely production schedule, but the intent is that income and poverty estimates can be updated on an annual basis and made available in the year following data collection. If ACS direct estimates can be used for SAIPE, averaged over 1 or more years, they will be more timely than the current model-based CPS estimates. However, if ACS estimates are used indirectly in models, reducing the time lag in the estimates will require efforts to improve the timely availability of other data used by the models or perhaps changes in how the data are used (e.g., perhaps using an earlier year of food stamp data as a predictor variable; see Chapter 3).

Comparability and Response Quality

The third key consideration for use of survey data for small-area estimates is the quality of the survey responses on variables used to measure income and poverty and the comparability of the information with that provided by other surveys. In particular, because the SAIPE Program estimates are currently based on the March CPS as the dependent variable in prediction models, it is important to understand how using another survey for this role, such as the ACS, would affect the consistency of the time series of SAIPE.

Research has documented significant differences between income and poverty estimates from CPS and SIPP, as well as between estimates from CPS and the census long form. Comparisons of CPS and SIPP poverty rates find that SIPP rates are consistently lower than CPS rates, although the difference in the two rates was less in the 1996 panel than in earlier panels. In 1991, the SIPP poverty rate for the total population was 15 percent below the CPS rate, and differences for some groups were even larger. For example, the SIPP poverty rate for the elderly in 1991 was 27 percent below the CPS rate, apparently due largely to more reports of Social Security income in SIPP than CPS (Martini and Dowhan, 1996; see also Short et al., 1998). As noted above, comparisons of census and CPS estimates of income and poverty generally find that the census produces higher estimates of median household income and lower estimates of poverty than the CPS.[10]

[10]No direct comparisons have been performed of SIPP and census income and poverty estimates.

Such differences in income and poverty estimates among these surveys are not surprising. As discussed above, even though they use the same income and poverty definitions, the surveys differ in many other ways that could affect the estimates. To date research has not been able to establish which factors are most important in producing differences in income and poverty estimates across surveys, nor to determine which estimates are of higher quality. Even for SIPP and CPS, for which the most measurement research has been conducted, it is not possible to assess the total error in their poverty or income estimates nor to fully understand differences between them. Generally, it is believed that CPS estimates are of higher quality than census estimates and that SIPP estimates may be of higher quality yet, due largely to SIPP's focus on income measurement, which has produced more complete reporting of many sources of income than the CPS or other surveys. Under its current design, however, SIPP suffers from cumulative attrition over the life of a panel, which likely contributes to biases, such as an upward bias in measured income and a downward bias in measured poverty, that increase in later waves of a panel. (See Citro, 1995, for an assessment of CPS and SIPP income data; see also U.S. Census Bureau, 1998c.)

Income and poverty estimates from the ACS will likely differ not only from CPS and SIPP estimates, but also from census estimates, even though the ACS is designed to be very similar to the census long form. A study that compared median household income in the 1996 ACS test sites and the 1990 census, adjusted to 1996 dollars, found that the ACS produced significantly lower medians than the census in all four sites (Posey and Welniak, 1998). The ACS median incomes were also lower than 1993 median incomes (adjusted to 1996 dollars) from the SAIPE program for three of the four sites. It is not possible to determine what proportion of these differences is due to differences in measurement among the data sources and what proportion is due to socioeconomic changes for the areas over time. We note below some of the key differences between the ACS, the 2000 census, the March CPS, and SIPP that are likely to affect data comparability.

Type of Income Questions

The ACS questionnaire and the census long form include only seven questions on separate sources of income in contrast to the March CPS and SIPP, which include many more such questions. Evidence suggests that asking more questions on income elicits more complete reporting because it prompts recall of small or occasionally received income amounts (see Ycas and Lininger, 1983:27; Martini and Dowhan, 1996). However, that evidence applies to voluntary surveys such as CPS and SIPP. Whether income reporting in mandatory surveys, such as the ACS and the census long form, is impaired by having fewer questions is not known. One study of the 1960 census, which showed income estimates higher than those from the March 1960 CPS, attributed most of the difference to a shortage of higher income families in the CPS, possibly due to higher rates of nonresponse to income items by such families in the voluntary CPS (Miller, 1966).

Respondent and Mode

Both the ACS and the 2000 census use mailout/mailback techniques as the principal mode of response: one household member is asked to fill out the income and other questions for all household members. The March CPS also allows one household member to respond for other members. SIPP, in contrast, strives to obtain self-reports from each adult household member, although proxy responses are often accepted. Interviewing for both the CPS and SIPP uses a combination of personal and telephone techniques. Research to date is not conclusive on the effects of either interview mode or proxy response on income reporting.

In addition to encouraging self response, SIPP encourages respondents to consult records, such as pay stubs, in reporting income sources and amounts, on the assumption that record use contributes to more complete income reporting. In the 1990-1993 panels, about 20 percent of respondents used at least one type of record.

Reference Period

Both the 2000 census and the March CPS ask about receipt of income over the most recent calendar year at a time when many people have just completed or are preparing their income tax returns. SIPP asks about income receipt on a monthly basis for many income types and for the 4-month period prior to each interview for other income types. Estimates for calendar years can be constructed from the SIPP data. In contrast, the ACS asks about annual income for a reference period that is the 12 months prior to the interview month.

Because the ACS neither has the short recall period of SIPP nor refers to a specific calendar year (except for the ACS households interviewed in January), it may exhibit higher levels of income underreporting than the census, March CPS, or SIPP. A split-sample experiment with the ACS questionnaire in fall 1997 determined no significant differences in median total income of individuals between respondents who were asked to report income for the preceding calendar year and those who were asked to report income for the past 12 months (Posey and Welniak, 1998); however, more research will be needed to assess the effects of the ACS reference period on income and poverty statistics.

Because the ACS monthly samples have different income reporting periods, it will not be possible to construct 12-month averages of annual income and poverty rates that refer only to a specific calendar year. Each month's sample will reflect a different 12-month reporting period, and the 12 monthly samples for year t will be centered on July of year $t - 1$ through June of year t (spanning 24 months in all). Estimates can be constructed for which the reporting period is centered on a calendar year by averaging monthly samples for July of year t through June of year $t + 1$. However, such estimates will still represent an average of different reporting periods, and, consequently, may differ from the estimates that would be obtained from the March CPS or the census.[11]

Residence Rules

The ACS current or 2-month residence rule (see "American Community Survey," above) will classify some people differently from either the census or CPS and SIPP "usual residence" concept. For example, college students in dormitories will be counted at the dormitory location in the census; at the dormitory location or the location of their family residences in the ACS, depending on the outcome of applying the 2-month rule; and at the location of their families in the March CPS and SIPP. Whether college students are counted at school or home will affect not only the size of their families, but also their families' income level and poverty threshold.

When annual (or longer) averages are constructed from the ACS for small areas, an issue arises that is similar to the issue of different reporting periods for income. Namely, in localities that experience large shifts in population (either seasonal shifts as may occur in college towns and retirement communities or secular shifts due to changing economic conditions or other factors), monthly samples may differ substantially in the number and characteristics of residents for a locality. Estimates of the numbers of residents and their income and poverty status that are con-

[11]The Census Bureau is presently adjusting ACS income dollar amounts for inflation to represent a common calendar reference year. For example, a household interviewed in March of 1996 reported its income for March 1995 through February 1996. The Census Bureau adjusted that income to a 1996 reference calendar year by multiplying it by the 1996 average annual consumer price index for all urban consumers (CPI-U) for January-December 1996 and then dividing by the average CPI-U for March 1995-February 1996. This procedure, however, does not address the problem that monthly ACS samples may experience a different mix of economic conditions over the reporting period.

structed from the ACS by averaging over the relevant 12 months to obtain estimates for a calendar income reference year may not correspond to the estimates obtained for the people who are contacted in March or April following that year in the CPS or census.[12]

Survey Procedures

The 2000 census, ACS, March CPS, and SIPP differ in many aspects of their survey operations, which could affect such features of data quality as household response rates, questionnaire item response rates, completeness of coverage of the population, accuracy of reporting, and accuracy of editing and imputation procedures. As an example, the ACS hopes to achieve quality improvements, in comparison with the census, by having a permanent interviewing staff instead of the army of temporary enumerators who are employed for the short time period in which census follow-up enumeration is conducted. Preliminary results from the 1996 ACS test sites (see "American Community Survey," above) suggest that the ACS interviewers may achieve lower item nonresponse rates, but not necessarily better population coverage, than the census.

ANALYSIS AND CONCLUSIONS

Having considered the reliability, timeliness, and likely quality of data from the 2000 census long form, ACS, March CPS, and SIPP and the alternative uses that could be made of them for the SAIPE program (e.g., providing direct estimates, serving as predictor or independent variables in models), we have reached several conclusions and recommendations. We present our analysis under four headings–general, role of the ACS, role of the 2000 census long form, and revised poverty measure–and then list our formal recommendations.

General

Model-Based Estimates

From our review of the 2000 census long-form, ACS, March CPS, and SIPP, it is clear that the SAIPE Program must continue to use models to

[12]It is possible that the smoothing used to produce calendar-year estimates from the ACS may prove to be advantageous. For example, population and income estimates from averages of monthly samples for a college town may better represent the "typical" experience of that town over a year than estimates that are based on the population in March or April of the following year.

produce indirect estimates of income and poverty for small areas. None of these surveys can provide direct estimates that are of sufficient reliability, quality, and timeliness to replace all of the small-area estimates produced by SAIPE.

The 2000 census long-form estimates will be reliable for all states and many counties, in that they will have acceptably low levels of error due to sampling variability, but the census estimates are only available for income year 1999. Also, the census estimates will not be reliable for most subcounty areas, such as most school districts, even for income year 1999.

One-year average estimates from the monthly ACS, once it is fully implemented, will be reliable only for states and a small percentage of counties, while 5-year average estimates will be reliable for a larger percentage of counties. However, there will still be a sizable proportion of counties and many smaller areas for which the estimates will have low reliability. Also, 5-year average estimates will not begin to be available until very late in this decade, and they could be viewed as problematic for some program uses because they will reflect changes in income and poverty with a considerable lag. For example, two areas may have the same 5-year average poverty rate, but one area may have a sharply increasing poverty rate over the period and the other area a sharply decreasing poverty rate.[13] Moreover, the quality of the ACS income and poverty estimates has yet to be established. Consequently, using the ACS to provide direct estimates for the SAIPE Program, except for states, does not seem warranted absent considerable evaluation work.

The March CPS provides high-quality annual estimates, but it does not currently provide reliable direct estimates for any subnational areas, except for the very largest states. However, the CPS may provide reliable state estimates in the future, given the recent appropriation to adjust the sample size and design to provide reliable state estimates of low-income children who lack health insurance coverage. The estimates from SIPP at present are neither reliable for any subnational area nor available on a

[13]For fund allocation, the use of 5-year averages would gradually shift funds from areas with declining poverty rates to areas with increasing poverty rates, which could be viewed as beneficial if localities value stability of funding more than faster response to changing levels of need (see Chapter 6; see also Waksberg, Levine, and Kalton, 1999). Also, 5-year averages from the ACS could be preferable to the currently available SAIPE model-based estimates because the ACS estimates could likely be produced on a faster time schedule and so use more current data. For example, it should be possible in late 2010 to produce 5-year average estimates for counties from ACS data for 2005-2009. In contrast, it is likely that estimates released in late 2010 from the current county model would be based on 3-year average data from the CPS for 2006-2008 because of the lags in obtaining administrative data for the model (see Chapter 3).

timely basis. Thus, we conclude that some type of modeling must be used for most SAIPE estimates for the foreseeable future, which may involve using one or more or all of the available surveys—2000 census long form, ACS, March CPS, and SIPP.

Measurement Research

Although the 2000 census, ACS, CPS, and SIPP currently measure the same concepts of income and poverty, differences in their measurements can be expected due to the many differences in their design and operation. Detailed understanding of measurement differences is essential to determine the best ways to use the data from these surveys in the SAIPE Program. To date, only limited data and information are available for this purpose.

As part of a measurement research program, we urge the Census Bureau to conduct a planned exact match of the March 2000 CPS and the 2000 census long-form sample (exact CPS-census matches were performed for the 1950-1980 censuses). The Census Bureau should also conduct an exact match of the 1996 SIPP panel, for which the last year of interviews covers 1999 income, with the 2000 census.[14] The Census Bureau should also carry out a planned set of aggregate comparisons between the 2000 ACS and the 2000 census. An exact ACS-census match for 2000 will not be possible because of a decision not to send long-form questionnaires to any of the ACS households in the sample around the time of the census, in order to minimize respondent burden and confusion between the two surveys. However, a planned exact match of the ACS with the census short-form may help evaluate within-household population coverage in the census and ACS and should be carried out.

Another useful set of comparisons would be exact matches of the 2000 census, 2000 ACS, 2000 March CPS, and 1996 SIPP with Internal Revenue Service (IRS) tax return records for 1999.[15] Census-IRS, CPS-IRS, and SIPP-IRS matches have been performed in the past (see, e.g., Childers and Hogan, 1984; Coder, 1991, 1992; David et al., 1986). Such matches for income year 1999 could provide valuable information not only for comparing income reports among the household surveys as they relate to the IRS records, but also for assessing the performance of IRS

[14]A SIPP-census match might be restricted to SIPP rotation groups that were interviewed close to census day.

[15]The Census Bureau obtains limited tax return information each year from the IRS, such as wages and salaries and adjusted gross income, for research and estimation purposes (see Chapter 5).

data in small-area estimation models. One issue that could be addressed, for example, is the extent to which the IRS records cover the low-income population (see further discussion in Chapter 5). For matching purposes, it will be important to include 1999 tax returns that were filed late as well as returns that were filed on time.

The Census Bureau should explore ways to make the exact matches of census, IRS, and household survey data available to the research community–for example, by providing access to the files at the secure research centers that the Bureau has established in cooperation with several universities around the country. The availability of such files, with appropriate safeguards to protect the confidentiality of individual responses, would likely stimulate research on measurement error and modeling that would be beneficial to the SAIPE Program.

Role of the ACS

County Models

Careful evaluation of the strengths and weaknesses of the ACS, including in-depth comparisons with other surveys, will be needed to determine the best strategy for using ACS data for SAIPE estimates.[16] For states, it appears possible to use direct estimates from the ACS, averaged over a year, once the survey has been fully implemented. For counties, our necessarily preliminary review of reliability, timing, and response quality issues suggests that two possible uses of ACS data merit serious consideration. Both uses, for which the Census Bureau should begin research and development now, involve indirect rather than direct estimation.

One approach is for the Census Bureau to continue to base county estimates on statistical models for which the March CPS estimates form the dependent variable and ACS estimates are used as one of the predictor variables, along with the other variables that are currently in the models. (For the school-age poverty model, these variables include IRS tax return data, food stamp data, census data, and population estimates.) For this purpose, the ACS estimates could be averaged over the same 3 years as the CPS estimates to make them consistent for the time period covered. This averaging would also reduce the sampling variability of the ACS estimates, which could improve the predictive power of the ACS variable in the models. It could also be possible to use ACS estimates for several years in a time-series or multivariate modeling approach (see Chapter 3).

[16]See also National Research Council (2000b) for discussion of issues in using the ACS.

Continuing to base the county models on the CPS, which is the official source of poverty statistics, could be advantageous because the CPS can be expected to have less bias in the measurement of annual income and poverty than the ACS. However, CPS-based models for income and poverty estimates for counties do have some limitations. Even when 3-year averages are used, the sampling variability of the CPS county estimates is high, so that very few counties receive a significant weight on the direct estimates when they are combined with the model estimates in the estimation procedure (see National Research Council, 2000c). Also, many counties are excluded from the modeling because they have no sample households (due to the clustered sample design), or, in the case of poverty estimates, no poor households (or no poor households with school-age children) in the sample. In contrast, the ACS uses an unclustered design with sample households in every county each month.

Hence, a second strategy to investigate is to construct statistical models for county income and poverty estimates in which the dependent variable is taken from the ACS estimates. An issue for evaluation is whether the dependent variable is best constructed as an annual average of 12 monthly samples centered on the calendar year (i.e., using months from July of year t to June of year $t + 1$) with appropriate inflation adjustments, or as an average of, say, 24 or 36 monthly samples centered on the calendar year. In either case, there would be less reliance on the models, compared with the CPS-based models, because ACS direct estimates would be available for all (or almost all) counties. The use of 2-year or 3-year average ACS estimates would place more weight on the direct estimates when they are combined with the model estimates than if average annual estimates, which have greater sampling variability, were used.[17]

Given the likely measurement biases for ACS income and poverty estimates, estimates from the ACS-based county models could perhaps be improved by calibrating them in some way to selected estimates from the March CPS. For example, counties could be grouped into broad categories on such dimensions as race, ethnicity, and geographic region, and raking factors could be developed that would achieve consistency between the ACS model-based estimates for each county group and the corresponding March CPS estimates. For this purpose, the CPS estimates could be based on weighted 3-year averages in order to reduce their sampling variability. Alternatively, calibration could be achieved by a bivariate model in which ACS and CPS estimates form the dependent variables in two linked equations (see Chapter 3).

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[17]However, average annual estimates may have less bias than 2-year or 3-year average estimates.

If a calibration procedure is adopted, it should then be applied to ACS estimates for states as well as counties, so as to achieve a consistent measurement standard for the direct state estimates and the model-based county estimates. The goal of any calibration procedure would be to reduce the mean square error of adjusted ACS estimates by taking advantage of the lower variance of the ACS data and the presumed lower bias of the March CPS data. If SIPP becomes the preferred source for national estimates of poverty (see "A Revised Poverty Measure," below), there would be reason to calibrate the ACS estimates to the SIPP estimates and not to the March CPS estimates. Substantial research and development could be required to develop an appropriate calibration approach in either case.

School District Estimates

A possible role for the ACS (once it is fully implemented) that could improve the SAIPE estimates for school districts and other subcounty areas is to use ACS data to form within-county shares to apply to updated county poverty estimates.[18] The advantage of this approach, in comparison with the current estimation procedure in which the most recent census data are used to develop within-county shares to apply updated county model estimates, is that the ACS estimates will be more current. Also, if the ACS estimates of shares are applied to estimates from an ACS-based county model, the two sets of estimates would reflect the same measurement standard.

However, the ACS estimates of shares will exhibit higher sampling variability than the census estimates of shares, particularly if the ACS estimates are averaged over, say, a 3-year rather than a 5-year period. For use in a shares model, statistical smoothing of the ACS estimates for subcounty areas within counties should be investigated to reduce their sampling variability (see Chapter 3).[19] Another possibility for investigation is whether the ACS estimates could be combined in some way with 2000 census estimates to form within-county shares. If in the future it

[18]The shares would be each subcounty area's proportion of the total number of poor school-age children (or other population group) in the county.

[19]Whether smoothing county and subcounty estimates in order to reduce the mean square error of the latter would be successful with the ACS is not clear, given the sizable sampling variability of the ACS estimates for many counties. The other techniques suggested in Chapter 3 for reducing the variability of census long-form estimates for subcounty areas, which involve using short-form and long-form data in a simple or stratified ratio adjustment, are not applicable to the ACS. There is no ACS short form that is to be completed for all households.

proves feasible to assign IRS tax return data to subcounty areas (see Chapter 5), then it might be possible to combine ACS estimates and IRS data for this purpose.

For the greatest improvement in subcounty estimates of income and poverty, it will likely be necessary to develop a statistical regression model for these areas that makes use of administrative data for predictor variables (see Chapter 5). However, development of appropriate administrative data is a long-range effort, so the Census Bureau should pursue the alternative of using ACS estimates, perhaps together with 2000 census estimates, in a within-county shares model.

ACS Funding

The ACS has the potential to play a major role in the SAIPE Program because of its large sample size and continuous operation. To do so, the ACS has to have consistent levels of sufficient funding over the next decade for the planned sample sizes. Reductions in funding would likely lead to reduced sample sizes and other discontinuities in the data that could jeopardize the usefulness of the ACS for SAIPE and make it difficult to evaluate how effective the ACS could be for SAIPE if carried out as now planned. More generally, if the ACS does not receive consistent funding, it will be difficult to properly assess its potential for small-area estimation for such important purposes as fund allocation and program evaluation.

Role of 2000 Census Long Form

Models

Estimates from the 1990 census long form are used–in somewhat different ways–as predictor variables in the current SAIPE state and county regression models, and these variables contribute importantly to the models (see National Research Council, 2000c:Ch.6). It makes sense to plan for a similar role for estimates from the 2000 census long form and perhaps to include predictor variables from both the 1990 and 2000 censuses for a time.[20]

Long-form census estimates may prove to be less effective predictors in the models for some years than others because of different economic

[20]Planning to use the 2000 census as a predictor variable in models for states as well as counties is necessary, given that it will not be feasible to use ACS direct state estimates at least until data are available for income reference year 2003 and the quality of the data has been determined.

conditions. Economic changes may occur immediately following a census as well as later in a decade. This problem is naturally handled in a modeling framework, given that the model is refitted for each estimation year.

For smaller areas, such as school districts and other subcounty areas, it is likely that income and poverty estimates from the 2000 census will exhibit high sampling variability even with oversampling of small governmental units and the use of the most effective procedures to reduce variance. The census estimates will also be available for only one year. However, it will be necessary to use the 2000 long-form estimates to form within-county shares to apply to updated county estimates until it becomes possible to use the ACS for this purpose or until it becomes possible to develop a subcounty model similar to the state and county models. The development of such a model depends on obtaining appropriate subcounty administrative records data. If such data can be developed for use in a subcounty model, then the 2000 census estimates are a likely candidate to serve as one of the predictor variables. The sampling variability in the census estimates would weaken the predictive power of the census variable, but the model would produce unbiased predictions.

Direct Estimates for 1999

While it clearly makes sense to plan to use 2000 long-form estimates as predictor variables in SAIPE state and county regression models and, for the time being, in a county shares model for school districts and other subcounty areas, it is far from clear what use, if any, to make of the direct long-form estimates for income year 1999. On the one hand, direct estimates will be reliable for states and many counties, and they will have considerable face validity for users, so that not to use these estimates for income year 1999 seems problematic. However, their use would likely produce anomalies in the time series of estimates because the standard of measurement provided by the census direct estimates would not likely be the same as that underlying the estimates produced for prior years from the SAIPE CPS-based models nor that underlying the estimates produced for subsequent years from another model (e.g., one based on the ACS or CPS or either of these two surveys adjusted to SIPP controls). Moreover, the census estimates for 1999 will not be reliable for many counties and most subcounty areas, and they may not be available in time to meet the Census Bureau's SAIPE schedule, which calls for 1999 estimates to be delivered to users by fall 2002 (although the census estimates could be used to produce revised 1999 SAIPE estimates when they become available).

We do not believe there is a clearly preferred answer for whether and how to use 2000 long-form direct estimates for SAIPE for income year 1999. We urge the Census Bureau to consider several options, which include: using the direct long-form estimates (either for release on the SAIPE schedule or for later release); using the long-form estimates with a ratio adjustment to short-form data to reduce the sampling variability of the estimates (see Chapter 3); using the long-form estimates with a calibration to CPS aggregate estimates; not using the direct long-form estimates but, instead, using the current SAIPE models to produce indirect estimates for income year 1999. The Bureau should convene a meeting of key users to discuss these options so that the basis for the Bureau's decision is well understood.

A Revised Poverty Measure

U.S. poverty statistics for the total population and population groups are currently based on a measure in which annual before-tax money income for a family or unrelated individual is estimated from the March CPS and compared with the applicable poverty threshold for the family size. A report of the National Research Council (1995a) concluded that the current measure is not adequate to inform public policy and recommended that it be replaced with a revised measure, in which disposable after-tax money and near-money income would be estimated from SIPP and compared with an appropriate poverty threshold (see also Betson, Citro, and Michael, 2000). An earlier National Research Council report (1993) also recommended that SIPP become the basis for measuring poverty.

The revised poverty measure would differ from the current measure in how the thresholds are developed, updated, and adjusted for different size families and areas of the country. The revised measure would also differ in how family resources are measured from survey data. Starting with gross money income, as in the current measure, the revised measure would add the value of near-money in-kind benefits (e.g., food stamps, subsidized housing, school lunch, energy assistance), and subtract the following items: payroll taxes; net federal and state income taxes (for some recipients of the earned income tax credit, a positive amount would be added to income); expenses necessary for work, including work-related transportation and child care costs; child support payments to another family; and out-of-pocket medical expenditures.

Whether some or all of the recommendations in the 1995 report will be adopted is not known at this time. A report of the U.S. Census Bureau (1999c) illustrated the use of the revised poverty measure with March CPS data for 1990-1997, and the Bureau plans to regularly release revised estimates (labeled "experimental") on its Internet web site at the same time that the official estimates are released each fall. The Bureau is also working to make it possible to implement a revised measure with SIPP by adding some questions and seeking funding to revise the design so that a new panel is introduced each year, which could be used to equalize the bias due to sample attrition across years. Additional funding is also being sought for expanded sample size to support direct estimates for the largest states.

Using a revised official poverty measure in SAIPE would mean changing the measurement standard to a measure that is more appropriate for policy purposes–because it takes account of taxes and in-kind transfer programs and other family circumstances that are not reflected in the current measure. Such a change raises issues of implementation.

The 2000 census does not ask questions on in-kind benefits or nondiscretionary expenses (e.g., work expenses) that would be needed to calculate family resources under the revised measure. The ACS includes questions on in-kind benefits (food stamps, energy assistance, school lunch, and subsidized housing), but not on nondiscretionary expenses, and it is unlikely that the questionnaire could be expanded to provide all of the elements of the revised definition. In contrast, the revised definition of disposable money and near-money income can fairly readily be calculated from either the March CPS or the SIPP, although imputations for some kinds of expenses needed to calculate disposable money and near-money income are required (more so in CPS than in SIPP).

The limitations of the 2000 census and ACS income data constrain but do not preclude the use of these sources in estimating poverty for small areas with a revised measure. Direct estimates could not be obtained from either the 2000 census or the ACS that fully implemented a revised measure. However, if the CPS remains the official source of poverty statistics with a revised measure, then 2000 census or ACS estimates that are based on the current measure could be used as predictor variables in CPS-based regression models that use a revised measure. Poverty estimates reflecting the current measure from the 2000 census or the ACS could also be used to form within-county shares for subcounty areas to apply to updated county poverty estimates developed from CPS-based models for which the dependent variable reflected a revised measure.[21] Finally, if ACS estimates are calibrated in some manner to March CPS estimates of poverty developed with a revised measure, then the calibrated ACS estimates could be used as a dependent variable in regression

[21] This use of census long-form or ACS estimates of shares would require the assumption that the distribution of poverty within counties is similar under the current and revised poverty measures.

models. If SIPP replaces the CPS as the official source of poverty statistics, then such calibrations should be implemented with that survey instead of the CPS.

Conclusions and Recommendations

4-1 The Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program must continue to rely primarily on models for updated income and poverty estimates for small areas. None of the existing or planned surveys can produce direct estimates of sufficient reliability, timeliness, and quality to provide all of the SAIPE income and poverty estimates.

4-2 To inform decisions about the use of the 2000 census long form, American Community Survey, CPS March Income Supplement, and Survey of Income and Program Participation for SAIPE, the Census Bureau should conduct research to understand and document the differences in their measurement of income and poverty. For this purpose, the Census Bureau should conduct a series of exact matches and analyses:

- the planned exact match of the March 2000 CPS and the 2000 census long form;
- an exact match of interviews from the 1996 SIPP panel covering 1999 income and the 2000 census long form;
- the planned set of aggregate comparisons of income and poverty estimates from the 2000 ACS and the 2000 census long form;
- an exact match of the 2000 ACS and the 2000 census short form to examine differences in measurement of household composition and demographic characteristics that relate to income and poverty; and
- exact matches of Internal Revenue Service tax returns for income year 1999 with the 2000 census long form, 2000 ACS, March 2000 CPS, and 1996 SIPP.

4-3 Research and development by the Census Bureau should begin now to explore two possible uses of ACS estimates in SAIPE models for counties: to form one of the predictor variables in statistical models for which the March CPS continues to provide the dependent variable and to serve as the dependent variable in county models. For the latter use, the ACS estimates might possibly be calibrated in some way to selected estimates from the March CPS.

4-4 The Census Bureau should conduct research on using ACS estimates for school districts and other subcounty areas, possibly combined with 2000 census estimates, to form within-county shares or proportions to apply to updated county model poverty estimates.

4-5 If the ACS is to fulfill its potential to play a major role in the SAIPE Program, it is important that the survey have sufficient funding for planned sample sizes over the next decade. Reductions in funding could jeopardize its usefulness for SAIPE and, more generally, make it difficult to properly assess the potential uses of ACS data in small-area estimation.

4-6 The Census Bureau should plan to use 2000 census long-form estimates to form one of the predictor variables in the SAIPE state and county models.

4-7 For SAIPE estimates for income year 1999, it may be possible to use the direct estimates from the 2000 census long form, but whether this is feasible or desirable is not clear. The Census Bureau should consider the available options and discuss them fully with users.

4-8 If the recommendations of the National Research Council for changes in the official measure of poverty are adopted, the Census Bureau will need to consider the implications for the SAIPE Program. In particular, it may become feasible and desirable to use estimates from the Survey of Income and Program Participation for calibration purposes.

5

Future Model Development: The Role of Administrative Records

OVERVIEW

Estimates for school districts and other subcounty areas from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program currently cannot be produced by using regression models similar to the state and county models. The latter models are advantageous not only because they use updated information to form the dependent and predictor variables, but also because the modeling procedure improves the precision of the resulting estimates. Instead, for subcounty areas a shares approach must be used: estimates from the previous census long-form sample of the shares or proportions for each subcounty area of the county total are applied to updated estimates from the county model. The estimates of shares are subject to high levels of sampling variability for many small areas and also necessarily assume that the relative proportions of poor people among areas within each county have not changed since the census. If appropriate variables could be found to use in regression models to predict poverty or income for subcounty areas, such models would likely be better than the current shares procedure.

The difficulty is that no administrative records data sources currently exist that can provide consistently measured, updated predictor variables for a subcounty model, in the way that tax return and food stamp data are used in the state and county models. There is also an issue of the source of the dependent variable in a subcounty model–the American Community Survey (ACS) might serve this purpose, perhaps calibrated in some manner to the March Current Population Survey (CPS), as discussed in Chapter 4. Alternatively, it might be possible to develop a bivariate model (see Chapter 3) in which both the ACS and March CPS provide dependent variables.[1]

In this chapter we first review the advantages and problems of developing two possible data sources for predictor variables for subcounty income and poverty regression models: IRS tax return records, which could be used in modeling both income and poverty, and food stamp records, which could be used in modeling poverty. Both of these sources currently provide significant variables in the Census Bureau's state and county models; their use for subcounty models would require further development of the Census Bureau's capabilities for geocoding addresses to small geographic areas.

Use of food stamp data would also require arrangements to obtain microlevel data on a regular basis from state agencies, or, alternatively, to enable state agencies to geocode the records and provide area summaries to the Census Bureau. In contrast, the Census Bureau already has access to selected information on individual income tax returns. If geocoding capabilities can be improved for subcounty areas but access arrangements cannot be worked out for food stamp data, it would not be possible to develop a subcounty poverty model with both IRS and food stamp data.

However, it is possible that an acceptable subcounty poverty regression model could be developed with IRS data (including 2000 census data as another predictor variable), but not including food stamp data.[2] Alternatively, it may be possible to use geocoded IRS data to develop within-county shares to apply to updated estimates from the Census Bureau's county model. Whether it is preferable to form within-county shares by using IRS data or ACS data (once they become available) is a question. Another question is whether it might be possible to combine ACS and IRS data in some manner to form the shares. It is possible that a shares model along these lines, which would use more current data to form the shares

[1]The recent availability of funding to adjust the CPS sample size and design to support reliable state estimates of low-income children who lack health insurance coverage could make that survey more valuable as the source of a dependent variable for subcounty areas.

[2]The Census Bureau conducted preliminary work on estimating a county model of poor school-age children, excluding the food stamp variable that is used as a predictor variable in the current model. The results demonstrated somewhat poorer performance for the model without food stamps, based on comparisons with 1990 census estimates and an estimate of goodness of fit. However, the model without food stamps was still an improvement for estimates of poor school-age children in 1989 over simpler procedures that assumed little change from the 1980 census (see Siegel, 1997; National Research Council, 2000c).

than is currently done by using the previous census, could be as effective as a regression model.

Following the discussion of IRS and food stamp data and the prospects for geocoding these sources to subcounty areas, we consider the potential uses of data from the National School Lunch Program for improving poverty estimates specifically for school districts. School lunch data, which do not require geocoding, might be used, alone or in some combination with ACS data (and possibly 2000 census data, as well), to form within-county shares to apply to updated county model estimates. Alternatively, school lunch data might be used as a predictor variable in a regression model for school district poverty estimates.

We then discuss data needs for improved population estimates, which are required for many uses of small-area income and poverty estimates from SAIPE: for example, in fund allocation formulas for which SAIPE estimates of numbers of poor need to be converted to poverty rates. Population estimates between censuses are developed by using administrative records, such as tax returns (see Chapter 3), and we discuss future directions for research and development to improve the data sources and methods for producing small-area estimates of total population and population by age.

Based on its review and analysis, the panel lastly presents its recommendations on the possible uses of administrative records data to improve income, poverty, and population estimates from the SAIPE Program, which are listed at the end of the chapter. The panel is cognizant that enhancements to administrative records data sources and improved geocoding are likely to be costly. As part of planning its research program for the next decade, the Census Bureau should consult with user agencies about their needs for small-area estimates, particularly at the subcounty level. It could be useful to develop rough cost-benefit calculations jointly with these agencies to help guide further research and development. Such calculations might assess, for example, the benefits for fund allocation and other program purposes from improving the accuracy of estimates of poor school-age children for school districts (in terms of bias and variance) against the costs of developing the necessary data and geocoding capabilities. In its planning, the Census Bureau should also consider the benefits of possible improvements to administrative data and geocoding capabilities for other Bureau programs.

TAX RETURN DATA

Tax return data from the Internal Revenue Service (IRS) have long been used by the Census Bureau for small-area estimation. Each year the Bureau obtains a file from IRS of selected information on 1040 tax returns, including type of return (e.g., joint, single), adjusted gross income, and other variables, which the Bureau uses for statistical purposes.[3] The 1040 data contribute importantly to the Census Bureau's population estimates for states and counties by providing the basis for estimating year-to-year net internal migration rates for people under age 65 between counties (see Chapter 3).[4] They were used from 1971 to 1987 to estimate per capita income for local government jurisdictions for fund allocation under the General Revenue Sharing program. Currently, they are being used to form predictor variables in the SAIPE income and poverty models for states and counties. The process of assigning poverty status to the IRS records, in general terms, involves comparing adjusted gross income for families on tax returns to a poverty threshold that corresponds to the number of adult and child exemptions reported on the return (including exemptions reported for children away from home).

Although there are differences between the definitions of families and income in IRS records and in household surveys and the census, they are not critical for purposes of developing a predictive model. More important is that the data provide consistent measures across areas. IRS data have the advantage that the rules for reporting of income are uniform across the nation. However, the Census Bureau's evaluation has found some differences across states in the completeness of the tax files that it obtains from IRS that may affect use of the data in models (Cardiff, 1998). These differences occur because the Census Bureau receives an early version of the data for each tax filing year from the IRS. The Census Bureau should further investigate the quality of the data from the early version and determine if a somewhat later version would be preferable and could be used without delaying preparation of the estimates.

In the Census Bureau's current CPS-based state model for estimating proportions of poor school-age children, the IRS data contribute two of the four predictor variables for each state: (1) proportion of child exemptions reported by families in poverty on tax returns and (2) proportion of people under age 65 who are not included on an income tax return, which is obtained by subtracting the estimated number of exemptions on in-

[3]There is no reverse flow of information: that is, the Census Bureau does not provide individually identifiable information of any kind to the IRS (nor to anyone outside the Bureau).

[4]Demographic information contained in the Social Security Administration Numident File, linked to IRS tax returns, also contributes to state population estimates by age, sex, race, and Hispanic origin (see Chapter 3). The Numident File will likely play an even more important role in small-area population estimates, now that the Census Bureau has access to 100 percent of the records and not only a 20 percent sample of them (see "Data Needs for Population Estimates" below; see also National Research Council, 1994).

come tax returns for people under age 65 from the estimated total population under age 65 that is derived from demographic analysis. The reason for including a variable to estimate people not reported on tax returns is because they are believed to be poorer on average than other people. IRS data also contribute to the SAIPE state models for median household income, total poverty, poverty for children under age 5, and poverty for people under age 18 (see Table 3-2 in Chapter 3).

In the Census Bureau's current CPS-based model to estimate numbers of poor school-age children for counties, the IRS data contribute two of the five predictor variables for each county with poor sampled households containing school-age children in 3 years of the March CPS: (1) log (number of child exemptions reported by families in poverty on tax returns) and (2) log (number of child exemptions reported on tax returns). The second variable is included together with another predictor variable–log (estimated population under age 18 from demographic analysis)–to cover children not reported on tax returns (i.e., in nonfiling families), who are assumed to be poorer on average than other children. IRS data also form predictor variables in the SAIPE county models for median household income, total poverty, and poverty for people under age 18 (see Table 3-3 in Chapter 3).

IRS tax return data are not identified by county of residence. In order to use the data in the county model, the Census Bureau must first assign the address on each tax return record to a geographic area. Over the years, the Census Bureau has refined its methods for geocoding addresses to counties so that the process is believed to work well in most instances. For both states and counties, there are errors in assigning addresses to area of residence because some tax returns are filed from a person's business address or the address of the tax preparer, which may not be in the same state or county as the taxpayer's residence. The extent of nonresidential tax-filing addresses, and, in particular, the number of such addresses that differ from the filer's state or county of residence, is not known.

To use IRS tax return data to form predictor variables for an income or poverty model for school districts or other subcounty areas, or, alternatively, to form within-county shares or proportions for subcounty areas, the Census Bureau will need to further refine its geocoding capabilities so that addresses can be assigned geographic codes below the county level. As discussed below (see "Geocoding with TIGER and MAF"), the development of the 2000 census Master Address File (MAF) and the refinement of the TIGER geocoding system may make it possible to geocode addresses to subcounty areas with acceptable accuracy, although the problem of nonresidential tax-filing addresses will remain.

FOOD STAMP DATA

States and Counties

The Census Bureau uses the proportions and numbers of food stamp recipients, respectively, as predictor variables in the SAIPE state and county poverty models. Two key eligibility requirements for food stamps, which make it suitable for modeling poverty, are that households must have gross income below 130 percent of the applicable poverty guideline and net income, after certain deductions, below 100 percent of the applicable poverty guideline.[5] The gross and net income limits for eligibility and the ceilings on allowable deductions are higher in Alaska and Hawaii than in the other states due to their higher cost of living.

The Census Bureau obtains monthly totals of food stamp recipients for states from the U.S. Department of Agriculture (USDA). The Bureau conducted research to determine how best to use these data for input to the state poverty models. Based on that research, the Bureau decided to use the monthly counts averaged over a 12-month period centered on January 1 of the calendar year subsequent to the income reference year for the poverty estimates. The Census Bureau further refines the food stamp counts in three ways: it subtracts counts by state of the numbers of people who received food stamps due to specific natural disasters from the counts of the total number of recipients; it uses the results of time-series analysis of monthly state food stamp data from October 1979 through September 1997 to smooth outliers; and it adjusts the counts of food stamp recipients in Alaska and Hawaii downward to reflect the higher eligibility thresholds for those states.

For counties, the Census Bureau obtains counts of food stamp recipients from USDA and, in some instances, from state agencies, but the information obtained is not always the same for different counties: in most counties, the counts of food stamp recipients pertain to July; for some counties, they are an average of the monthly counts for the year. For input to the county poverty models, the Census Bureau rakes the county food stamp numbers to the adjusted food stamp state numbers.

Although there are nearly uniform rules for administration of the Food Stamp Program across states, estimated participation rates–the proportion of eligible households that apply for and receive benefits–differ appreciably by state. Such differences, which may stem from differences

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[5]The poverty guidelines used for determining program eligibility are derived by smoothing the official poverty thresholds for different size families (see Fisher, 1992).

in outreach efforts, the stigma associated with participation, or other factors, could affect the comparability of food stamp counts across areas in terms of how well they relate to poverty. Interarea comparability may also be affected by changes in the design and administration of income support programs consequent to recent welfare reform legislation (the 1996 Personal Responsibility and Work Opportunity Reconciliation Act [PRWORA] and subsequent amendments). The legislation denied food stamp benefits to many immigrants, who are not distributed uniformly across the country. It also greatly limited benefits for able-bodied adults without dependents who do not meet work requirements, and, at the same time, it permitted waivers from those provisions for high-unemployment areas, which could affect interarea comparability.

Another possible effect on interarea comparability may result from the marked decline that has occurred in welfare caseloads under the Temporary Assistance to Needy Families (TANF) program established by PRWORA. The extent of the decline has differed among states, in part due to differences in state efforts to move families off the welfare rolls. These differences appear to have affected food stamp caseloads as well, perhaps because families who leave TANF are discouraged from applying for other benefits, such as food stamps, for which they may still be eligible.

A priority for the Census Bureau should be to assess the comparability across states and counties of food stamp data for years subsequent to passage of the 1996 legislation in terms of how well the data relate to poverty (see Recommendation 5-1, below). For example, analysis of trends in estimated participation rates for states before and after welfare reform could indicate whether some states have diverged from national trends. If comparability appears to have markedly decreased, it may not be appropriate to use food stamp data as a predictor variable in the state poverty models or even in the county poverty models as they are currently specified. However, if the data remain reasonably comparable across counties within states, then it might be possible to use food stamps in estimating poverty for counties. For example, it might be possible to develop a form of county model that would predict changes in poverty on the basis of changes in food stamp data and other predictor variables. The results could then be controlled to estimates from state models that did not include food stamp recipients as a predictor variable.

Another problem with the use of food stamp data in the county models concerns the time that is required to obtain the data, which are not available until almost 2 years after the year to which they refer. This delay is one of the reasons that the Census Bureau currently produces county poverty estimates with a minimum 3-year time lag (e.g., estimates for income year 1995 were completed in fall 1998). As discussed in Chapter 3, a priority for the Census Bureau should be to evaluate ways to reduce the time lag between the reference year of the estimates and the date when they are released. A way to reduce the delay that is due to lags in obtaining food stamp data could be to use the data for the year prior to the income reference year in the models.

Subcounty Areas

For subcounty poverty estimates, it is not now possible to consider using counts of food stamp recipients in a model because, in contrast to tax returns, the Census Bureau does not have access to individual food stamp records and hence cannot undertake to geocode the addresses to local areas. State agencies, and not the USDA, have custody of and control over the individual food stamp records, and state record systems differ in format and provisions for access. In some states, county agencies maintain and control their own food stamp databases.

To obtain food stamp data for subcounty poverty models would most likely require a substantial investment of staff time and resources to build cooperative arrangements for geocoding and tabulating the data (see Becker, 1998). Such a cooperative enterprise would need to involve the Census Bureau, the U.S. Department of Education, the U.S. Department of Agriculture, other federal agencies interested in subcounty poverty estimates, and state agencies. Arrangements would need to be worked out for geocoding the microlevel records (assuming geocoding capabilities are improved, as discussed below) and for resolving discrepancies and errors in geocoding. Arrangements would also need to be worked out for access to the geocoded files. Given state confidentiality requirements as well as the benefits of local knowledge for geocoding, it may be that a workable arrangement would be to have state agencies perform the geocoding of food stamp records in their state and then provide summaries to the Census Bureau for counties, school districts, and other subcounty areas.

Critical to the success of a decentralized system, such as that just outlined, or any other arrangement for geocoding and tabulating food stamp records for small areas, is to develop compelling incentives for the different stakeholders to participate. The benefits to the Census Bureau and to the Department of Education and other federal agencies of having food stamp data available for use in poverty models are clear, provided that the costs are bearable. State agencies perhaps could benefit from having geocoded food stamp records available for such purposes as streamlining the enrollment of children in school lunch and breakfast programs who are automatically eligible because their families are enrolled in food stamps.[6] However, it would likely require resources from federal agencies for the geocoding work and other aspects of running a successful cooperative program, such as training and documentation, to enlist the full participation of state agencies.

We are not optimistic about the prospects for developing an effective federal-state cooperative program for geocoding food stamp records to subcounty areas. Moreover, changes in the operation of the Food Stamp Program, as discussed above, raise questions about the usefulness of food stamp data for modeling in the future. But, if the demand for updated small-area poverty estimates continues to grow, then the benefits, costs, and feasibility of some type of cooperative program could be investigated. A possible approach would be for the Census Bureau and the Department of Education to identify one or two interested states that might be willing to establish pilot programs to serve as feasibility studies.

GEOCODING WITH TIGER AND MAF

The Census Bureau's TIGER (Topologically Integrated Geographic Encoding and Referencing System) database was developed after the 1980 census to provide a complete mapping of every line segment in the United States, including streets, rivers, other physical features, and invisible boundaries of governmental and statistical areas; to link address ranges for city-style addresses to line segments;[7] and to link codes for census geographic areas (counties, census tracts, blocks, etc.) to the map spaces defined by the line segments. TIGER thus makes it possible to geocode (assign) addresses on administrative records to small census geographic units when the addresses are in city-style format–that is, when the address has a house or building number and street name, such as "104 Main St." For larger areas, such as counties, the Census Bureau has developed methods for geocoding not only city-style addresses to those areas, but also non-city-style addresses, which include rural route numbers and post office box numbers. Essentially, the Bureau uses ZIP-plus-4 codes to assign non-city-style addresses to counties. The geocoded records can then be tabulated to provide statistics of interest for small areas.

However, TIGER cannot now be used for geocoding addresses to

[6] For this purpose, each school district in a state might be provided with the list of families participating in food stamps whose addresses were geocoded to that district, provided that such a procedure is compatible with the state's confidentiality provisions for food stamp data.

[7] Because TIGER generates public use products, it contains address ranges and not individual street addresses; the Census Bureau regards the latter as confidential under Title 13 of the U.S. Code.

subcounty areas because a significant percentage of addresses are not in city-style format, and the coding method used for counties for such addresses would not likely be accurate for subcounty areas. In addition, the address ranges in TIGER do not reflect all of the city-style addresses that exist. The completion of the Master Address File (MAF) for the 2000 census will make it possible to expand the address coverage in TIGER. The MAF contains individual addresses for housing units–separately identifying units in apartment buildings and other multiunit structures–together with the applicable codes for the state, county, census block, and other geographic entities. The MAF includes not only units with city-style addresses, but also units that lack such addresses. For the latter type of unit, MAF contains not only an address that, together with an appropriately marked map, can be used by an enumerator to locate the unit (e.g., "white trailer with green shutters"), but also, to the extent field staff are able to obtain the information, the mailing address for that unit (e.g., P.O. Box 8).

Development of TIGER/MAF

TIGER began with 1:100,000-scale maps of the entire country from the U.S. Geological Survey and obtained input from three previously separate sources of geographic information that were used in the 1980 census. These sources were Geographic Base Files, developed for the densely settled portions of metropolitan areas, which linked address ranges to blocks, census maps, and Geographic Reference Files, which linked blocks to other geographic units (census tracts, towns, counties, etc.). Originally, about 65 percent of total addresses were contained in the address ranges that were associated with line segments in TIGER. By adding information from the Address Control File that was developed for the 1990 census, TIGER address range coverage was expanded to about 85 percent of addresses. The remaining addresses could not be linked to line segments because they were not in city-style format.

By adding information from the 2000 MAF, the address range coverage in TIGER is being expanded yet further. The MAF began with the 1990 census address list and has been updated over the decade with the U.S. Postal Service's Delivery Sequence File (DSF). As the MAF is updated, new addresses are geocoded by TIGER to the extent possible. When new street names are identified from the DSF (e.g., a new subdivision), the Census Bureau's regional offices check them using local maps or in the field, and the map locations and address ranges for the segments are added to TIGER.

Several operations were conducted to further update the MAF and TIGER in preparation for the 2000 census. These operations included field canvassing of the entire country by Census Bureau staff and review of the MAF and TIGER-derived maps by localities and tribal governments. Also, before the census, consistency checks were run between MAF and TIGER.

To update governmental unit boundaries in TIGER, the Census Bureau every year conducts a Boundary and Annexation Survey to ascertain boundary changes for counties, cities, townships, and American Indian areas. In addition, beginning with the 1995-1996 school year, the Department of Education is providing funding for school district boundaries to be updated and put into TIGER every 2 years.

Prospects for Improved Geocoding

The Census Bureau hopes to obtain funding for a TIGER/MAF modernization and continuous updating program following the 2000 census. As part of this program, the Census Bureau would exchange electronic files of addresses and geocodes with local and tribal governments, when possible, and perhaps use satellite imagery data for more precision for structures and physical features. The Census Bureau will in any case use the U.S. Postal Service's DSF to update both the MAF and TIGER on a continuous basis after 2000–at least as often as every year, and perhaps two or three times a year.

In addition, plans are being developed in conjunction with the American Community Survey for a Community Address Updating System (CAUS) as another source of input for the continuous development of TIGER/MAF, primarily in parts of the United States that do not have city-style addresses for mail delivery. In turn, the MAF will be used as the sampling frame from which the ACS monthly samples are drawn. As outlined in Alexander (1999), the CAUS will involve field work conducted by ACS and other Census Bureau survey field staff to check areas of housing growth and to correct errors and omissions in TIGER/MAF. In areas with city-style addresses, CAUS staff will field check growth areas to identify omissions in the DSF updates. In areas with non-city-style addresses that have locally developed geocoded address lists, CAUS staff will validate the local lists and check growth areas for errors and omissions. In non-city-style areas without local lists, CAUS enumerators will field check all areas of growth. Growth areas will be identified from community sources, administrative records, and observations by interviewers.

As a result of all these activities, the address range coverage in TIGER should become ever more complete for geocoding purposes. The 2000 version of TIGER, after a full cross-check with the 2000 MAF, should have more complete address range coverage than the pre-2000 census version because many addresses that were added to the MAF in areas for which TIGER did not have address ranges will turn out to be city-style addresses. Furthermore, during the next decade, as more and more counties in rural areas adopt city-style addresses to provide locatable addresses to emergency personnel (the E-911 Program), and as these addresses are added to TIGER/MAF through the updating programs just described, the address range coverage in TIGER will become yet more complete.[8]

There will still likely be some areas of the country for which there are no city-style addresses or for which there are E-911 addresses but they are not adopted, for one or another reason, by the U.S. Postal Service or by localities and, hence, do not appear in the DSF or local lists. These areas, primarily rural, will then continue to have addresses (e.g., rural route 2, box 4) that are included in the MAF but are not geocodable by TIGER as it is presently configured.

Also, post office box addresses that exist because people prefer to have mail delivered to a box rather than to a street address will be geocodable to the post office location through TIGER but not to the residence. Similarly, addresses that are for third parties (e.g., tax accountant addresses on tax returns) or for someone's business may be geocodable, but not to the appropriate residential address. Indeed, changes to administrative records (e.g., a requirement to list residential addresses on tax returns) may be needed to achieve high levels of geocoding for some small areas for some types of records.

The Census Bureau has indicated that it may be possible, if there is demand, to use the MAF to geocode addresses that cannot be geocoded in TIGER. Thus, the MAF will have addresses of the form of "rural route 2, box 4" that are assigned to census geographic units and could be geocoded via a match to the MAF record with that same address. However, the Census Bureau does not yet have software to use the MAF in this way. Another alternative, if resources could be obtained, might be to modify TIGER to recognize address ranges of the form "rural route 2, box 4 through box 50," if such addresses have a well-defined sequence that permits mapping them to TIGER line segments.

In deciding how far to develop geocoding capabilities in TIGER or MAF, the Census Bureau will need to consider the demand for geocoding to subcounty areas and the cost-effectiveness of alternative strategies. Generally, the Bureau's ability to make better use of administrative

[8]The National Emergency Numbering Association encourages counties to adopt the E-911 Program. As counties develop street addresses for E-911 purposes, the addresses are usually added to the U.S. Postal Service's DSF, which is periodically matched to TIGER and the MAF.

records for small-area estimates and other purposes will require resources for a TIGER/MAF modernizing and updating program.

In order to use administrative records to form variables for income and poverty models for school districts and other subcounty areas, it should be a high priority once the 2000 census TIGER/MAF has been completed to study the extent of geocoding that can be achieved with TIGER at that time (see Recommendation 5-2, below).[9] We understand that the Census Bureau plans geocoding studies. We recommend that one such study assess the success of geocoding tax return records (to which the Census Bureau already has access) to school districts. The study should select states or parts of states for which school districts are not county equivalents. It should analyze the reasons for nongeocodable addresses (e.g., rural route address, post office box in city-style area, other reason) and attempt to identify nonresidential addresses. The study should also assess to what extent it is possible to use ZIP codes or perhaps town or place names to assign addresses to school districts.

A study along these lines could inform the prospects for developing improved poverty and income estimation models for school districts and other subcounty areas that use IRS and other administrative records data or, alternatively, for improving the census shares method by using IRS shares or changes in IRS shares. It could also inform decisions about new uses of IRS data to improve small-area population estimates that are needed for the SAIPE Program. Finally, to the extent that such a study demonstrated the benefits of improving the address information on IRS records for geocoding purposes, it would then be possible to make a case for requiring a residential address on tax returns.

SCHOOL LUNCH DATA

Another possible source of information on poverty from administrative records that is available specifically for school districts comprises counts of students who are approved to receive free meals under the National School Lunch Program. School lunch data have the advantage that they are compiled for schools and school districts and, hence, do not require geocoding of individual addresses.[10]

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[9]The proposed study would update the results of a study of geocoding 1995 tax returns to census blocks that was conducted in 1998. At that time, TIGER was successful in assigning only 72 percent of the addresses to census blocks. The percentage of addresses assigned varied considerably across counties (U.S. Census Bureau, 1998a).

[10]However, for school districts that cross county lines (27% of total districts in 1990), it can be difficult to allocate school lunch counts appropriately to the county parts of the district. Such allocation would be required if school lunch data were used to form within-county shares in the model used by the Census Bureau to develop updated estimates of poor school-age children for districts.

Basis for Excluding School Lunch Data from SAIPE

The Census Bureau did not use school lunch data in developing updated estimates of poor school-age children for school districts from its county shares model for two major reasons. First, there is at present no complete and accurate set of school lunch data for all school districts that is readily available at the national level. The National Center for Education Statistics (NCES) obtains school lunch counts as part of its Common Core of Data (CCD) system, in which state educational agencies report a large number of data items for public school systems.[11] The school lunch data are not published and have not been a priority of NCES. The center does not follow up with states when there is no information provided for a school district or to evaluate the accuracy of the reports. Hence, the data are far from complete, and the quality of the data is not established (see National Research Council, 2000c:Ch.7).

On the files provided by NCES to the panel for 1990-1995, the reports of school lunch participants were more than 90 percent complete for only 18 states. The reports were less than 50 percent complete in all 6 years for 10 states; most of these states did not report school lunch data at all. In addition, although states are asked to report counts of students approved for free lunches, it appears that many states report the combined total number of students approved for free or reduced-price lunches, which have different income eligibility limits. Clearly, if school lunch data are to be used to estimate the number of poor school-age children, it would be necessary to make school lunch reporting a priority for follow-up and evaluation in the CCD system.

Second, the Census Bureau does not use school lunch data in developing a consistent set of school district estimates nationwide because the counts of students approved for free lunches differ from poor school-age children in at least three respects and the differences are probably not the same across jurisdictions:

- The eligibility standard to qualify for free lunches is family income that is less than 130 percent of the poverty guideline, which means that students approved for free lunches include near-poor as well as poor children. Children in families with incomes as high as 185 percent of poverty can receive reduced-price lunches.
- Participation in the school lunch program is voluntary and may be

[11]NCES is the only federal agency that attempts to obtain school lunch data for school districts. The Department of Agriculture obtains aggregate counts each October at the state level of the number of children approved for free lunches and reduced-price lunches in both public and participating private schools.

affected by such factors as perceived stigma (it is believed that high school students are less likely to participate than elementary school students for this reason) and the extent of outreach by school officials to encourage families to sign up for the program.

- Students approved for free lunches include children enrolled in participating schools in the district, whereas the Census Bureau is charged to produce estimates of poor school-age children who reside in the district. The two populations differ to the extent that poor resident children attend nonparticipating private schools or schools outside their district (nonresident poor children may also attend schools in the district).[12]

If the relationship between students approved for free lunches and poor school-age children varies across jurisdictions, it would not be possible to use school lunch data to estimate school-age poverty for school districts directly (e.g., by applying a constant factor to the school lunch counts to obtain estimated numbers of poor school-age children). If school district estimates are obtained by suballocating or distributing county-level estimates, as is done in the current county shares approach, then school lunch data could be used in modeling the suballocation if the relationship between school lunch participants and poor school-age children is constant across school districts within counties. However, variations in the relationship within counties would be a problem for such modeling.

There are two other reasons that such modeling could be problematic if school lunch data appeared suitable to use in models for some but not all states and counties. First, there would be practical difficulties for the Census Bureau to collect the data and develop and evaluate different estimation procedures for different sets of school districts, even when it might be possible to improve the accuracy of the estimates in some cases. Second, if the use of different estimation procedures produced estimates with different biases across school districts, there could be a problem of equity for education programs, such as Title I concentration grants, that have a sizable threshold for allocating funds: given a fixed appropriation and a threshold, the allocations to one area can affect the allocations to other areas (see Chapter 6).

Yet the number of students approved for free lunches is an indicator of low income that relates specifically to the population of school-age

[12]The increased numbers of charter schools, which may have ill-defined boundaries that overlap existing school districts, could also make it difficult to relate the number of students approved for free lunches to the number of poor school-age children who reside in a district.

children and is available annually. Moreover, it is not subject to the sampling error that is such a serious problem for school district estimation for indicators based on sample data, such as the census long form and the American Community Survey. Thus, if school lunch data were available and determined to relate in a reasonably consistent manner to school-age poverty across jurisdictions, the Census Bureau could consider using such data to modify its current estimation process. For example, as noted above, it could use school lunch counts instead of 1990 (or 2000) census data to develop within-county shares for school districts to apply to updated estimates from the county poverty model. Or it could consider using a combination of school lunch and census data or school lunch and ACS data (when those become available) to form within-county shares. Alternatively, changes in school lunch counts, instead of shares, could be applied to updated county estimates.[13] Yet another alternative is the possibility of developing a school district poverty model similar to the state and county regression models, and using school lunch counts, or year-to-year changes in those counts, as a predictor variable in the model (assuming comparability of school district school lunch data over counties and states).

Evaluations

The panel undertook a limited evaluation of a school lunch-based shares approach for estimating school-age poverty in two states for which it was able to obtain complete free and reduced-price school lunch data for almost all public schools and assign them to school districts: 1989-1990 for New York and 1990-1991 for Indiana.[14] The analysis compared three sets of estimates of poor school-age children in 1989 for school districts in each of the two states with 1989 estimates from the 1990 census. The three sets were developed by allocating 1990 census county estimates of poor school-age children to school districts using three different methods: (1) a method, similar to the Census Bureau's shares model, in which 1980 cen-

[13]To illustrate, a change model could produce school-district estimates for, say, 2005 by calculating the ratios of school lunch counts from 2005-2006 to the counts in 1999-2000, applying those ratios to 2000 census estimates, and then controlling the sums of the adjusted 2000 census estimates for the school districts (or school district parts) within each county to the 2005 estimates from the county model.

[14]The New York State evaluation was carried out at the State University of New York-Albany by Dr. James Wyckoff, a member of the panel, assisted by Frank Papa (see National Research Council, 2000c:App.D). The Indiana evaluation was carried out at the University of Notre Dame by Dr. David Betson, a member of the panel (see Betson, 1999b).

sus within-county school district shares of poor school-age children were applied to the 1990 census county estimates;[15] (2) a method in which 1989-1990 (or 1990-1991) within-county school district shares of the number of students approved for free lunches were applied to the 1990 census county estimates; and (3) a method in which 1989-1990 (or 1990-1991) within-county school district shares of the combined number of students approved for free or reduced-price lunches were applied to the 1990 census county estimates.

We found that even though the school lunch data pertained to the same year as (or 1 year later than) the 1990 census comparison estimates, neither set of school lunch-based estimates was much more accurate in either state than the estimates that were based on 1980 census data, which were 10 years out of date. Looking at both overall differences and differences for categories of school districts, the use of the number of students approved for free lunches as the basis for estimates of poor school-age children was marginally more accurate than the other two methods that were evaluated.[16]

These results are not encouraging for the use of school lunch data as a consistent measure of poverty for school-age children. However, the finding that free lunch counts are marginally more effective than the previous census for estimating within-county shares of poor school-age children for school districts suggests that it could be worthwhile for the Census Bureau to further evaluate the potential uses of school lunch data for SAIPE (see recommendation 5-3, below). Also, school lunch data are widely used by states as a proxy measure for poverty in allocating state funds and suballocating federal funds to school districts (see Chapter 2), and they carry considerable face validity in that context. Further evaluations by the Census Bureau could thus be helpful not only for the SAIPE Program, but also for other uses of school lunch data.

For further evaluation, the Census Bureau could replicate the panel's analysis for a few more states if there are states other than Indiana and New York for which it is possible to obtain 1989-1990 (or 1990-1991) school lunch counts for school districts. When 2000 census data become avail-

[15]The 1980 census share estimates were not ratio-adjusted, as was done for the 1990 census share estimates (see Chapter 3).

[16]For example, the average absolute difference between the 1990 census estimates of poor school-age children for school districts in New York State and the estimates from each of the three methods, as a percentage of the average number of poor school-age children in the districts, was 23.9 percent for the method that used 1980 census data, 22.3 percent for the method that used free lunch data, and 24.2 percent for the method that used free and reduced-price lunch data.

able, the Bureau could also conduct similar evaluations that compare estimates of school-age poverty for 1999 (instead of 1989). The evaluations could examine the performance of models in which changes in the numbers of students approved for free lunches are used to develop estimates, as described above, as well as the performance of a method in which school lunch data and census data are used in combination to develop estimates of within-county school district shares of poor school-age children.

Because some formula allocations, such as Title I concentration grants, impose a threshold for receiving funds, it is important for the evaluations to include an analysis of the threshold effects. For example, the panel's analysis for New York found that using school lunch data that were not adjusted to county estimates greatly overestimated the number of districts that exceeded the Title I concentration grant eligibility threshold of more than 15 percent or more than 6,500 poor school-age children. The reason for this result is that school lunch counts include children in families with incomes that are near but not below the poverty threshold. Adjusting the school lunch data to add up to county estimates of poor school-age children–that is, using the school lunch data to form within-county shares–greatly improved the accuracy of estimates of districts that were eligible for Title I concentration grants.

The results of a more extensive set of evaluations along the lines suggested could indicate whether the Census Bureau should continue to consider the use of school lunch data for school district poverty estimates. If these data are to be used, a major effort would be needed to improve the reporting of the data to NCES for use by the Census Bureau for estimation purposes.

DATA NEEDS FOR POPULATION ESTIMATES

Uses

The Census Bureau's program of population estimates serves a variety of needs of federal, state, and local government agencies. National-level estimates by age, sex, race, and Hispanic origin are used as controls for weighting the responses to such surveys as the CPS and the Survey of Income and Program Participation (SIPP), and the ACS uses county-level estimates by age, sex, race, and Hispanic origin for weighting. Population estimates are also used as denominators for vital rates (e.g., birth and death rates), and they have extensive uses in fund allocation: currently, $180 billion of federal dollars are allocated to states and other areas by formulas that include population estimates in the formula (U.S. Census Bureau, 1999d; see also U.S. General Accounting Office, 1999).

Fund allocation programs that use poverty estimates from the SAIPE state and county models often require state and county population estimates to convert estimated numbers of poor to estimated proportions of poor and vice versa. This use requires population estimates of persons under age 5 (states only), aged 5-17, under age 18, and total population. The SAIPE poverty models for states require state estimates of total population and persons under age 65 to serve as predictor variables in one or more of the models. State estimates of total population and population by age are also used to convert estimated poverty rates from the state poverty models to estimated numbers of poor. The SAIPE poverty models for counties require county estimates of total population and people under age 18 to serve as predictor variables in one or more of the models.

For school districts, population estimates of children aged 5-17 are needed to convert SAIPE model estimates of numbers poor to proportions poor for determining eligibility for Title I concentration grants. Also needed for Title I allocations are school district estimates of total population–due to a provision in the legislation whereby states can use estimates other than SAIPE estimates to allocate funds to school districts with fewer than 20,000 people.

Future Research and Development

Although evaluations have shown that the population estimates are considerably more accurate than the poverty estimates for counties and school districts and appear to have relatively little effect on the poverty estimates (see Chapter 3), there is still room to improve the population estimates, particularly for school districts. In this section we discuss how improvements in population estimates may be achieved in the next decade either by the use of new data becoming available or by new applications of existing data series (see recommendation 5-4, below).

Administrative records have been the mainstay in the preparation of population estimates for many decades (see Chapter 3), and we discuss possible new uses and improvements in two major administrative sources: tax returns (linked with Social Security data for population estimates by age) and school enrollment data. We also consider possible new roles in the population estimates program of the Master Address File and the American Community Survey.

Tax Returns

Total Population Federal income tax return (form 1040) files are critical for state and county total population estimates because they are used to estimate the intercounty migration component of the county estimates (for people under age 65), which, in turn, are summed to states. About 85-90 percent of the population is covered by the tax files but with significant geographic variation: the lowest state population coverage is about 80-85 percent, but for many small counties, the population coverage averages under 70 percent (Creech and Sater, 1999). The proportion of the population serving as the basis of the estimates is further reduced by the year-to-year matching process used to estimate net migration. Thus, a large proportion of the population is being estimated indirectly by using the migration rates of matched taxfilers as proxies for persons not covered or matched in the tax files. It would likely improve the population estimates if the tax files covered a higher proportion of the population.

A possible approach for improving overall coverage is illustrated by research done at the IRS (Sailar and Weber, 1998), which involved unduplicating files of information documents (Forms 1099 and W-2) and matching them to 1040 forms. Information documents are forms that employers, government agencies, and other organizations are required to file to report income paid to individuals. The Information Returns Master File (IRMF) includes information from many different information documents, the bulk of which (1993 tax year) are Form W-2, wages and salaries (27%); Form 1099-INT[erest] and Form 1099-DIV[idends] (42%); Form 1099-B, sales of capital assets other than real estate (10%); Form 1099-G, government transfer payments, and Form 1099-SSA, Social Security benefits (11%).

The challenge in using the IRMF is to identify the small percentage of forms that relate to people who are not already included on the individual tax returns (Form 1040). The frequency of appearance of a type of 1099 form in the IRMF is no indicator of its importance to improving the population count. For example, there are many more 1099-INT and 1099-DIV forms than there are 1099-SSA forms; however, most recipients of 1099-INT and 1099-DIV forms already file tax returns, whereas many Social Security recipients do not, so the 1099-SSA forms will make a greater contribution to the population count. In Sailer's study, unduplicating and merging the 1099 forms into the IRS 1040 files by using Social Security numbers (SSNs) and other information increased the overall coverage of the population from 85-90 percent to 97 percent, which likely reduced the geographic variation in coverage as well. This magnitude of coverage increase would likely improve the quality of the migration estimates–and, in turn, the population estimates–derived from the tax files. Methods and procedures for regularly using information returns–which amount to some 1 billion documents annually–are yet to be developed, but they warrant the Census Bureau's close attention.

The Census Bureau is planning to conduct research and experimentation as part of the 2000 census on the use of tax return and other administrative records to obtain population and housing information. In a limited set of sites, the Census Bureau will merge and unduplicate several administrative files obtained from other federal agencies, geocode the addresses to census blocks, and compare the block-level population counts to census counts.[17] The Census Bureau will also match the merged administrative records file to the MAF to compare household-level data. The results of this work could lead to improved data for developing population estimates, particularly if files are included that expand coverage of the population beyond what federal agency files are likely to provide. A National Research Council (2000a) panel has recommended that the Census Bureau obtain food stamp files for the areas for which the experiment is to be conducted.

Population by Age At present, tax files are used only to derive migration rates in developing county estimates of total population. However, since SSNs for filers and all dependents are now required on tax returns, it should be possible to generate county estimates by age group using the same methodology as for the total population, assuming that IRS can provide the full file with all the necessary codes to the Census Bureau.[18]

At present, IRS provides SSNs for filers and the first four dependents on each tax return on the file extract furnished to the Census Bureau. If SSNs were provided for all dependents, the Census Bureau could obtain their ages and those of the filers by matching to the Social Security Numident File, which the Bureau now regularly receives and which contains birthdates. With this information, the Census Bureau could develop updated county estimates by age directly instead of using the current raking-ratio procedure in which county age estimates from the previous census are adjusted to agree with updated county total population estimates and updated state population estimates by age. This method would need to be evaluated, including the extent of errors in SSNs, particularly for dependents.

If the information documents (1099 forms) could be merged with the 1040 tax files and the population coverage of the files thereby increased significantly, it would be possible to develop simpler methods with which

[17]Files the Census Bureau plans to use include IRS 1040 tax returns and 1099 information documents, the SSA Numident file, Medicare enrollment records, Selective Service registration files, Department of Housing and Urban Development tenant rental assistance certification files, and Indian Health Service patient registration files.

[18]The Census Bureau is experimenting with such an approach for state population estimates by age; see Chapter 3.

to estimate the population under age 18 for counties by making use of aggregate data on tax returns on the number of child dependents. However, it is not clear to what extent merging information documents with the 1040 tax files will improve coverage of children (rather than adults).

School District Estimates For school districts, there are no indicators of population change that are currently available for use in the population estimation process, either for the total population or for the population aged 5-17. As a consequence, school district population estimates are less accurate than county or state population estimates. The small size of most school districts also makes estimates for them less accurate than estimates for states and counties.

Data from IRS tax files could likely contribute to improved school district population estimates if the individual records could be geocoded to school districts, as they are for other levels of geography. We recommend that the Census Bureau assign high priority to evaluating the extent of geocoding of tax records to school districts that can be achieved with the TIGER system after the 2000 census. Assuming the results are reasonably positive, the Census Bureau should proceed with research to determine how best to use tax records for improved small-area population estimates, as well as improved small-area income and poverty estimates. Research will also be required to determine how best to maintain and improve the geocoding capabilities of TIGER/MAF throughout the decade.

School Enrollment

For many decades, information on school enrollment, both public and private, was an important element of the Census Bureau's population estimation methodology for counties and states. Data on enrollment in the elementary grades were especially useful because school attendance at the relevant ages is compulsory. As a result, the number of children enrolled in elementary school was close to the total population of elementary school-age children, and the relationship between the two numbers was fairly stable over time. This close relationship permitted the development of a methodology (component method 2) to derive relatively reliable net migration rates of the school-age population, which in turn were used to estimate net migration rates of the total population of areas (for a detailed description of the methodology, see U.S. Census Bureau, 1987).

The method was dropped in the 1980s for a variety of reasons, including the disappointing results of evaluations, carried out with 1980 census data, of population estimates that used school enrollment for estimating total migration; the extensive data collection required to obtain reliable data for all counties in the United States; and the deterioration of the relationship between enrollment and school-age population over time, possibly due to the growth of private schools, for which county of residence and area of attendance do not always coincide, and of busing across county lines, among other reasons. However, school enrollment information is still used in estimating the population by age for states (for people under age 65).

In light of the need for estimates of the population aged 5-17 at the county level and the very close relationship between school enrollment and the age group of interest, we encourage the Census Bureau to reexamine the school enrollment approach for developing these estimates, including an assessment of data sources. This methodology should be evaluated as part of the Bureau's 2000 census test program for evaluating population estimates.

School enrollment data may be useful in two ways. They could be used to derive migration estimates to feed into county population estimates for children aged 5-17. They could also perhaps be used directly to measure changes in the distribution of the school-age population among counties within states and among school districts within counties. For this purpose, the U.S. Department of Education's Common Core of Data school enrollment information may be useful, although the data pertain only to public school enrollment. For school districts, it could be possible to estimate within-county changes over time in contrast to the current system of maintaining the relative distribution from the last decennial census.

Master Address File

The Master Address File, the list of addresses on which the 2000 census enumeration is based, will be maintained and updated continuously throughout the decade (see above, "Geocoding with TIGER and MAF"). Sources for updating the MAF, and the associated TIGER geocoding system for assigning addresses to geographic areas, will include the U.S. Postal Service's Delivery Sequence File, input from local communities, and listing operations in selected areas by enumerators for the American Community Survey. A continuously updated MAF will provide a current nationwide inventory of residential addresses and housing units.

The MAF can very likely be used to improve the methods for population estimates in future years. To begin with, it would provide a firm starting point and control for the housing unit method of population estimation, which the Census Bureau currently uses for population estimates for places and other county subdivisions.[19] Beyond that, a more far-reaching application of the MAF for population estimates would be to explore matching administrative records and merging population information onto MAF address records to provide data on the characteristics of the population for areas of interest. For example, it might be possible to develop population estimates by age from such matching operations. As noted earlier, work along these lines is planned as part of the Census Bureau's 2000 census research and experimentation program for use of administrative records.

The ACS might also be able to contribute to improved population estimates. For example, ACS data on vacancy rates, household size, and type of structure, averaged over several years, might be used together with housing unit control counts from the MAF to improve the housing unit estimation method. ACS data on measures of change over time, including migration, could perhaps also augment measures derived from other sources, such as tax files, to improve estimates for the total population and age groups. These and other uses of the ACS for population estimation will require evaluation of such aspects of the ACS as the sampling variability in the estimates and the differences between ACS and census residence rules (see Chapter 4). The census is the basis for carrying forward population estimates, and differences in residence rules could affect the comparability of census and ACS data, particularly for areas with transient populations.

RECOMMENDATIONS

5-1 The Census Bureau and other agencies that produce small-area estimates by using administrative records, such as tax returns and food stamp data, should regularly devote resources to reviewing the quality, comparability, and timeliness of those administrative data for their use in estimation. The review should consider possible changes to administrative records systems that would benefit estimation without undue cost to the data collection agency or undue burden on respondents. For the Census Bureau's small-area models of poverty, it is particularly important to review the interarea comparability of food stamp data before and after the 1996 welfare reform legislation in terms of how these data relate to differences in poverty.

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[19]In this method, estimates of changes since the previous census in the housing stock, derived from building permits and other sources, are combined with census-based estimates of housing vacancy rates and the number of people per housing unit to estimate the change in population for an area since the previous census.

5-2 The Census Bureau should give high priority to enhancing the capabilities of its TIGER/MAF system to geocode addresses from administrative records to small areas. The Bureau should conduct a study, as soon as possible after the 2000 census is completed, of the extent to which TIGER can be used to geocode addresses on IRS tax returns to school districts.

5-3 The Census Bureau should consider conducting evaluations of the possible uses of National School Lunch Program data to develop improved estimates of poor school-age children for school districts.

5-4 The Census Bureau should conduct research on improved data and methods for small-area estimates of total population and population by age. In particular, such research should include:

- ways to improve population coverage in tax return files on the basis of information documents, to use tax returns for estimates of population by age, and to geocode tax returns to subcounty areas;
- reassessment of the usefulness of school enrollment data for county and school district estimates of school-age children; and
- ways to use the Master Address File and, perhaps, the American Community Survey to improve population estimates.

6

Using Estimates in Allocation Formulas

OVERVIEW

In this chapter we return to the topic of using estimates from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program (or other sources) for program purposes, specifically, for allocation of funds by the use of formulas. The chapter illustrates some problems for allocations that errors in estimates–not only persistent biases, but also random variability across areas and over time–may cause and how some kinds of formula provisions may exacerbate the effects of such errors.[1]

As discussed in Chapter 2, many federal programs include small-area income and poverty estimates as factors in formulas to allocate funds to states or other areas, such as school districts and service delivery areas. Many state programs also allocate funds to substate areas by formulas that use measures related to poverty or income. Typically, such funding formulas are complex: they often include more than one factor in addition to poverty or income, such as population of a certain age, total population, condition of housing stock, or relevant expenditures by the jurisdiction. They also often have other complex provisions, such as thresholds for eligibility, minimum allocation amounts, or hold-harmless require-

[1]See Fellegi (1981) for a discussion of similar issues in the context of whether census estimates should be adjusted for measured undercount for use in allocation formulas.

BOX 6-1
Formula Provisions for Title I Basic and Concentration Education Grants

The Title I program, which supports compensatory education programs to benefit educationally disadvantaged children, currently funds two different types of allocations to school districts–basic grants and concentration grants. The formulas for both types of grants allocate funds to school districts on the basis of their numbers of formula-eligible children: poor school-age children (as estimated by the Census Bureau) and other small groups of children—those in foster homes, in families above the poverty level that receive Temporary Assistance to Needy Families benefits, and in local institutions for neglected and delinquent children. The formulas also take account of state per-pupil expenditures. Both formulas includes thresholds and hold-harmless provisions, as well as state minimum allocation amounts.

Thresholds Basic grants allocate funds to school districts that meet two threshold criteria: at least 10 formula-eligible children and a percentage of formula-eligible children that exceeds 2 percent of their total school-age children. The thresholds for basic grants are low and exclude only about 10 percent of school districts. Concentration grants, in contrast, allocate funds only to school districts with high numbers (more than 6,500) or high proportions (more than 15%) of formula-eligible children: less than half of all school districts are eligible for concentration grants.

Hold Harmless The Title I legislation specified a 100 percent guarantee of the prior year's amount for basic and concentration grants for school year 1996-1997. For later years, it specified a sliding hold-harmless provision for basic grants and no hold-harmless provision for concentration grants. Under the sliding provision school districts with 30 percent or more formula-eligible children are guaranteed at least 95 percent of the prior year's grant; the guarantee is 90 percent for districts with 15-30 percent formula-eligible children and 85 percent for districts with fewer than 15 percent formula-eligible children. For school years 1998-1999, 1999-2000, and 2000-2001, Congress passed legislation providing a 100 percent guarantee for all eligible school districts for both basic and concentration grants. In addition, beginning with the 1999-2000 school year, Congress extended the concentration grant hold-harmless provision to eligibility as well as amounts: that is, any school district that was eligible for a concentration grant in the previous year would continue to receive the amount of that grant even if it was no longer eligible on the basis of the new SAIPE estimates of poor school-age children for school districts.

ments (that jurisdictions receive not less than all or some fraction of their allocation amounts of the preceding year). The Title I education program provides an example of formulas with multiple provisions; see Box 6-1.

Formulas are complex because legislators and other policy makers often seek to satisfy multiple, sometimes conflicting, objectives. For example, they may wish to both target funds to poorer jurisdictions and to provide some funds to as many jurisdictions as possible. They may also wish to respond to changes in short-term need but not cause a disruption by suddenly and sharply cutting back funding to jurisdictions where needs have declined. There may also be a desire to provide incentives to localities to contribute more funding of their own. Budget constraints overlay all of these considerations, further complicating matters.[2]

In considering how to structure fund allocation formulas to satisfy various objectives, it is important to consider the properties of the estimates that will be used for the formula factors and how features of those estimates may interact with formula provisions (see Federal Committee on Statistical Methodology, 1978; National Research Council, 2000b). It should not be assumed that estimates, even if they meet requirements of timeliness, geographic specificity, population specificity, and concept of poverty or income desired, are entirely accurate or unbiased.

Indeed, income and poverty estimates, whether from the decennial census, a household survey, an administrative records file, or a model like those in the SAIPE Program that uses multiple data sources, are just what the term implies: they are estimates that are subject to error. Survey estimates (from the census long form and other surveys) are subject to variability from sampling error. Model-dependent estimates are subject to both model error and sampling error. Income and poverty estimates from all sources are subject to other kinds of error as well. For example, they may exhibit variability due to random reporting errors (e.g., random misreporting of income in a survey or administrative records file). Estimates may also exhibit systematic bias for many reasons: they may be out of date, represent a somewhat different concept from that desired (e.g., participants in a program may not be a good proxy for people in poverty), or not pertain to the specified population group (e.g., estimates for poor children aged 5-17 may not be a good proxy for poor children aged 15-19). Furthermore, the estimates may be biased because of problems in data collection–for example, because people who fail to answer a survey or fill out an administrative form differ systematically from those who respond or because the question wording on a survey consistently elicits underreporting of income.

The extent of error in estimates can almost never be known precisely. Error, too, must be estimated. It is usually straightforward to estimate the sampling variability in direct estimates from a survey, but an estimate of sampling error understates the total variability in the estimates and does

[2]The history of changes to the matching formula for the now-defunct Aid to Families with Dependent Children program illustrates some of the competing goals that legislators often confront (see Peterson and Rom, 1990; see also National Research Council, 1995a:Ch.8).

not address the issue of systematic bias. The magnitude of nonsampling errors is much harder to estimate. Users should require from producers as much information as possible about the error properties of small-area income and poverty estimates and use that information to assess the implications of using alternative estimates for fund allocation. It is particularly important to conduct such assessments when a new allocation program is being developed, an existing formula is being modified, or consideration is being given to changing from one source of estimates to another. Users need to recognize that errors in the estimates may have unintended consequences when they are used with a particular formula specification.

Ideally, users would consider both the benefits and costs of alternative sources of estimates for fund allocation, although it can be difficult to develop and implement an appropriate metric for doing so. It is not straightforward to estimate the costs of producing estimates or of improving their accuracy or other features (e.g., timeliness) that can affect accuracy. For example, it is not clear how much of the costs of collecting the survey and administrative data that are used in the SAIPE Program estimates should be assigned to those estimates. It is also not straightforward to estimate the benefits of improved estimates in terms of the effects on formula allocations. Nonetheless, users should consider the effects of error in estimates on allocations and the costs of alternative ways of reducing error, which may include replacing one set of estimates with another set, improving the accuracy of a given set of estimates, or changing a provision in the formula so that errors in estimates are less consequential for the resulting allocations.

BIAS

As noted in Chapter 2, persistent bias in estimates is of particular concern because it means that, over time, some areas may consistently receive more or less funding than they would with unbiased estimates. Users may determine that, over time, one type of bias is less serious than another (e.g., that it is preferable to use more up-to-date estimates of poor school-age children as a proxy for poor children aged 15-19 instead of using decennial census estimates for the intended age group). That determination should be made, as much as possible, on the basis of careful analysis and consideration of alternatives.

Users should also recognize that some formula provisions may exacerbate the effects of bias in the estimates on fund allocations. For example, if there is a bias such that income is underreported and, hence, poverty is overestimated, and if the allocation formula includes a threshold for eligibility, then the use of biased estimates will likely lead to a larger number of jurisdictions receiving funds than would occur with unbiased estimates. The effect of allocating funds to jurisdictions that are truly not eligible is to reduce the amount of funding that is available for truly eligible districts. This outcome probably occurred for Title I concentration grants in instances when states used school lunch counts to suballocate county amounts to school districts, given that students approved for free or reduced-price school lunches include near-poor as well as poor children.[3]

It is likely that biases will be greater for some types of areas than others. For example, if poverty is consistently overestimated for urban areas and consistently underestimated for rural areas, then urban areas will likely receive a greater proportion of total funding than intended by a formula. Morever, if the formula has a threshold, some rural areas will receive no funding, even though they are truly eligible, and, conversely, some urban areas will receive funds when they truly are not even eligible.

An alternative that policy makers could consider instead of thresholds, particularly when there is reason to suspect bias in the estimates, would be to make fund allocations a smooth function of the estimates. For example, the dollar amount allocated per poor child could increase with the proportion of poor children in the area. In this way, there would be no danger of a poor district receiving nothing, yet funds would still be targeted toward poorer areas.[4]

VARIABILITY

While a persistent and sizable bias is generally of most concern for the use of small-area income and poverty estimates in fund allocation formulas, variability in the estimates, due to sampling error and other sources, can have unintended effects on allocations as well. Panel members conducted simulations to illustrate the effects of variability in estimates on fund allocations under several different scenarios.

The analysis by panel members Alan Zaslavsky and Allen Schirm (reported in the Appendix) was originally prepared for a workshop on methodological issues for the planned American Community Survey

[3]The formula for Title I basic grants also includes a threshold, but it is very low (see Box 6-1).

[4]However, high variability in the estimates due to sampling error could reduce the targeting of funds to poorer areas with either the use of a smooth function or a threshold (see Betson, 1999a; see also "Variability," below). As an alternative approach for better targeting of funds to poorer areas, it might be possible to keep a threshold for eligibility and use a different type of estimator that reflects uncertainty in the estimates (see National Research Council, 2000b).

(ACS) (see National Research Council, 2000b). Their work considered the effects of changing from using outdated decennial census estimates to using more current ACS estimates with higher sampling error. The analysis illustrates the problems that variability can cause, particularly when formulas include thresholds and hold-harmless provisions. It also suggests that alternative forms of estimates (such as moving averages) may reduce variability and be as effective as hold-harmless requirements in cushioning areas against sharp declines in funding. The analysis does not strictly apply to model-dependent estimates, such as those in SAIPE; nonetheless, the general conclusions are likely to hold.

Panel member David Betson (1999a) conducted related analyses that further illustrate the unintended consequences that variability in estimates can have on fund allocations. Some of these analyses are summarized below (see "Illustrative Scenarios") .

Census Versus More Current Survey Estimates

Traditionally, the decennial census long-form survey has supplied many of the income and poverty estimates used in allocation formulas. Census estimates have the advantage of comparatively small sampling error for many areas, although even census estimates have high sampling error for very small areas, such as many school districts. Census estimates are subject to other kinds of variability and to bias from several sources, including bias for annual allocations because the census measures poverty and income only at 10-year intervals although income and poverty can change markedly over shorter periods.

The use of census data in funding formulas provides a fixed stream of allocations to an area over 10 or more years (assuming no changes in appropriation levels) with no recognition of changes in need during that period. Moreover, even though, in the long run, some areas may receive funding on the basis of census data that is equivalent to the funding they would receive on the basis of their true average income or poverty over the period, this result will almost certainly not occur for all areas. Because a census takes place only once every decade, there may be areas that receive more (less) than their fair long-run share over, say, a 30-year period because their poverty rate in the 3 census years is above (below) their average rate, either due to chance variability or a systematic upward (downward) bias in their census measurements. Also, the use of census estimates in formulas that include hold-harmless provisions at a fairly high level can favor areas that have a higher-than-typical poverty rate (or lower-than-typical median income) in a census year. It could take decades for the allocations for such an area to return to a level that is more appropriate to the area's typical income or poverty level.

Measuring income and poverty more frequently, as is planned for the ACS, can make it possible for allocations to respond more quickly to changes in need. However, household surveys have considerably higher sampling error than the census and are subject to other kinds of error as well (although for some areas the mean square error of the survey estimates may be smaller than that of the census estimates). If separate samples are drawn each year, as is planned for the ACS, then sampling error can be reduced by cumulating the data over more than 1 year to calculate moving averages, but this approach again makes funding less responsive to changes in need.

There may also be other problems with using averages of estimates over time. For example, changes in appropriation levels could mean that the average funding shares received by an area over a long period, calculated by using a weighted moving average of estimates with fixed weights for each year combined with a linear allocation formula (i.e., a formula with no thresholds or other nonlinear provisions), may be larger (or smaller) than the average funding shares that would obtain with annual estimates (see Appendix). When a formula has a substantial threshold (like that for Title I concentration grants), the use of moving averages may also lead to a different allocation than would obtain with annual estimates (e.g., an area that experiences an increase in need in a particular year may not cross the threshold with a moving average). However, the use of moving averages may be advantageous to the extent that localities value continuity of funding. Detailed analysis of these and other situations is needed to fully understand the implications for allocations of various formula provisions and sources of error in income and poverty estimates that are used in formulas.

Illustrative Scenarios

To look at more complex interactions of nonlinear funding formula provisions with variability in estimates of income and poverty, panel members developed several illustrative scenarios for which simulations were run, some of which we summarize here (see also the Appendix). These scenarios necessarily incorporate simplifying assumptions. Yet they call attention to how there can be unintended consequences for allocations due simply to sampling error in the estimates used for formula factors and to certain formula provisions.

The scenarios focused on two types of nonlinear formula provisions: thresholds and hold-harmless provisions. Thresholds are used in some allocation formulas to target areas most in need while meeting a budget constraint, and hold-harmless provisions are used in many allocation formulas to cushion the effects of a decrease in funds due to a decline in measured need (see Box 6-1). Summarized below are three kinds of scenarios: for a single area in a single year, assuming open-ending funding; for a single area for more than 1 year, assuming open-ended funding; and comparisons of open- and closed-ended funding.

Single Area, Single Year, Open-Ended Funding

One scenario (see Appendix:Table A-1) looked at the effects of different levels of sampling error in the direct estimate of a poverty rate on allocations for an area when the formula includes a threshold poverty rate below which the area receives zero funding. If an area's estimated rate exceeds the threshold, it receives funds directly in proportion to the estimated rate. Four different true poverty rates were used in the simulations, two above and two below the threshold rate. These simulations ignore the fact that the allocation for a single area typically depends–at least to some extent–on the allocations for other areas because the total funding amount for a program is usually fixed and not open-ended.

The results showed that the higher the sampling error, the greater is the expected value of the funding that an ineligible area (i.e., with a low true poverty rate) would receive, when it should receive no funding at all with an exact measurement. Conversely, with increasing sampling error, the smaller is the expected value of the funding that an eligible area (i.e., with a high true poverty rate) would receive compared with the amount it would receive with an exact measurement.[5] These results occur because, as a result of sampling error, the estimate for an ineligible area will sometimes lead to it being classified as eligible, and the estimate for an eligible area will sometimes lead to it being classified as ineligible. The negative relationship between sampling error and expected value of funding for an eligible area is not strictly linear: at very high levels of sampling error, the expected value of funding increases again for an eligible area instead of continuing to decline, although it always remains below the allocation that would be received with an exact measurement.

The above results apply only in expectation. The expected value of funding that an area would receive is the average value over the set of values in the simulation. The particular allocation that an area will receive is subject to chance variability: it will be a single value and not the expected value. It is clearly desirable that the variability of the individual

[5] The expected value of funding is the average amount from a large number of simulations for a given level of assumed sampling error. See Fuller (1995) for a mathematical demonstration that is related to this result.

values around the average value not be large. For example, it is problematic for an eligible area to have a sizable chance of receiving no funds or a large amount.

The above results also apply only to a single year. Over time, eligible areas are likely to value continuity in their levels of funding. In this case, they clearly benefit from lower levels of sampling error because the relationship between sampling error and the variability of the expected funding for an eligible area for which the poverty rate does not change over time is linear (see Betson, 1999a).

Overall under this first scenario, as sampling error increases, the sharp cutoff envisioned by the threshold in the formula (zero funds, some funds) is replaced with a relationship that is almost linear between an area's poverty rate (or other measure) and its expected funding. Because smaller areas will have higher sampling error than larger areas for most survey-based estimates, it is more likely that smaller areas, if they are truly ineligible, will incorrectly obtain some funding, or, if they are truly eligible, will obtain less funding than intended by the formula. The relationship of error to size of area is not so clear for model-dependent estimates, for which, in general, errors will tend to vary less across areas than they will for direct survey estimates.

Models may often provide a more cost-effective means of reducing variability than the alternative of paying to increase the sample size in a survey. However, an assessment would be needed of whether the total error (mean square error) is less for model or survey estimates.

Single Area, More than 1 Year, Open-Ended Funding

One scenario with a time dimension (see Appendix:Figure A-1) looked at the effects of different levels of sampling error on allocations over a 4-year period for a single area when the formula includes an 80 percent hold-harmless provision and there is no change in the poverty rate for the area. In this scenario an area receives funds in direct proportion to its estimated poverty rate without a threshold constraint. For an area with high sampling error, there is a considerably higher probability that the area will receive more funding in the second year than it would with an exact measurement and that the area will increasingly benefit from this windfall for years 3 and 4. By decreasing sampling error, the use of a 3-year moving average greatly reduces this effect. The results for another such scenario in which the formula includes both a threshold and a hold-harmless provision were even more pronounced than the results for each provision alone (see Appendix:Table A-2).

Yet other scenarios with a time dimension (see Appendix) looked at allocations for an area experiencing a downward trend in poverty rates and compared the effectiveness of hold-harmless provisions and moving-average estimates in dampening the magnitude of declines in funding from year to year. The results suggested that 3-year moving-average estimates could be as effective as a hold-harmless provision in moderating downswings in allocations.

Open-Ended and Closed-Ended Funding

Betson (1999a) ran simulations for scenarios with open-ended and closed-ended funding formulas and an 80 percent hold-harmless provision. The assumption for the open-ended formula was that additional funds would be appropriated to accommodate any increase needed because some areas received more funds with the hold-harmless provision than they would have otherwise. The results for closed (i.e., fixed) funding showed that the operation of the hold-harmless provision would work against higher poverty areas in comparison with lower poverty areas. The disadvantage for the higher poverty areas was greater with higher sampling error.

Betson's analyses showed that, in general, a higher sampling error, together with a threshold, or (for a closed-ended program) a hold-harmless provision, or both, tended to equalize the funding amount per eligible person (poor child in the Title I program) across areas. This result is counter to the goal of a program, such as Title I concentration grants, that is designed to provide extra funding (beyond the basic grant) to needier areas.

CONCLUSION

The analyses conducted by panel members of the interactions of sampling error in poverty estimates with such provisions of funding formulas as eligibility thresholds and hold-harmless provisions are just a first step in probing all of the issues involved in specifying formulas that can achieve their intended goals. Complicating the problem is that, as we note above, programs generally have multiple and often competing goals that can make it difficult to specify an effective formula even without the added effects of errors in the estimates used for allocations.

Some level of error in estimates is inevitable. While further analysis of the effects of error is needed, the panel's work strongly suggests that policy makers need to take account of expected levels of bias and variability in the estimates that are considered for formulas. Policy makers need to ask analysts to evaluate both alternative formulas and alternative estimates to determine those formula provisions and kinds of estimates that are best able to achieve such goals as targeting funds to more needy areas and avoiding sudden, large funding decreases on local budgets. For example, the panel's work suggests that moving-average estimates could serve the goal of cushioning budgets against fund decreases without misallocating funding as much as a hold-harmless provision. These and other options deserve a full-scale research effort that can inform policy makers about the likely advantages and disadvantages of alternative funding formulas and sources and kinds of estimates to use in them.

The Committee on National Statistics is planning to conduct more work in this area. With the participation of our panel, it held a workshop in spring 2000 on issues in using estimates for fund allocation, and a more intensive study of the interactions of properties of estimates with features of funding formulas is planned to begin this year. We believe such an effort can usefully inform both users and producers of small-area estimates.

7

Recommendations for Producers and Users

Small-area income and poverty estimates are increasingly in demand for important public policy purposes, such as allocation of funds to states and localities. No estimates can satisfy all requirements perfectly or be without error, but for fund allocation and related program uses, it is critical that they meet the highest possible standards with regard to their development and use.

In this concluding chapter we recommend practices that we believe should be followed in the production of small-area estimates, documentation that users of estimates should expect from producers, studies that users should undertake of the effects of estimates on programs, and the need for policy makers to consider carefully the strengths and weaknesses of alternative sources of estimates in selecting which ones to use for fund allocation and other program purposes. Policy makers also need to consider the design of formula provisions as they interact with the properties of estimates.

Our recommendations apply specifically to the model-dependent estimates produced from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program. It seems likely to us that the regularly updated SAIPE estimates will become more widely used for fund allocation–not only as new programs for allocating funds to subnational areas are introduced, but also as existing programs are modified to use the SAIPE estimates in place of outdated census estimates. However, many of our recommendations apply equally to other sources of small-area estimates, including direct estimates produced from surveys or administrative records.

At present, for counties and smaller areas, model-based estimates of income and poverty are generally the only possible source of estimates that are more up-to-date than those from the decennial census. Even when it is possible to produce direct survey estimates by averaging over several years or months, as is sometimes done for states from the Current Population Survey and as is planned for states and smaller areas from the American Community Survey, model-based estimates should be considered by users and may be preferred.

PRODUCTION OF ESTIMATES

The production of model-based estimates (such as those provided by SAIPE) that use multiple data sources and sophisticated statistical techniques is a major effort that includes many operations. These operations include data acquisition and review, database development, geographic mapping and coding of data, methodological research, model development and testing, production of estimates (together with estimates of their error properties), and thorough evaluation and documentation of procedures and outputs. For the estimates to be of the highest quality possible for such important uses as fund allocation, it is essential that the producing agency have adequate staff and other resources for all components of the estimation program.

Below we identify practices that we believe are critically important to follow for each of the major components of a small-area estimation program. In addition, the producing agency should maintain regular contact with key users, so that the estimation program is producing those estimates that are most needed and appropriate within the constraints of available resources.

Input Data

As a matter of routine practice, each time a new round of estimates is prepared, the producing agency should check the input data for errors (e.g., check to see that state food stamp reports look reasonable compared with the previous year's reports and do not have transcription or other errors). Such checking should also include the procedures used to geocode or otherwise assign data to areas for which estimates are to be produced.

A producing agency should regularly review each data source to determine its continued suitability for use in estimation model(s). Such reviews should address the comparability of the data over time and across areas. If changes, such as program changes for administrative records, are determined to affect either temporal or spatial comparability, it will be necessary to carry out research and development to determine if alternative model formulations can still make use of the data or if the particular data source needs to be dropped.

A producing agency should regularly search for possible new data sources and consider pilot efforts as appropriate to establish the value of a new source. The search for new data sources is particularly important because some sources currently being used may change in ways that adversely affect their usefulness for estimation. A producing agency should also identify changes that might be made to existing survey and administrative records sources to enhance the usefulness of the data for modeling, while not adding undue burden for the source agency. Because it may not be easy to gain agreement to make changes to ongoing administrative records systems and surveys, the producer agency should seek the cooperation of users to understand and support the need for change.

Another dimension of data to pursue is timeliness. A producing agency should make efforts to reduce the lag in availability of key data sources so that the lag in releasing estimates can be reduced. Strategies for more timely estimates could include changes to modeling procedures, as well as working with data originators to reduce the time between collection and delivery of data to the producing agency.

Finally, every producing agency should regularly document its use of data sources in estimation models and, to the extent possible, make available assessments of the effects of each source on the production of estimates. It is particularly important to document the effects on estimates when there is a change in data sources–for example, if the existing Current Population Survey-based SAIPE models are turned into American Community Survey-based models.

Methodological Research: Model Development and Testing

It is important for a producing agency to have resources to carry out research on methods that may improve the estimates in terms of their variability, bias, and timeliness (see Chapter 3). Such research should include provision for early testing of promising ideas in models for which the estimates can be evaluated in comparison with estimates from existing production models. A new model can be crude for this purpose; the intent is to learn early on if improvements from a new model appear substantial enough to warrant work toward full-scale development. Methodological research and model testing should always be accompanied by documentation and archiving to maintain a record of ideas that were tried but did not work out, ideas that appear promising but need considerably more work, and ideas that appear to be prime candidates for development in the short term.

Evaluation

It is the responsibility of an agency that produces model-dependent estimates to conduct a thorough assessment of them. Every time that a set of production estimates is produced, evaluations should be carried out before the estimates are released. Such evaluations should include checking of input data and software program code to make sure that all specifications were correctly implemented. Such checking is especially important whenever there are changes in the data (which will likely happen each year for which estimates are produced) or the software (which may happen less frequently).

Regular evaluations should include internal evaluations of the model outputs each time that estimates are produced–for example, examining patterns of residuals and other features of regression models (see Chapter 3). Over time, the internal evaluations should focus on identifying consistent biases that may appear for multiple estimation years, and research and development should be directed to understanding and reducing those biases to the extent possible. It is expected that random variation will produce anomalies in estimates in any given year; however, persistent patterns need to be investigated and addressed through such means as trying out alternative model specifications. One-time anomalies, which might be due to a problem with the input data rather than random variation, should also be investigated.

Regular evaluations should include external evaluations to the extent possible, by comparing the production estimates with estimates from other sources (see Chapter 3). Whenever a production model is being revised in its specifications or sources of data, there should be the fullest external evaluation possible, including comparisons with alternative model formulations. In this instance there should also be an internal evaluation of alternative models.

Documentation of Procedures and Evaluations

An integral part of the evaluation effort outlined above is the preparation of detailed documentation, which should cover both the evaluation results and the modeling procedures in sufficient detail to permit replication of the estimates. No small-area estimates should be published without full documentation. Such documentation is needed for analysts both inside and outside the producing agency to judge the quality of the estimates and to identify areas for research and development to improve the estimates in future years.

The producing agency should make arrangements for researchers outside the agency to have access to the input data and models, taking care to address confidentiality concerns. Such access is important to permit independent replication and evaluation.

USE OF ESTIMATES

Users of small-area income and poverty estimates need to ensure that the estimates provided by the producing agency are used effectively and appropriately. Thus, an agency such as the U.S. Department of Education should have an active program to understand estimates and assess their effect on such uses as Title I allocations to school districts.

A user agency should convey its expectations that the producing agency will provide complete, understandable, and timely documentation of the methods for developing estimates and evaluation results to accompany each new release of estimates. The user agency should carefully review the documentation so that it fully understands the properties of the estimates.

A user agency should also regularly undertake studies of the effects of the estimates on fund allocations (or other program uses) that are made of them. Studies of fund allocation effects will require maintaining a database of each year's allocations and having the capability to analyze allocation patterns in relation to program provisions and the type and quality of estimates, including the capability to simulate alternative provisions and estimates. Such studies should help inform policy makers about the operation of formulas and how changes in formulas or the estimates used could achieve the program's goals more effectively. Such studies could also help identify priority areas for improvements in estimates to provide to producer agencies.

For federal funding programs in which states suballocate federal amounts to localities, the responsible federal agency should not only study the effects of estimates on the initial funding amounts determined by the agency, but also review the methods and data used by states for suballocation. At a minimum, the responsible federal agency should regularly collect data on state suballocation amounts, procedures, and sources of estimates. In addition, to the extent possible, the agency should conduct evaluation studies of the effects of state procedures and data on the resulting allocations. (Studies could perhaps subsample states for this purpose.) Such studies may be helpful to the responsible federal agency in developing guidance for use of estimates by states.

Finally, a user agency may find it useful periodically to commission in-depth reviews of the estimates that are used for its programs and possible alternatives to them by individuals or groups not affiliated with the producer or user agency. Such reviews should be carried out not only when the program estimates are dependent on a model, but also when they are obtained directly from a survey or administrative records. A full-scale review should include the strengths and weaknesses of alternative sources of estimates in terms of program requirements for the income or poverty definition, level of geographic and population detail, timeliness, and accuracy (including both bias and variability across areas and over time).

DECIDING TO USE ESTIMATES FOR PROGRAMS

If producing agencies follow good practice in developing, evaluating, and documenting estimates, and user agencies are vigilant in seeking to understand estimates and assess their effects on fund allocations and other program uses, then policy makers will have information with which to periodically reassess the laws and regulations that cover use of estimates for program purposes. As we discuss in Chapter 6 for fund allocation formulas, it is critical that policy makers be aware of the unintended consequences that errors in estimates can have on allocations. It is also important that information about the effects of alternative formula provisions and the kinds and quality of estimates be considered in decisions about how to construct or modify formulas and which estimates to use in them. Because it may be difficult to take account of such information in the heat of debate on particular legislation, it is important for policy makers to commission periodic assessments or take other steps to identify key issues and develop detailed alternatives for consideration in the early stages of crafting new or modified program legislation. Such assessments can also contribute to regular reviews by policy makers of the provisions of existing allocation formulas that use small-area estimates.

APPENDIX

Interactions Between Survey Estimates and Federal Funding Formulas

Alan M. Zaslavsky and Allen L. Schirm

Federal programs that allocate funds to states and localities for the low-income population have typically used estimates from the decennial census in the allocation formula. As one example, the Title I education program historically used census estimates of poor school-age children for allocations; recently, however, the program has used more up-to-date estimates. These estimates are from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) Program, which uses data from the March Current Population Survey (CPS), the census, and administrative records in statistical models. Looking to the future, the American Community Survey (ACS), if it is implemented as planned, will be a source of continuously updated estimates from a large sample of households that could be used in allocation formulas.

The introduction of a new data source for the allocation of federal funds to states and localities can affect allocations substantially, for two reasons. First, the new data source may measure a concept differently from previously used sources. For example, the CPS and the decennial census long form find different levels and distributions of poverty (National Research Council, 2000c:Ch.3). Such differences may be consequences of differing survey items, modes of administration, survey protocols, and other details of survey design, and are particular to each survey. Second, even if two surveys provide unbiased estimates of the same quantity, statistical characteristics of the surveys may differ. Among the relevant statistical characteristics are the distributions of errors and the frequency of the survey.

In this paper we consider the second of these issues by drawing out some of the potential implications of introducing a new survey, such as the ACS, for calculation of fund allocations. Our intent is to address some general characteristics of federal funding formulas and the ways in which they might be affected by a shift to a new data source that provides sample data on a continuous basis. We do not attempt to predict the effects of using the ACS on particular units or to assess quantitatively the potential effects of use of the ACS.

We begin by discussing some of the data sources and estimation approaches that are currently used for distribution of federal program funds. We then describe generic features of funding formulas and some potential anomalies inherent in applying the current formulas to sample data. We illustrate these anomalies with simulations. Finally, we argue that when data sources change, properties of the formulas change as well; consequently, consideration should be given to modifying the formulas in light of the original objectives for which they were designed.

Our paper was originally developed for a workshop on the American Community Survey, sponsored by the Committee on National Statistics, in September 1998 (see National Research Council, 2000b). However, the analysis applies not only to the use of estimates from the ACS, but also to the use of estimates from any survey.

DATA SOURCES AND ESTIMATION APPROACHES

Funding formulas typically require estimates of numbers of people who are eligible to receive a benefit distributed through some intervening agency. For example, the number of children in certain age ranges that are in low-income families is required for calculation of grants to states for the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) or for distribution of Title I education aid. The number of low-income children who are uninsured is required for estimates of need for the State Children's Health Insurance Program (SCHIP) initiative. The fraction of a population that falls into the eligible category may also be important for determining where need is concentrated. Hence, estimates of the total population in a broad category (usually by age, such as the number of children), the number falling into an eligibility category within that population (such as the number of poor children), and the fraction of the population falling into the eligibility category (such as the poverty rate among children) are all potentially of interest.

Estimates of total population are derived from the most recent census, updated to the present year by the use of administrative records. These demographic estimates are subject to some error, especially for relatively small areas and towards the end of the postcensal decade. Still, comparisons made in the SAIPE program at the Census Bureau suggest that error from this source is smaller than that due to estimation of eligibility rates and numbers (National Research Council, 2000c:Ch.8).

Estimates of eligible population are based on the census, survey data, and possibly auxiliary data sources. Estimation procedures may be simple and direct or quite complex. For example, before the 1997-1998 school year, Title I education funds were distributed to counties on the basis of the last decennial census, and so allocations were only updated once each decade (apart from minor adjustments due to school district boundary changes and updating of the small part of the counts, such as children in institutions for neglected and delinquent children, based on noncensus data). Since then, however, state and county estimates of children in poverty have been estimated using a complex empirical Bayes model fitted to CPS data, in which decennial census estimates appear as a covariate along with income tax poverty and nonfiling rates and numbers of food stamp recipients. (School district estimates are developed by applying the proportions of poor school-age children in each school district within a county from the 1990 census to updated estimates from the county model.)

Even the CPS data that are inputs to the model are not simply annual estimates, but instead are cumulated (averaged) over a 3-year period, centered on the reference year, for the county small-area estimation model. CPS data are sparse for all but the largest states and counties, and the models that are used only imperfectly fit the data. Nonetheless, assessments by the Census Bureau and by the panel (National Research Council, 1998, 1999) concluded that the model-based estimates were on the whole superior to those obtained by simply carrying forward rates or shares from the previous decennial census. (For small domains, such as small counties and school districts, sampling error in census long-form estimates may be substantial, perhaps even larger than model error.) Numbers of WIC eligibles by state are calculated using a similar, although more complex, model.

Among the most important perceived advantages of the ACS is that it will provide a relatively dense sample in each year, bridging the gap between the current census long form, with its dense but temporally infrequent sample, and the CPS and other current surveys, which are collected almost continuously but with relatively sparse samples. This feature offers the possibility of developing current estimates using simple models or cumulation procedures. Depending on the size of the target area (and the sampling rate applied there), ACS estimates may be based on simple cumulation of 1 to 5 years of data.

Aside from the purely statistical advantages of such an approach, it may also achieve superior public acceptability because of its apparently greater directness. Direct estimates are usually defined as those based only on data collected within the domain for which the estimates are being made; indirect estimates are those that also use data for other domains. Domains may be defined cross-sectionally (as geographical areas or other parts of the population), temporally, or both. Simple indirect estimators may average over spatial domains (e.g., combining several school districts in a county to estimate a single poverty rate that will be used for all of them) or over time (cumulation over years). More complex indirect estimators include the range of small-area estimation models (Ghosh and Rao, 1994), such as synthetic estimation, regression estimation, and hierarchical Bayes models.

The former Title I estimation method using long-form data was direct for the year of the census. (It was temporally indirect when used in later years.) The new estimation procedure, which uses a regression model fit to a national CPS data set, is indirect. The procedure proposed for adjustment of the 1990 census population counts for states was also indirect (Hogan, 1993). Use of an indirect method for such a high-profile objective was evaluated in hindsight by the Census Bureau as too controversial (Fay and Thompson, 1993), and a decision was made to use only direct estimates at the state level in the procedures for the 2000 census (Schindler, 1998). This decision was reversed after use of adjusted counts for congressional apportionment was prohibited, and current plans call for indirect estimation for most domains.

The cumulation procedures proposed for the ACS are at an intermediate level of directness between those used in Title I estimation before and after the shift to model-based estimates. Geographically they are direct, but temporally they are indirect in that current estimates are based on a collection of temporally distinct domains, namely, populations as they were in the same geographic area in previous years. From a purely statistical point of view, both forms of indirectness raise similar issues of model error. Temporal indirectness of the form found in the ACS, however, can hardly be criticized if it replaces the even more indirect procedure of estimating the present situation from a single previous year (the decennial census year) with no current data.

FUNDING FORMULAS

Formulas for distribution of federal funds to states and substate units can be quite complex. A single program may distribute parts of its funds according to several different formulas. Nonetheless, the issues we are concerned with in this paper can be discussed in terms of a few common features.

Funding formulas typically involve distribution of funds in proportion to a measure of need, such as the number of members of a subpopulation that are in poverty by some standard. Generally, the total pie to be divided is determined by the appropriations for the program, although the level of the appropriation may itself be affected by Congress's perception of total need. Consequently, funding formulas have an aspect of indirectness, in the sense that an increase in allocation to one domain implies a decrease somewhere else, although the effect of each domain's allocation on each other domain is generally small.

Proportional allocation of funds may be modified by hold-harmless provisions and thresholds. A hold-harmless provision limits the amount by which the allocation to a unit can decrease from one year to the next. With a 100 percent hold-harmless provision, no unit's allocation is allowed to decrease. With an 80 percent provision, no unit's allocation may decrease by more than 20 percent in any year. The hold-harmless level may vary from year to year as part of the appropriations process. The hold-harmless level may also depend on some other characteristic of the unit, such as its poverty rate. The rationale for a hold-harmless provision is that it moderates fluctuations in the allocation to each governmental unit, softening the effects of cuts on a unit that has budgeted services in anticipation of an allocation similar to the previous year's. With a high hold-harmless level and static or declining total appropriations, allocations may be essentially frozen regardless of shifts in the distribution of need indicated by more recent data. With growing budgets, the effect of a hold-harmless provision is ameliorated, if the provision is stated in terms of absolute amounts (as is typical), rather than shares of the total amount distributed. For example, if the total budget grows by 5 percent, a 100 percent hold harmless allows a unit's share to fall by almost 5 percent.

A threshold is a minimum level below which a unit is not entitled to receive funds from a program (or a component of a program). A threshold may be an absolute count (e.g., a minimum number of children in poverty) or a rate (e.g,. a minimum poverty rate). A threshold on counts operates to prevent dispersal of funds across small units in which the scale of the local program would be too small to administer effectively or efficiently. A threshold on rates directs funds to units where the relative burden of need is greatest, and the governmental unit is presumably least able to meet it with its own resources.

The allocation provisions described above are illustrated by two important programs: the WIC nutrition program and the Title I compensatory education program. In WIC, allocations are based on state estimates; in Title I, allocations are based on county and school district estimates.

WIC is a federal grant program for states that is administered by the Food and Nutrition Service of the U.S. Department of Agriculture. The program provides nutrition and health assistance services for low-income childbearing women, infants, and children. The current rule for allocating WIC food funds to states became effective on October 1, 1999, and specifies that if there is sufficient funding, each state receives a grant equal to its final prior year grant. Thus, there is a 100 percent hold-harmless provision. (If there is insufficient funding to give all states their prior year grants, each state's grant is reduced pro rata.) After prior year grants have been provided, up to 80 percent of remaining funds are allocated as inflation adjustments. Then, all remaining funds are allocated based on each state's estimated "fair share," that is, its share of the estimated national population of persons who are eligible for the program on the basis of income. Thus, a state with 1 percent of the eligible persons has a fair share of 1 percent of the total available funds, and the dollar amount that is 1 percent of the total is the fair share target funding level. States whose prior year grants adjusted for inflation are less than their fair share targets receive "growth funds." The amount of growth funds received by an "under fair share" state is directly proportional to the difference between the prior year grant adjusted for inflation and the fair share. States with prior year grants adjusted for inflation in excess of their fair share targets do not receive growth funds (unless all the "under fair-share" states decline to accept the full amount of growth funds available).

States' fair shares are calculated from estimates of the numbers of infants and children in families with incomes at or below 185 percent of poverty, the income eligibility threshold for WIC. Beginning with fiscal year 1995, state allocations have been determined from model-based estimates obtained using CPS, decennial census, and administrative records data (Schirm and Long, 1995); the model was revised for fiscal 1996 (Schirm, 1996) and has undergone further development since then. In prior years (under somewhat different allocation rules), state grants were calculated from decennial census estimates. Estimates from the 1980 census were used from the early 1980s until fiscal year 1994, when 1990 census estimates were used.

Title I of the Elementary and Secondary Education Act provides federal funds to school districts for education programs for disadvantaged children. To date, Congress has appropriated funds for two types of Title I grants, basic grants and concentration grants, which totaled about $7 billion and $1 billion, respectively, for the 1999-2000 school year. Through the 1998-1999 school year, Title I funds were allocated to school districts through a two-stage process; the U.S. Department of Education allocated funds to counties, and states suballocated funds to school districts within each county. Direct allocations to school districts began with the 1999-2000 school year, but we describe here the former system.

Allocations are based on the estimated numbers and percentages of school-age children who are poor. The rules for allocating funds are complex and include both hold-harmless provisions and eligibility thresholds. For example, a variable hold-harmless rate pertains to basic grants. A school district is guaranteed at least 95 percent of its prior year grant if at least 30 percent of its school-age children are poor. The guarantee falls to 90 percent if the percentage poor is between 15 and 30 and to 85 percent if the percentage poor is below 15.[1] To receive basic grant funds, a school district must have at least 10 eligible children who constitute more than 2 percent of the district's population aged 5 to 17. To receive concentration grant funds, a district must have more than 6,500 eligible children or more than 15 percent of children aged 5 to 17 who are eligible. Further complicating the allocation process, Title I grants also depend on other factors, such as state average per-pupil expenditures.

Model-based estimates of the numbers and percentages of school aged children who are poor in states and counties were first used to allocate Title I funds for the 1997-1998 school year. These estimates were developed by the Census Bureau from CPS, census, and administrative records data. In prior years, direct estimates from the census were used to allocate Title I funds. Recently, the Census Bureau developed model-based estimates for school districts that have been evaluated (National Research Council, 2000c:Ch.7) and were used in allocating funds directly to school districts for the 1999-2000 school year.

INTERACTIONS AMONG DATA SOURCES, ESTIMATION PROCEDURES, AND ALLOCATION FORMULAS

General Findings

Data sources, estimation procedures, and allocation formulas each play a role in the successive steps of calculation of fund allocations. In practice, the distinction between the roles played by the estimation procedure that generates the inputs to the funding formula and the formula itself can be formal and legalistic because the same calculations often may be positioned either in the estimator or in the formula. For example, the law may specify that allocations are based on a 3-year moving average, and that each year's estimate is based on a single year's data. The same effect is obtained, however, if the formula uses a single year's estimate but

[1]For the 1998-1999, 1999-2000, and 2000-2001 school years, Congress has enacted a 100 percent hold harmless for both basic and concentration grants.

the estimate for that year is calculated (for purely statistical reasons) as a 3-year moving average. For another example, a formula may specify that a school district's eligibility for a category of funds depends on the poverty rate in the district, but if estimates are calculated only for counties and then applied directly to the districts, the effect is the same as if eligibility were calculated at the county level. In that case, developing a capability to estimate poverty rates by district effectively changes the formula. In contrast, some formula provisions do not have natural counterparts in estimation procedures: hold-harmless provisions are common examples.

Keeping this relationship between estimation and formulas in mind, we consider the effect of various choices of formula and estimator under various scenarios for sampling error (determined in part by the size of the domain) and year-to-year patterns in the population value (number or rate) for the target group (e.g., children in poverty). Before setting out detailed scenarios, we note several facts. First, reliance on census data implies that the data will be seriously out of date much of the time. Because of the time it takes to process long-form data, they are about 2 years old by the time they are tabulated, and the reference year of the data is the year previous to the year in which they are collected. Therefore, by the time census data become available, data from the previous census will have been used to allocate funds up to 13 years past the reference year. Analyses of CPS data for Title I allocations suggested that substantial shifts in the geographical distribution of poverty can take place in periods of 3 or 4 years, a finding that should be unsurprising to students of regional business trends. Consequently, reliance on census data implies unresponsiveness to significant short-term regional trends in poverty.

Second, even in terms of long-run averages, reliance on census data is problematical because the census only gives a few widely separated snapshots. For example, over a 30-year period, only three censuses take place, and it would not be surprising if some states happen to have poverty rates at all three censuses that are substantially below their average rates over the 30-year period. Such states would not receive their fair share of allocations, even averaged over the 30-year period. Similarly, a state (or county) could fall below a threshold in a single year that happens to be a census year and, hence, lose its entitlement to funding that it might have obtained if the census had occurred in any other year. In effect, the estimates suffer from small temporal sample size. This problem can be solved only by measuring poverty in more of the intervening years.

Third, the effect of hold-harmless provisions depends on both the frequency with which new data become available and the frequency of reallocation. For example, after new census data become available, shares could be reallocated only once, or they could be reallocated annually, applying a hold-harmless each year, so that a state whose share has fallen would move to its new share through a series of annual steps. With decennial adjustments of allocations and a fairly high hold-harmless level, it may take several decades for a state with a single spike in its poverty rate to return down to allocations appropriate to its more typical level. With annual adjustments, even with a hold-harmless level very close to 100 percent, the cumulative change in allocations over a decade is likely to be larger: for example, 10 decreases of 7 percent are about equivalent to a single decrease of 50 percent. In practice, hold-harmless levels are decided legislatively. Consequently, the actual effect of changing the schedule of recalculation is unpredictable, because Congress may be influenced by the change in the estimation method to set a different hold-harmless level than it would if allocations were adjusted only after each decennial census. (We point out below that the effect of hold harmless is further complicated by the role of sampling error.)

Fourth, if each year's samples are independent, or almost so as in the ACS, then variances can be reduced by cumulation, that is, by calculation of a moving average. Assuming uncorrelated sampling error with equal variances in each year, using a 3-year equally weighted moving average multiplies variances by a factor of one-third (.333). Less obviously, an exponentially weighted moving average using 3 years of data with weights proportional to $0.7^0 = 1$, 0.7^1, and 0.7^2 (at lags 0, 1, 2 years) multiplies variances by a factor of .361, very close to the reduction obtained by equal weighting, while giving greater weight to the most recent data. (The weighting factor of 0.7 might be seen as a compromise value because it reduces the weight on data from 2 years back substantially, to half that of the most recent year, but does not too greatly affect variances.) These results on cumulation do not apply to the CPS because of the positive correlation between annual estimates caused by its rotation group design. Although this design can be exploited to obtain improved estimates of changes, simple cumulation will not reduce variance as much as with an independent design.

Fifth, holding procedures and annual appropriations constant over time, a linear estimation procedure (i.e., a weighted moving average with fixed weights for each lag) combined with a linear formula gives allocations that tend to agree, in the aggregate, with those corresponding to average shares over a long time period. This result follows from the fact that every year is given equal total weight (appearing at each relevant lag) except those close to the beginning or the end of the interval. The premises of this argument are not entirely realistic. Annual appropriations for a program are not constant (in current or constant dollars). Hence, it is inevitable that some states will have the good fortune (or political influence) to be entitled to their largest shares of the pie in the years in which the pie is largest. Such a state will receive an aggregate share over the period that is larger than the average of its annual shares; conversely, another state will receive a smaller aggregate share. Furthermore, it is not evident that "unbiased" aggregates in this sense are a particularly desirable property from the standpoint of fair or efficient allocation, when needs change from year to year. Nonetheless, this result suggests that some of the complexities of the interaction between the estimation procedures and formula arise because one or both is nonlinear.

Illustrations

We now consider some of the more complex interactions among the elements of the allocation process by developing several illustrative scenarios. We assume that allocation is based on a single variable, which may be interpreted as a standardized poverty rate, set on a scale (for simplicity of presentation) for which a typical value is about 1.

We ignore the dependence of allocations on levels in other domains. In practice, each domain is affected by the others because they share a prespecified total appropriation, but this is not important to the illustrations in this section, in which we focus on the *differential* effects on different units. In the next section, we show more rigorously how this form of dependency among domains affects our results.

We simulate annual reallocations over a 4-year period. Each scenario is defined by four elements, drawn from a set of alternatives: sampling standard deviation, estimation method, formula, and population process.

- The sampling standard deviation assumes one of four values: 0.1, 0.25, 0.5, and 1.0. These values may be regarded as corresponding to a moderately large domain, mid-sized domains, and a small domain, defined in terms of sample size. We also consider a domain with no sampling variance, representing a very large domain, as a standard of comparison. We assume that sampling error is normally distributed with a mean of zero. (This is a reasonable approximation for small values of the sampling standard deviation, but not for a value of 1, for which normality would imply a substantial probability of a negative estimate of the rate.)
- The estimation method is a single-year estimate (SINGLE), a 3-year moving average with equal weights (MA3), or a 3-year moving average with weights proportional to 0.7^0, 0.7^1, and 0.7^2 (MAE3).
- The formula has four possibilities: allocation is equal to the standardized poverty rate (PROP); allocation is equal to the rate with an 80 percent hold-harmless provision (HH), meaning that the allocation is the maximum of the current rate and 80 percent of the last allocation; allocation is equal to the rate if it is above a threshold of 1 and 0 if it is below 1

(THRESH); combination of threshold and hold harmless, equal to the maximum of the current rate (or 0, if the current rate is less than 1) and 80 percent of the last allocation (HH-THRESH). In any case we assume that the hold harmless does not affect allocations in the first year.

• For the population process, the population standardized poverty rate is either constant (CONS) at one of several rates, trending upward from .75 to 1.25 (UP) over a 4-year period, or trending downward from 1.25 to .75 (DOWN).

Rather than simulating all possible combinations of these factors, we focus on a few sets of scenarios to illustrate specific points. In many of our simulations, we emphasize the effect of sampling variability on the expected allocation for an area under a particular scenario. Because sampling variability is so much affected by the size of the domain, this approach focuses attention on possible inequities to large or small domains that are otherwise similar—that is, the tendency for one or the other type of domain to systematically obtain disproportionately smaller allocations for a given trajectory of population rates.

Scenario 1: Effects of Sampling Variability with a Threshold

Table A-1 illustrates the effect of sampling variability when there is a threshold and each year is estimated independently (SINGLE, THRESH, CONS, with constant true rates 1.3, 1.1, 0.9, or 0.7). The entries are expected values (averaging over the sampling distribution of the estimates).

TABLE A-1 Results for Scenario (1): Effects of Sampling Variability with a Threshold, Single-Year Estimator

True Standardized Poverty Rate	1.3	1.1	0.9	0.7
Sampling Standard Deviation (SD)	Expected Allocation			
SD = 0 (exact)	1.30	1.10	0.00	0.00
SD = 0.1	1.30	0.95	0.17	0.00
SD = 0.25	1.20	0.81	0.40	0.13
SD = 0.5	1.11	0.84	0.57	0.36
SD = 1	1.19	0.99	0.82	0.65

NOTE: See text for specification of scenario.

In this simulation, as in the others, for each value of truth and standard error, a large number of values (20,000) are drawn from the corresponding normal distribution. The allocation is calculated under the THRESH rule, and then the allocations are averaged. Because each year is independent in this simulation, it suffices to simulate a single year.

Note that with exact information (no sampling variance), each domain receives its proportional allocation if above the threshold, and nothing if below, as required by the funding formula. However, with increasing sampling variance, the below-threshold domains have increasing probabilities of estimates above the threshold and therefore an increasing expected benefit. This effect, of course, kicks in more quickly in domains for which the true rate is just below the threshold, as shown by comparing the last two columns of Table A-1. The situation for above-threshold domains is more complex. With modest amounts of sampling variability, the probability that the sample estimate falls below the threshold, causing the domain to lose all of its funding for the year, becomes large enough to reduce the domain's expected benefit. When sampling variability becomes sufficiently (perhaps unrealistically) large, however, the expected payoff begins to increase again, because the positive errors (which are in theory unbounded) begin to compensate for the negative errors (which are bounded because the payoff is never negative). This increase in expectation is accompanied by a drastic increase in variance, as eligibility for any funding approaches a coin toss (assuming, again unrealistically, that the sampling distribution is symmetrical).

Reading down any column of Table A-1, one can see how changing sampling variance affects the expected payoff to a domain at each value of the "truth." Particularly for true rates close to the threshold, the differences down the column can be large. It is difficult to imagine a rationale for giving an area a larger expected payoff because a decision was made about sample design for a survey that caused that area's rate to be estimated less precisely.

As sampling error increases, the sharp cutoff envisioned in the formula is replaced with an increasingly smooth (ultimately almost linear) relationship between population rate and expected payoff. It is arguable that sharp thresholds in funding formulas are not entirely sensible and that a smoother transition would give more stability and less importance to very small shifts near the threshold. However, smoothing expected payoff around the threshold through sampling noise is a poor way to do this. For areas with substantial sampling variability, the threshold magnifies annual variability in allocations relative to a smooth transition, even though the expected allocation over time is smoothed. Furthermore, the amount of smoothing around the transition is dependent on the design for each area, and the cutoff at the transition is sharpest for areas with small sampling variability.

Scenario 2: Effects of Sampling Variability with a Hold-Harmless Provision

Figure A-1 shows the effect of sampling variability when there is a hold-harmless provision at 80 percent and the underlying standardized population poverty rate is constant at 1 (HH, CONS). Each panel pertains to a different estimator (SINGLE, MA3, MAE3). The solid line in each panel shows the "correct" allocation (based on the true value 1), and the dotted lines show the expected allocations with annual SD = 0.1 (triangle), 0.25 (+), and 0.5 (X). In this simulation, we draw the estimated rates independently in each year (simulating independent sampling). Nonetheless, the calculated allocation is affected, through the hold-harmless provision, by the allocation in the previous year.

Expected allocations in the first year are all equal to 1 because we assume no effect of hold harmless in the first year. In successive years the expectation climbs because the allocation is "ratcheted up"—that is, when it is increased by sampling variability in one year, it cannot decrease very much in the following year. Comparing the three panels, we find that use of a moving-average estimator of the rate greatly mitigates this effect, more than would be expected simply due to the reduction in variance. With a 3-year moving average, the standard deviation of the estimates for the scenario with annual SD = 0.5 is reduced to $0.5/\sqrt{3} = .289$, but the bias in year 4 is reduced to .029 (estimated by simulation), much less than the bias of .057 that is found with single-year estimates with SD = 0.25. This reduction in bias is a consequence of the fact that the 3-year moving-average estimates for consecutive years use data from two of the same years (and one different year at each end), so the series of estimates is positively autocorrelated (i.e., a year with a positive estimation error will tend to be followed by another year with a positive error). Hence, the moving-average estimates are smoother over time than independent annual estimates with the same standard deviation, and big jumps in estimates that trigger the hold-harmless provision are less likely to occur. (See Scenario (5) below for an analysis of this greater smoothness.) This result illustrates that a linear smoothing procedure can give some of the stability that is sought with a hold-harmless provision, without the size-related bias that hold harmless can engender.

The combined effect of a hold harmless and a threshold is even more drastic than the effect of either alone. Table A-2 is comparable to Table A-1 above for the effects of a threshold, but it assumes that there is an 80 percent hold-harmless provision as well. (The results shown are for year

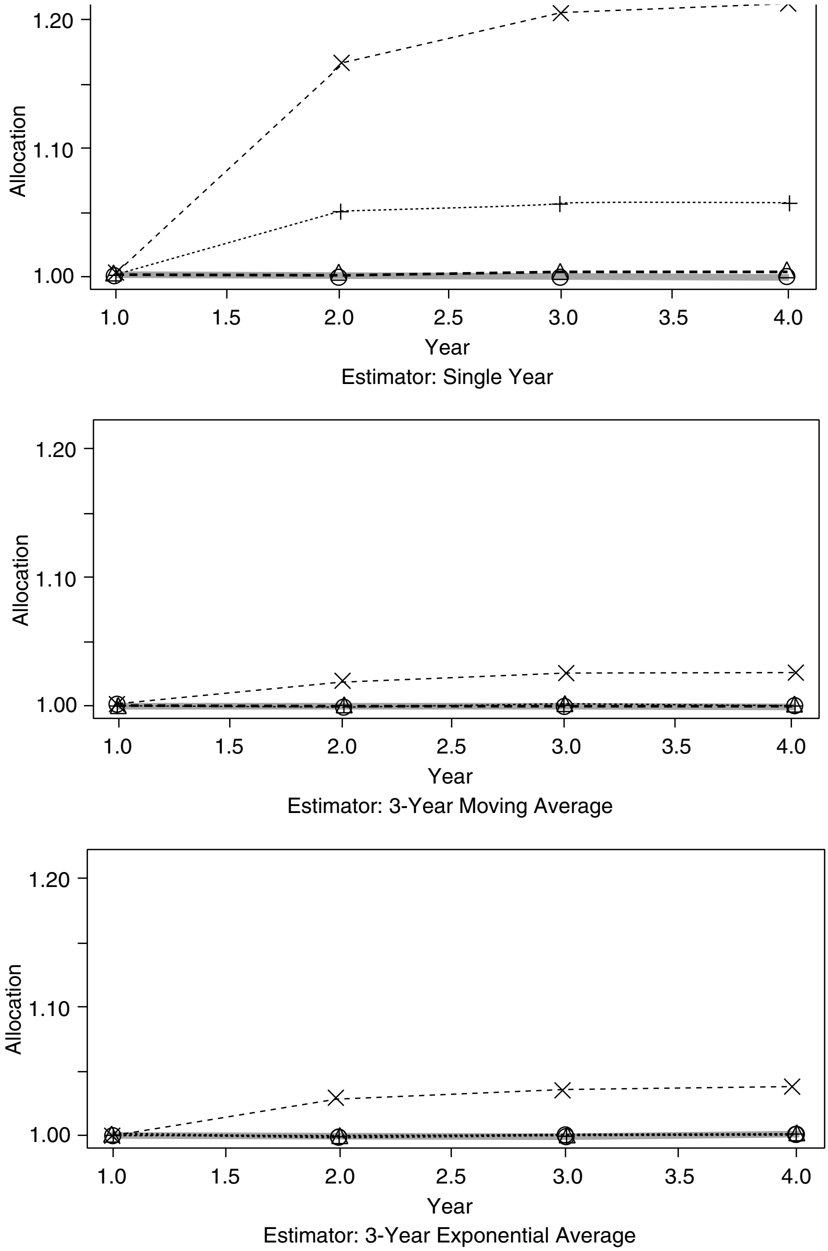

FIGURE A-1 Effects of sampling variability with a constant poverty rate and hold-harmless provision: Correct allocations and three estimation methods. NOTE: Results for scenario (2); see text for details.

TABLE A-2 Results for Scenario (2) (Modified): Effects of Sampling Variability with a Threshold and an 80 Percent Hold-Harmless Provision; Single-Year Estimator

True Standardized Poverty Rate	1.3	1.1	0.9	0.7
Sampling Standard Deviation (SD)	Expected Allocation			
SD = 0 (exact)	1.30	1.10	0.00	0.00
SD = 0.1	1.30	1.09	0.41	0.00
SD = 0.25	1.34	1.12	0.78	0.33
SD = 0.5	1.47	1.27	1.04	0.77

NOTE: See text for specification of scenario.

4, when the effects of hold harmless have approached steady state.) The results are extremely sensitive to sampling variances. Domains for which the actual standardized poverty rate is just below the threshold (set at 1), but that have a large measurement standard deviation, have very high expected allocations relative to what they would have received if there were no measurement error. This result occurs because once a domain goes above the threshold and receives funding, it takes a long time for it to drift down toward zero funding even if its estimates are below the threshold for the following several years.

Scenario 3: Effects of Various Linear Estimation Methods with a Trend

Figure A-2 shows a hypothetical downward trend (solid line) in standardized population poverty rates, assumed to start in year 2 after a period of constant rates, and the expected allocations with three estimation methods: single-year data (SINGLE = triangles), 3-year moving average (MA3 = +), and exponentially weighted MA (MAE3 = X). Sampling standard deviation is not relevant to the calculation of expected allocations in this case: the estimators and formulas are linear, so that adding variability does not affect the expectation of the estimators. As expected, the single-year estimates track (in expectation) the correct allocations, but the moving averages trail them. The exponentially weighted average, because it weights more recent years more heavily, trails slightly less far behind. This result illustrates the bias-variance tradeoff inherent in modeling. Note that as long as "what goes up must come down," the upward bias during a decline is balanced by a downward bias during an increase.

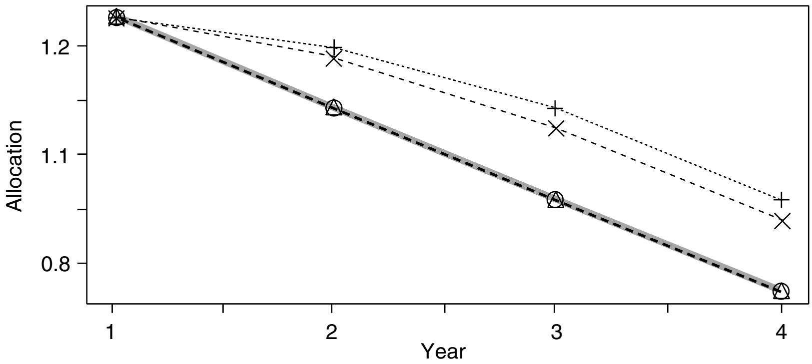

FIGURE A-2 Effects of a downward trend with no hold-harmless provision: Correct allocations and three estimation methods. NOTE: Results for scenario (3); see text for details.

The optimal weighting method (number of years and weights on each lag) depends on sampling variances, the magnitude and pattern of process variability over time, and the importance attached to timeliness and accuracy of estimates.

Scenario 4: Effects of Trends with a Hold-Harmless Provision

Figure A-3 shows a scenario similar to that in scenario (3) but with a hold-harmless provision. The sampling standard deviation is now relevant, and the three values of the standard deviation are labeled as in scenario (2). The effects are a combination of those seen in (2) and (3): moving averages lag behind the trend, and domains with large standard deviations tend to be "ratcheted" upwards.

Figure A-4 shows the same scenarios except with an upward trend in rates. Here, the bias due to hold harmless has been mitigated: with increasing rates, the hold-harmless provision is less likely to have an effect.

Scenario 5: Comparison of Hold Harmless and Moving Average as Methods for Moderating Downward Jumps

In this set of three scenarios, estimates fluctuate around a mean of 1 with SD = 0.5. These fluctuations represent the sum of sampling error and uncorrelated year-to-year variability in the population rate. We compare three approaches to reducing the magnitude of downward jumps from year to year. In the first, an 80 percent hold-harmless provision is applied to annual data with SD = 0.5 (HH). The second is like the first

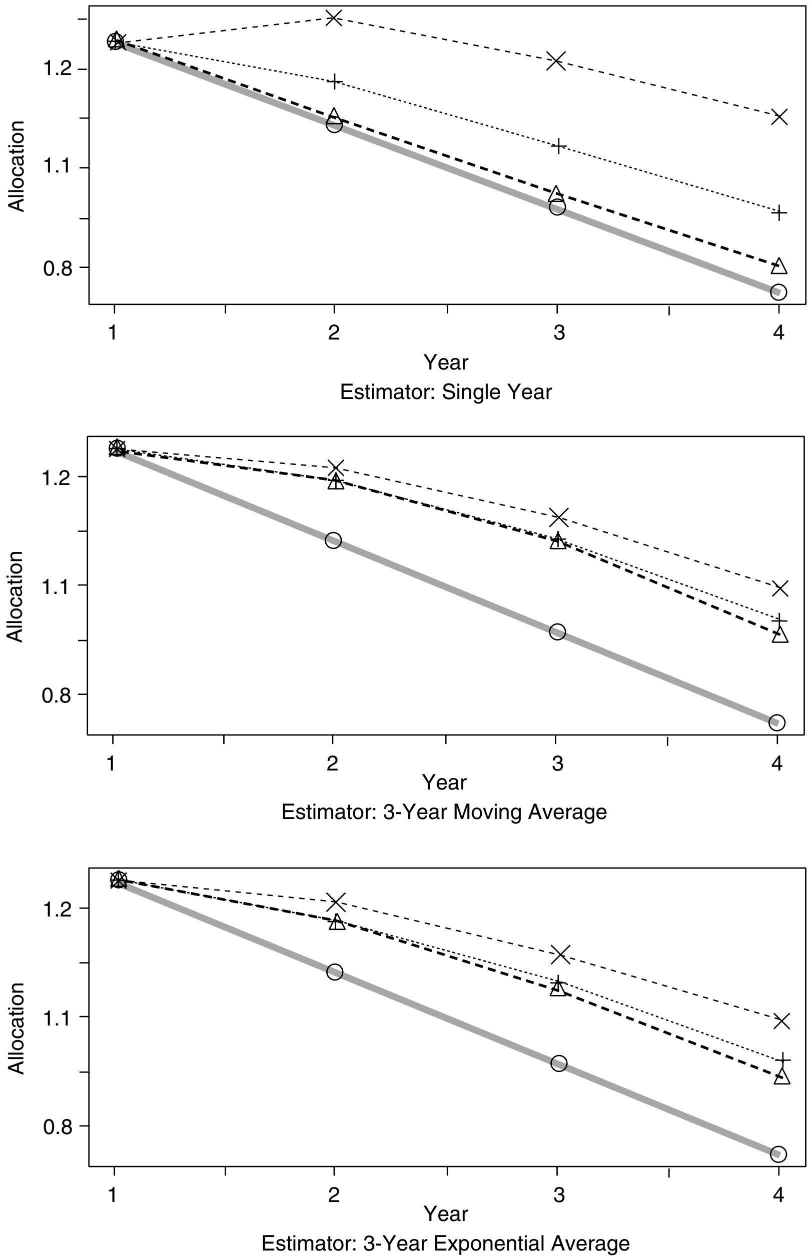

FIGURE A-3 Effects of a downward trend with a hold-harmless provision: Correct allocations and three methods. NOTE: Results for scenario (4); see text for details.

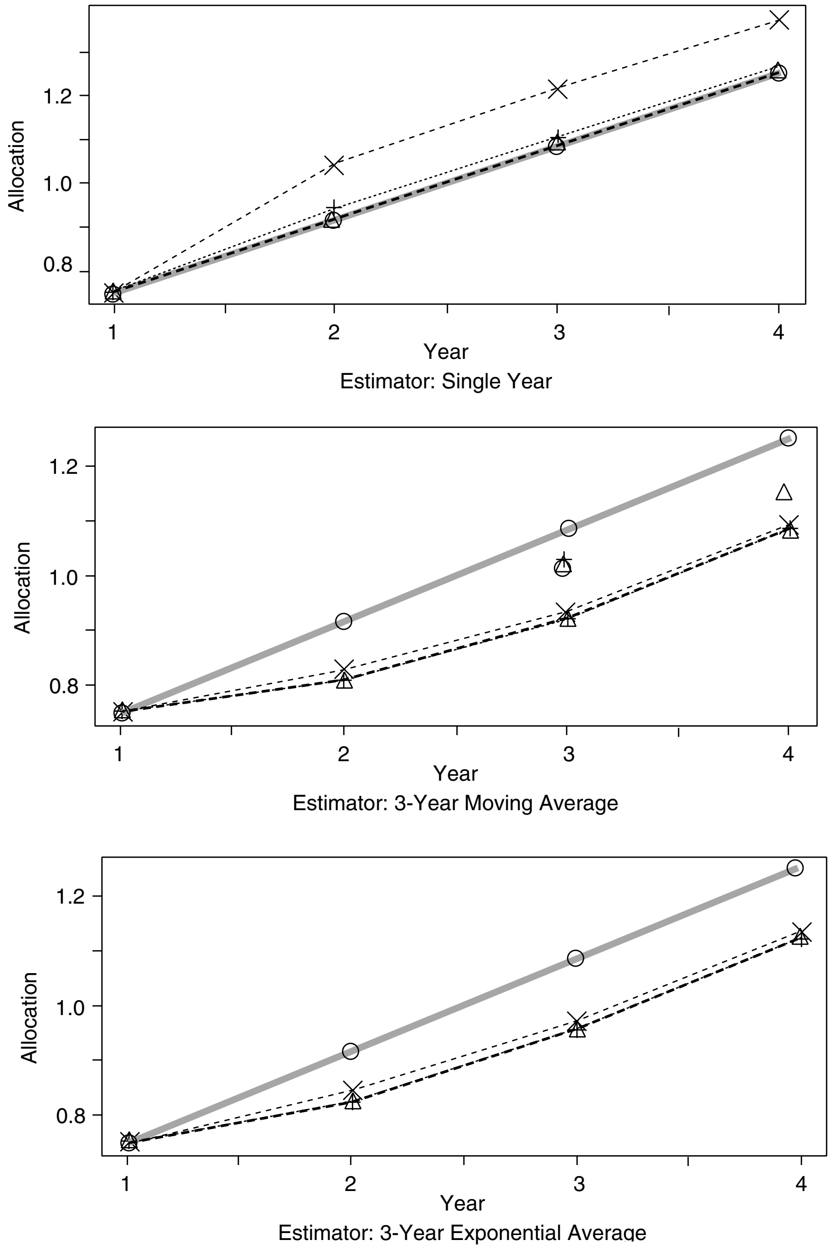

FIGURE A-4 Effects of an upward trend with a hold-harmless provision: Correct allocations and three methods. NOTE: Results for scenario (4); see text for details.

TABLE A-3 Results for Scenario (5): Hold Harmless and Moving Average as Methods for Moderating Downward Jumps in Allocations

Changes in Allocations from Year 3 to Year 4	Estimation Scheme		
	HH	HH(3)	MA3, no HH
Fraction of Changes Down	0.624	0.576	0.500
Mean Down	–0.236	–0.189	–0.188
Mean Up	0.415	0.261	0.188

NOTES: HH, hold harmless; HH(3), hold harmless with sampling standard deviation reduced by $\sqrt{3}$; MA3, no HH, 3-year moving average, no hold harmless; see text for specification of estimation schemes.

except that we assume that the standard deviation is reduced to SD = $0.5/\sqrt{3}$ (HH3). (If variability is entirely due to sampling error, this reduction in the standard deviation could be obtained by multiplying sample size by 3.) The third scenario assumes that a formula without a hold-harmless provision is applied to a 3-year moving average (MA3, no HH) and SD = $0.5/\sqrt{3}$, the same as that for the second scenario. For evaluation, we look at the changes in allocation from year 3 to year 4, when the hold harmless has almost reached steady state. We calculate the fraction of changes that go in the downward direction, the mean of those changes, and the mean of the changes in the upwards direction; see Table A-3.

As expected, the moving average is equally likely to go up or down in the absence of hold harmless. The asymmetry of the hold harmless leads to more downward than upward shifts: because the downward shifts are limited in magnitude, there must be more of them. Another way of explaining this effect is that the upward bias of the hold harmless with a large standard deviation means that the current allocation tends to be higher than the long-run mean rate and will take more downward than upward steps.

Comparing the mean magnitude of the steps, we find that in the realistic comparison of the first and third columns of Table A-3, both the downward and upward steps engendered by the hold-harmless provision are larger on the average than those engendered by a moving-average estimator with a proportional formula. Even the second column (representing a somewhat unrealistic scenario, since it assumes that an expansion of sample size could be afforded) has downward changes no smaller than those obtained with a moving average. This result suggests that use of a moving average can be as effective as a hold-harmless provision in moderating downward swings in allocations. The cost of using a moving average, however, is that it is less responsive than a single-year estimate to upward jumps in the rate; such sensitivity might be valued if one of the purposes of the allocation formula is to be responsive to rapidly rising needs.

EFFECTS OF A FIXED GLOBAL BUDGET FOR ALLOCATIONS

The preceding simulations have been based on the assumption that each area's fund allocation is independent of those received by all other areas. Often, this assumption is unrealistic. A common situation is that in which there is a fixed global budget for a program, so that the funding of each domain is dependent on the "demand" for funding of each of the other domains. On the surface, this appears to be the case for programs such as the Title I education program. We must note, however, that the assumption of a fixed global budget may also be an oversimplification, since Congress may respond to an increased demand for funds—due to increasing poverty rates—by increasing the total amount available for distribution. Congress may also increase the total amount when reallocation of a fixed global budget would reduce funds to some areas by more than it can collectively tolerate, even if poverty rates have not increased on average. For the analysis in this section, nonetheless, we assume a fixed global budget.

In addressing the effects of the interactions among allocations to different areas, it is critical to note that they are mediated through some parameters of the fund allocation formula. For example, suppose that a globally budgeted amount is distributed among domains in proportion to the number of individuals who fall under a criterion of need. If the population eligible for aid is overestimated in some area (holding estimates for other areas constant), the amount distributed per eligible person (the key parameter of this funding formula) would be driven down, which would affect the allocations for other areas. In general, if the number of areas is large, the aggregated magnitude of the effects on allocations due to applying a nonlinear formula with imprecise data may be close to its expectation, simply because it is the average of contributions from a large number of areas. Hence, it may be highly predictable from mathematical calculations or simulations of bias, such as those illustrated in the previous section. The total effect of sampling error may then be calculated by estimating the effect of these biases on the formula parameter and, consequently, the expected effect on the estimate for the single area of interest.

We now restate this argument using a more formal notation. Let $f(x_i,\theta)$ be the formula allocation for domain i, which has a measurable characteristic x_i related to need if the overall formula parameter is θ. The parameter θ may be something that is calculated in the process of applying a formula in which θ is not specified: for example, if a fixed budget is distributed over a variable pool of recipients, the amount per recipient depends on the number of recipients. For simplicity of presentation, we assume that f is nondecreasing in both x_i and θ: that is, needier areas receive more than they would if they were less needy, and increasing the formula parameter increases (or leaves constant) the amount allocated to each area. Simple illustrations include the following:

(i) $f(x_i,\theta) = x_i\theta$, simple proportional allocation, where x_i is the number in need in the area. In this formula, θ is simply the amount allocated per needy person.

(ii) $f(x_i,\theta) = w_i h(x_i)\theta$, where w_i is a measure of size (e.g., total population), and $h(x_i)$ is a possibly nonlinear function of a rate (e.g., $h(x_i) = 0$ for $x_i < c$, $h(x_i) = x_i$ otherwise, representing a rate threshold for receiving an allocation). We regard w_i as a fixed quantity, which does not need to be included in the formula explicitly. Example (i) is a special case of this class of formulas.

(iii) $f(x_i,\theta) = aw_ix_i$ for $x_i > -\theta$, 0 otherwise, with a a predetermined constant. Suppose again that x_i represents a rate. Then under this formula, the neediest areas, defined as those exceeding a certain threshold rate of need $-\theta$, receive a predetermined allocation a per needy person, while those below the threshold receive nothing. (Note that we use $-\theta$ to maintain the condition that f is increasing in θ.) Here, there is a "floating threshold" in the sense that the threshold (rate) for receiving benefits is determined by the level at which the budget is exhausted.

If x_i is estimated from a sample, the allocation to domain i is $f(x_i + \varepsilon_i, \theta)$, where ε_i is measurement (sampling) error. The statistical sampling distribution of ε_i depends on x_i and some sampling characteristic or characteristics s_i, which one might think of as the sampling standard error of the estimate and perhaps some more complex properties of the error distribution. Finally, suppose that the expected allocation for an area, taking the expectation over the distribution of ε_i given s_i, is $f_s(x_i, s_i, \theta)$. Note that this is essentially the quantity that was studied through the simulations of the preceding section; in particular, we were concerned about the sensitivity of $f_s(x_i, s_i, \theta)$ to s_i.

Given a fixed budget A, the value of θ used in the allocation is determined by the relationship $\sum_i f(x_i + \varepsilon_i, \theta) = A$. If the number of areas is fairly large, we may approximate the sum by its expectation, $\sum_i f_s(x_i, s_i, \theta) = A$. Hence, the expected allocation to domain i, $f_s(x_i, s_i, \theta)$, is affected by the sampling properties for the measurement in that domain and by the effect of sampling properties averaged over other domains.

It is difficult to draw any fully general conclusions about the effect of sampling error on allocations to each area. It is possible to draw fairly general conclusions, however, for allocation formulas of the forms (i) and (ii) above, where θ appears as a proportionality constant in the formula. In that case, the ratio of allocations for any two areas is free of θ; furthermore, the ratio of the ratio of expectations to the ratio of correct allocations is also free of w_i. The latter ratio (for comparison of two domains labelled i, j) is given by

$$\frac{\dfrac{f_s(x_i, s_i, \theta)}{f_s(x_j, s_j, \theta)}}{\dfrac{f(x_i, \theta)}{f(x_j, \theta)}} = \frac{\dfrac{h_s(x_i, s_i)}{h(x_i)}}{\dfrac{h_s(x_j, s_j)}{h(x_j)}},$$

where h_s is defined analogously to f_s. The proportional bias

$$\frac{h_s(x_i, s_i)}{h(x_i)},$$

and the way it is affected by sampling properties s_i, is precisely what the previous simulations studied. Hence, we conclude that for a large class of formulas, the results we have obtained for single areas apply straightforwardly to comparisons of the relative effect of sampling error in different areas. We anticipate that in many situations that do not quite fit the structure of (ii), fairly similar results would nonetheless apply: that is, areas for which the sampling properties of their estimates augment their expected allocations the most with fixed values of θ are also advantaged when they must share a global budget with other areas.

CONCLUSIONS

From a legalistic and formal standpoint, modification of the estimation procedure and modification of the formula are two entirely different enterprises. There are good reasons from the standpoint of the division of labor among the agencies of government to maintain this distinction. In fact, though, the formula, estimation procedure, and data sources are parts of a coherent whole. As pointed out in an example above, the distinction between the estimation procedure and the formula is often entirely arbitrary, an expression of the same calculation with different labels. Given this fact, it would be shortsighted to give attention to estimation and data collection while ignoring formulas. The goal cannot be simply to devise an estimation procedure that replicates allocations that were obtained with outmoded data sources. First, new data may be superior to old data, so that the old system can only be replicated by throwing away valuable information. Second, procedures used with older sources may reflect only the limitations of those data, not an intention to obtain a specific outcome.

As the illustrations suggest, interactions among sampling properties of the data, estimation methods, and funding formulas may produce unanticipated and sometimes undesirable effects. The long-term effects of linear estimators and formulas are fairly predictable. Results of some nonlinear methods, however, may be greatly affected, even on the average and in the long run, by sampling variances. This effect is problematical, because it almost inevitably leads to situations in which larger or smaller units tend systematically to get more than their proportional shares, other factors (poverty rates) being constant. Furthermore, decisions about sample allocation should be made on technical grounds related to optimizing the overall accuracy of the survey, but these decisions have implications for outcomes for specific areas when the outcomes are sensitive to variances. Such a link between methodological choices and outcomes puts the data collection and estimation agencies of government in an untenable position.

Widely used nonlinear allocation procedures include hold-harmless provisions and thresholds. These could be replaced to some extent by estimation and allocation procedures that accomplish some of the same goals but have less paradoxical properties, so their use should be reconsidered. Yet some nonlinear and indirect procedures, such as empirical Bayes estimation, can be shown to produce estimates with improved accuracy relative to direct estimators. Therefore, they are likely to be useful when high-precision direct estimators are not available. Indirect estimators tend to have sampling characteristics (such as variation from year to year) that are less dependent on sample size than those of direct estimators, but they may be affected by model biases that tend to persist over time. Their interaction with allocation procedures needs to be better understood as they become more widely used.

Funding formulas are often ingenious "ad hockeries," hammered out from a political process based on compromise. Although notions of equitable and efficient allocation of resources are implicit in them, they do not, by themselves, define those notions. It is the responsibility of those who generate data and implement formulas, and best understand how they work together in practice, to consider the ways that new procedures and data change a formula's effects and to suggest revisions to formulas that best serve their original objectives.

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NOTE: Many of the cited reports and papers by U.S. Census Bureau staff are available on the Bureau's web site: http://www.census.gov. Other Bureau reports related to small-area income and poverty estimates may be obtained by contacting David Waddington, Division of Housing and Household Economic Statistics, U.S. Census Bureau, Washington, D.C. 20233.

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