THE SURVEY OF INCOME AND PROGRAM PARTICIPATION

EXPLORING CHANGES IN HEALTH CARE COVERAGE USING THE SIPP LONGITUDINAL RESEARCH FILE

No. 41

D. Burkhead and
A. Feldman-Harkins
Bureau of the Census

U. S. Department of Commerce BUREAU OF THE CENSUS

Survey of Income and Program Participation

EXPLORING CHANGES IN HEALTH CARE
COVERAGE USING THE SIPP LONGITUDINAL
RESEARCH FILE

No. 8719

Dan Burkhead and Angela Feldman-Harkins
Bureau of the Census

December 1987

THE SURVEY OF INCOME AND PROGRAM PARTICIPATION

EXPLORING CHANGES IN HEALTH CARE COVERAGE USING THE SIPP LONGITUDINAL RESEARCH FILE

No. 8719

Dan Burkhead and Angela Feldman-Harkins
Bureau of the Census

December 1987

U.S. Department of Commerce BUREAU OF THE CENSUS

TABLE OF CONTENTS

EXPLORING CHANGES IN HEALTH CARE COVERAGE USING THE SIPP LONGITUDINAL RESEARCH FILE

By Dan Burkhead and Angela Feldman-Harkins, Bureau of the Census

INTRODUCTION

The SIPP Longitudinal Research File provides a data base from which changes in health care coverage can be examined and related to labor force participation, separation and divorce, retirement, program participation, etc. This paper presents the findings from the first analysis of health care coverage from the SIPP Longitudinal File. Several important areas are explored. First, a description on the longitudinal file creation and its limitations is given. Second, the survey's questions on health care coverage are described. Third, the health care coverage estimates from SIPP are compared with estimates derived from the Current Population Survey. Finally, estimates of change in employer-provided health insurance coverage and associated changes in other socioeconomic characteristics are profiled.

DESCRIPTION OF THE SIPP LONGITUDINAL RESEARCH FILE

During the period between October 1985 and August 1986 the Bureau of the Census constructed the first longitudinal data file based on the Survey of Income and Program Participation (SIPP). The data file was created by linking together cross-sectional WAVE-file data and then performing a series of longitudinal edits. Longitudinal edits were implemented to improve consistency for a select group of data items and to correct for a small number of errors related to the cross-sectional processing system. The main objective of this first longitudinal effort was to provide a data base for research and evaluation on SIPP data quality and for exploration of the uses of intra-year income, household composition, and work experience data.

The longitudinal research file was developed from the 1984 SIPP household panel. This panel consisted initially of about 19,900 interviewed households (the institutionalized population is excluded from the survey). The panel was divided into four equal-size subpanels, termed rotations. The first rotation was interviewed in October 1983. Subsequent interviews were conducted at 4-month intervals with one rotation being interviewed each month. Hence, by January 1984 each sample household had completed one interview. The interviews for October, November, and December 1983, and January 1984 taken collectively constituted a "WAVE", in this case, WAVE 1. In February 1984 the second interviewing cycle or WAVE 2 began. Monthly interviews continued in this sequence through July 1986.

Since SIPP is a longitudinal survey which attempts to follow persons when they move to new residences the designated sample is not the housing units selected but the members of the sample housing units interviewed in WAVE 1.

Each interview in SIPP contains a basic set of "core" questions covering labor force activities and receipt of income. This core of questions relates to labor force and income during the contiguous four-month period immediately preceding the month of interview. The four-month period is termed the "reference period." In most cases, the core data collection procedures were designed to obtain individual monthly observations for the key data items. Monthly core data were the building blocks used to construct the longitudinal research file.

The longitudinal research file contains data covering a total time period of 12 months for each sample person. This 12-month period varies depending on the

rotation to which the person belonged since a monthly interviewing scheme was used. Approximately one-fourth of the observations pertain to each of the following 12-month periods: June 1983 to May 1984, July 1983 to June 1984, August 1983 to July 1984, and September 1983 to August 1984.

A detailed description of the longitudinal processing procedures can be found in a working paper, "Preliminary Data from the SIPP 1983-1984 Longitudinal Research File," John F. Coder, et. al., Bureau of the Census, U.S. Department of Commerce.

DESCRIPTION OF QUESTIONS ON PRIVATE HEALTH INSURANCE COVERAGE

The SIPP questionnaire includes questions pertaining to the health insurance and medical care coverage of all household members. While each interview contains questions on this subject the manner in which this information is collected varies depending on the type of health or medical coverage. Medicare and Medicaid are two public medical benefit programs covered specifically. Private health insurance is included, with a distinction made between insurance provided through employers (or previous employers) and insurance obtained through other sources. Other questions concerning private health insurance include the type of plan and the proportion of cost paid by the employer, if the plan was provided through an employer. This paper is solely concerned with private health insurance coverage.

Private health insurance coverage data are collected in each interview. The private health insurance coverage is updated independently as no data collected in previous interviews is used. Figure 1 shows the items dealing with private

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage

24a. During the 4-month period, did . . . have group or individual health insurance in . . .'s own name? (Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)	1536	1 ☐ Yes — *SKIP to 24c* 2 ☐ No
ASK OR VERIFY — **b.** Was . . . covered by a health insurance plan in somebody else's name?	1537	1 ☐ Yes 2 ☐ No } *SKIP to Check Item R22*
c. Did . . . have this health insurance plan during the entire 4-month period?	1538	1 ☐ Yes — *SKIP to 24e* 2 ☐ No
d. In which months did . . . have the plan? *Mark (X) all that apply.*	1540 1542 1544 1546	1 ☐ Last month 2 ☐ 2 months ago 3 ☐ 3 months ago 4 ☐ 4 months ago
e. Was . . .'s plan provided through an employer or union (or through a former employer or a pension plan)?	1548	1 ☐ Yes 2 ☐ No — *SKIP to 24g*
f. Did the employer or union (former employer or pension plan) pay for part or all of the cost of this plan?	1550	1 ☐ All 2 ☐ Part x3 ☐ None
g. Was this an individual plan or a family plan?	1552	1 ☐ Individual — *SKIP to Check Item R22* 2 ☐ Family
h. Did . . .'s health plan cover all the persons living here?	1554	1 ☐ Yes — *SKIP to 25* 2 ☐ No
i. Other than . . ., which persons in this household were covered by . . .'s plan?	1556 1558 1560 1562 1564 1566	Person No. ☐☐☐ Name ______ ☐☐☐ ______ ☐☐☐ ______ ☐☐☐ ______ ☐☐☐ ______ x3 ☐ None
CHECK ITEM R22 *Refer to Control Card item 27.* Is . . . the designated parent or guardian of children under 18 who live in this household?	1568	1 ☐ Yes 2 ☐ No — *SKIP to 25*
CHECK ITEM R23 Have each of these children already been identified as members of a family health insurance plan?	1570	1 ☐ Yes 2 ☐ No x1 ☐ DK } *SKIP to 24k*
24j. I have recorded that all of . . .'s children were covered by a health insurance plan — is that correct?	1572	1 ☐ Yes — *SKIP to 25* 2 ☐ No

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage--Continued

24k. Are any of (Which of) . . .'s children (were) covered by a health insurance plan? **(Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)**	1574	x5 ☐ All children OR	
		Person No.	Name
	1576	☐☐☐	
	1578	☐☐☐	
	1580	☐☐☐	
	1582	☐☐☐	
	1584	☐☐☐	
	1586	x3 ☐ None	

health insurance coverage which is asked specifically for all household members age 15 years and over. Coverage of household members under age 15 is derived by asking which household members are covered by insurance policies obtained by adult members.

The update for private health insurance coverage identifies persons having coverage at any time during the 4-month reference period but does not provide a monthly accounting of coverage. This monthly accounting is derived in one of two ways. For persons with private health insurance in their "own name" (policyholders) a questions is asked directly concerning the months of coverage. The months of coverage for all other household members were derived by linking their coverage to the adult household members reporting that their coverage extended to these other household members.

The longitudinal editing process for the private health insurance coverage data was designed mainly to remove a very small number of inconsistencies caused by cross-sectional imputations. In most of these cases the reported coverage statuses (for the 4-month reference period) in two of the three interviews are consistent (the same) with each other but inconsistent with an imputed value in the remaining interview. The edit changed the inconsistent covered status to be consistent (the same) with the two reported values.

The edit of the health insurance covered status required that a post-edit modification be made to the monthly coverage fields. The covered status may have been altered from "covered" to "not covered" or from "not covered" to "covered." If the status was altered to "not covered," all monthly coverage fields for the

individual were modified to indicate this new status. Changing the status to "covered" required that the monthly coverage fields be established. In these cases the monthly status fields for all months of the 4-month reference period were modified to indicate a status of "covered" for the individual. No changes were made to the coverage status of other household members who derived their coverage from this individual even though some may have been justified. Given the small number of cases edited, this should not present a serious problem.

The private health insurance variables on the longitudinal file are structured differently than those on the WAVE files. They do not replicate the detail as collected in the individual 4-month reference periods but have been restructured into three variables; a variable indicating coverage in the person's "own name," a variable indicating coverage in "someone else's name," and a variable indicating if the insurance was obtained through an employer. This last variable applies only to persons with coverage in their own name. We did not attempt to establish covered units, i.e., which household members were covered by which member's policy.

PRIVATE HEALTH INSURANCE COVERAGE

Table 1 contains estimates of persons covered by private health insurance for the SIPP 1983-84 reference period. The household relationship categories apply to month 12. The figures in table 1 on private health insurance are not additive since persons may have been in more than one coverage status during the period.

The data in table 1 show that a total of 189.8 million persons were covered by private health insurance for one or more months during the SIPP 12-month

Table 1. Estimated Number of Persons Ever Covered by Private Health Insurance, Mean Number of Months Covered, and Mean Number of Persons Covered Per Month for 1983-84: SIPP Longitudinal Research File

(Relationship as of Month 12)

Characteristics	Number ever covered (thous.)	Mean number of months covered	Mean number covered per month (thous.)
COVERED BY PRIVATE HEALTH INSURANCE			
Total	189,813	10.9	172,715
Householders	75,087	10.9	68,416
Family	55,952	11.0	51,521
Nonfamily	19,132	10.6	16,895
Other family members	111,505	10.9	101,690
Other unrelated individuals	3,221	9.7	2,609
HAD OWN PRIVATE HEALTH INSURANCE			
Total	103,670	10.2	88,451
Householders	68,428	10.7	61,045
Family	50,185	10.8	45,006
Nonfamily	18,244	10.6	16,039
Other family members	32,764	9.3	25,402
Other unrelated individuals	2,478	9.7	2,003
HAD PRIVATE HEALTH INSURANCE THROUGH SOMEONE ELSE			
Total	99,498	10.2	84,264
Householders	10,903	8.1	7,371
Family	9,317	8.4	6,515
Nonfamily	1,586	6.5	856
Other family members	87,602	10.5	76,288
Other unrelated individuals	993	7.3	605

reference period and that these persons were covered for an average of 10.9 months. Of the total persons with private health insurance coverage, 103.7 million had coverage in their own name for at least one or more months, i.e., these persons were the primary "policyholders." SIPP estimated about 99.5 million persons with one or more months of coverage as a "family" member. The estimates in table 1 of private health insurance based on the SIPP data file are not directly comparable to estimates published from the March CPS because the CPS data are restricted to employer-related insurance coverage for persons (and their dependents) working during the calendar year.

COMPARISON OF ESTIMATES OF EMPLOYER-PROVIDED HEALTH CARE COVERAGE FROM SIPP AND CPS

An examination of SIPP and CPS annual estimates must be accompanied by a brief description of the two data sets and differences that may affect their relationship. Estimates available from the SIPP and CPS are for different, but overlapping time periods. The CPS provides figures for calendar years (1983 and 1984 are applicable in this examination) whereas estimates from the SIPP research file span four 12-month periods each containing months in calendar years 1983 and 1984.

The SIPP estimates of numbers are based on weights reflecting independent estimates of the noninstitutional population as of December 1983. Persons included in the SIPP research file have weights only if they were included in the original sample. In this analysis, persons entering or leaving the sample within the 3 interview periods are not included. Only persons interviewed all 12 months are included.

According to the SIPP research file, 113.4 million persons received wage or salary income. This figure is higher than estimates of wage and salary workers from the CPS for either 1983 or 1984 (see table 2). The SIPP estimates that 69 percent of all wage and salary workers had employer-provided health insurance coverage at some time during the 12-month period. This is about 8 percentage points higher than the 1983 CPS estimate. About 72 percent of male workers and 55 percent of female workers had employer-provided health insurance according to the SIPP research file. Coverage rates by selected characteristics for male and female wage and salary workers appear in tables 3 and 4, respectively. the percent distributions of wage and salary workers covered by employer-provided health insurance are shown in tables 5 and 6 by selected characteristics.

EXAMINING THE LOSS OF EMPLOYER-PROVIDED COVERAGE

A simple tabulation from the SIPP longitudinal data file indicates that about 11.7 million workers who began the year with employer-provided health insurance coverage lost that coverage during one or more of the remaining 11 months. This figure represents 17.2 percent of the 68.5 million workers who had employer-provided health insurance coverage during the first month. The data in table 7 show the composition of this group based on their access to other health insurance and significant changes in work/job activities.

Of the 11.7 million losing employer-provided health insurance, approximately 48 percent experienced no change in employers or in their employment status (see figure 2 for a list of statuses). Since no data are collected on specific reasons for loss of health insurance we can only speculate on the cause of these changes. One important factor is probably response error and confusion

Table 2. Comparison of Number of Persons with Wage and Salary Income Covered by an Employer-Provided Health Care Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Sex

(Numbers in thousands)

Characteristic	SIPP	CPS 1984	CPS 1983
BOTH SEXES			
Number with wage and salary income................	113,408	112,024	108,502
Percent with employer-provided health insurance.	68.6	59.8	61.0
MEN			
Number with wage and salary income................	61,732	59,787	58,443
Percent with employer-provided health insurance.	72.0	66.8	68.4
WOMEN			
Number with wage and salary income................	51,676	52,237	50,059
Percent with employer-provided health insurance.	54.8	51.7	52.3

Table 3. Comparison of Estimates of Number of Male Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total............	61,732	72.0	59,787	66.8	58,443	68.4
Race and Spanish Origin						
White................	54,021	73.2	52,527	67.8	51,569	69.4
Black................	5,975	62.7	5,682	59.4	5,533	60.3
Spanish origin[1]........	4,188	64.5	4,194	54.4	3,400	59.3
Age						
15 to 24 years.........	14,283	36.9	13,333	31.4	13,314	33.1
25 to 34 years.........	17,435	78.2	17,144	74.6	16,459	76.4
35 to 44 years.........	13,050	85.2	12,583	81.9	12,095	83.0
45 to 54 years.........	8,563	87.0	8,480	81.4	8,421	83.7
55 to 64 years.........	6,618	87.9	6,483	79.0	6,447	81.7
65 years and over......	1,784	63.8	1,765	37.7	1,707	39.1
Relationship to Family Householder						
In family..............	52,992	71.6	49,986	66.7	49,226	68.1
Householder..........	36,679	84.5	34,927	79.5	34,565	80.9
Spouse...............	2,881	76.4	1,915	70.8	1,823	69.3
Other................	13,432	35.3	13,145	32.1	12,838	33.6
In subfamily...........	144	39.6	174	32.8	132	53.0
Unrelated individuals..	8,596	75.0	9,627	68.1	9,085	70.4
Weeks Worked						
Worked full time.......	51,408	81.1	51,540	75.0	49,953	77.2
50 to 52 weeks.......	37,863	89.7	39,433	83.6	37,176	85.3
40 to 49 weeks.......	4,325	76.0	3,974	65.4	3,944	69.9
27 to 39 weeks.......	3,695	65.4	2,860	52.0	3,071	62.1
26 weeks or less.....	5,526	36.6	5,272	31.2	5,762	38.0
Worked part time.......	10,311	26.6	8,247	15.6	8,491	17.0

[1]Persons of Spanish origin may be of any race.

Table 4. Comparison of Estimates of Number of Female Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total.............	51,676	54.8	52,237	51.7	50,059	52.3
Race and Spanish Origin						
White.................	44,212	54.2	44,705	51.1	43,176	51.9
Black.................	6,167	57.8	6,122	55.5	5,635	55.4
Spanish origin[1]........	2,932	51.4	2,998	48.6	2,627	51.7
Age						
15 to 24 years.........	12,710	35.4	12,617	31.0	12,251	32.4
25 to 34 years.........	14,527	63.2	14,816	61.1	14,117	61.6
35 to 44 years.........	10,635	58.4	10,885	57.1	10,325	57.1
45 to 54 years.........	7,226	60.6	7,269	59.2	6,965	59.2
55 to 64 years.........	5,193	65.8	5,200	59.8	5,063	60.5
65 years and over......	1,384	45.4	1,449	27.6	1,338	30.3
Relationship to Family Householder						
In family..............	43,955	51.6	43,643	48.9	41,838	49.3
Householder..........	8,374	66.3	7,686	64.1	7,338	64.1
Spouse...............	25,495	53.6	25,882	51.1	25,039	51.6
Other................	10,086	34.0	10,075	31.8	9,461	31.8
In subfamily...........	231	45.9	326	47.5	283	50.2
Unrelated individuals..	7,491	74.1	8,268	66.4	7,938	68.0
Weeks Worked						
Worked full time.......	32,310	74.1	35,629	68.1	33,780	68.7
50 to 52 weeks.......	22,298	84.2	25,319	76.7	24,024	77.2
40 to 49 weeks.......	3,600	73.9	3,299	64.2	2,905	66.2
27 to 39 weeks.......	2,493	57.1	2,408	52.1	2,241	57.3
26 weeks or less.....	3,920	27.3	4,604	31.4	4,610	31.7
Worked part time.......	19,361	22.7	16,608	16.6	16,279	18.1

[1]Persons of Spanish origin may be of any race.

Table 5. Comparison of Estimates of Number of Men 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Percent	Number covered	Percent	Number covered	Percent
Total	44,427	100.0	39,966	100.0	40,004	100.0
Race and Spanish Origin						
White	39,518	89.0	35,599	89.1	35,786	89.5
Black	3,749	8.4	3,376	8.4	3,335	8.3
Spanish origin[1]	2,702	6.1	2,281	5.7	2,017	5.0
Age						
15 to 24 years	5,269	11.9	4,184	10.5	4,402	11.0
25 to 34 years	13,631	30.7	12,791	32.0	12,573	31.4
35 to 44 years	11,118	25.0	10,309	25.8	10,040	25.1
45 to 54 years	7,452	16.8	6,899	17.3	7,051	17.6
55 to 64 years	5,818	13.1	5,119	12.8	5,270	13.2
65 years and over	1,139	2.6	665	1.7	668	1.7
Relationship to Family Householder						
In families	37,920	85.4	33,349	83.4	33,536	83.8
Householder	30,976	69.7	27,775	69.5	27,957	69.9
Spouse of householder	2,201	5.0	1,355	3.4	1,264	3.2
Other relative of householder	4,743	10.7	4,220	10.6	4,315	10.8
In unrelated subfamilies	57	0.1	57	0.1	70	0.2
Unrelated individuals	6,450	14.5	6,560	16.4	6,398	16.0
Weeks Worked						
Worked full time	41,683	93.8	38,678	96.8	38,562	96.4
50 to 52 weeks	33,955	76.4	32,947	82.4	31,711	79.3
40 to 49 weeks	3,287	7.4	2,598	6.5	2,758	6.9
27 to 39 weeks	2,418	5.4	1,487	3.7	1,906	4.8
26 weeks or less	2,022	4.6	1,647	4.1	2,187	5.5
Worked part time	2,744	6.2	1,288	3.2	1,441	3.6

[1]Persons of Spanish origin may be of any race.

Table 6. Comparison of Estimates of Number of Women 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Per-cent	Number covered	Per-cent	Number covered	Per-cent
Total	28,317	100.0	26,998	100.0	26,163	100.0
Race and Spanish Origin						
White	23,948	84.6	22,863	84.7	22,408	85.6
Black	3,563	12.6	3,395	12.6	3,121	11.9
Spanish origin[1]	1,507	5.3	1,458	5.4	1,357	5.2
Age						
15 to 24 years	4,499	15.9	3,915	14.5	3,974	15.2
25 to 34 years	9,182	32.4	9,054	33.5	8,703	33.3
35 to 44 years	6,210	21.9	6,215	23.0	5,896	22.5
45 to 54 years	4,381	15.5	4,304	15.9	4,123	15.8
55 to 64 years	3,416	12.1	3,108	11.5	3,061	11.7
65 years and over	629	2.2	400	1.5	406	1.6
Relationship to Family Householder						
In families	22,661	80.0	21,353	79.1	20,625	78.8
Householder	5,554	19.6	4,930	18.3	4,704	18.0
Spouse of householder	13,673	48.3	13,224	49.0	12,915	49.4
Other relative of householder	3,433	12.1	3,199	11.8	3,006	11.5
In unrelated subfamilies	106	0.4	155	0.6	142	0.5
Unrelated individuals	5,551	19.6	5,489	20.3	5,396	20.6
Weeks Worked						
Worked full time	23,927	84.5	24,246	89.8	23,215	88.7
59 to 52 weeks	18,775	66.3	19,426	72.0	18,547	70.9
40 to 49 weeks	2,659	9.4	2,119	7.8	1,924	7.4
27 to 39 weeks	1,424	5.0	1,255	4.6	1,283	4.9
26 weeks or less	1,069	3.8	1,446	5.4	1,460	5.6
Worked part time	4,390	15.5	2,752	10.2	2,949	11.3

[1]Persons of Spanish origin may be of any race.

Table 7. Workers Losing Employer-Provided Health Insurance by Job or Employment Status Change and Marital Status Change

(Numbers in thousands)

Change status	Total	With continuous overall coverage	Without continuous overall coverage
Total losing coverage........................	11,744	5,582	6,162
Percent................................	100.0	100.0	100.0
With a job or employment status change.......	52.2	34.8	67.9
Without a job or employment status change....	47.8	65.2	32.1
With a marital status change.................	(NA)	2.9	(NA)
Without a marital status change..............	(NA)	97.1	(NA)

Figure 2. Employment Status Recodes for Each Month

1 = With a job entire month, worked all weeks
2 = With a job entire month, missed one or more weeks, no time on layoff
3 = With a job entire month, missed one or more weeks, spent time on layoff
4 = With a job one or more weeks, No time spent looking or on layoff
5 = With a job one or more weeks, Spent one or more weeks looking or on layoff
6 = No job during month, spent entire month looking or on layoff
7 = No job during month, spent one or more weeks looking or on layoff
8 = No job during month, No time spent looking or on layoff

on the part of respondents, i.e., no "real" change took place in health insurance coverage. We suspect that a large proportion of these cases fit in this category. In some cases the employer may have cancelled employee policies. In other cases employees may have cancelled coverage in order to take advantage of a "better" or cheaper policy available from the spouse's employer.

About 48 percent of those workers losing employer-provided health coverage had continuous coverage through other private health insurance, either in their own name or as a family member in a policy obtained by another household member. The remaining 52 percent of these workers were without private health insurance coverage for one or more months.

It is interesting to note that a much higher proportion of workers <u>with</u> continuous overall private health insurance coverage experienced <u>no</u> job or employment status changes than workers <u>without</u> continuous overall coverage (65 percent vs. 32 percent). We suspect that this higher proportion reflects higher response error on the type of coverage for the group with continous coverage. Our hypothesis is that a significant proportion of this group 1) reported changes from a policy in their own name with their employers, to other types of coverage when, in fact, no change took place and 2) reported employer-provided health insurance in their own name in the initial interview incorrectly and corrected this error in a subsequent interview. We believe, therefore, that the estimated number of workers losing employer-provided health insurance (11.7 million) during the year is biased upward significantly.

As another part of our examination of the loss of employer-provided health insurance, we analyzed the relative timing of these losses and changes in job and employment status. These data are summarized in table 8. An estimated 59 percent of the losses in employer-provided health insurance occurred during the month in which the job or employment changed. This rate was much lower for the group with continuous coverage than for the without-continuous-coverage group (43 percent vs. 66 percent). We believe that the weaker association between the timing of these events for the group with continuous coverage is additional evidence of response error problems with this group. In fact, this group may largely be defined by respondents with similar response error problems.

TIMING OF COVERAGE CHANGES

Of the estimated 83.7 million wage and salary workers with employer-provided health insurance, 56.7 million workers (67.8 percent) had coverage through their employer all 12 months. Of the remaining 27.0 million workers, 19.4 million (71.8 percent) changed their private health insurance coverage only between the months that marked the ends of the interview periods (i.e., between the 4th and 5th months or between the 8th and 9th months). This appears to be further evidence of the suspected response errors discussed in the previous section and of recall problems in general. For a further discussion of this phenomenon see "Gross Changes in Income Recipiency from the Survey of Income and Program Participation," Dan Burkhead and John Coder, Bureau of the Census, paper presented at the annual meeting of the American Statistical Association, Las Vegas, Nevada, August 5-8, 1985.

CONCLUSION

The SIPP longitudinal research file provides an opportunity to examine employer-provided private health insurance coverage for individuals through a 12-month

Table 8. Workers With Both Loss of Employer-Provided Health Insurance and a Change in Job or Employment Status by Month of Occurrence for These Events

(Numbers in thousands)

Month of job/employment status change	Total	With continuous overall coverage	Without continuous overall coverage
Total losing employer coverage and having a job/employment status change.................	6,130	1,944	4,186
Percent................................	100.0	100.0	100.0
Same month as coverage change.................	58.7	42.9	66.0
One month before coverage change..............	7.9	6.4	8.7
Two months before coverage change.............	6.2	11.2	3.9
Three or more months before coverage change...	7.3	12.9	4.8
One or more months after coverage change......	19.9	26.7	16.7

period, and to compare those estimates with another source of such data--the March CPS. The SIPP research file estimates a higher percentage of wage and salary workers with employer-provided health insurance coverage than reported in the CPS.

The investigation has uncovered some possible response error which may affect the change in coverage status. Among individuals who lost employer-provided health insurance coverage during the 12-month period, it appears likely that a considerable amount of misreporting occurred. Not only is it probable that the number of persons losing coverage are significantly overreported, but the timing of the changes in coverage are clustered at the breaks between the interview reference periods.

To address this problem an examination of the questionnaire wording might be useful. When a respondent is asked whether he/she had insurance in his/her "own name" it may not be apparent to the respondent that the purpose of the question is to find the primary policyholder. The respondent may think that if a person is covered by the insurance policy then the policy is in his/her name. Perhaps the item might be reworded to emphasize this distinction.

Original copy

U.S. DEPARTMENT OF COMMERCE

August 12, 1987

To : John

From: Dan

Thanks for sending me Dan and Angela's paper on Health Care Coverage using the SIPP research file for consideration as a SIPP Working Paper. I had Cindy T. review it; her comments are attached. She revised a few minor points. When Dan and Angela revise the paper send it back to me. Of course, if they have a problem with Cindy's comments let me know. Thanks.

TRANSMITTAL FORM CD-82A (10-67)
PRESCRIBED BY DAO 214-2

USCOMM-DC 1232-P67

U.S. DEPARTMENT OF COMMERCE

July 13, 1987

To : Cindy

From: Dan Dan

I would like to make this paper a SIPP Working Paper. Would you do a peer review for me?

Thanks!

Attachment

"Exploring Changes in Health Care Coverage Using the SIPP Longitudinal Research File," by Dan Burkhead and Angela Feldman-Harkins

TRANSMITTAL FORM CD-82A (10-67)
PRESCRIBED BY DAO 214-2

USCOMM-DC 1231

10-20-86

REVIEW OF PAPER

TITLE: Health-Care Coverage -SIPP REVIEWER: C. Taeuber

AUTHOR: Burkhead - Feldman/Hoskins DATE SENT:

FOR (conference/journal): EXPECTED RETURN DATE:

DATE RETURNED:

****

REVIEWERS COMMENTS:

Strengths of paper and advice:

(1) Nice paper - well written

(2) Would like to see more discussion of Tables 3 and 4 - for example, I am puzzled by the ~~[illegible]~~ major differences between CPS & SIPP

(3) Are there race-age-sex differences in the change in coverage status? That is, is the "response error" consistent among age-sex-race groups? If yes, strengthens your conclusions. If no . . . . ?

****

AUTHORS RESPONSES:

April 13, 1987

EXPLORING CHANGES IN HEALTH CARE COVERAGE USING THE SIPP LONGITUDINAL RESEARCH FILE

By Dan Burkhead and Angela Feldman-Harkins, Bureau of the Census

INTRODUCTION

The SIPP Longitudinal Research File provides a data base from which changes in health care coverage can be examined and related to labor force participation, separation and divorce, retirement, program participation, etc. This paper presents the findings from the first analysis of health care coverage from the SIPP Longitudinal File. Several important areas are explored. First, a description on the longitudinal file creation and its limitations is given. Second, the survey's questions on health care coverage are described. Third, the health care coverage estimates from SIPP are compared with estimates derived from the Current Population Survey. Finally, estimates of change in employer-provided health insurance coverage and associated changes in other socioeconomic characteristics are profiled.

DESCRIPTION OF THE SIPP LONGITUDINAL RESEARCH FILE

During the period between October 1985 and August 1986 the Bureau of the Census constructed the first longitudinal data file based on the Survey of Income and Program Participation (SIPP). The data file was created by linking together cross-sectional WAVE-file data and then performing a series of longitudinal edits. Longitudinal edits were implemented to improve consistency for a select group of data items and to correct for a small number of errors related to the cross-sectional processing system. The main objective of this first longitudinal effort was to provide a data base for research and evaluation on SIPP data quality and for exploration of the uses of intra-year income, household composition, and work experience data.

The longitudinal research file was developed from the 1984 SIPP household panel. This panel consisted initially of about 19,900 interviewed households (the institutionalized population is excluded from the survey). The panel was divided into four equal-size subpanels, termed rotations. The first rotation was interviewed in October 1983. Subsequent interviews were conducted at 4-month intervals with one rotation being interviewed each month. Hence, by January 1984 each sample household had completed one interview. The interviews for October, November, and December 1983, and January 1984 taken collectively constituted a "WAVE", in this case, WAVE 1. In February 1984 the second interviewing cycle or WAVE 2 began. Monthly interviews continued in this sequence through July 1986.

Since SIPP is a longitudinal survey which attempts to follow persons when they move to new residences the designated sample is not the housing units selected but the members of the sample housing units interviewed in WAVE 1.

Each interview in SIPP contains a basic set of "core" questions covering labor force activities and receipt of income. This core of questions relates to labor force and income during the contiguous four-month period immediately preceding the month of interview. The four-month period is termed the "reference period." In most cases, the core data collection procedures were designed to obtain individual monthly observations for the key data items. Monthly core data were the building blocks used to construct the longitudinal research file.

The longitudinal research file contains data covering a total time period of 12 months for each sample person. This 12-month period varies depending on the

rotation to which the person belonged since a monthly interviewing scheme was used. Approximately one-fourth of the observations pertain to each of the following 12-month periods: June 1983 to May 1984, July 1983 to June 1984, August 1983 to July 1984, and September 1983 to August 1984.

A detailed description of the longitudinal processing procedures can be found in a working paper, "Preliminary Data from the SIPP 1983-1984 Longitudinal Research File," John F. Coder, et. al., Bureau of the Census, U.S. Department of Commerce.

DESCRIPTION OF QUESTIONS ON PRIVATE HEALTH INSURANCE COVERAGE

The SIPP questionnaire includes questions pertaining to the health insurance and medical care coverage of all household members. While each interview contains questions on this subject the manner in which this information is collected varies depending on the type of health or medical coverage. Medicare and Medicaid are two public medical benefit programs covered specifically. Private health insurance is included, with a distinction made between insurance provided through employers (or previous employers) and insurance obtained through other sources. Other questions concerning private health insurance include the type of plan and the proportion of cost paid by the employer, if the plan was provided through an employer. This paper is solely concerned with private health insurance coverage.

Private health insurance coverage data are collected in each interview. The private health insurance coverage is updated independently as no data collected in previous interviews is used. Figure 1 shows the items dealing with private

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage

Question	Item	Response
24a. During the 4-month period, did . . . have group or individual health insurance in . . .'s own name? **(Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)**	1536	1 ☐ Yes — *SKIP to 24c* 2 ☐ No
ASK OR VERIFY — **b. Was . . . covered by a health insurance plan in somebody else's name?**	1537	1 ☐ Yes 2 ☐ No } *SKIP to Check Item R22*
c. Did . . . have this health insurance plan during the entire 4-month period?	1538	1 ☐ Yes — *SKIP to 24e* 2 ☐ No
d. In which months did . . . have the plan? *Mark (X) all that apply.*	1540 1542 1544 1546	1 ☐ Last month 2 ☐ 2 months ago 3 ☐ 3 months ago 4 ☐ 4 months ago
e. Was . . .'s plan provided through an employer or union (or through a former employer or a pension plan)?	1548	1 ☐ Yes 2 ☐ No — *SKIP to 24g*
f. Did the employer or union (former employer or pension plan) pay for part or all of the cost of this plan?	1550	1 ☐ All 2 ☐ Part x3 ☐ None
g. Was this an individual plan or a family plan?	1552	1 ☐ Individual — *SKIP to Check Item R22* 2 ☐ Family
h. Did . . .'s health plan cover all the persons living here?	1554	1 ☐ Yes — *SKIP to 25* 2 ☐ No
i. Other than . . ., which persons in this household were covered by . . .'s plan?		Person No. — Name
	1556	☐☐☐ ______
	1558	☐☐☐ ______
	1560	☐☐☐ ______
	1562	☐☐☐ ______
	1564	☐☐☐ ______
	1566	x3 ☐ None
CHECK ITEM R22 *Refer to Control Card item 27.* Is . . . the designated parent or guardian of children under 18 who live in this household?	1568	1 ☐ Yes 2 ☐ No — *SKIP to 25*
CHECK ITEM R23 Have each of these children already been identified as members of a family health insurance plan?	1570	1 ☐ Yes 2 ☐ No x1 ☐ DK } *SKIP to 24k*
24j. I have recorded that all of . . .'s children were covered by a health insurance plan — is that correct?	1572	1 ☐ Yes — *SKIP to 25* 2 ☐ No

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage--Continued

24k. **Are any of (Which of) . . .'s children (were) covered by a health insurance plan?** **(Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)**	1574	x5 ☐ All children OR	
		Person No.	Name
	1576		
	1578		
	1580		
	1582		
	1584		
	1586	x3 ☐ None	

health insurance coverage which is asked specifically for all household members age 15 years and over. Coverage of household members under age 15 is derived by asking which household members are covered by insurance policies obtained by adult members.

The update for private health insurance coverage identifies persons having coverage at any time during the 4-month reference period but does not provide a monthly accounting of coverage. This monthly accounting is derived in one of two ways. For persons with private health insurance in their "own name" (policyholders) a questions is asked directly concerning the months of coverage. The months of coverage for all other household members were derived by linking their coverage to the adult household members reporting that their coverage extended to these other household members.

The longitudinal editing process for the private health insurance coverage data was designed mainly to remove a very small number of inconsistencies caused by cross-sectional imputations. In most of these cases the reported coverage statuses (for the 4-month reference period) in two of the three interviews are consistent (the same) with each other but inconsistent with an imputed value in the remaining interview. The edit changed the inconsistent covered status to be consistent (the same) with the two reported values.

The edit of the health insurance covered status required that a post-edit modification be made to the monthly coverage fields. The covered status may have been altered from "covered" to "not covered" or from "not covered" to "covered." If the status was altered to "not covered," all monthly coverage fields for the

individual were modified to indicate this new status. Changing the status to "covered" required that the monthly coverage fields be established. In these cases the monthly status fields for all months of the 4-month reference period were modified to indicate a status of "covered" for the individual. No changes were made to the coverage status of other household members who derived their coverage from this individual even though some may have been justified. Given the small number of cases edited, this should not present a serious problem.

The private health insurance variables on the longitudinal file are structured differently than those on the WAVE files. They do not replicate the detail as collected in the individual 4-month reference periods but have been restructured into three variables; a variable indicating coverage in the person's "own name," a variable indicating coverage in "someone else's name," and a variable indicating if the insurance was obtained through an employer. This last variable applies only to persons with coverage in their own name. We did not attempt to establish covered units, i.e., which household members were covered by which member's policy.

PRIVATE HEALTH INSURANCE COVERAGE

Table 1 contains estimates of persons covered by private health insurance for the SIPP 1983-84 reference period. The household relationship categories apply to month 12. The figures in table 1 on private health insurance are not additive since persons may have been in more than one coverage status during the period.

The data in table 1 show that a total of 189.8 million persons were covered by private health insurance for one or more months during the SIPP 12-month

Table 1. Estimated Number of Persons Ever Covered by Private Health Insurance, Mean Number of Months Covered, and Mean Number of Persons Covered Per Month for 1983-84: SIPP Longitudinal Research File

(Relationship as of Month 12)

Characteristics	Number ever covered (thous.)	Mean number of months covered	Mean number covered per month (thous.)
COVERED BY PRIVATE HEALTH INSURANCE			
Total	189,813	10.9	172,715
Householders	75,087	10.9	68,416
Family	55,952	11.0	51,521
Nonfamily	19,132	10.6	16,895
Other family members	111,505	10.9	101,690
Other unrelated individuals	3,221	9.7	2,609
HAD OWN PRIVATE HEALTH INSURANCE			
Total	103,670	10.2	88,451
Householders	68,428	10.7	61,045
Family	50,185	10.8	45,006
Nonfamily	18,244	10.6	16,039
Other family members	32,764	9.3	25,402
Other unrelated individuals	2,478	9.7	2,003
HAD PRIVATE HEALTH INSURANCE THROUGH SOMEONE ELSE			
Total	99,498	10.2	84,264
Householders	10,903	8.1	7,371
Family	9,317	8.4	6,515
Nonfamily	1,586	6.5	856
Other family members	87,602	10.5	76,288
Other unrelated individuals	993	7.3	605

reference period and that these persons were covered for an average of 10.9 months. Of the total persons with private health insurance coverage, 103.7 million had coverage in their own name for at least one or more months, i.e., these persons were the primary "policyholders." SIPP estimated about 99.5 million persons with one or more months of coverage as a "family" member. The estimates in table 1 of private health insurance based on the SIPP data file are not directly comparable to estimates published from the March CPS because the CPS data are restricted to employer-related insurance coverage for persons (and their dependents) working during the calendar year.

COMPARISON OF ESTIMATES OF EMPLOYER-PROVIDED HEALTH CARE COVERAGE FROM SIPP AND CPS

An examination of SIPP and CPS annual estimates must be accompanied by a brief description of the two data sets and differences that may affect their relationship. Estimates available from the SIPP and CPS are for different, but overlapping time periods. The CPS provides figures for calendar years (1983 and 1984 are applicable in this examination) whereas estimates from the SIPP research file span four 12-month periods each containing months in calendar years 1983 and 1984.

The SIPP estimates of numbers are based on weights reflecting independent estimates of the noninstitutional population as of December 1983. Persons included in the SIPP research file have weights only if they were included in the original sample. In this analysis, persons entering or leaving the sample within the 3 interview periods are not included. Only persons interviewed all 12 months are included.

According to the SIPP research file, 113.4 million persons received wage or salary income. This figure is higher than estimates of wage and salary workers from the CPS for either 1983 or 1984 (see table 2). The SIPP estimates that 69 percent of all wage and salary workers had employer-provided health insurance coverage at some time during the 12-month period. This is about 8 percentage points higher than the 1983 CPS estimate. About 72 percent of male workers and 55 percent of female workers had employer-provided health insurance according to the SIPP research file. Coverage rates by selected characteristics for male and female wage and salary workers appear in tables 3 and 4, respectively. the percent distributions of wage and salary workers covered by employer-provided health insurance are shown in tables 5 and 6 by selected characteristics.

EXAMINING THE LOSS OF EMPLOYER-PROVIDED COVERAGE

A simple tabulation from the SIPP longitudinal data file indicates that about 11.7 million workers who began the year with employer-provided health insurance coverage lost that coverage during one or more of the remaining 11 months. This figure represents 17.2 percent of the 68.5 million workers who had employer-provided health insurance coverage during the first month. The data in table 7 show the composition of this group based on their access to other health insurance and significant changes in work/job activities.

Of the 11.7 million losing employer-provided health insurance, approximately 48 percent experienced no change in employers or in their employment status (see figure 2 for a list of statuses). Since no data are collected on specific reasons for loss of health insurance we can only speculate on the cause of these changes. One important factor is probably response error and confusion

Table 2. Comparison of Number of Persons with Wage and Salary Income Covered by an Employer-Provided Health Care Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Sex

(Numbers in thousands)

Characteristic	SIPP	CPS 1984	CPS 1983
BOTH SEXES			
Number with wage and salary income................	113,408	112,024	108,502
Percent with employer-provided health insurance.	68.6	59.8	61.0
MEN			
Number with wage and salary income................	61,732	59,787	58,443
Percent with employer-provided health insurance.	72.0	66.8	68.4
WOMEN			
Number with wage and salary income................	51,676	52,237	50,059
Percent with employer-provided health insurance.	54.8	51.7	52.3

Table 3. Comparison of Estimates of Number of Male Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total.............	61,732	72.0	59,787	66.8	58,443	68.4
Race and Spanish Origin						
White..................	54,021	73.2	52,527	67.8	51,569	69.4
Black..................	5,975	62.7	5,682	59.4	5,533	60.3
Spanish origin[1]........	4,188	64.5	4,194	54.4	3,400	59.3
Age						
15 to 24 years.........	14,283	36.9	13,333	31.4	13,314	33.1
25 to 34 years.........	17,435	78.2	17,144	74.6	16,459	76.4
35 to 44 years.........	13,050	85.2	12,583	81.9	12,095	83.0
45 to 54 years.........	8,563	87.0	8,480	81.4	8,421	83.7
55 to 64 years.........	6,618	87.9	6,483	79.0	6,447	81.7
65 years and over......	1,784	63.8	1,765	37.7	1,707	39.1
Relationship to Family Householder						
In family..............	52,992	71.6	49,986	66.7	49,226	68.1
Householder..........	36,679	84.5	34,927	79.5	34,565	80.9
Spouse...............	2,881	76.4	1,915	70.8	1,823	69.3
Other................	13,432	35.3	13,145	32.1	12,838	33.6
In subfamily...........	144	39.6	174	32.8	132	53.0
Unrelated individuals..	8,596	75.0	9,627	68.1	9,085	70.4
Weeks Worked						
Worked full time.......	51,408	81.1	51,540	75.0	49,953	77.2
50 to 52 weeks.......	37,863	89.7	39,433	83.6	37,176	85.3
40 to 49 weeks.......	4,325	76.0	3,974	65.4	3,944	69.9
27 to 39 weeks.......	3,695	65.4	2,860	52.0	3,071	62.1
26 weeks or less.....	5,526	36.6	5,272	31.2	5,762	38.0
Worked part time.......	10,311	26.6	8,247	15.6	8,491	17.0

[1]Persons of Spanish origin may be of any race.

Table 4. Comparison of Estimates of Number of Female Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total............	51,676	54.8	52,237	51.7	50,059	52.3
Race and Spanish Origin						
White..................	44,212	54.2	44,705	51.1	43,176	51.9
Black..................	6,167	57.8	6,122	55.5	5,635	55.4
Spanish origin[1]........	2,932	51.4	2,998	48.6	2,627	51.7
Age						
15 to 24 years.........	12,710	35.4	12,617	31.0	12,251	32.4
25 to 34 years.........	14,527	63.2	14,816	61.1	14,117	61.6
35 to 44 years.........	10,635	58.4	10,885	57.1	10,325	57.1
45 to 54 years.........	7,226	60.6	7,269	59.2	6,965	59.2
55 to 64 years.........	5,193	65.8	5,200	59.8	5,063	60.5
65 years and over......	1,384	45.4	1,449	27.6	1,338	30.3
Relationship to Family Householder						
In family..............	43,955	51.6	43,643	48.9	41,838	49.3
Householder..........	8,374	66.3	7,686	64.1	7,338	64.1
Spouse...............	25,495	53.6	25,882	51.1	25,039	51.6
Other................	10,086	34.0	10,075	31.8	9,461	31.8
In subfamily...........	231	45.9	326	47.5	283	50.2
Unrelated individuals..	7,491	74.1	8,268	66.4	7,938	68.0
Weeks Worked						
Worked full time.......	32,310	74.1	35,629	68.1	33,780	68.7
50 to 52 weeks.......	22,298	84.2	25,319	76.7	24,024	77.2
40 to 49 weeks.......	3,600	73.9	3,299	64.2	2,905	66.2
27 to 39 weeks.......	2,493	57.1	2,408	52.1	2,241	57.3
26 weeks or less.....	3,920	27.3	4,604	31.4	4,610	31.7
Worked part time.......	19,361	22.7	16,608	16.6	16,279	18.1

[1]Persons of Spanish origin may be of any race.

Table 5. Comparison of Estimates of Number of Men 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Percent	Number covered	Percent	Number covered	Percent
Total	44,427	100.0	39,966	100.0	40,004	100.0
Race and Spanish Origin						
White	39,518	89.0	35,599	89.1	35,786	89.5
Black	3,749	8.4	3,376	8.4	3,335	8.3
Spanish origin[1]	2,702	6.1	2,281	5.7	2,017	5.0
Age						
15 to 24 years	5,269	11.9	4,184	10.5	4,402	11.0
25 to 34 years	13,631	30.7	12,791	32.0	12,573	31.4
35 to 44 years	11,118	25.0	10,309	25.8	10,040	25.1
45 to 54 years	7,452	16.8	6,899	17.3	7,051	17.6
55 to 64 years	5,818	13.1	5,119	12.8	5,270	13.2
65 years and over	1,139	2.6	665	1.7	668	1.7
Relationship to Family Householder						
In families	37,920	85.4	33,349	83.4	33,536	83.8
Householder	30,976	69.7	27,775	69.5	27,957	69.9
Spouse of householder	2,201	5.0	1,355	3.4	1,264	3.2
Other relative of householder	4,743	10.7	4,220	10.6	4,315	10.8
In unrelated subfamilies	57	0.1	57	0.1	70	0.2
Unrelated individuals	6,450	14.5	6,560	16.4	6,398	16.0
Weeks Worked						
Worked full time	41,683	93.8	38,678	96.8	38,562	96.4
50 to 52 weeks	33,955	76.4	32,947	82.4	31,711	79.3
40 to 49 weeks	3,287	7.4	2,598	6.5	2,758	6.9
27 to 39 weeks	2,418	5.4	1,487	3.7	1,906	4.8
26 weeks or less	2,022	4.6	1,647	4.1	2,187	5.5
Worked part time	2,744	6.2	1,288	3.2	1,441	3.6

[1]Persons of Spanish origin may be of any race.

Table 6. Comparison of Estimates of Number of Women 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Per-cent	Number covered	Per-cent	Number covered	Per-cent
Total.................	28,317	100.0	26,998	100.0	26,163	100.0
Race and Spanish Origin						
White........................	23,948	84.6	22,863	84.7	22,408	85.6
Black........................	3,563	12.6	3,395	12.6	3,121	11.9
Spanish origin[1].............	1,507	5.3	1,458	5.4	1,357	5.2
Age						
15 to 24 years...............	4,499	15.9	3,915	14.5	3,974	15.2
25 to 34 years...............	9,182	32.4	9,054	33.5	8,703	33.3
35 to 44 years...............	6,210	21.9	6,215	23.0	5,896	22.5
45 to 54 years...............	4,381	15.5	4,304	15.9	4,123	15.8
55 to 64 years...............	3,416	12.1	3,108	11.5	3,061	11.7
65 years and over............	629	2.2	400	1.5	406	1.6
Relationship to Family Householder						
In families..................	22,661	80.0	21,353	79.1	20,625	78.8
Householder................	5,554	19.6	4,930	18.3	4,704	18.0
Spouse of householder......	13,673	48.3	13,224	49.0	12,915	49.4
Other relative of householder...............	3,433	12.1	3,199	11.8	3,006	11.5
In unrelated subfamilies.....	106	0.4	155	0.6	142	0.5
Unrelated individuals........	5,551	19.6	5,489	20.3	5,396	20.6
Weeks Worked						
Worked full time.............	23,927	84.5	24,246	89.8	23,215	88.7
59 to 52 weeks.............	18,775	66.3	19,426	72.0	18,547	70.9
40 to 49 weeks.............	2,659	9.4	2,119	7.8	1,924	7.4
27 to 39 weeks.............	1,424	5.0	1,255	4.6	1,283	4.9
26 weeks or less...........	1,069	3.8	1,446	5.4	1,460	5.6
Worked part time.............	4,390	15.5	2,752	10.2	2,949	11.3

[1]Persons of Spanish origin may be of any race.

Table 7. Workers Losing Employer-Provided Health Insurance by Job or Employment Status Change and Marital Status Change

(Numbers in thousands)

Change status	Total	With continuous overall coverage	Without continuous overall coverage
Total losing coverage	11,744	5,582	6,162
Percent	100.0	100.0	100.0
With a job or employment status change	52.2	34.8	67.9
Without a job or employment status change	47.8	65.2	32.1
With a marital status change	(NA)	2.9	(NA)
Without a marital status change	(NA)	97.1	(NA)

Figure 2. Employment Status Recodes for Each Month

1 = With a job entire month, worked all weeks
2 = With a job entire month, missed one or more weeks, no time on layoff
3 = With a job entire month, missed one or more weeks, spent time on layoff
4 = With a job one or more weeks, No time spent looking or on layoff
5 = With a job one or more weeks, Spent one or more weeks looking or on layoff
6 = No job during month, spent entire month looking or on layoff
7 = No job during month, spent one or more weeks looking or on layoff
8 = No job during month, No time spent looking or on layoff

on the part of respondents, i.e., no "real" change took place in health insurance coverage. We suspect that a large proportion of these cases fit in this category. In some cases the employer may have cancelled employee policies. In other cases employees may have cancelled coverage in order to take advantage of a "better" or cheaper policy available from the spouse's employer.

About 48 percent of those workers losing employer-provided health coverage had continuous coverage through other private health insurance, either in their own name or as a family member in a policy obtained by another household member. The remaining 52 percent of these workers were without private health insurance coverage for one or more months.

It is interesting to note that a much higher proportion of workers <u>with</u> continuous overall private health insurance coverage experienced <u>no</u> job or employment status changes than workers <u>without</u> continuous overall coverage (65 percent vs. 32 percent). We suspect that this higher proportion reflects higher response error on the type of coverage for the group with continous coverage. Our hypothesis is that a significant proportion of this group 1) reported changes from a policy in their own name with their employers, to other types of coverage when, in fact, no change took place and 2) reported employer-provided health insurance in their own name in the initial interview incorrectly and corrected this error in a subsequent interview. We believe, therefore, that the estimated number of workers losing employer-provided health insurance (11.7 million) during the year is biased upward significantly.

As another part of our examination of the loss of employer-provided health insurance, we analyzed the relative timing of these losses and changes in job and employment status. These data are summarized in table 8. An estimated 59 percent of the losses in employer-provided health insurance occurred during the month in which the job or employment changed. This rate was much lower for the group with continuous coverage than for the without-continuous-coverage group (43 percent vs. 66 percent). We believe that the weaker association between the timing of these events for the group with continuous coverage is additional evidence of response error problems with this group. In fact, this group may largely be defined by respondents with similar response error problems.

TIMING OF COVERAGE CHANGES

Of the estimated 83.7 million wage and salary workers with employer-provided health insurance, 56.7 million workers (67.8 percent) had coverage through their employer all 12 months. Of the remaining 27.0 million workers, 19.4 million (71.8 percent) changed their private health insurance coverage only between the months that marked the ends of the interview periods (i.e., between the 4th and 5th months or between the 8th and 9th months). This appears to be further evidence of the suspected response errors discussed in the previous section and of recall problems in general. For a further discussion of this phenomenon see "Gross Changes in Income Recipiency from the Survey of Income and Program Participation," Dan Burkhead and John Coder, Bureau of the Census, paper presented at the annual meeting of the American Statistical Association, Las Vegas, Nevada, August 5-8, 1985.

CONCLUSION

The SIPP longitudinal research file provides an opportunity to examine employer-provided private health insurance coverage for individuals through a 12-month

Table 8. Workers With Both Loss of Employer-Provided Health Insurance and a Change in Job or Employment Status by Month of Occurrence for These Events

(Numbers in thousands)

Month of job/employment status change	Total	With continuous overall coverage	Without continuous overall coverage
Total losing employer coverage and having a job/employment status change	6,130	1,944	4,186
Percent	100.0	100.0	100.0
Same month as coverage change	58.7	42.9	66.0
One month before coverage change	7.9	6.4	8.7
Two months before coverage change	6.2	11.2	3.9
Three or more months before coverage change	7.3	12.9	4.8
One or more months after coverage change	19.9	26.7	16.7

period, and to compare those estimates with another source of such data--the March CPS. The SIPP research file estimates a higher percentage of wage and salary workers with employer-provided health insurance coverage than reported in the CPS.

The investigation has uncovered some possible response error which may affect the change in coverage status. Among individuals who lost employer-provided health insurance coverage during the 12-month period, it appears likely that a considerable amount of misreporting occurred. Not only is it probable that the number of persons losing coverage are significantly overreported, but the timing of the changes in coverage are clustered at the breaks between the interview reference periods.

To address this problem an examination of the questionnaire wording might be useful. When a respondent is asked whether he/she had insurance in his/her "own name" it may not be apparent to the respondent that the purpose of the question is to find the primary policyholder. The respondent may think that if a person is covered by the insurance policy then the policy is in his/her name. Perhaps the item might be reworded to emphasize this distinction.

U.S. DEPARTMENT OF COMMERCE

July 13, 1987

To : Cindy

From: Dan Dan

I would like to make this paper a SIPP Working Paper. Would you do a peer review for me? Thanks!

Attachment

"Exploring Changes in Health Care Coverage Using the SIPP Longitudinal Research File," by Dan Burkhead and Angela Feldman-Harkins

Chron

U.S. DEPARTMENT OF COMMERCE

August 12, 1987

To : John

From: Dan

Thanks for sending me Dan and Angela's paper on Health Care Coverage using the SIPP research file for consideration as a SIPP Working Paper. I had Cindy T. review it; her comments are attached. She revised a few minor points. When Dan and Angela revise the paper send it back to me. Of course, if they have a problem with Cindy's comments let me know. Thanks.

10-20-86

REVIEW OF PAPER

TITLE: Health Care Coverage -SIPP REVIEWER: C. Taeuber

AUTHOR: Burkhead - Feldman/Hoskins DATE SENT:

FOR (conference/journal): EXPECTED RETURN DATE:

DATE RETURNED:

****

REVIEWERS COMMENTS:

Strengths of paper and advice:

(1) Nice paper - well written

(2) Would like to see more discussion of Tables 3 and 4 - for example, I am puzzled by the major differences between CPS & SIPP

(3) Are there race-age-sex differences in the change in coverage status? That is, is the "response error" consistent among age-sex-race groups? If yes, strengthens your conclusions. If no . . . . ?

****

AUTHORS RESPONSES: In response to Point 1, thanks!

In response to Point 2, we have added further discussion of possible reasons for the differences in employer-provided health insurance coverage between SIPP and CPS.

In response to Point 3, data for this universe was not tabulated by age, race, and sex, because it was felt that the small number of sample cases per category would not provide reliable estimates.

10-20-86

REVIEW OF PAPER

TITLE: Health Care Coverage -SIPP REVIEWER: C. Taeuber

AUTHOR: Burkhead - Feldman/Hawkins DATE SENT: ______

FOR (conference/journal): ______ EXPECTED RETURN DATE: ______

______ DATE RETURNED: ______

****

REVIEWERS COMMENTS:

Strengths of paper and advice:

(1) Nice paper - well written

(2) Would like to see more discussion of Tables 3 and 4 - for example, I am puzzled by the major differences between CPS & SIPP

(3) Are there race-age-sex differences in the change in coverage status? That is, is the "response error" consistent among age-sex-race groups? If yes, strengthens your conclusions. If no . . . . ?

****

AUTHORS RESPONSES:

September 10, 1987

EXPLORING CHANGES IN HEALTH CARE COVERAGE USING THE SIPP LONGITUDINAL RESEARCH FILE

By Dan Burkhead and Angela Feldman-Harkins, Bureau of the Census

INTRODUCTION

The SIPP Longitudinal Research File provides a data base from which changes in health care coverage can be examined and related to labor force participation, separation and divorce, retirement, program participation, etc. This paper presents the findings from the first analysis of health care coverage from the SIPP Longitudinal File. Several important areas are explored. First, a description on the longitudinal file creation and its limitations is given. Second, the survey's questions on health care coverage are described. Third, the health care coverage estimates from SIPP are compared with estimates derived from the Current Population Survey. Finally, estimates of change in employer-provided health insurance coverage and associated changes in other socioeconomic characteristics are profiled.

DESCRIPTION OF THE SIPP LONGITUDINAL RESEARCH FILE

During the period between October 1985 and August 1986 the Bureau of the Census constructed the first longitudinal data file based on the Survey of Income and Program Participation (SIPP). The data file was created by linking together cross-sectional WAVE-file data and then performing a series of longitudinal edits. Longitudinal edits were implemented to improve consistency for a select group of data items and to correct for a small number of errors related to the cross-sectional processing system. The main objective of this first longitudinal effort was to provide a data base for research and evaluation on SIPP data quality and for exploration of the uses of intra-year income, household composition, and work experience data.

The longitudinal research file was developed from the 1984 SIPP household panel. This panel consisted initially of about 19,900 interviewed households (the institutionalized population is excluded from the survey). The panel was divided into four equal-size subpanels, termed rotations. The first rotation was interviewed in October 1983. Subsequent interviews were conducted at 4-month intervals with one rotation being interviewed each month. Hence, by January 1984 each sample household had completed one interview. The interviews for October, November, and December 1983, and January 1984 taken collectively constituted a "WAVE", in this case, WAVE 1. In February 1984 the second interviewing cycle or WAVE 2 began. Monthly interviews continued in this sequence through July 1986.

Since SIPP is a longitudinal survey which attempts to follow persons when they move to new residences the designated sample is not the housing units selected but the members of the sample housing units interviewed in WAVE 1.

Each interview in SIPP contains a basic set of "core" questions covering labor force activities and receipt of income. This core of questions relates to labor force and income during the contiguous four-month period immediately preceding the month of interview. The four-month period is termed the "reference period." In most cases, the core data collection procedures were designed to obtain individual monthly observations for the key data items. Monthly core data were the building blocks used to construct the longitudinal research file.

The longitudinal research file contains data covering a total time period of 12 months for each sample person. This 12-month period varies depending on the

rotation to which the person belonged since a monthly interviewing scheme was used. Approximately one-fourth of the observations pertain to each of the following 12-month periods: June 1983 to May 1984, July 1983 to June 1984, August 1983 to July 1984, and September 1983 to August 1984.

A detailed description of the longitudinal processing procedures can be found in a working paper, "Preliminary Data from the SIPP 1983-1984 Longitudinal Research File," John F. Coder, et. al., Bureau of the Census, U.S. Department of Commerce.

DESCRIPTION OF QUESTIONS ON PRIVATE HEALTH INSURANCE COVERAGE

The SIPP questionnaire includes questions pertaining to the health insurance and medical care coverage of all household members. While each interview contains questions on this subject the manner in which this information is collected varies depending on the type of health or medical coverage. Medicare and Medicaid are two public medical benefit programs covered specifically. Private health insurance is included, with a distinction made between insurance provided through employers (or previous employers) and insurance obtained through other sources. Other questions concerning private health insurance include the type of plan and the proportion of cost paid by the employer, if the plan was provided through an employer. This paper is solely concerned with private health insurance coverage.

Private health insurance coverage data are collected in each interview. The private health insurance coverage is updated independently as no data collected in previous interviews is used. Figure 1 shows the items dealing with private

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage

24a. During the 4-month period, did . . . have group or individual health insurance in . . .'s own name? (Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)	1536	1 ☐ Yes — *SKIP to 24c* 2 ☐ No
ASK OR VERIFY — **b.** Was . . . covered by a health insurance plan in somebody else's name?	1537	1 ☐ Yes 2 ☐ No } *SKIP to Check Item R22*
c. Did . . . have this health insurance plan during the entire 4-month period?	1538	1 ☐ Yes — *SKIP to 24e* 2 ☐ No
d. In which months did . . . have the plan? *Mark (X) all that apply.*	1540 1542 1544 1546	1 ☐ Last month 2 ☐ 2 months ago 3 ☐ 3 months ago 4 ☐ 4 months ago
e. Was . . .'s plan provided through an employer or union (or through a former employer or a pension plan)?	1548	1 ☐ Yes 2 ☐ No — *SKIP to 24g*
f. Did the employer or union (former employer or pension plan) pay for part or all of the cost of this plan?	1550	1 ☐ All 2 ☐ Part x3 ☐ None
g. Was this an individual plan or a family plan?	1552	1 ☐ Individual — *SKIP to Check Item R22* 2 ☐ Family
h. Did . . .'s health plan cover all the persons living here?	1554	1 ☐ Yes — *SKIP to 25* 2 ☐ No
i. Other than . . ., which persons in this household were covered by . . .'s plan?	 1556 1558 1560 1562 1564 1566	Person No. / Name ☐☐☐ ☐☐☐ ☐☐☐ ☐☐☐ ☐☐☐ x3 ☐ None
CHECK ITEM R22 *Refer to Control Card item 27.* Is . . . the designated parent or guardian of children under 18 who live in this household?	1568	1 ☐ Yes 2 ☐ No — *SKIP to 25*
CHECK ITEM R23 Have each of these children already been identified as members of a family health insurance plan?	1570	1 ☐ Yes 2 ☐ No x1 ☐ DK } *SKIP to 24k*
24j. I have recorded that all of . . .'s children were covered by a health insurance plan — is that correct?	1572	1 ☐ Yes — *SKIP to 25* 2 ☐ No

Figure 1. SIPP Questionnaire Items on Private Health Insurance Coverage--Continued

24k. **Are any of (Which of) . . .'s children (were) covered by a health insurance plan?** **(Exclude Medicaid, Medicare, CHAMPUS, CHAMPVA and plans paying benefits only for accidents or specific diseases.)**	1574	x5 ☐ **All children** **OR**
		Person No. / **Name**
	1576	
	1578	
	1580	
	1582	
	1584	
	1586	x3 ☐ **None**

health insurance coverage which is asked specifically for all household members age 15 years and over. Coverage of household members under age 15 is derived by asking which household members are covered by insurance policies obtained by adult members.

The update for private health insurance coverage identifies persons having coverage at any time during the 4-month reference period but does not provide a monthly accounting of coverage. This monthly accounting is derived in one of two ways. For persons with private health insurance in their "own name" (policyholders) a questions is asked directly concerning the months of coverage. The months of coverage for all other household members were derived by linking their coverage to the adult household members reporting that their coverage extended to these other household members.

The longitudinal editing process for the private health insurance coverage data was designed mainly to remove a very small number of inconsistencies caused by cross-sectional imputations. In most of these cases the reported coverage statuses (for the 4-month reference period) in two of the three interviews are consistent (the same) with each other but inconsistent with an imputed value in the remaining interview. The edit changed the inconsistent covered status to be consistent (the same) with the two reported values.

The edit of the health insurance covered status required that a post-edit modification be made to the monthly coverage fields. The covered status may have been altered from "covered" to "not covered" or from "not covered" to "covered." If the status was altered to "not covered," all monthly coverage fields for the

individual were modified to indicate this new status. Changing the status to "covered" required that the monthly coverage fields be established. In these cases the monthly status fields for all months of the 4-month reference period were modified to indicate a status of "covered" for the individual. No changes were made to the coverage status of other household members who derived their coverage from this individual even though some may have been justified. Given the small number of cases edited, this should not present a serious problem.

The private health insurance variables on the longitudinal file are structured differently than those on the WAVE files. They do not replicate the detail as collected in the individual 4-month reference periods but have been restructured into three variables; a variable indicating coverage in the person's "own name," a variable indicating coverage in "someone else's name," and a variable indicating if the insurance was obtained through an employer. This last variable applies only to persons with coverage in their own name. We did not attempt to establish covered units, i.e., which household members were covered by which member's policy.

PRIVATE HEALTH INSURANCE COVERAGE

Table 1 contains estimates of persons covered by private health insurance for the SIPP 1983-84 reference period. The household relationship categories apply to month 12. The figures in table 1 on private health insurance are not additive since persons may have been in more than one coverage status during the period.

The data in table 1 show that a total of 189.8 million persons were covered by private health insurance for one or more months during the SIPP 12-month

reference period and that these persons were covered for an average of 10.9 months. Of the total persons with private health insurance coverage, 103.7 million had coverage in their own name for at least one or more months, i.e., these persons were the primary "policyholders." SIPP estimated about 99.5 million persons with one or more months of coverage as a "family" member. The estimates in table 1 of private health insurance based on the SIPP data file are not directly comparable to estimates published from the March CPS because the CPS data are restricted to employer-related insurance coverage for persons (and their dependents) working during the calendar year.

COMPARISON OF ESTIMATES OF EMPLOYER-PROVIDED HEALTH CARE COVERAGE FROM SIPP AND CPS

An examination of SIPP and CPS annual estimates must be accompanied by a brief description of the two data sets and differences that may affect their relationship. Estimates available from the SIPP and CPS are for different, but overlapping time periods. The CPS provides figures for calendar years (1983 and 1984 are applicable in this examination) whereas estimates from the SIPP research file span four 12-month periods each containing months in calendar years 1983 and 1984.

The SIPP estimates of numbers are based on weights reflecting independent estimates of the noninstitutional population as of December 1983. Persons included in the SIPP research file have weights only if they were included in the original sample. In this analysis, persons entering or leaving the sample within the 3 interview periods are not included. Only persons interviewed all 12 months are included.

According to the SIPP research file, 113.4 million persons received wage or salary income. This figure is higher than estimates of wage and salary workers from the CPS for either 1983 or 1984 (see table 2). The SIPP estimates that 69 percent of all wage and salary workers had employer-provided health insurance coverage at some time during the 12-month period. This is about 8 percentage points higher than the 1983 CPS estimate. About 72 percent of male workers and 55 percent of female workers had employer-provided health insurance according to the SIPP research file. Coverage rates by selected characteristics for male and female wage and salary workers appear in tables 3 and 4, respectively. The higher coverage rates obtained by SIPP may be attributed, in part, to survey design. In SIPP, the health insurance questions are asked of each person aged 15 years old and over. Each person with health insurance in his (or her) own name is then asked about employer-provided coverage. In CPS, questions concerning employer-provided health insurance coverage are asked only of wage and salary workers. Because of the shorter recall period in SIPP (4 months versus 1 year for CPS), more marginally employed persons should be identified which should result in more short-term health insurance coverage. The percent distributions of wage and salary workers covered by employer-provided health insurance are shown in tables 5 and 6 by selected characteristics.

EXAMINING THE LOSS OF EMPLOYER-PROVIDED COVERAGE

A simple tabulation from the SIPP longitudinal data file indicates that about 11.7 million workers who began the year with employer-provided health insurance coverage lost that coverage during one or more of the remaining 11 months. This figure represents 17.2 percent of the 68.5 million workers who had employer-provided health insurance coverage during the first month. The data in table 7 show the composition of this group based on their access to other health insurance and significant changes in work/job activities.

Of the 11.7 million losing employer-provided health insurance, approximately 48 percent experienced no change in employers or in their employment status (see figure 2 for a list of statuses). Since no data are collected on specific reasons for loss of health insurance we can only speculate on the cause of these changes. One important factor is probably response error and confusion on the part of respondents, i.e., no "real" change took place in health insurance coverage. We suspect that a large proportion of these cases fit in this category. In some cases the employer may have cancelled employee policies. In other cases employees may have cancelled coverage in order to take advantage of a "better" or cheaper policy available from the spouse's employer.

About 48 percent of those workers losing employer-provided health coverage had continuous coverage through other private health insurance, either in their own name or as a family member in a policy obtained by another household member. The remaining 52 percent of these workers were without private health insurance coverage for one or more months.

It is interesting to note that a much higher proportion of workers <u>with</u> continuous overall private health insurance coverage experienced <u>no</u> job or employment status changes than workers <u>without</u> continuous overall coverage (65 percent vs. 32 percent). We suspect that this higher proportion reflects higher response error on the type of coverage for the group with continous coverage. Our hypothesis is that a significant proportion of this group 1) reported changes

Figure 2. Employment Status Recodes for Each Month

1 = With a job entire month, worked all weeks
2 = With a job entire month, missed one or more weeks, no time on layoff
3 = With a job entire month, missed one or more weeks, spent time on layoff
4 = With a job one or more weeks, No time spent looking or on layoff
5 = With a job one or more weeks, Spent one or more weeks looking or on layoff
6 = No job during month, spent entire month looking or on layoff
7 = No job during month, spent one or more weeks looking or on layoff
8 = No job during month, No time spent looking or on layoff

from a policy in their own name with their employers, to other types of coverage when, in fact, no change took place and 2) reported employer-provided health insurance in their own name in the initial interview incorrectly and corrected this error in a subsequent interview. We believe, therefore, that the estimated number of workers losing employer-provided health insurance (11.7 million) during the year is biased upward significantly.

As another part of our examination of the loss of employer-provided health insurance, we analyzed the relative timing of these losses and changes in job and employment status. These data are summarized in table 8. An estimated 59 percent of the losses in employer-provided health insurance occurred during the month in which the job or employment changed. This rate was much lower for the group with continuous coverage than for the without-continuous-coverage group (43 percent vs. 66 percent). We believe that the weaker association between the timing of these events for the group with continuous coverage is additional evidence of response error problems with this group. In fact, this group may largely be defined by respondents with similar response error problems.

TIMING OF COVERAGE CHANGES

Of the estimated 83.7 million wage and salary workers with employer-provided health insurance, 56.7 million workers (67.8 percent) had coverage through their employer all 12 months. Of the remaining 27.0 million workers, 19.4 million (71.8 percent) changed their private health insurance coverage only between the months that marked the ends of the interview periods (i.e., between the 4th and 5th months or between the 8th and 9th months). This appears to be further evidence of the suspected response errors discussed in the previous section and of recall problems in general. For a further discussion of this phenomenon see "Gross Changes in Income Recipiency from the Survey of Income and Program Participation," Dan Burkhead and John Coder, Bureau of the Census, paper presented at the annual meeting of the American Statistical Association, Las Vegas, Nevada, August 5-8, 1985.

CONCLUSION

The SIPP longitudinal research file provides an opportunity to examine employer-provided private health insurance coverage for individuals through a 12-month period, and to compare those estimates with another source of such data--the March CPS. The SIPP research file estimates a higher percentage of wage and salary workers with employer-provided health insurance coverage than reported in the CPS.

The investigation has uncovered some possible response error which may affect the change in coverage status. Among individuals who lost employer-provided health insurance coverage during the 12-month period, it appears likely that a considerable amount of misreporting occurred. Not only is it probable that the number of persons losing coverage are significantly overreported, but the timing of the changes in coverage are clustered at the breaks between the interview reference periods.

To address this problem an examination of the questionnaire wording might be useful. When a respondent is asked whether he/she had insurance in his/her "own name" it may not be apparent to the respondent that the purpose of the question is to find the primary policyholder. The respondent may think that if a person is covered by the insurance policy then the policy is in his/her name. Perhaps the item might be reworded to emphasize this distinction.

Table 1. Estimated Number of Persons Ever Covered by Private Health Insurance, Mean Number of Months Covered, and Mean Number of Persons Covered Per Month for 1983-84: SIPP Longitudinal Research File

(Relationship as of Month 12)

Characteristics	Number ever covered (thous.)	Mean number of months covered	Mean number covered per month (thous.)
COVERED BY PRIVATE HEALTH INSURANCE			
Total	189,813	10.9	172,715
Householders	75,087	10.9	68,416
Family	55,952	11.0	51,521
Nonfamily	19,132	10.6	16,895
Other family members	111,505	10.9	101,690
Other unrelated individuals	3,221	9.7	2,609
HAD OWN PRIVATE HEALTH INSURANCE			
Total	103,670	10.2	88,451
Householders	68,428	10.7	61,045
Family	50,185	10.8	45,006
Nonfamily	18,244	10.6	16,039
Other family members	32,764	9.3	25,402
Other unrelated individuals	2,478	9.7	2,003
HAD PRIVATE HEALTH INSURANCE THROUGH SOMEONE ELSE			
Total	99,498	10.2	84,264
Householders	10,903	8.1	7,371
Family	9,317	8.4	6,515
Nonfamily	1,586	6.5	856
Other family members	87,602	10.5	76,288
Other unrelated individuals	993	7.3	605

Table 2. Comparison of Number of Persons with Wage and Salary Income Covered by an Employer-Provided Health Care Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Sex

(Numbers in thousands)

Characteristic	SIPP	CPS	
		1984	1983
BOTH SEXES			
Number with wage and salary income................	113,408	112,024	108,502
Percent with employer-provided health insurance.	68.6	59.8	61.0
MEN			
Number with wage and salary income................	61,732	59,787	58,443
Percent with employer-provided health insurance.	72.0	66.8	68.4
WOMEN			
Number with wage and salary income................	51,676	52,237	50,059
Percent with employer-provided health insurance.	54.8	51.7	52.3

Table 3. Comparison of Estimates of Number of Male Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total.............	61,732	72.0	59,787	66.8	58,443	68.4
Race and Spanish Origin						
White.................	54,021	73.2	52,527	67.8	51,569	69.4
Black.................	5,975	62.7	5,682	59.4	5,533	60.3
Spanish origin[1]........	4,188	64.5	4,194	54.4	3,400	59.3
Age						
15 to 24 years.........	14,283	36.9	13,333	31.4	13,314	33.1
25 to 34 years.........	17,435	78.2	17,144	74.6	16,459	76.4
35 to 44 years.........	13,050	85.2	12,583	81.9	12,095	83.0
45 to 54 years.........	8,563	87.0	8,480	81.4	8,421	83.7
55 to 64 years.........	6,618	87.9	6,483	79.0	6,447	81.7
65 years and over......	1,784	63.8	1,765	37.7	1,707	39.1
Relationship to Family Householder						
In family..............	52,992	71.6	49,986	66.7	49,226	68.1
Householder..........	36,679	84.5	34,927	79.5	34,565	80.9
Spouse..............	2,881	76.4	1,915	70.8	1,823	69.3
Other...............	13,432	35.3	13,145	32.1	12,838	33.6
In subfamily...........	144	39.6	174	32.8	132	53.0
Unrelated individuals..	8,596	75.0	9,627	68.1	9,085	70.4
Weeks Worked						
Worked full time.......	51,408	81.1	51,540	75.0	49,953	77.2
50 to 52 weeks.......	37,863	89.7	39,433	83.6	37,176	85.3
40 to 49 weeks.......	4,325	76.0	3,974	65.4	3,944	69.9
27 to 39 weeks.......	3,695	65.4	2,860	52.0	3,071	62.1
26 weeks or less.....	5,526	36.6	5,272	31.2	5,762	38.0
Worked part time.......	10,311	26.6	8,247	15.6	8,491	17.0

[1]Persons of Spanish origin may be of any race.

Table 4. Comparison of Estimates of Number of Female Wage and Salary Workers 15 Years Old and Over and Percent Covered by an Employer-Provided Health Insurance Plan at any time during the year: SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number	Percent covered	Number	Percent covered	Number	Percent covered
Total.............	51,676	54.8	52,237	51.7	50,059	52.3
Race and Spanish Origin						
White..................	44,212	54.2	44,705	51.1	43,176	51.9
Black..................	6,167	57.8	6,122	55.5	5,635	55.4
Spanish origin[1]........	2,932	51.4	2,998	48.6	2,627	51.7
Age						
15 to 24 years.........	12,710	35.4	12,617	31.0	12,251	32.4
25 to 34 years.........	14,527	63.2	14,816	61.1	14,117	61.6
35 to 44 years.........	10,635	58.4	10,885	57.1	10,325	57.1
45 to 54 years.........	7,226	60.6	7,269	59.2	6,965	59.2
55 to 64 years.........	5,193	65.8	5,200	59.8	5,063	60.5
65 years and over......	1,384	45.4	1,449	27.6	1,338	30.3
Relationship to Family Householder						
In family..............	43,955	51.6	43,643	48.9	41,838	49.3
Householder..........	8,374	66.3	7,686	64.1	7,338	64.1
Spouse...............	25,495	53.6	25,882	51.1	25,039	51.6
Other................	10,086	34.0	10,075	31.8	9,461	31.8
In subfamily...........	231	45.9	326	47.5	283	50.2
Unrelated individuals..	7,491	74.1	8,268	66.4	7,938	68.0
Weeks Worked						
Worked full time.......	32,310	74.1	35,629	68.1	33,780	68.7
50 to 52 weeks.......	22,298	84.2	25,319	76.7	24,024	77.2
40 to 49 weeks.......	3,600	73.9	3,299	64.2	2,905	66.2
27 to 39 weeks.......	2,493	57.1	2,408	52.1	2,241	57.3
26 weeks or less.....	3,920	27.3	4,604	31.4	4,610	31.7
Worked part time.......	19,361	22.7	16,608	16.6	16,279	18.1

[1]Persons of Spanish origin may be of any race.

Table 5. Comparison of Estimates of Number of Men 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Percent	Number covered	Percent	Number covered	Percent
Total	44,427	100.0	39,966	100.0	40,004	100.0
Race and Spanish Origin						
White	39,518	89.0	35,599	89.1	35,786	89.5
Black	3,749	8.4	3,376	8.4	3,335	8.3
Spanish origin[1]	2,702	6.1	2,281	5.7	2,017	5.0
Age						
15 to 24 years	5,269	11.9	4,184	10.5	4,402	11.0
25 to 34 years	13,631	30.7	12,791	32.0	12,573	31.4
35 to 44 years	11,118	25.0	10,309	25.8	10,040	25.1
45 to 54 years	7,452	16.8	6,899	17.3	7,051	17.6
55 to 64 years	5,818	13.1	5,119	12.8	5,270	13.2
65 years and over	1,139	2.6	665	1.7	668	1.7
Relationship to Family Householder						
In families	37,920	85.4	33,349	83.4	33,536	83.8
Householder	30,976	69.7	27,775	69.5	27,957	69.9
Spouse of householder	2,201	5.0	1,355	3.4	1,264	3.2
Other relative of householder	4,743	10.7	4,220	10.6	4,315	10.8
In unrelated subfamilies	57	0.1	57	0.1	70	0.2
Unrelated individuals	6,450	14.5	6,560	16.4	6,398	16.0
Weeks Worked						
Worked full time	41,683	93.8	38,678	96.8	38,562	96.4
50 to 52 weeks	33,955	76.4	32,947	82.4	31,711	79.3
40 to 49 weeks	3,287	7.4	2,598	6.5	2,758	6.9
27 to 39 weeks	2,418	5.4	1,487	3.7	1,906	4.8
26 weeks or less	2,022	4.6	1,647	4.1	2,187	5.5
Worked part time	2,744	6.2	1,288	3.2	1,441	3.6

[1]Persons of Spanish origin may be of any race.

Table 6. Comparison of Estimates of Number of Women 15 Years Old and Over Covered by an Employer-Provided Health Insurance Plan Between the SIPP Longitudinal Research File and the CPS (1984 and 1983) by Selected Characteristics

(Numbers in thousands)

Characteristic	SIPP		CPS			
			1984		1983	
	Number covered	Per-cent	Number covered	Per-cent	Number covered	Per-cent
Total.................	28,317	100.0	26,998	100.0	26,163	100.0
Race and Spanish Origin						
White..........................	23,948	84.6	22,863	84.7	22,408	85.6
Black..........................	3,563	12.6	3,395	12.6	3,121	11.9
Spanish origin[1]..............	1,507	5.3	1,458	5.4	1,357	5.2
Age						
15 to 24 years................	4,499	15.9	3,915	14.5	3,974	15.2
25 to 34 years................	9,182	32.4	9,054	33.5	8,703	33.3
35 to 44 years................	6,210	21.9	6,215	23.0	5,896	22.5
45 to 54 years................	4,381	15.5	4,304	15.9	4,123	15.8
55 to 64 years................	3,416	12.1	3,108	11.5	3,061	11.7
65 years and over............	629	2.2	400	1.5	406	1.6
Relationship to Family Householder						
In families..................	22,661	80.0	21,353	79.1	20,625	78.8
Householder................	5,554	19.6	4,930	18.3	4,704	18.0
Spouse of householder......	13,673	48.3	13,224	49.0	12,915	49.4
Other relative of householder..............	3,433	12.1	3,199	11.8	3,006	11.5
In unrelated subfamilies.....	106	0.4	155	0.6	142	0.5
Unrelated individuals........	5,551	19.6	5,489	20.3	5,396	20.6
Weeks Worked						
Worked full time.............	23,927	84.5	24,246	89.8	23,215	88.7
59 to 52 weeks.............	18,775	66.3	19,426	72.0	18,547	70.9
40 to 49 weeks.............	2,659	9.4	2,119	7.8	1,924	7.4
27 to 39 weeks.............	1,424	5.0	1,255	4.6	1,283	4.9
26 weeks or less...........	1,069	3.8	1,446	5.4	1,460	5.6
Worked part time.............	4,390	15.5	2,752	10.2	2,949	11.3

1Persons of Spanish origin may be of any race.

Table 7. Workers Losing Employer-Provided Health Insurance by Job or Employment Status Change and Marital Status Change

(Numbers in thousands)

Change status	Total	With continuous overall coverage	Without continuous overall coverage
Total losing coverage	11,744	5,582	6,162
Percent	100.0	100.0	100.0
With a job or employment status change	52.2	34.8	67.9
Without a job or employment status change	47.8	65.2	32.1
With a marital status change	(NA)	2.9	(NA)
Without a marital status change	(NA)	97.1	(NA)

Table 8. Workers With Both Loss of Employer-Provided Health Insurance and a Change in Job or Employment Status by Month of Occurrence for These Events

(Numbers in thousands)

Month of job/employment status change	Total	With continuous overall coverage	Without continuous overall coverage
Total losing employer coverage and having a job/employment status change.................	6,130	1,944	4,186
Percent.................................	100.0	100.0	100.0
Same month as coverage change.................	58.7	42.9	66.0
One month before coverage change..............	7.9	6.4	8.7
Two months before coverage change.............	6.2	11.2	3.9
Three or more months before coverage change...	7.3	12.9	4.8
One or more months after coverage change......	19.9	26.7	16.7

9012

CONGRESSIONAL BUDGET OFFICE
U.S. CONGRESS
WASHINGTON, DC 20515

June 26, 1990

Daniel Kasprzyk
SHEP, Suite 2A
U.S. Bureau of the Census
Washington, D.C. 20233

Dear Dan:

The enclosed diskette contains the text (TEXT.AEA) and tables (TAB1.AEA, TAB2.AEA, TAB3.AEA) from the Long/Rodgers paper, "The Effects of Being Uninsured on Health Care Service Use: Estimates from the Survey of Income and Program Participation."

I am sure that you will be able to use the text with little additional work. The tables, on the other hand, were formatted all wrong when I put them on my screen. Hopefully, your secretary can straighten out the problem. (The Figure 1 exists only on paper.)

Please call me if you have any questions.

Sincerely,

Jack Rodgers

Jack Rodgers

Enclosures

202-226-2654

FORM CD-27
(5-85)
DAO 219-4

U.S. DEPARTMENT OF COMMERCE

PUBLICATION CLEARANCE REQUEST 87-28

1. DATE OF REQUEST 4/14/87

2. ☒ New ☐ Revision ☐ Reprint

INSTRUCTIONS

Complete this form before work has begun on proposed project. Any additional explanation needed to help reviewers in determining the need for the project not fitting form spaces should be typed on a separate sheet with three copies attached to this form. Submit CD-27 first to unit publication and public affairs offices for approval, then to the Department Office of Public Affairs. Periodicals, after Department approval of this form, must also be cleared by OMB through Publications Service. The approved CD-27 assigned number must be included in the appropriate box of the CD-10 printing requisition before production can begin. Consult DAO 219-4, June 18, 1982, for further assistance.

NOTE: If the Office of Public Affairs requires submission of the final copy and CD-10 for review, this must be done prior to submitting final copy and CD-10 to Publications Service.

WARNING: Contracting must be done in conformance with Government and Department procurement regulations.

3. REQUESTOR (Unit, bureau and/or office)
Bureau of the Census, PopulatiOonDivision

4. PROJECT DIRECTOR (Name, title, location, telephone)
Daniel Kasprzyk, Special Assistant, Office of the Chief
Rm. 2024-3, 763-5784

5. TITLE OF PUBLICATION Survey of Income and Program Participation:1986 (Selected Papers given at the Annual Meeting of the American Statistical Association in Chicago, Ill., August 18-21, 1986) and following versions through September 1988.

6. DESCRIPTION AND JUSTIFICATION (Purpose, how the publication will benefit user, how it will add to existing area of knowledge, importance to unit mission and policy.)
The booklet highlights the research papers presented on the Survey of Income and Program Participation (SIPP) at the 1986 Annual Meeting of the ASA in Chicago, Ill. This booklet is shared by Federal, State, local, and International statistical agencies; academia; and the private sector. The use of this booklet is to keep SIPP users informed of ongoing research and foster new research. (Attached is a sample of the 1985 version of this report.)

7. SPECIFICATIONS
Ink color: Number 1 Cover only ☐ Cover and text ☒
If more than one color, when approved by Publications Service? ______
Graphics: Cover only ☒ Cover and text ☐ Photos: Number (est.) ______ Pages: Number 152

8. Audience
Immediate audience is the SIPP session attendees at the meeting and the SIPP mailing list.

9. NUMBER OF COPIES Sales ______ Sold through GPO ☐, NTIS ☐, other ☐ (specify) ______
Free 600 (If free, justify benefits to taxpayer of distribution.) Research conducted by the Census Bureau is indirectly supported with taxpayers' dollars. Sharing its research and learning of new research and techniques helps the Bureau keep up with the state of the art and keeps the users informed.

10. SALES PROMOTION
No X (If no, why not?) Booklet not intended for public sale since only intended
Yes ___ (How?) for present/potential users of SIPP.
☐ Require assistance with promotion plans

11. ESTIMATED COST
a. Editorial, copy preparation, research $ 300
b. Printing, composition, graphics $ 3,700

12. APPROPRIATION NUMBER
34-1465-244-701-135-0401

13. PROPOSED DATE FOR SUBMISSION OF COMPLETED COPY FOR PRINTING 4/14/87

14. APPROVALS: I certify this publication is essential to unit mission, all reasonable cost-reduction measures will be followed, alternate methods of communicating the message have been investigated and the proposed publication does not duplicate existing literature.

	Signature	Date	Address	Telephone
Project Chief	PAULA SCHNEIDER	4-15-87	Rm. 2011-3	763-7646
Unit Publications Clearance Officer	WALTER C. ODOM	4-23-87	Rm. 1061-4	763-7292
Unit Office of Public Affairs 4-23	JAMES F. GORMAN	4/23/87	Rm. 2705-3	763-4051

15. FINAL APPROVALS

Office of Public Affairs O/S	Publications Service	Signature	Date	Assigned No.
☐ Cleared ☐ Copy and CD-10 to be submitted to OPA prior to submitting to PS ☐ Disapproved	☒ Cleared ☐ Disapproved		/ /	12941
		T. Cass	6/26/87	

USCOMM DC 85 17758

SURVEY OF INCOME

AND

PROGRAM PARTICIPATION

SESSION II

The five papers presented in this session were sponsored by the Survey Research Methods Section of the American Statistical Association.

SIPP: LONGITUDINAL ESTIMATION FOR PERSONS' CHARACTERISTICS

Edward L. Kobilarcik and Rajendra P. Singh, U.S. Bureau of the Census

I. INTRODUCTION

The Bureau of the Census has been conducting interviews for the Survey of Income and Program Participation (SIPP) since October 1983. The SIPP is a national survey and is designed to provide improved information on the income and participation in government programs of the noninstitutional United States population. Person and household characteristics that may influence income and program participation are also available from the SIPP. This information is vital to improve the capability of federal agencies to formulate and evaluate their policies and programs in the areas of income and social welfare.

Two types of estimates will be produced from the survey--cross-sectional and longitudinal. The method developed for producing cross-sectional estimates is described in [5]. This paper presents estimation methodology to provide longitudinal estimates of person characteristics from SIPP data. We define longitudinal estimates to be those that are obtained by linking two or more interview data files. These estimates include the length of time in a particular state (spell estimate), transition estimates at any given time or interval, annual estimates of income and estimates of change of certain characteristics. The method presented in this paper is developed for the first SIPP longitudinal file covering the first three interviews of the survey. This method consists of several stages of weight adjustments designed to reduce the bias in the survey caused by undercoverage and nonresponse. (These estimation stages do not differ appreciably from those used in SIPP cross-sectional estimation.)

This file has been developed to be used primarily for research purposes and the estimation method may be revised for future longitudinal products. Because of the urgency to make this file available for the summer of 1986, some of the decisions concerning the estimation method may not be conceptually sound, for example, treating those households in which at least one household member failed to respond to the first interview as a nonresponding household. However, the increase in bias and/or variance due to these decisions is expected to be negligible.

II. BACKGROUND AND SAMPLE DESIGN

The SIPP 84 panel is a multistage stratified systematic sample of the noninstitutionalized resident population of the United States. This population includes persons living in group quarters, such as dormitories, rooming houses, and religious group dwellings. Noncitizens of the United States who work or attend school in this country and their families were eligible. All other persons were ineligible. This includes crew members of merchant vessels, Armed Forces personnel living in military barracks, and institutionalized persons, such as correctional facility inmates and nursing home residents. With these qualifications, persons who were residing in the United States at the time of the first interview were eligible for SIPP. However, only persons who were at least 15 years of age were eligible for interview.

Initially, a sample of living quarters in 174 Primary Sampling Units (PSUs) was selected. (Living quarters are those in which the occupants do not live and eat with any other person in the structure and that have either direct access from the outside of the building or through a common hall, or complete kitchen facilities for that unit only.) These 174 PSUs were subsampled from the Current Surveys (CS) A design PSUs [1]. To subsample these PSUs, SIPP strata were formed by combining CS strata having sample PSUs with similar proportion of non-white persons (1970), urban persons (1970), and families with income below the poverty level (1969). Forty-five of the CS strata were single-PSU strata and were selected in SIPP with certainty. To select the remaining 129 nonself-representing (NSR) PSUs, a CS stratum was selected from each SIPP stratum with probability proportional to its size. The CS PSUs in the selected CS strata were the designated NSR sample SIPP PSUs.

The SIPP sample is divided into four groups of equal size called rotation groups. One rotation group is interviewed each month. In general, one cycle of four interviews is called a wave. This design provides a smooth and steady work load for data collection and processing. Persons in the sample are interviewed once every four months for approximately two and one-half years. The reference period for the interview questions is the four months preceding the interview month. For example, the reference period for the November 1983 interview month is July through October 1983. These sample persons are interviewed again in March 1984 for the November 1983 through February 1984 period.

Persons 15 years old and over present as household members at the time of first interview are to be part of the survey for the entire two and one-half year period. With certain restrictions, these sample persons are followed if they move to a new address. "New" persons living with sample persons are considered to be part of the sample only while residing with these sample persons. More details on the SIPP design are given in [2], [3], and [4].

III. LONGITUDINAL UNIVERSE OF PERSONS

Before defining the longitudinal universe, it is first necessary to consider the SIPP universe at the beginning of the survey and the possible ways persons can enter and exit this universe.

As mentioned previously, the SIPP universe at the beginning of the survey is persons who are members of the civilian non-institutional population and members of the military not residing in military barracks. Persons can enter the SIPP universe in two ways: 1) persons can move from foreign living quarters, institutions or military barracks (call these places ineligible addresses) to an eligible address or 2) persons can be born to members of the universe. Likewise, persons can exit the universe in two ways: 1) moving to an ineligible address or 2) dying. A more comprehensive discussion is presented in [9].

With the above in mind, the longitudinal universe is defined to be the noninstitutional population (excluding military barracks) on December 1, 1983. This date is the midpoint of the wave 1 interview months. With this definition, the sample from the universe is restricted to only those persons who were eligible for the first SIPP interview. Because of this, persons who relocate from an ineligible address to an eligible one during the time period after the first interview are excluded from the universe since they were not in the eligible population during the first interview. However, eligible persons who die or move to an ineligible address are included since they were in the eligible population.

IV. SAMPLE OF UNIVERSE

The sample from the longitudinal universe consists of eligible persons living in the selected living quarters at the time of the first interview. Not all of these persons were interviewed. Those who did respond to the initial interview are called original sample persons. Longitudinal analysis will only be appropriate for these original sample persons. This sample can be viewed as a sample of cohorts, with the cohorts being those persons in the SIPP sample between October 1983 and January 1984, inclusive. (By definition, a cohort is a group of individuals sharing a common characteristic.) Longitudinal analysis will only be appropriate for these cohorts. Longitudinal tabulations for such persons can be produced for the time period covered by the first 12 interview months of the survey. This corresponds to the period October 1983-September 1984 and represents the first three SIPP interviews. Data for 12 months will be available for each original sample person except for those who are known to have left the universe. The specific 12 months available depends on the person's rotation. For example, rotation 1 has data for reference months June 1983-May 1984.

V. OVERVIEW OF LONGITUDINAL ESTIMATION

Three strategies are generally suggested as solutions to handle whole interview nonresponse - a weighting adjustment, imputation, or a combination of the two [10], [11]. In our estimation procedure, all such cases will be handled by a weighting adjustment. We decided on this approach because of time and resource constraints and the unavailability of a good longitudinal imputation system.

The estimation procedure defined below is used to develop longitudinal weights for original sample persons. Certain processes in the procedure were developed to reduce for some, but not all, of the known biases in the SIPP such as bias due to undercoverage and attrition. These biases are briefly discussed in VII.

A ratio estimation technique is used in the longitudinal estimation. A set of variables correlated to estimates of interest is used to define ratio adjustment cells for various adjustments. For a given cell, the ratio adjustment factor for each respondent in that cell R_c is obtained as

$$R_c = \frac{T_c}{W_c}, \text{ where } T_c \text{ is a control}$$

total and W_c are the weighted counts that are adjusted to the control total. The control total may be obtained from the sample or from an independent source.

The following three assumptions are implicit in the formation of these cells:

1. There is a significant correlation between the important survey estimates and the variables used to form weighting cells.
2. Two different weighting cells have different means.
3. Within each weighting cell, the means for the sample respondents and the nonrespondents are equal.

Thus, it is desirable to form a new cell if the mean of characteristics of interest and the response rate for this cell are different from the mean and response rate from all other weighting cells.

Each sample person is given a single longitudinal weight, with this weight being assigned to each of a person's 12 reference months.

VI. DESCRIPTION OF ESTIMATION PROCEDURE

Several processes are included in the construction of longitudinal weights. Each process has its own specific objective. As explained below, the processes consist of several adjustments.

The following sample persons will be treated as "interviewed" persons in the estimation procedure: 1) those who responded to each of the first three interviews and who during the first interview lived in a household in which all eligible members responded to the interview (call this a wave 1 interviewed household) and 2) those who resided in a wave 1 interviewed household but who during the time period covered by the second and third interview are known to have died or moved to an ineligible address (foreign living quarters, institutions or military barracks). For persons who are known to have died or moved to an ineligible address, the months that such persons were deceased or residing in an ineligible address will be identified.

The following sample persons will be treated as "noninterviewed" persons in the estimation procedure: 1) those who at the time of the first interview lived in a household in which at least one household member failed to respond to the first interview (call this a wave 1 noninterviewed household), 2) those who resided in a wave 1 interviewed household but failed to respond to the second and/or third interview because of household or person nonresponse, and 3) those who resided in a wave 1 interviewed household but who moved in with members of another wave 1 interviewed household after the first interview. (This occurred for only four households.) These persons are treated as noninterviews because an imputation system for handling missing interviews is not yet available and because the processing system is unable to handle households defined in 1) and 3) above.

All persons classified as interviewed are assigned positive weights, while those classified as noninterviewed are assigned zero weights.

B. Preparation of Unbiased Estimates

A common method of estimation, weighting by the reciprocal of the probability of selection (P_i), is the first step in the weighting process. This procedure results in an unbiased estimator of a population total assuming 100% response. With this procedure, the unbiased weight for the i^{th} sample person is

$$W_i = \frac{1}{P_i}$$

where P_i is the selection probability of the household containing the i^{th} sample person. For some households a factor is included in the selection probability because different overall sampling fractions were used for certain parts of the population. In particular, certain units were subsampled because their actual size was much larger than anticipated.

C. Adjustment for Noninterviews

The next step in the estimation process is the adjustment for noninterviews. In general, noninterview weight adjustment consists of the reassignment of the weights of noninterviewed households or persons to groups of interviewed households or persons that hopefully have similar characteristics. This is equivalent to assigning the mean value of the cell to all nonrespondents. Noninterview adjustment will take place in two phases. The first phase consists of a household adjustment, while the second phase is a person adjustment.

In the first phase, a household adjustment is made to account for persons who resided in a wave 1 noninterviewed household. The adjustment consists of the computation of weight adjustment factors within cells defined by cross-classifications of the following variables:

1. Census region (Northeast, Midwest, South, West)
2. Residence (Metropolitan, non-Metropolitan)
3. Race of reference person (Black, non-Black)
4. Tenure (owner, renter)
5. Household size (1, 2, 3, 4 or more)

The cell assigned to each household is based on the values of these variables as of the initial SIPP interview.

The second phase of the adjustment accounts for persons who resided in a wave 1 interviewed household but who failed to respond to at least one of the remaining two interviews for reasons other than death or moving to an ineligible address. The adjustment is on a person basis and consists of the computation of weight adjustment factors within cells defined by cross-classifications of the following variables:

1. Average monthly household income of the person's household
2. Program participation status of person's household
3. Person's labor force status
4. Person's race and ethnicity

5. Years of school completed by person
6. Assets ownership status of person's household

For each of the two adjustment phases, the following ratio is computed within each noninterview adjustment cell using the weighted counts of households (or persons):

$$1 + \frac{\text{Noninterviewed households (persons)}}{\text{Interviewed households (persons)}}$$

(For variance considerations, individual cells are combined together if the ratio in a given cell is too large or contains too few cases.) For a given cell, this ratio is F_{1c} for the household adjustment phase and F_{2c}, for the person phase. These ratios are applied to the initial weight W_i of each interviewed person within a given noninterview cell. At the completion of the noninterview adjustment procedure, each person bears a weight equal to the following product:

$$W_i \times F_{1c} \times F_{2c}.$$

After the noninterview adjustments are made, noninterviewed persons are assigned zero weights. Further processing is limited to interviewed persons.

C. Adjustments To Demographic Differences From Total Population

The weighted distribution of the sample generally differs somewhat from the distribution of the total population with respect to demographic variables. This is due to two reasons. First, the distribution of the sample PSUs may not accurately represent the distribution of all PSUs due to sampling errors. This arises because in some areas one PSU is selected to represent an entire stratum of PSUs. Secondly, there exists undercoverage of households and persons within these households.

In order to reduce the mean square error (MSE) of survey estimates, two stages of adjustment are used to help bring the weighted sample distribution and the population distribution into closer agreement. This is accomplished by post-stratifying using demographic variables that are highly correlated with the variables to be measured. The first stage is designed to adjust for the sampling error associated with the sample PSUs. Undercoverage is adjusted in the second stage. Both stages are explained in greater detail below.

1. First Stage Adjustment

First stage adjustment employs a cell by cell weight adjustment procedure applied to households. For various categories of race and residence defined by the variables specified below, ratios were calculated within each adjustment cell reflecting the relationship between the estimated 1980 census household counts generated from the SIPP sample to the total population at the time of the 1980 census. (Adjustment cells are collapsed if the ratio in a given cell is too large or contains too few cases.)

a. Census region
b. Residence
c. Central city status
d. Race of household head

The weight after this adjustment is called the "first-stage weight" and is equal to the following product:

$$W_i \times F_{1c} \times F_{2c}, \times \text{(First-stage ratio)}$$

2. Second Stage Adjustment

The second stage of adjustment is applied to interviewed persons to account for undercoverage by bringing the distribution of sample persons into closer agreement with independently derived current estimates. These independent estimates are obtained using a Current Population Survey (CPS) estimation procedure developed for the CPS March income supplement [5]. The CPS estimates are used because they have a lower variance than SIPP estimates. This in turn increases the precision of the SIPP estimates.

Separate procedures are applied to sample persons aged 14 and under (children) and sample persons age 15 and over (adults). For children, a cell by cell adjustment is applied in several race x age x sex cells. For adults, a "raking" procedure is applied to adjustment tables defined by the following variables: race, age, sex, householder status, and relationship to householder status. A cell by cell adjustment for Hispanics is applied to both children and adults.

a. General Description

1. Raking Procedure for Adults

In brief, the "raking" procedure is an iterative weight adjustment procedure which aligns weighted sample counts with known marginal distributions. The method of iterative proportions which provides a best asymptotic normal (BAN) estimator in [7] is used. The procedure is used in our weighting process as one part of the second-stage adjustment for persons aged 15 years and over. It is applied here to the first-stage ratio estimates of these persons.

2. Description of Raking Procedure

The raking procedure defined below is for two marginal distributions. Define:

W_{ijk} = first-stage weight of k^{th} person in i^{th} row, j^{th} column.

$Y_{i.}$ = CPS control estimate for i^{th} row.

$Y_{.j}$ = CPS control estimate for j^{th} column.

We wish to obtain adjusted first-stage weights W_{ijk} such that $\sum_{jk} W_{ijk} = Y_{i.}$ and $\sum_{ik} W_{ijk} = Y_{.j}$

The above is accomplished by applying the Deming-Stephan method of deriving the W_{ijk} by proportionally adjusting the interior cell values until in turn each of the marginal equations is satisfied [7]. Each adjustment begins with the outcome of the previous adjustment. The process is completed when the condition equations are satisfied to a specified tolerance.

The procedure is conducted in the following manner. Below, all row adjustments are labelled with an odd superscript, while column adjustments are given an even superscript.

First, a ratio adjustment factor for the rows is computed as

$$f_{i.}^{(1)} = Y_{i.}/\sum_j \sum_k W_{ijk} \quad ,$$

followed by the computation of a column ratio adjustment factor:

$$f_{.j}^{(2)} = Y_{.j}/\sum_i \sum_k W_{ijk}\, f_{i.}^{(1)} \quad ,$$

followed by the computations of another row ratio adjustment factor

$$f_{i.}^{(3)} = Y_{i.}/\sum_j \sum_k W_{ijk}\, f_{i.}^{(1)}\, f_{.j}^{(2)}.$$

Then, an estimate of the column marginals after the third iteration is computed:

$$\hat{Y}_{.j}^{(3)} = \sum_i \sum_k W_{ijk}\, f_{i.}^{(1)}\, f_{.j}^{(2)}\, f_{i.}^{(3)}$$

$$\text{If } | Y_{.j} - \hat{Y}_{.j}^{(3)} | \quad T \text{ for all } j$$

(where T is some defined level of tolerance), then the procedure is terminated and each interior cell is assigned an overall ratio adjustment factor computed as

$$g_{ij} = f_{i.}^{(1)}\, f_{.j}^{(2)}\, f_{i.}^{(3)} \quad .$$

If the tolerance is not met, the process is continued. After each odd iteration

$| Y_{.j} - \hat{Y}_{.j}^{(Z)} |$ is checked to see if the tolerance is met for all j. The procedure is terminated when all columns meet the specified tolerance. If the process is terminated after z iterations, each cell is then assigned a ratio adjustment factor

$$g_{ij} = f_{i.}^{(1)}\, f_{.j}^{(2)} \ldots f_{.j}^{(Z-1)}\, f_{i.}^{(z)}.$$

3. **Adjustment for Hispanics**

Part of the overall second-stage procedure consists of a Hispanic adjustment procedure in order to reduce the MSE of SIPP Hispanic estimates.

For various sex by age categories of the Hispanic population, ratios are calculated based on the relationship between weighted Hispanic estimates and independent Hispanic estimates. The ratio is applied only to Hispanic persons.

4. **Age Adjustment for Children**

For persons 14 years of age and under, an age adjustment procedure is applied to reduce the MSE of children estimates.

For such persons, ratios are computed based on the weighted estimates of children to the CPS estimates of children within cells defined by race x age x sex.

b. **2nd Stage Adjustment for Children**

The overall second-stage adjustment procedure for children consists of the following steps.

STEP 1: Hispanic adjustment
STEP 2: Age adjustment

c. **Second-Stage Adjustment for Adults**

For adults, the following steps are employed in second-stage adjustment.

STEP 1: Raking procedure (all adults)
STEP 2: Hispanic adjustment
STEP 3: Raking procedure (all adults)
STEP 4: Hispanic adjustment
STEP 5: Raking procedure (non-Hispanic)

d. **Final Longitudinal Weights**

The final longitudinal weight (FW) for each person is equal to the weight generated after the second stage adjustment: FW = W_i x F_{1c} x $F_{2c'}$ x (First-stage ratio) x g_{ij}

VII. DISCUSSION

Below we raise specific issues concerning SIPP longitudinal estimation.

A. Nonresponse is a particularly serious problem for a longitudinal survey such as SIPP since cumulative nonresponse increases as the life of the panel increases. A study on nonresponse behavior in SIPP has identified groups with differential nonresponse in the survey [12]. The effectiveness of the estimation procedure described above in reducing bias due to nonresponse is unknown. Research needs to be conducted to evaluate the effectiveness of this procedure in reducing these biases. If necessary, alternative adjustment methods should be explored.

B. Research in other areas of estimation need to be conducted. In particular:

1. Kalton, Lepkowski and Lin in [10] have examined the weighting adjustment versus imputation issue for handling nonresponse. They suggest that a combination of the two may be appropriate for certain types of wave nonresponse. Research needs to continue to determine wave nonresponse patterns that should be adjusted for by an imputation approach instead of a weighting adjustment.

2. The method described in this paper does not make use of those persons who failed to respond to the first interview but responded to subsequent interviews. The reliability of SIPP longitudinal estimates would improve if data on these persons could be utilized in an estimation procedure. Thus, we need to explore ways to use the data on such persons.

3. Longitudinal imputation in SIPP may adversely affect transition and spell estimates. Research needs to be conducted in this area to determine the effect of imputation on these type of estimates.

C. It is well known that a time-in-sample bias exists for other Census Bureau demographic surveys [8], [13]. Such a bias is likely to exist in SIPP. We have been unable to evaluate this bias in SIPP because of lack of data. As data accumulates for more SIPP panels, this bias should be evaluated.

D. There are other sources of biases in SIPP. For example, as respondents learn more about the survey their response to certain questions may be affected. Due to lack of knowledge on this and other biases, the procedure in this paper does not attempt to adjust for such biases. The effect of these biases need to be examined as they may affect SIPP estimates such as transition estimates. Research is now in progress to develop estimators with smaller bias for such estimates. A large scale effort in this area is needed. The research to be conducted should identify estimates with large biases as well as how to adjust for such biases.

REFERENCES

[1] U.S. Department of Commerce, Bureau of the Census. "The Current Population Survey: Design and Methodology," by Robert Hanson. Technical Paper 40. Superintendent of Documents No.: C2.212:40; January 1978. Reprinted July, 1985.

[2] Census Bureau memorandum from G. Shapiro for T. O'Reagan, "Sampling Specifications for the 1984 Panel of the Survey of Income and Program Participation (SIPP)," January 31, 1983.

[3] Census Bureau memorandum from D. Hubble for Documentation, "SIPP 1984 Sample Design: Selection of 1984 Panel SIPP Sample PSUs," February 28, 1983.

[4] Nelson, D. McMillen, D., and Kasprzyk, D., "An Overview of the Survey of Income and Program Participation," SIPP Working Paper Series No. 8401, Update. U.S. Bureau of the Census, Washington, D.C., 1985.

[5] Census Bureau memorandum from C. Jones for T. Walsh, "SIPP 85: Cross-Sectional Weighting Specifications for Wave 1 -Revision," November 21, 1985.

[6] Census Bureau memorandum from C. Jones for T. Walsh, "SIPP 1984 - Specifications for Longitudinal Weighting of Persons," July 17, 1986.

[7] Ireland, C.T. and Kullback, S. "Contingency Tables with Given Marginals", Biometrika, vol. 55, 179-188, 1969.

[8] Census Bureau memorandum from R. Singh to G. Shapiro, "Nonsampling Errors in the National Crime Survey (NCS)," February 15, 1985.

[9] Judkins, D.R., Hubble, D. L., Dorsch, J. A., McMillen, D. B., Ernst, L. R., "Weighting of persons for SIPP Longitudinal Tabulation"; Proceedings of the Section on Survey Research Methods, American Statistical Association, 676-680, 1984.

[10] Kalton, G., Lepkowski, J., Lin, T., "Compensating for Wave Nonresponse in the 1979 ISDP Research Panel", Proceedings of the Section on Survey Research Methods, American Statistical Association, 372-377, 1985.

[11] Little, R. and David, M., "Weighting Adjustment for Nonresponse in Panel Surveys," Bureau of the Census Working Paper, 1983.

[12] Census Bureau memorandum from McArthur and Short for distribution list, "Measurement of Attrition from the SIPP through the Fifth Wave of 1984 Panel," April 10, 1986.

[13] U.S. Department of Commerce, Bureau of the Census, "An Error Profile: Employment as Measured by the Current Population Survey" by Camilla A. Brook and Barbara Bailar, Statistical Policy Working Paper 3., September 1973.

ACKNOWLEDGEMENTS

David Judkins did much of the planning and early work upon which this paper is based. This paper owes much to Mr. Judkins' excellent insight. We also wish to thank Graham Kalton, Martin David and Daniel Horwitz for their contributions in helping us resolve issues related to SIPP longitudinal weighting.

The authors also wish to thank Joan George for her excellent typing and good humor through numerous revisions.

LABOR FORCE TRANSITIONS: A COMPARISON OF UNEMPLOYMENT ESTIMATES FROM TWO LONGITUDINAL SURVEYS[1]

Martha S. Hill, Survey Research Center, Univ. of Michigan
Daniel H. Hill, Survey Research Center, Univ. of Michigan

Introduction

The existence of micro-level panel data has lead to some major revisions in the way we think of labor market behaviors, bringing into question the very concept of unemployment as an unanticipated negative shock (e.g., Heckman and MaCurdy, 1980; MaCurdy, 1981; and Ashenfelter and Ham, 1979) and providing a novel perspective on the costs and even the definition of unemployment (e.g., Adams, 1985, and Abowd and Ashenfelter, 1981). The policy implications[2] are of such importance that we need to be very careful that they are not merely artifacts of the data due perhaps to measurement problems. Certain aspects of this issue have already been investigated. A number of studies of the quality of self-reported unemployment, for example, have recently appeared in the literature (e.g., Bowers and Horvath, 1984; or Poterba and Summers, 1985), and these present evidence of appreciable errors in reports of unemployment status and duration. Evidence of failure to report spells of unemployment is presented by Mathiowetz (1984), who finds that omission of reports of unemployment spells increases substantially with the length of time from the termination of the spell to the interview.

While validation studies would offer the most reliable evidence on the quality of unemployment data, comparisons across different surveys can also be informative. The Survey of Income and Program Participation (SIPP)[3] and the Panel Study of Income Dynamics (PSID)[4] are two studies with similar unemployment information collected from comparable segments of the population, but with an important design difference. The SIPP data is collected on a more frequent basis than the PSID, and Mathiowetz's findings suggest that the more frequent interviewing schedule of the SIPP should result in more complete reporting of unemployment spells, especially short ones.

The purpose of the present analysis is to compare reports of unemployment experiences for the July through December 1983 period obtained from the PSID and the SIPP. Duration of unemployment and transitions to employment are the focus since these labor force experiences are frequent enough during a short time span to allow relatively precise estimates with moderate sized surveys. The research will address two questions. First, do measures of the incidence and duration of unemployment differ between the studies?, and, second, do these differences result in different estimates of the parameters of a multivariate model of unemployment?

Survey Methodologies

In addition to the differences in frequency of contact between the PSID and the SIPP, there are also differences in question design. The PSID tends to require more of the respondent in terms of contextual detail but also provides more structure to the questions. On the other hand, the SIPP provides more precise dating of employment events. The PSID obtains information on the timing of unemployment events in two distinct question sequences. One sequence proceeds iteratively asking a series of questions about increasingly remote jobs and unemployment spells between jobs, until the entire previous calendar year is accounted for. Since this would not capture periods of temporary layoff, with returns to the same job, a second set of questions asks for total amounts and timing of work lost due to specific labor force events such as illness, unemployment, strike, or vacation during the reference year. The data from these sequences are extensively edited for completeness and consistency in the SRC's Ann Arbor Coding/Editing facility and are processed in the form of monthly dating for a variety of employment events. The SIPP procedure consists of providing the respondent a calendar of the weeks of the four-month reference period just proceeding the interview and requesting a report of which weeks were ones with a job, which were weeks with unpaid absences from a job and what the main reason was for them, and which were weeks looking for a job.

In addition to differences in frequency of interviews (and, thereby, length of recall), and the method of eliciting the information from the respondent, the two studies differ in designations of individuals (and "families") and in who is to be the informant. Since it was designed in 1966, the PSID uses the, now archaic, "head of household" definition of the designated respondent, with husbands reporting for their wives. The SIPP, on the other hand, designates each adult as a respondent and attempts to interview all such persons in its sample households. Both studies allow proxy reports when the designated respondent is not available.

Sample Restrictions and Definition of Variables

Whenever one wishes to compare or combine two datasets it is necessary to restrict them to their common content. Often this intersection of elements is only a small fraction of the total content of either study. The present analysis is no exception.

Because of differences in the timing of reference period of the two studies we are forced to restrict our reference period to a rather short segment of the total era of history covered by either study. Since the period of history covered by the two studies is the beginning of what has come to be known as the Reagan Recovery, we cannot assume any stationarity in unemployment behavior and, thus, must restrict the comparisons of the unemployment data in the two studies to the same time period. Since the SIPP sample was introduced to the study on a rotating basis, we face a trade-off between the number of SIPP rotation groups included and the length of the period over which we will measure unemployment experiences. A compromise was struck with the selection of the six month period from July to December 1983, a period covered by the first two rotation groups of the SIPP. This, of course, throws out far more information from the SIPP than from the PSID, and analysts must remember in evaluating our findings that more precise estimates for both studies, and especially the SIPP, are possible if the studies were to be analyzed independently. The period is, however, long enough to capture important aspects of transitions out of unemployment, as Feldstein's (1976) work stressing the importance of temporary layoffs suggests.

A further restriction in the comparative analysis concerns the sample. In the PSID, the detailed employment questions are asked only of 'heads' for themselves and of heads about their 'wives'. This means that in order to make comparisons across the studies it is necessary to restrict the larger SIPP population of inference to individuals who would be so classified by the PSID. Thus, we restrict the SIPP sample to those individuals whose relationship to the "reference person" is either self or spouse. Furthermore, since in 1983 individuals who were out of the labor force (i.e. retired, 'housewife', permanently disabled or student) at the time of the PSID interview were not asked the detailed employment sequence,[5] it is necessary to further restrict the SIPP sample to persons who are either employed or unemployed in the second week of April 1984 (the modal 1983 PSID interview week). Finally, a natural extension of the restriction to those either employed or unemployed at the time of interview was to confine the sample further to people always in the labor force (i.e., employed or unemployed) throughout the July to December period. The net result of these restrictions are comparable samples of 5218 in the PSID and 6212 in the SIPP for what we will term the "adult persistent labor force".

Our analysis also attempts to maintain comparability in the definition of variables. In both studies unemployment includes both time looking for work when without a job and temporary layoff. Time with a job and either working, sick, on strike, or on vacation is counted as employment time. The variables used as covariates in the multivariate analysis are limited to a set available in both studies. These variables are believed to affect either the individual's potential wage in a new job (i.e. age, education, race, and gender) or his reservation wage (i.e. family income needs level, the earnings of other family members, asset income, means tested transfer income, and whether receiving unemployment compensation). A table describing the variables is available from the authors upon request.

The bivariate analyses are weighted to correct for differences in initial probabilities of selection and for differential nonresponse. Limitations of the computer programs precluded weighting in the multivariate analysis.

Results

Table 1 presents the proportions of individuals in the two (restricted) samples who experience some unemployment during the six months for which the studies overlap. These figures are presented separately for men and women as well as both genders combined. As we would expect given the shorter recall period, respondents in the SIPP are somewhat more likely to report having some unemployment. The 11.2% average incidence estimate from the SIPP is roughly fifteen percent greater than the estimate from the PSID. While we have not yet computed complex sampling errors for either estimate, sampling errors computed under the assumption of simple random sampling would suggest that this difference is significant at the five percent level. With typical design effects from the PSID of less than 1.5, we would expect this difference to remain close to the margin of significance even with complex sampling errors. The slightly greater difference between SIPP and PSID unemployment incidence estimates for women is not, however, significant even under the assumption of simple random sampling.

Table 1
Percent of Adult Persistent Labor Force Members Unemployed at Some Time July-December 1983
[Sample Sizes in Brackets]

	Men	Women	All
SIPP	11.4% [3,666]	11.0% [2,552]	11.2% [6,218]
PSID	10.0% [2,970]	9.3% [2,242]	9.7% [5,212]

When attention is confined to the subsample reporting some unemployment, more dramatic differences between the studies appear (see Table 2). The average amount of time reportedly lost from work due to unemployment for males in the SIPP is nearly a month (4.11 weeks) longer than that in the PSID. This difference of nearly forty percent is highly significant and, combined with the fact that the average number of transitions out of unemployment reported for males in the SIPP (.53) is lower than that in the PSID (.65), suggests that a major difference between the two studies is a higher proportion of long-term unemployed in the SIPP. The corresponding differences for women are barely perceptible and are far from significant. These same patterns persist when the spell itself is used as the unit of analysis. Since we expected short spells of unemployment rather than long ones to be better reported in the SIPP because of its more frequent interviewing schedule, these results are somewhat puzzling.

Table 2
Mean Total Unemployment and Transitions Out of It Among Adult Persistent Labor Force Members Unemployed Some Time July- December 1983

	Men	Women	All
Mean (Standard Deviation) Weeks Unemployed			
SIPP	15.08 (8.45)	14.00 (8.30)	14.65 (8.40)
PSID	10.97 (8.98)	14.10 (8.97)	12.17 (9.10)
Mean (Standard Deviation) Number of Unemployment to Employment Transitions			
SIPP	0.53 (0.68)	0.56 (0.70)	0.55 (0.69)
PSID	0.65 (0.78)	0.51 (0.55)	0.59 (0.69)
Number of the Unemployed Persons in Samples			
SIPP	422	272	694
PSID	360	261	621

A Proportional Hazards Model of Transitions to Employment

In order to obtain some preliminary notion of how the study differences might affect the estimates of structural model parameters, we use the data to estimate a proportional hazards model of the transition from unemployment to employment. Following Cox (1972) we assume that the hazard rate of re-employment for individual i at time t is of the form:

$$\lambda^i(t) = \lambda(t)\, e^{\beta' x_i} \qquad 1)$$

where x_i is a vector of the characteristics of the individual. As was the case in selecting the time period and types of individuals to include in the analysis, we are limited in our selection of individual characteristics to those which are collected in a comparable manner in each of the studies. These consist of demographic characteristics (age, race, gender) and measures of their resources (asset income, income of other family members, welfare income, and a dummy for whether they received unemployment compensation during the period) and needs (the official poverty needs standard for the family in which they lived during the period). Estimates of the parameter vector β are obtained by maximizing the partial likelihood function:

$$L_1 = \prod_{i=1}^{n} [(e^{\beta' x_i}) \div (\sum_{h \epsilon R(t_i)} e^{\beta' x_h})] \qquad 2)$$

where n is the number of completed spells and $R(t_i)$ is the set of uncompleted spells at time t_i. This latter group can be thought of as the set of individuals still 'at risk' (unemployed) when individual i moves from unemployment to employment.

We estimate the proportional hazards model under a variety of conditions—first for the PSID and the SIPP separately and then combining the two studies to test for differences. Variants of the same comparisons suggested by differences in this analysis are then explored.

Table 3 presents the parameter estimates obtained when the proportional hazards model is estimated on the two data sets separately. Both studies tend to exhibit effects in the expected directions, and there is considerable agreement in the signs and magnitudes of their coefficients. In both studies the rate of exit from unemployment to employment is higher the younger and the better educated the individual is. Positive coefficients on the non-black indicator also appear in both studies. Being better educated and non-black would tend to raise wage offers whereas being younger would tend to increase the number of job offers since the pay-back period for training a worker would be longer. Higher wage offers and more job offers would facilitate earlier exit from unemployment to employment.

Table 3
Maximum Partial-Likelihood Estimates
of the Proportional Re-employment Hazard Model,
Separately for PSID and SIPP

	SIPP	PSID
Age	-.0135* (.0069)	-.0105 (.0078)
Education	.0083 (.0189)	.0072 (.0258)
Non-Black	.1229 (.1585)	.4831** (.1174)
Male	-.0502 (.1093)	.2127+ (.1212)
Needs	.0275 (.0203)	.0322 (.0240)
Others' Earnings	.0007 (.0053)	.0041 (.0063)
Asset Income	-.0464 (.0356)	.0001 (.0241)
Welfare Income	-.0667+ (.0347)	-.1387** (.0385)
Whether Unemployment Compensation	-.2758** (.1046)	-.1437 (.1136)
Chi-Square	19.92	53.36
d.f.	9	9
N	797	692

– Significant at .10 level.
* Significant at .05 level.
** Significant at .01 level.

The income and needs covariates represent factors affecting the individual's reservation wage, the wage level that a job offer would have to match or exceed in order to attract the individual to employment. The greater the availability of income other than that to be realized by the individual working, the higher the individual would tend to set his or her reservation wage, which would tend to prolong unemployment. The financial needs of the person's family would also tend to influence the reservation wage, but in the opposite direction—lowering it and thus making the individual more likely to accept a job offer. Welfare income, whether unemployment compensation received, and the needs variables produce coefficients in both studies consistent with this model.

Differences between the two studies do appear, however. When the two samples are combined, and a study indicator is included both additively and multiplicatively with other predictors, we are able to reject the hypothesis that the two studies yield equivalent measures of employment transitions and their determinants.[6] Significantly different effects appear for the non-black indicator and welfare income. The PSID estimates larger absolute effects of these two factors than does the SIPP. While part of the difference in the effects of welfare income could be due to differences in the components included (the SIPP includes WIC and energy assistance, whereas the PSID does not), it is unlikely that definitional differences are the underlying factor in the differential effects of race.

The results of Table 3, with the two studies examined separately, indicate that, in terms of the overall goodness-of-fit, the PSID measures have a significantly stronger systematic component than do the SIPP data. While the χ-square of 19.92 with 9 degrees of freedom for the SIPP does indicate a significant overall relationship between the hazard rate for re-employment and its explanatory variables, it is only marginally significant, and is much less significant than the χ-square of 53.36 obtained with the same variables for the PSID. This difference is due primarily to the much stronger effects of the race and welfare income on the probability of exiting unemployment to employment in the PSID. We should note, of course, that a superior fit of our re-employment model does not, in itself, indicate that the PSID data are better. It indicates only that the types of transitions observed with the PSID data are more strongly related to the predictor variables. It may well be that SIPP is better at detecting short spells of unemployment but that these episodes are less predictable than are the more salient spells reported in the PSID.

Both the SIPP and the PSID samples contain spells with observed start dates and 'left-censored' spells (ones with start dates predating the onset of the observation period). Left-censored spells present problems for hazard analysis, thus the sensitivity of our results to their inclusion is an issue. To investigate this, we stratified the samples in both the SIPP and the PSID into left-censored spells and non-left-censored spells and calculated distinct survival probabilities for each type of spell in each study.[7] The results are presented in Figures 1 and 2, for the PSID and the SIPP, respectively.

Focusing first on the non-left-censored curves, we find substantial agreement between the two studies, but some notable differences. For both the SIPP and PSID the survival curve becomes flatter with time, as we would expect.[8] In addition, during the four weeks following the onset of unemployment and over the long-duration range of unemployment (16–26 weeks) the curves are similar in shape. In the intermediate range, though, they differ; there the SIPP shows a more gradual re-employment process than does the PSID. Thus, again, the SIPP indicates more unemployment. But, apparently, the additional unemployment is from more intermediate-length spells rather than more spells of short duration.

Deviations of left-censored curves from expectations provide additional insight into the SIPP-PSID difference. A left-censored curve can be expected to follow a particular pattern in terms of its shape and placement relative to its corresponding non-left-censored curve. Since the non-left-censored survival curve flattens with time, we would expect the left-censored curve to start at a higher level, be even flatter, and so end at an even higher level than the non-left-censored one[9].

With the PSID (Figure 1) we find roughly this pattern, although the left-censored curve flattens less rapidly than expected. Measurement error from using monthly data to measure weekly dating could be causing this divergence from expectations.

A more dramatic divergence from expectations arises in the SIPP data (Figure 2). For the left-censored cases the likelihood of exit to employment is much greater in the 13–17 week range than anywhere else. This distorts the pattern of

Figure 1

Unemployment Survival PSID

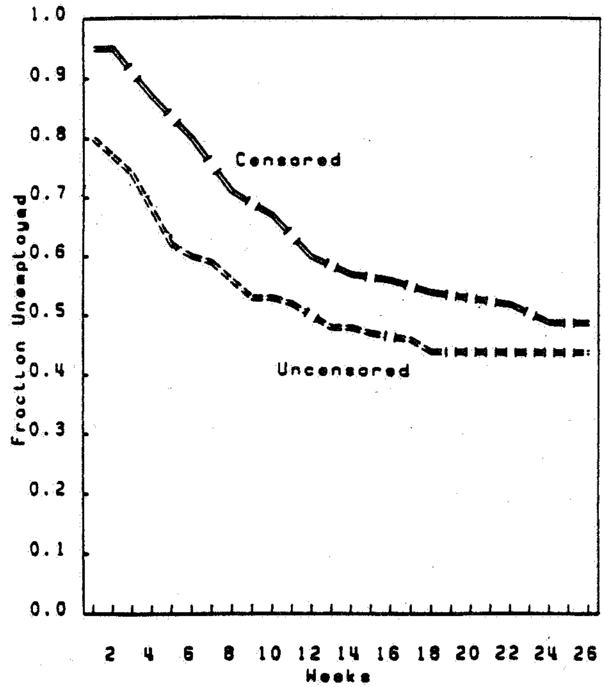
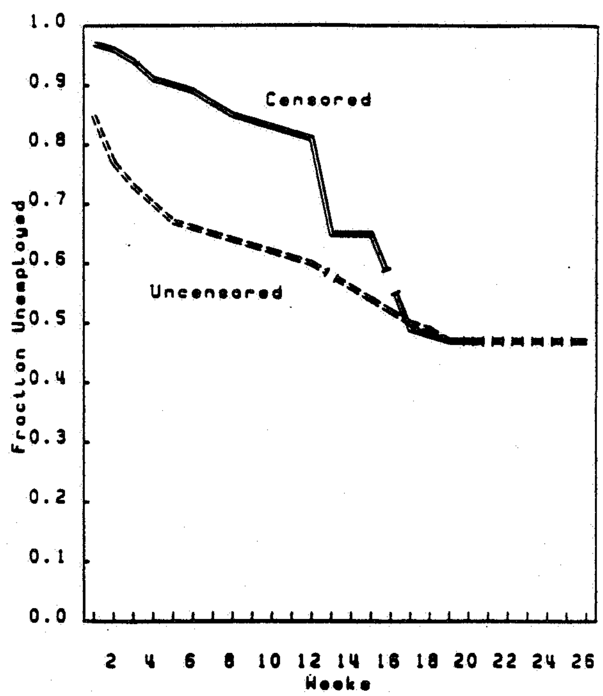


Figure 2

Unemployment Survival Functions-SIPP



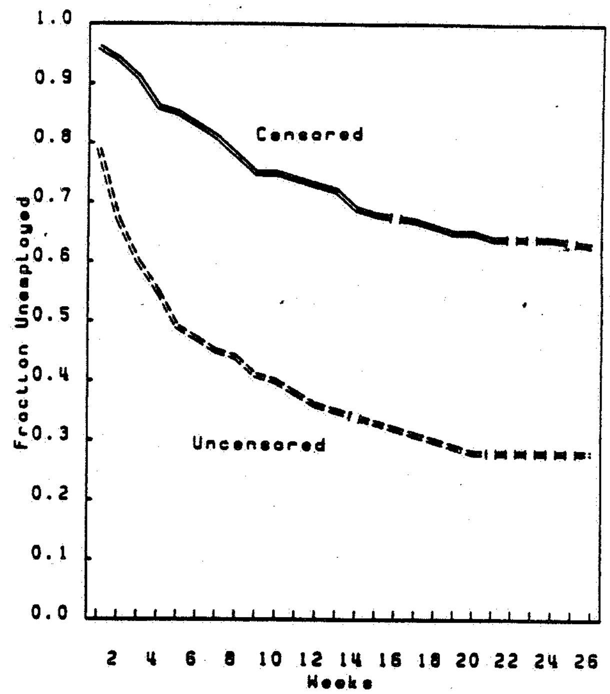
an ever-flatter slope, and, in fact, causes the left-censored curve to merge with the non-left-censored curve beginning at the 17-week point.

This length for an unemployment spell coincides very closely with the spell length associated with a transition from unemployment throughout Wave 1 of the SIPP* to employment at the very beginning of Wave 2.[10] Since the reported unemployment-to-employment transitions in the SIPP are, indeed, more likely to happen at the wave-to-wave seam than at other times[11], it is likely that transitions reported then are more prone to misreporting. Erroneous reports cannot be individually identified, but likely candidates include those obtained for persons with movements from unemployment to employment, or the reverse, at the wave-to-wave seam <u>and no other time</u>. While eliminating the seam-only transition cases is <u>not</u> a viable solution to the reporting error problem, it is a useful technique for obtaining an approximate idea of its magnitude.[12]

With the SIPP sample modified in this way, the pattern of the left-censored survival curve relative to the non-left-censored one is exactly as expected—initially higher and flattening out more quickly over time so that the final level difference is greater than the initial one.[13] (See Figure 3.) Further, reestimating the combined PSID-SIPP model with the SIPP portion modified as described above substantially reduces the differences in the model estimates for the two studies: the global χ-squares of the models with and without the study-specific interactions drops from about 30 (d.f. 10) to 17, a difference which is significant at only the .10 rather than .01 level. Erroneous seam transitions, thus, appear to account for a substantial part of the difference in the multivariate results of the SIPP and the PSID.

Figure 3

Unemployment Survival SIPP w/o Seams



Conclusions and Directions for Future Research

While the PSID and SIPP employment event history sequences are intended to measure the same labor market behaviors, differences in the two study designs do have significant effects on the measures obtained. While both studies undoubtedly miss some episodes of unemployment, the more frequent interviewing schedule of the SIPP does seem to result in a more complete accounting of unemployment than does the PSID. For comparable periods of history and populations of inference the SIPP obtains estimates of unemployment incidence which are roughly fifteen percent higher than those obtained in the PSID. Since it is less likely that individuals will report unemployment when they have had none than it is that casual unemployment will be forgotten, this result, along with the larger sample size of the SIPP, would argue in favor of analysis of SIPP data over PSID data for studies of unemployment incidence. However, the SIPP is not without some questionable aspects.

For males with some unemployment the estimated amounts of unemployment are dramatically higher in the SIPP than in the PSID. In this case, however, it is less clear that more is better. This result could, for instance, be a reflection of a greater tendency, say, with a short reference period to report being unemployed the entire reference period, even when there were, in reality, some periods of employment. Proxy reports may, however, be the major problem. SIPP and PSID results concerning levels of unemployment among women are quite similar, and for that subgroup the frequency of proxy reports is of comparable size in the two studies. The PSID, however, has a much lower frequency of proxy reports for men than does the SIPP.

It is unclear which study is superior for the purpose of estimating the parameters of multivariate behavioral models of unemployment durations or transitions. Although the signs of the parameters of our proportional hazards model were found to be in general agreement across the studies, there were marginally significant differences in the magnitudes of a number of parameters and strongly significant differences in the overall goodness of fit. Overall the PSID data tended to yield stronger associations between the probability of re-employment and the exogenous variables included in the analysis. The effects of race and welfare income on the probability of becoming re-employed are much stronger in the PSID. The larger sample and better coverage of short spells of unemployment would seem to argue for use of the SIPP data in studies focusing on only one of the two studies, but the problem of inordinately large numbers of reported transitions occurring at the seams of the waves is sufficiently serious and difficult to model as to make the PSID an attractive alternative.

The study differences we have detected in the present analysis suggest a number a lines of potential future research. First, whether the SIPP does in fact obtain better reports of short spells needs to be investigated with longer observation periods which will allow the inclusion of all four SIPP rotation groups. Second, the predominance of transitions at the seams needs to be further investigated to see to what extent it is a reflection of the higher proportion of proxy interviews in the SIPP overall, and thereby more switches between self- and proxy reports. Imputations may also be a source of concern. Finally, the 'seam problem' should be investigated in the PSID as soon as the second wave of detailed employment event histories are merged to the first. Since in the PSID the reference periods are designed to overlap by six months, it should provide considerable methodological leverage in the analysis of these within/between wave inconsistencies.

REFERENCES

Abowd, J.M. and Ashenfelter, O.C. (1981) "Unemployment and Compensating Wage Differentials" (Report No. 7923) (Chicago: University of Chicago, Center for Mathematical Studies in Business and Economics).

Adams, J.D. (1985) "Permanent Differences in Unemployment and Permanent Wage Differentials" *Quarterly Journal of Economics* 26–56.

Ashenfelter, O.C. and Ham, J. (1979) "Education, Unemployment, and Earnings" *Journal of Political Economy*, 87:99–116.

Bowers, N. and Horvath, F.W. (1984) "Keeping Time: An Analysis of Errors in the Measurement of Unemployment Duration", *Journal of Business and Economic Statistics* 2:140–149.

Cox, D.R. (1972) "Regression Models and Life Tables." *Journal of the Royal Statistical Society* ser. B. 34:187–220.

Cox, D.R. (1975) "Partial Likelihood", *Biometrika* 62:269–844.

Feldstein, M. (1976) "Temporary Layoffs in the Theory of Unemployment", *Journal of Political Economy* 937–957.

Heckman, J.J. and MaCurdy, T. (1980) "A Life Cycle Model of Female Labor Supply" *Review of Economic Studies* 47: 47–74.

MaCurdy, T. (1981) "An Empirical Model of Labor Supply in a Life Cycle Setting" *Journal of Political Economy* 89: 1059–1085.

Mathiowetz, N.A. (1984) "The Problems of Omissions and Telescoping Error: New Evidence from a Study of Unemployment" *Proceedings of the American Statistical Society, Session on Survey Methods*.

Poterba, J.M. and Summers, L.H. (1985) "Adjusting the Gross Changes Data: Implications for Labor Market Dynamics", *Proceedings of the Conference on Gross Flows in Labor Force Statistics* (Washington: U.S. Departments of Commerce and Labor) 81–95.

FOOTNOTES

[1]The authors gratefully acknowledge the helpful comments of Greg Duncan, Nancy Mathiowetz, and Willard Rodgers.

[2]See Feldstein (1976) for discussion of the theoretical and policy implications of temporary layoffs.

[3]The SIPP is a survey of households conducted by the Bureau of the Census with data for a panel collected every four months for a two and one-half year period. It began collecting information in October 1983.

[4]The PSID is an annual survey of households conducted by the Survey Research Center of the University of Michigan. It has been collecting income and labor market information, among other things, from the same sample of individuals, and their descendents, each year since 1968. Since 1983, detailed information on the timing of work history events has also been collected.

[5]Beginning in 1984 the detailed sequences were asked of all PSID primary adults regardless of labor-force status at the time of the interview.

[6]The global χ-square obtained when all the study indicator interactions plus a study indicator dummy are included is 82.82 (d.f. 19), which is significantly higher than the 52.94 (d.f. 9) when they are not. A table with the full detail of this analysis is available from the authors upon request.

[7]Ideally one would like to know the beginning date of all spells, but when this is not possible a second-best solution is to restrict the sample to non-left-censored spells. For our samples, though, the left-censored spells constitute a very sizable portion of all unemployment spells: in the SIPP 424 of the 797 unemployment spells sampled are left-censored, and in the PSID 339 out of 692 spells are left-censored. Thus eliminating left-censored spells would also present problems. The stratification approach was the third best alternative.

[8]The flattening with time is expected since, over time, the people remaining at risk will increasingly become ones with circumstances not amenable to becoming employed.

[9]This follows because spells classified as left-censored are known to have begun on or before what we treat as their beginning, and thus most are at a further stage of unemployment than a non-left-censored case tracked for the same length of time.

[10]The length of time between the end of our observation period and the 'seam' between the waves is thirteen weeks for rotation group 1 (as is the length of time from the beginning of our reference period and the seam for rotation group 2), and seventeen weeks is the length of a full wave.

[11]When we partition the SIPP into pairs of weeks for each person we obtain 17,350 person-week pairs. The mean fraction of these pairs which involve a transition from unemployment to employment is .0218. Looking only at the 694 pairs of weeks at the seam, however, the comparable figure is 1211. and for those 16,656 person-week pairs not at the seam it is .0177 The chances of a re-employment event being reported between waves is, therefore, slightly less than seven times as great as within a wave.

[12]We do not, of course, recommend this as a solution to the seam problem in estimating behavioral models from the SIPP. We drop the 'seam' cases merely as a means of seeing if the study differences could be caused by them.

[13]The level of the modified-SIPP-sample curves should not be taken as representative of the unemployment process since removing those individuals from the SIPP who reported transitions from one homogeneous state to the opposite at the wave seam can be expected to eliminate a disproportionate number of long spells of unemployment. It is not surprising, then, that the modified SIPP sample yields lower survival probabilities than either the PSID or the full SIPP sample.

AN ADDITIVE MODEL OF RECALL ERROR: ANALYSIS OF SIPP DATA

Daniel H. Hill, Survey Research Center[1]

Interviewing in the Survey of Income and Program Participation (SIPP) is conducted on a four month rotating schedule and respondents are asked about their experiences over the last four months. One consequence of this design is that, for any particular calendar month, reports involve anywhere from one to four months of recall, depending upon which 'rotation group' the respondent has been assigned. Furthermore, since SIPP is a panel study, the extent of respondent 'conditioning' also varies from one rotation group to the next after the first four interviewing months. It is quite possible that the quality of the data is affected by both the length of recall and extent of conditioning. If so, efficient estimation of monthly population parameters would require that this heterogeneity of the data quality be taken into account.

The purpose of this paper is to capitalize on the SIPP design to test for the *existence of* (rather than the precise patterns of) differences in data quality which are systematically related to length of recall or extent of respondent conditioning. The paper is organized into three sections. In Section I we briefly describe the SIPP design and incorporate it into an additive model of recall error. In the next section we describe our sample and estimating procedures, while in the third section we present the empirical results.

Before going on to our description of the SIPP design, a couple of words on why length of recall and extent of respondent conditioning should affect data quality are in order. Length of recall affects data quality because as the recall period increases so does the probability that the respondent will fail to recall the particular events which are used to construct the full response to the survey question. Furthermore, respondent errors in the placing of events in time, also increase with length of recall, but at a decreasing rate. Respondent conditioning may also affect data quality, although it is difficult to know in what direction. On the one hand, as the respondent becomes experienced with the survey, he learns what is expected of him and can better prepare himself to provide accurate answers. On the other hand, as the novelty of the survey experience wears off, the respondent may be more willing to simplify reality in order to take short-cuts in the interviewing process.

Section 1
SIPP Design and a Model of Recall Error

The SIPP questionnaire is administered every four months to the same representative sample of adults in the U.S. Each respondent is asked about his earnings from each of his jobs in each of the four months of the reference period. These reports are taken as our dependent variables. To save costs in training interviewers, the sample is split into four random sub-samples, or 'rotation groups', which are interviewed sequentially in a monthly rotating fashion. The first rotation group was interviewed in October 1983 and was asked about monthly earnings for the June through September period. The second rotation group was first interviewed in November and asked about the July through October period. Etc., etc. The result of this design is that, for any given calendar month, reports involve anywhere from one to four months of recall depending upon rotation group membership. Table 1 summarizes the relationship between rotation group and the amount of recall, as well as the number of times-in-sample, associated with each monthly earnings report for the September 1983 through May 1984 period.

TABLE 1
LENGTH OF RECALL

Reference Month	Length of Recall by Rotation Group 1	2	3	4
Sep ('83)	1	2	3	4
Oct	4	1	2	3
Nov	3	4	1	2
Dec	2	3	4	1
Jan	1	2	3	4
Feb	4	1	2	3
Mar	3	4	1	2
Apr	2	3	4	1
May	1	2	3	4

In order to see how this design can be used to address the question of whether differential recall errors represent a significant problem, it is necessary first to develop a model of the reporting process. Following, to the extent possible, O'Muircheartaigh's (1986) notation person j's earnings as reported during trial t for month m can be expressed as:

$$y_{mjt} = \bar{y}_m + \Delta_{mj} + \beta_j + \epsilon_{jt} \qquad 1)$$

where: $\bar{y}(m)$ is the true average earnings in month m of the population of inference; $\Delta(mj)$ is individual j's true deviation from this average; $\beta(mj)$ is the 'fixed response error' or bias; and $\epsilon(mjt)$ is the variable response error associated with trall t. Both the β and the variance of the ϵ can be expected to be affected by the length of recall and number of times in sample, while neither $\bar{y}$ nor Δ—the structural portion of the response—will be affected by these conditions of the interview.

The expectation of 1) across both trials and individuals is:

$$E_{tj}(y_{mjt}) = \bar{y}_m + \bar{\beta} \qquad 2)$$

where $\bar{\beta}$ is the average of the fixed response errors or the bias. For the reasons noted in the introduction, we would expect $\bar{\beta}$ to be a function of length of recall and number of times in sample. While we could attempt parameterizations of this function, a more flexible alternative is to treat the various combinations of length of recall and number of times in sample as distinct discrete shift parameters where $\beta(cr)$ is the mean fixed response error associated with reports involving r months recall and c times in sample.

With this notation in mind we are now ready to see how the SIPP rotating design allows investigation of our hypotheses. Table 2 illustrates some of what can and cannot be estimated using the SIPP design. The top panel includes the expected value of reports for September and October 1983 from each of the four rotation groups. In all there are eight linear equations in seven unknowns ($\bar{Y}$(s), $\bar{\beta}(11)$, $\bar{\beta}(12)$, $\bar{\beta}(13)$, $\bar{\beta}(14)$, $\bar{Y}$(o), and $\bar{\beta}(24)$). Unfortunately, however, these eight equations are not independent. Just as age, period and cohort effects are inseparably linked, so are month, recall and time-in-sample effects. We can not identify the individual parameters.

TABLE 2
LENGTH OF RECALL

Reference Month	Expected Value of Monthly Reports by Rotation Group 1	2	3	4
Sep ('83)	$\bar{y}_s+\beta_{11}$	$\bar{y}_s+\beta_{12}$	$\bar{y}_s+\beta_{13}$	$\bar{y}_s+\beta_{14}$
Oct	$\bar{y}_o+\beta_{24}$	$\bar{y}_o+\beta_{11}$	$\bar{y}_o+\beta_{12}$	$\bar{y}_o+\beta_{13}$

Sep ('83);$\bar{y}^*_s$;$\bar{y}^*_s+(\beta_{12}-\beta_{11})$;$\bar{y}^*_s+(\beta_{13}-\beta_{11})$;$\bar{y}^*_s+(\beta_{14}-\beta_{11})$
Oct;$\bar{y}^*_o+(\beta_{24}-\beta_{11})$;$\bar{y}^*_o$;$\bar{y}^*_o+(\beta_{12}-\beta_{11})$;$\bar{y}^*_o+(\beta_{13}-\beta_{11})$

Where $\bar{y}^* \equiv \bar{y} + \beta_1$

What we can identify is the fixed recall error of all but one of the recall groups relative to that of the one. This is illustrated in the bottom panel of Table 2. In essence if we take one month recall as our 'norm' (i.e. $y^* \equiv y + \beta(11)$) then we can see how reports involving more than one month's recall differ from this norm.[2] While this is less than we would like, it is enough for our immediate purpose of testing for the existence of recall error.

While we could investigate the significance of recall group bias simply by performing ANOVAs of mean reports, a more efficient and flexible alternative procedure is available which allows some investigation of recall error variances. Since we do not want to attribute differences in levels or variances to recall groups which are due to differences in the systematic portion (Δ) we first formulate a traditional human capital model of labor earnings which is appropriate for the situation in which levels in some months are zero According to this model earnings are determined by the level of individual investments in human capital. The two principal forms of these investments are formal education and on-the-job learning which is generally measure by years of experience. Thus:

$$\Delta_j = F(Ed_j,Exp_j) \simeq \sum_{i=0}^{k} (\delta_{Ei}\, Ed_j^i + \delta_{xi}\, Exp_j^i) \qquad (3)$$

where Ed(j) and Exp(j) individual j's years of formal education and experience on the job, measured as deviations from average values, and the δ's are structural parameters relating earnings to human capital investments.

Of course, there is a very large number of other factors which will affect any given person's earnings, but, for the most part, the importance of each of these other factors, taken individually, is small. One important exception to this is race, the effect of which on earnings is far from negligible. All other factors can be collapsed into a single stochastic error term ψ_i. It is often argued that, as a result of the large number of excluded factors and the central limit theorem, this error term can be assumed to be normally distributed. It is also generally assumed that ψ is uncorrelated with earnings, education, experience and race and that its variance is constant.

With these assumptions the behavioral model becomes:

$$\Delta_j \simeq \sum_{i=0}^{k} (\delta_{Ei}\, Ed_j^i + \delta_{xi}\, Exp_j^i) + \delta_R\, Race_j + \psi_j \qquad 4)$$

$$= \Delta x_j + \psi_j$$

where Δ is the vector of structural parameters, and x(j) is the vector of powers of individual j's education and experience, and his race.

The combined measurement and behavioral model is obtained by substituting Δ from equation 4) into equation 1) to yield the following additive model of recall error:

$$y_{mj} = \beta_{cr} + \Delta x_j + \psi_i + \bar{\epsilon}_{mj} \qquad 5)$$

In addition to the assumptions identified above, identification requires that the measurement errors ϵ be uncorrelated with the behavioral errors (ψ), the determinants of earnings (x), and actual earnings.[3]

Section II
Data and Estimation

The model was estimated using data for prime-age (25–55 years old) males who had at least two months with some employment in the first three waves of the 1983 SIPP panel. In all there were approximately 6300 such individuals in rotation groups one through three.[4] In order to eliminate confounding effects of proxy respondents, however, the sample was limited to those men who provided their own reports in each of the three interviews. This apparently innocuous restriction resulted in approximately seventy percent of the cases being eliminated from our sample. Finally, roughly ten percent of the cases with imputations on wage and salary items, as well as cases with self-employed income, in any one month were filtered from the sample. The result of these eliminations is a rather special subsample of the population which is of a quite manageable size (1378 cases). Since it is, to a certain extent a 'self-selected' subsample,[5] inferences to the overall prime-age male workforce should be quite guarded. Nevertheless, unless there are different mechanisms operating in the various rotation groups which determine self- versus proxy-reporting behaviors, the behavioral model should still be common to each rotation group,[6] and tests of the effects of recall and conditioning should remain valid. Indeed, if there are systematic difference in the selection mechanism then they should show up as rotation group effects—the significance of which we will test in the following section.

Estimation was performed by comparing the product moment matrix implied by the model presented in equation 5) with the actual product moment matrix calculated from the sample. The product moment matrix implied by the model is:

$$\Sigma = n \begin{bmatrix} [\Gamma X]' X \Gamma + \Psi + \sigma_\epsilon & X' X \Gamma \\ \Gamma X' X & X' X \end{bmatrix} \qquad (6)$$

where $\Gamma' = [\Delta|\beta]$ and $X' = [x|R]$. The submatrix R is a (9x3) matrix composed of dummy variables for rotation group membership in each of the nine months.

The concentrated log-likelihood of the model given the sample is:

$$L = \mathrm{Log}|\Sigma| + tr(S\Sigma^{-1}) - \log|\Sigma| - C - 1 \qquad (7)$$

where C is the rank of S and Σ. We should note that with R containing dummy variables for all three rotation groups in our sample, X matrix is singular. We *must*, therefore, impose constraints on the β's to obtain unique solutions. The constraints we choose are those cross-month constraints which allow us to examine the hypotheses of recall bias, conditioning, and, alternatively, rotation group bias.

Once the constraints on the β are imposed, the above function can be minimized with respect to the β, δ, ψ and $\sigma(\epsilon)$ to yield full information maximum likelihood estimates. The various hypotheses regarding recall and conditioning biases can be tested by performing likelihood ratio tests using the minimum value of equation (7) under the alternative sets of constraints placed on the β's.

In actual practice we use the LISREL algorithm of Jöreskog and Sörbom (1976) to perform the estimation. Sufficient statistics for this consist of the (weighted) means and covariances of the sample. This formulation of the model is especially convenient for testing the various hypotheses regarding the form of the fixed response error. For testing hypotheses regarding error-variances, an alternative structure of the LISREL model is more convenient. This involves treating each recall group as a separate sample and estimating grouped systems of monthly data. The structural parameters (except the constant) are constrained to be equal across groups, while the relative measurement error variances are allowed to vary across groups.

Section III
Empirical Results

Table 3 presents the estimates of the mean difference in reporting bias in each recall/TIS group from that of the first recall and TIS group (i.e. $\beta(jk) - \beta(11)$) under various hypotheses. In addition to these estimated relative bias estimates (and their standard errors computed under the assumption of simple random sampling), the table provides the value of the likelihood function (and relevant degrees of freedom) from which likelihood ratio tests of the hypotheses can be performed. The first row of results in Table 4 refer to the hypothesis that length of recall is the only factor affecting reporting bias. Implementing this hypothesis involves relaxing three of the original 27 over-identifying restrictions incorporated in the model. When this is done twice the value of the likelihood function declines from its fully restricted value of 28.9 (not shown) by eight and one half units. Since twice the value of the likelihood function is distributed χ-square with degrees of freedom equal to the number of over-identifying restrictions, it is apparent that significant improvements in the goodness of fit are accomplished by allowing for recall-bias effects. Further significant improvements in goodness-of-fit are obtained when both time-in-sample and length of recall are allowed (see column 2). We accomplish this by permitting the effects of the recall group membership to vary from one TIS to the next. In all, this involves relaxing ten of the original 27 over-identifying restrictions, and results in a 19.4 unit decrease in the value of the fitting function.

While the recall and TIS effects are significant as a group, the estimated individual coefficients are sufficiently imprecise as to make it difficult to interpret their pattern. Only the coefficient on four-months recall when wave effects are allow is sufficiently large in relation to its estimated standard error to attain statistical significance. That it is significantly negative, is consistent with the type of memory model suggested by Sudman and Bradburn (1964) in which recall errors are assumed to stem from, in our application, a tendency for respondents to fail to recall more distant paychecks or to mis-place the occurrence of these payments in time. The Sudman and Bradburn model, however, would suggest a pattern of reported levels which would decline monotonically with length of recall. The point estimates in Table 3, if plotted against length of recall, would provide a very distinct impression of a 'saw-tooth' pattern. This pattern may reflect a tendency for respondents in the various recall groups to systematically misplace weekly paychecks from one month to the next.

An alternative hypothesis which is closely related to the recall error hypothesis, however, is that rotation group membership, *itself*, is associated with the response errors. A comparison of the values in the third row of Table 3 with those of the first indicates that we can reject this alternative interpretation of data. The χ-square statistic associated with the rotation group hypothesis is nearly four units larger than that associated with the recall error hypothesis yet involves the same number of over identifying restrictions.[7] Furthermore, relaxing the restrictions necessary to implement the former hypothesis does not significantly improve the overall goodness of fit, whereas relaxing those associated with the recall error hypothesis does.

Table 3
Estimated Biases Various Reporting Bias Hypotheses

	Hypothesis	
	Recall Bias Only	Recall and TIS Bias
$\beta_{12} - \beta_{11}$	-7.61 (10.42)	20.21 (30.28)
$\beta_{13} - \beta_{11}$	9.01 (10.05)	37.32 (55.15)
$\beta_{14} - \beta_{11}$	-19.17 (10.07)	- (-)
$\beta_{21} - \beta_{11}$	0 (-)	-32.83 (100.09)
$\beta_{22} - \beta_{11}$	-7.61 (10.42)	-52.67 (75.98)
$\beta_{23} - \beta_{11}$	9.01 (10.05)	-39.17 (52.47)
$\beta_{24} - \beta_{11}$	-19.17 (10.07)	-61.40* (30.23)
$\beta_{31} - \beta_{11}$	0 (-)	66.53 (51.72)
$\beta_{32} - \beta_{11}$	-7.61 (10.42)	26.01 (58.99)
$\beta_{33} - \beta_{11}$	9.01 (10.05)	39.87 (68.38)
$\beta_{34} - \beta_{11}$	-19.17 (10.07)	-3.36 (80.61)
χ-Square	20.4	9.4
d.f.	24	17

SRS Standard Errors in Parentheses.
Fully restricted χ-square: 28.9 (d.f.=27).
χ-square for Rotation bias only: 24.3 (d.f.=24). χ-square for Rotation and Wave bias: 11.1 (d.f.=19).

The preceding analysis suggests that there is significant differential bias resulting from length of recall and extent of conditioning in the SIPP reports of monthly earnings Furthermore. this differential bias is above and beyond that which can be accounted for by differences in subsample education. age and race, which are 'controlled' in our analysis. This analysis does not, however, shed light on the relative importance of differential bias and differential error-variance. In order to address this question it is necessary to analyze each recall group as a separate system of equations and impose the common structure of the behavioral model across these systems. Various hypotheses can then be incorporated in this group of systems by altering the cross-group constraints.

Table 4 presents the results of such analyses performed on the September, January and May earnings data for the same subsample employed in our preceding analysis.[8] Looking only at the first three rotation groups we are able to test hypotheses regarding the differential bias and error-variance for two and three months recall relative to one month recall.[9] The first row presents the value of the likelihood function obtained when neither differential bias nor error-variance are allowed. The second and third rows report the likelihood function values when bias only and bias and error-variance are allowed, respectively. As in the earlier analysis allowing for differential bias associated with length of recall and number of times in sample results in a relatively large and significant decrease in the value of the fitting function. This improvement in the goodness of fit, however, is not nearly as dramatic as that obtained when the restrictions that the error-variances are identical across recall and TIS groups are relaxed. Removing these over identifying restrictions results in a drop in the log-likelihood value of nearly fifty points (49.5). Since, under the null hypothesis that these improvements are due solely to chance, twice the decline in the log-likelihood is distributed χ-square with twelve degrees of freedom, we must conclude that differences in the reporting error variances across recall and TIS groups are quite important and extremely significant.

The pattern of relative recall-error variances for the various recall/TIS groups are quite interesting. For all three TIS groups, the rotation group associated with two months of recall has significantly lower estimated error variances than either the one or the three month recall groups. Although this initial decline in error-variance with recall seems quite peculiar, it is consistent with certain models of telescoping, such as Sudman and Bradburn's, based on "Weber's law of perceived time", according to which errors in the placement of events in time increase logarithmically with elapsed time. If the reporting process calls first for bounding the calendar month in perceived time and then summing the individual recalled paychecks received in that perceived period, then the first recall period will be longer in actual elapsed time than subsequent periods. If all people telescope at the same rate, are paid on the same schedules, do not forget entire paychecks, and are interviewed at the same time then this process would result in monotonically declining biases but relatively stable error-variances. To the extent that people do vary with respect to telescoping rates, pay schedules, and interviewing dates, however, then error variances can be expected to decline with recall length so long as the basic logarithmic pattern of telescoping errors holds.

Eventually, according to these models, telescoping errors will be overwhelmed by errors of omissions which increase monotonically with length of recall, and we should see a reversal in the direction of the pattern of biases and variances over time—something which seems to hold in the bottom panel of Table 4.[10]

Table 4
Estimated Relative Error Variances

	Goodness of Fit		
	χ-Square	d.f.	
Fully Restricted	130.5	48	
Bias Only	118.4	42	
Bias and Variance	19.4	30	
	Sep	Jan	May
$\sigma^2_{\epsilon(12)} - \sigma^2_{\epsilon(11)}$	-2798** (978)	-7775** (1222)	-4779** (1041)
$\sigma^2_{\epsilon(12)} - \sigma^2_{\epsilon(11)}$	-1758 (1016)	-4277** (1347)	-564 (1217)

Conclusions

In this initial analysis of SIPP earnings data we have found evidence of significant differential reporting bias and error-variance associated with length of recall and extent of respondent conditioning. The alternative hypothesis that data quality is a function of rotation group membership *itself* is not supported by the data.

For the particular model, subsample and measures we have examined, the statistical importance of the differential relative error-variances is much greater than that of the differential relative biases. Unlike our earlier work with the PSID Validity Study (see Duncan and Hill. 1985). however. we can not say reporting-error variance is in an absolute (nor even a mean-squared-error) sense more important than reporting bias.

The implication of these findings is that efficient estimation of monthly population parameters from the SIPP will require some corrections for the differential quality of the data from the various rotation/recall groups. Because there is evidence of significant differential bias as well as differential error variance, a proper treatment of the problem may require obtaining validating data for the SIPP instrument.

References

Duncan, Greg J. and Daniel H. Hill, (1985) "An Investigation of the Extent and Consequences of Measurement Error in Labor Economic Survey Data", *Journal of Labor Economics*.

Jöreskog, Karl, and Dag Sörbom (1976), *LISREL III: Estimation of Structural Equation Systems by Maximum Likelihood Methods*, (Chicago: National Educational Resources).

O'Muircheartaigh, Colm, (1986) "A General Model of Response Errors", *Response Variance in Surveys*, (Chicago: American Statistical Association Tutorial, August 1986).

Sudman, Seymour and Bradburn, Norman (1964), *Response Effects in Surveys*, (Chicago: Aldine Publishing Co.)

Footnotes

[1]The author would like to thank Bob Groves, Graham Kalton and Dan Kasperzyck for their helpful comments on earlier drafts of this paper.

[2]One month recall is equivalent to the CPS methodology.

[3]While these assumptions are frequently made in measurement error models, the latter two are particularly unfortunate in light of our previous findings the reporting error of annual earnings is negatively correlated both with actual earnings and experience (See Duncan and Hill, 1985).

[4]Rotation group four is omitted in the present analysis because the Wave 3 questionnaire was never administered.

[5]By this we mean that the individual's own behavior determines, in part, whether he is available at the time of the interview or whether someone else must report as a proxy for him.

[6]Initial selectivity bias analyses show only negligible differences between rotation groups in the self/proxy probit estimates. The dominant determinants in these models are relation to reference person, type of family, marital status, and employment status.

[7]A slightly different answer suggests itself when the combined hypotheses of recall and conditioning are contrasted with rotation group and wave hypotheses. In this case we can not reject one of the joint hypotheses in favor of the other. The fact that the wave and TIS hypotheses are operationally equivalent means that these joint hypotheses are, perhaps, too closely related to be differentiable.

[8]We limit our attention to this relatively small subset of months because the computational costs of examining groups of large systems is beyond our current resources.

[9]With these three rotation groups we can also estimate the differential bias and error variance of three and four months recall relative to two month recall using August, December and April.

[10]An alternative hypothesis is that the observed pattern of variances is not really related to length of recall so much as it is to rotation group membership. We might be able to test this alternative by repeating the analysis for October of 1983 and February 1984 when reports from rotation group 2 involve only one month's recall and those of rotation group 1 involve four months of recall. If the Sudman-Bradburn recall model is generating the data then we would expect to observe the same 'v' shaped pattern of estimated response variances. Unfortunately, we have not yet been able to develop a formal comparative test of these alternative models.

INVESTIGATION OF GROSS CHANGES IN INCOME RECIPIENCY FROM THE SURVEY OF INCOME AND PROGRAM PARTICIPATION

Lynn Weidman, U.S. Bureau of the Census

INTRODUCTION

The Survey of Income and Program Participation (SIPP) is a longitudinal survey of households that collects economic information about the U.S. population. For two and one-half years the members of a household are interviewed at four month intervals and information is obtained for each of the four months preceding an interview. (This four month period is also called a "wave.") One type of estimate that can be derived from this monthly data is that of the number of people who change their response to a question between consecutive months or between any two fixed time points. A previous study (Burkhead and Coder, 1985) examined month-to-month changes in receipt of five different income types and two noncash benefits. It showed that, for the first twelve months of SIPP, the number of reported changes in recipiency status between the last month of one interview period and the first month of the next interview period was far greater than the number reported between any two months of the same interview period. Burkhead and Coder discussed these differences in relationship to questionnaire wording/design and respondent recall error.

In this investigation we are looking for more direct causes of the discrepancy in the between/within interview numbers of gross changes. (A gross change between two times is the number of people in state A at the first time and state B at the second time. The distributions of gross changes refers to these numbers for a specified set of pairs of states. We will be looking at reported gross changes only.) There are three phases of this investigation.

1. Empirical analysis of data to determine if demographic characteristics of individuals are related to the discrepancy.
2. Description and estimation of models for the effect of time in sample, recall lag and other sources of response error on reported gross changes.
3. Estimation of response error from outside sources and use of it in conjunction with the models.

Here we will present an empirical analysis and examine any significant results. Two models for relating error sources to gross changes are then proposed and presented for use in the next phase of investigation.

EMPIRICAL ANALYSIS

The goal of empirical analysis is to use simple methods to detect the existence of obvious relationships between demographic/interview characteristics and changes in receipt status of seven income types and food stamps. There are four receipt states for two consecutive months: RR, RN, NR and NN, where R = receipt and N = nonreceipt. The income types of interest are social security, unemployment compensation, private pensions, VA compensations and pensions, supplemental security income, child support and AFDC. They will be examined with respect to age, sex, race, marital status, education, relationship to principal person, household size, tenure, SMSA size and interview status. The distribution of gross changes in receipt status between consecutive months for each income type will be computed with respect to all pairs of demographic characteristics. This will produce 360 sets of distributions for examination. Any apparent relationships may suggest other distributions for examination.

The categories used for demographic variables are defined as follows.

age: 15-30, 31-45, 46-60, 61+
sex: male, female
race: white, nonwhite
education: elementary, high school, above high school
marital status: married, (separated, divorced, widowed), never married

household size: 1,2,3,4-5,6+
tenure: home owned, not owned
relationship to reference person: reference person, spouse, child, other
SMSA size: not in an SMSA, 1 million +, less than 1 million
interview status for consecutive months: SS,SP,PS,PP where S=self, P=proxy

The file of monthly data was created from the first four waves of data available for each household. Each of these waves is searched for all persons who reported receipt of any of the income types of interest during any month of the wave. For each such person all the information available for the 16 month period is collected and placed on a record. This record will then be used if the person was interviewed for each of the four waves. (Restricting the analysis to these persons follows the Burkhead and Coder data set selection for the first twelve months.) A wave on the record was then used only if it was preceded by a wave of matching data. This ensures that the last three months of a wave are used in the calculations only if the first month is also. (An important fact to remember is that the large majority of people are not included on this file because they do not receive any of these income types.)

How will we determine if any relationships exist? When the monthly gross changes are computed there are usually two to five times as many RN and NR reported for the first month of a wave as there are for the other three months. (See Table 1.) For any pair of demographic variables to be a determinant of this change, we would have to observe a huge difference in the number of RN and NR reported in the first months of waves as compared to the last three months for some combination(s) of these variables, but not for others. We will be looking for one or more combinations to exhibit this behavior.

As a theoretical example of the distributions that were calculated see Table 2. There are two such tables for each comparison. The first is for all first months of a wave combined (between waves) and the second is for all months two, three, and four combined (within waves). This means that the total number of observations in the second table is three times the number of observations in the first.

TABLE 2
RACE

		white		non-white	
SEX	male	P_1RR P_1NR	P_1RN P_1NN	P_2RR P_2NR	P_2RN P_2NN
	female	P_3RR P_3NR	P_3RN P_3NN	P_4RR P_4NR	P_4RN P_4NN

Within each cell defined by a particular combination of demographic characteristics we calculate the probability of each receipt state, P_iAB=P (receipt state AB/cell i). Let P_iAB_w denote such a probability within waves and P_iAB_b the corresponding between wave probability. Compare P_iNR and P_iRN for between waves to those for within wave. If this demographic combination has no relationship to gross changes, the ratios P_iNR_b/P_iNR_w should be fairly constant for i, as should the ratios P_iRN_b/P_iRN_w. If one and/or both of these sets of ratios differ "greatly" between cells, this indicates the type of relationship we are looking for. (It is important to note that no statistical tests were performed. Comparisons are made by examining distributions for specified types of "noticeable" differences.)

When examining interview status the situation is somewhat different because two of the interview status pairs, PS and SP, cannot occur within waves. In this case we look for large differences in the distributions of P_iNR_b and P_iRN_b between cells.

Examination of these tables showed no major relationships between demographic variables and the gross changes. Some small differences in distributions occur, but nothing on the order of magnitude of the between/within wave gross change differences. As an example, see Table 3, sex x race for food stamps.

TABLE 3.A

Food Stamps: Between Waves
Race x Sex

Race	Sex	RR	RN	NR	NN
white	male	44.3 (547)	11.8 (146)	6.1 (75)	37.9 (468)
	female	59.7 (1560)	7.8 (205)	6.2 (163)	26.2 (684)
non-white	male	54.0 (262)	10.3 (50)	7.6 (37)	28.0 (136)
	female	68.9 (1086)	6.2 (97)	4.7 (74)	20.3 (320)

TABLE 3.B

Food Stamps: Within Waves
Race x Sex

Race	Sex	RR	RN	NR	NN
white	male	49.3 (1830)	2.0 (73)	3.1 (116)	45.6 (1695)
	female	64.2 (5031)	2.0 (154)	2.2 (172)	31.6 (2479)
non-white	male	61.2 (891)	1.4 (20)	1.6 (23)	35.8 (521)
	female	72.6 (3433)	1.4 (64)	1.7 (79)	24.4 (1155)

First entry in each cell is percent of total responses in row. Second entry is number of responses in cell.

Food stamps, social security and unemployment compensation were the sources with relatively large numbers of transitions reported. (I.e., with enough transitions to compare distributions for many cells.) The first two of these sources showed about the same patterns. Larger proportions of receipt of sources were reported by self-respondents than by proxies. There is usually a higher proportion of transitions between waves when at least one of two consecutive months has a proxy response than when both of the months are self-reported. As an example, see Table 4. Because the number of SS cases was much larger than the sum of SP, PS, and PP cases, these patterns did not have a noticeable effect on the within/between wave jumps. (For unemployment compensation there is a much larger number of cases with NN. The patterns are similar, but the difference in proportions are much smaller.)

TABLE 4.A

Food Stamps: Between Waves
Sex x Interview State

Sex	Interview State	RR	RN	NR	NN
Male	SS	54.5 (456)	9.4 (79)	6.0 (50)	30.1 (252)
	SP	45.7 (106)	12.5 (29)	8.6 (20)	33.2 (77)
	PS	38.2 (76)	16.1 (32)	8.0 (16)	37.7 (75)
	PP	37.7 (171)	12.4 (56)	5.7 (26)	44.2 (200)
Female	SS	65.5 (2326)	6.8 (240)	5.2 (184)	22.6 (802)
	SP	53.9 (125)	9.1 (21)	8.5 (20)	28.4 (66)
	PS	43.1 (103)	9.2 (22)	9.2 (22)	38.4 (92)
	PP	55.4 (92)	11.4 (19)	6.6 (11)	26.5 (44)

TABLE 4.B

Food Stamps: Within Waves
Sex x Interview State

Sex	Interview State	RR	RN	NR	NN
Male	SS	57.3 (1782)	1.5 (47)	2.5 (77)	38.7 (1202)
	PP	45.7 (939)	2.2 (46)	2.7 (56)	49.3 (1014)
Female	SS	68.1 (7750)	1.7 (198)	2.1 (236)	28.0 (3189)
	PP	59.8 (714)	1.7 (20)	1.3 (15)	37.3 (445)

MODELS

Since the empirical analysis failed to reveal any relationships between demographic variables and the distribution of gross changes, we must look for another way of determining their true distributions. For CPS it has long been known that there is a relationship between the responses to a question and (i) the amount of time that has elapsed between the month of interest and the month of interview, (ii) the interview status and (iii) the length of time a person has been in the sample. Here we propose models for gross changes that make use of similar relationships.

The dependent variable of interest for a given income type is the receipt state identified with the second of two consecutive months. The possible receipt states for month t are (1)=RR, (2)=RN, (3)=NR, (4)=NN. Let $y_{ijkt(\ell)}$ be the number of responses in receipt state ℓ in month t where

- i = number of times a person has been interviewed,
- j = number of months between month t and month of interview,
- k = interview status for months t-1 and t; PP,PS,SP and SS with S=self, P=proxy.

Then the vector $\underline{y}_{ijkt}$ = $(y_{ijkt(1)}, \; y_{ijkt(2)}, \; y_{ijkt(3)}, \; y_{ijkt(4)})$ represents the gross change counts for the combination ijkt.

Multivariate Normal Models

Since the $\underline{y}_{ijkt}$ are vectors of counts, they have a multinomial rather than a multivariate normal distribution. But because of the large sample sizes on which they are based (the total number of counts in y_{ijkt}), they have that distribution asymptotically. We propose a multivariate analysis of variance (MANOVA) model of the form

$$E(y_{ijkt(\ell)})=\mu_{(\ell)}+N_{i(\ell)}+M_{j(\ell)}+S_{k(\ell)}+ NM_{ij(\ell)}+NS_{ik(\ell)}+MS_{jk(\ell)}+\gamma_t \qquad (1)$$

where the terms are

- N_i = interview number i,
- M_j = months of recall between month of interview and month of occurrence,
- S_k = interview status,
- NM_{ij}, NS_{ik}, MS_{jk} are interactions of these effects, and
- γ_t = month t.

There are some difficulties we must take account of before using this model.

(1) Levels 2 and 3 of k occur only with j=4. This means that the cells which are defined with j=4 and k=1 or 4 contain structural zeros. The contrasts in the analysis that define the effects and their degrees of freedom must be consistent with these structural zeros.

(2) The effect for interview number is to determine if reporting of changes in state follows some pattern over time. For example, a person may report the specific month of transition in wave 1, but after that he reports all transitions as occuring in the first month of a wave. Suppose now that there is a proxy respondent for waves 2 and 3. Will the proxy behave as the self respondent did for wave 1, or as he would for wave 2, or in some different manner? In a strict sense this effect only has validity if the same respondent is available in each wave. However, we can still include this effect as an average response difference between successive interviews.

(3) Most of the data that is used in this modeling is not available on the file we are using. Recall that only persons who have received one of the eight income sources in the first 16 months of SIPPS are included in this file. The vast majority of persons have no receipt for the first 16 months and would thus have the receipt state NN for each of the months used in modeling. From the files for individual waves we would have to calculate the number of these persons in each cell defined by an ijkt combination. The most time-consuming part of this job would be matching records across waves.

Polytomous Logit Models

There is another approach we can take to this problem that does not require a multivariate normal distribution. Instead of modeling the frequency of each receipt state we can model the probabilities of the states with polytomous logit models. A brief description of these models is given.

Let an observation consist of a set of

independent variables $\underline{x}_i$ and a dependent variable y_i, where y_i falls into one of G mutually exclusive categories. Let $\underline{\beta}_g$ be a set of coefficients for category g,g=1,2,...G. Assume that

$$\text{Prob } (y_i=g) = \exp (\underline{x}_i' \underline{\beta}_g)/\sum_{g'=1}^{G} \exp (\underline{x}_i' \underline{\beta}_{g'}). \qquad (2)$$

The unknown $\underline{\beta}_g$, g=1,2,...G, can be estimated by maximum likelihood, where the likelihood function is

$$[\prod_{i=1}^{N} \exp (\underline{x}_i' \underline{\beta}_{h(i)})]/[\sum_{g=1}^{G} \exp (\underline{x}_i' \underline{\beta}_g)]^N$$

and h(i) is the category into which y_i falls. Note that the probability in (2) remains constant if all $\underline{\beta}_g$ are multiplied by a constant, so a single linear restriction must be placed on the $\underline{\beta}_g$'s to obtain unique maximum likelihood estimates.

We propose using this logit model approach to estimate the true proportion of responses in each receipt state at each time t. Let $\underline{x}_{ijkt}$ be the vector of 0-1 variables that indicate which main effects and interactions are present for each observation with a particular ijkt combination. Let $\underline{\beta}_\ell$ be the vector of corresponding effects for receipt state ℓ. Each observation that is counted in $y_{ijkt(\ell')}$ will contribute a term of the form

$$\exp (\underline{x}_{ijkt}' \underline{\beta}_{\ell'})/ \sum_{\ell=1}^{4} \exp (\underline{x}_{ijkt}' \underline{\beta}_\ell) \qquad (3)$$

to the likelihood function. Thus we only need to compute all the $\underline{y}_{ijkt}$ in order to determine the likelihood function and the resulting maximum likelihood estimates $\hat{\underline{\beta}}_\ell$, ℓ=1,2,3 or 4. Then the estimated proportion of observations in receipt state ℓ for combination ijkt is obtained by substituting the $\hat{\underline{\beta}}_\ell$ into (3).

The same difficulties that were described for MANOVA models are also present here.

When using either of these modeling approaches we would test for main effects and interactions being zero in order to determine which of them influence the reporting of changes in receipt. For MANOVA models standard procedures are available and for logit models likelihood ratio tests are used for nested models; i.e., for testing that certain entries in $\underline{\beta}_\ell$, ℓ=1,2,3,4, are zero.

SUMMARY

An empirical examination did not detect any relationships between gross change distributions and nine demographic variables and interview status. Modeling approaches are proposed for estimating the true number and proportion of each receipt state for a particular combination of interview number, months recall, interview status and month. Tests of significance for main effects and interactions can be carried out to determine which of them influence reporting of changes in receipt status. The resulting models could be used to adjust the reported gross changes toward the actual gross changes. More consideration of the validity of the models and the amount of work required to carry out estimation needs to be done before carrying this work further.

Mention should be made of another study that is in progress at the Census Bureau. A comparison of administrative records obtained from four states with SIPP data is being made to investigate the relationship between reported and actual changes in status. We hope to be able to use these results in conjunction with models to get an improved estimate of gross change distributions.

REFERENCES

Burkhead, Dan and John Coder (1985). "Gross Changes In Income Recipiency From the Survey of Income and Program Participation." American Statistical Association, Proceedings of the Social Statistics Section.

Hogue, Carma (1984). "History of the Problems Encountered in Estimating Gross Flows." Proceedings of the Conference on Gross Flows in Labor Force Statistics, Washington, D.C., 1-5.

Nerlove, Marc and S. James Press (1973). Univariate and Multivariate Log-linear and Logistic Models. Rand Corporation, R-1306-EDA/NIH.

TABLE 1

Month-to-Month Gross Changes: Food Stamps

Receipt Status	1st to 2nd	2nd to 3rd	3rd to 4th	4th to 5th	5th to 6th	6th to 7th	7th to 8th	8th to 9th	9th to 10th	10th to 11th	11th to 12th	12th to 13th	13th to 14th	14th to 15th	15th to 16th
RR	1240	1255	1274	1159	1270	1278	1287	1161	1260	1261	1265	1135	1216	1205	1219
RN	40	47	35	174	26	38	42	167	33	36	29	157	25	44	40
NR	62	54	61	129	46	51	51	123	37	33	40	97	33	54	43
NN	653	639	625	517	652	627	614	519	659	659	655	572	713	684	685

MEASURING THE BIAS IN GROSS FLOWS IN THE PRESENCE OF AUTO-CORRELATED RESPONSE ERRORS

David L. Hubble and David R. Judkins, U.S. Bureau of the Census

I. INTRODUCTION

Frequently, a categorical variable will be observed at two or more points in time. The interior cells of the cross-classification of two observations are commonly referred to as gross flows or gross changes. Gross flow estimates are potentially of tremendous value in understanding processes. However, estimates are subject to very complex nonsampling errors that have discouraged their use.[1] In fact, the concept may be fundamentally unmeasureable in the sense that any attempt to measure gross flows may change the characteristics of the process.[2] The most serious problems usually present are mismatched observations, observations not missing at random, and misclassification in the observations. In this paper, we focus on misclassifications for dichotomous variables. To the best of our knowledge, prior work on the effect of misclassifications has assumed that misclassifications on the two observations are independent. We have developed a technique that takes advantage of the design of the Survey of Income and Program Participation (SIPP) to estimate the effect in the presence of auto-correlated errors. Even though not all requirements for the technique are currently met by SIPP design, we did try applying it. In Section II, we present a summary of the technique and the exploratory application. In Section III, we make recommendations for design changes in SIPP and indicate areas for future study. In Section IV, we discuss the technique in detail. In Section V, we present the application.

II. SUMMARY

Several features of the SIPP design are essential to the technique.[3] First, the reference period covers more than one point in time. (The SIPP reference period is four months for most variables.) Second, interviewing is staggered over several points in time (four months); i.e. one fourth of the sample is interviewed each month. Third, each person is interviewed repeatedly with each reference period immediately following the preceding period; i.e. there are no gaps. Taken together, these features tures imply that there are four measurements of the gross flows between any pair of consecutive months. (See Figure 1.)

Figure 1. Time in Sample by Rotation and Reference Month

Reference Month	Rotation 1	2	3	4
February	3	2	2	2
March	3	3	2	2
April	3	3	3	2
May	3	3	3	3
June	4	3	3	3
July	4	4	3	3

Example: Gross flows between April and May are observed from the third interview for rotations 1, 2, and 3. For rotation 4, they are observed by matching the second and third interviews.

Three of the measurements come from single interviews (the gross flows are within a single reference period), while one measurement comes from a pair of consecutive interviews. A final feature that is required but only partially satisfied is a reinterview program to supply corrected gross flows within reference periods. (While there is a SIPP reinterview program, it was not designed with this objective.)

The combination of error rates, dual within/between reference period measurements, corrected within period gross flows, and a few extra assumptions, would allow us to get a rough feeling for the correlation between measurement errors for consecutive months when the measurements are taken four months apart. If we could get that far, there is some reason to hope that the correlation would be similar for nonconsecutive months when measurements are taken four or more months apart. Given the error rates and the correlation, the bias in the gross flows would then be estimable.

This technique is admittedly weak. Only the intensity of interest in gross flows and the comparable weakness of known alternatives induced us to present it. Its greatest weakness is the requirement for a rigorous reinterview program to produce accurate reinterview data on gross flows within periods. Current survey reinterview programs are most effective at detecting curbstoning (interviewer fraud).[4] Beyond that, they are notoriously unreliable. Note, however, that we do not require the common assumption that the reinterview be independent of the original interview.[5] Nor do we require multiple reinterviews of the same respondent as has been recommended as a technique for dealing with correlated misclassifications.[6] (Field staff is generally strongly opposed to multiple reinterview contacts.) The alternative to reinterview data is administrative data. It is not clear whether the record-matching problems there will be much less severe than the problems with reinterview data. Besides, the number of variables for which administrative data exist is very limited.

Faced then with this dilemma, we decided to forge ahead, making whatever assumptions were required, in order to get some feeling for the magnitude of the bias in estimated gross flows from SIPP. We are, of course, aware that our estimates are extremely crude; we only hope that they will be viewed as being at least marginally useful in understanding a very difficult and pressing problem.

Due to the lack of reliable data including the reinterview data, we were forced to restrict the scope of our analysis to the characteristic of food stamps. Even that was in the form of a sensitivity analysis. Varying the parameters (error rates, etc.) used in the technique was necessary to assess the robustness of our results. Our analysis showed the results to be fairly robust. For almost all combinations of the parameter values, the bias in the gross flow estimates appears to be quite serious.

III. RECOMMENDATIONS AND FUTURE STUDY

We have demonstrated that the user of these estimates is taking a serious risk. Estimates of exit and entrance rates (defined in Section IV) might easily be substantially biased. It is thus clear that further and better research is urgently needed. We outline some avenues for future study below and welcome additional suggestions. Unfortunately, this research will take time. Meanwhile, data users require some guidance. Our only suggestion at this point is that users examine the ratios of month-to-month exit and entrance rates as observed between reference periods to those observed within reference periods. For those characteristics with large ratios, statements about gross flows over longer periods should be very tentative.

- Perhaps we should focus more on how gross flows change over time than on the gross flows themselves. (This is done, for example, with CPS income estimates.) Note, however, that this requires stable instruments, procedures, and interviewing staff; so far, SIPP has changed a fair amount from panel to panel.

Areas for possible future study:

- Redesign reinterview program. Emphasize estimation of monthly error rates. Also, explore procedures other than simple repetition of original questions.
- Match SIPP into administrative databases. For some characteristics, obtain biases in gross flows directly. For others, obtain error rates for use in the technique proposed in this paper. Administrative data may also allow us to see if the relationship between true and observed gross flows depends on status at other points in time, such as, the time of interview or intervening time.
- Select special samples with known longitudinal characteristics from lists of program recipients, employees, taxpayers, etc.
- Subjectively examine gross flows to see if they "make sense."
- Explore reference periods of different lengths.
- Explore methods for increasing correlations between subsequent interviews such as conditioning response with a reminder of past response or longitudinal reconciliation.
- Explore the applicability of Colm O'Muircheartaigh's work on the correlation between interview and reinterview.

IV. DETAILED DESCRIPTION OF METHOD

Consider a Bernoulli variable observed at two points in time on one sample of a population. Assuming that the population is held constant, each unit can have one of four joint time statuses: (1,1), (1,0), (0,1), or (0,0). We will refer to these as flow types 1, 2, 3, and 4 respectively. Let $T=(T_1,\ldots,T_4)^T$ denote the population mean vector for the four gross flows. Let $Y=(Y_1,\ldots,Y_4)^T$ denote the vector of observed mean gross flows from the sample. We will assume that any under- coverage or nonresponse in the sample is ignorable and that the observations are perfectly matched. Thus the bias $EY-T$ in the observed gross flows is due solely to misclassification. Let m_{ij}=Pr(unit of flow type j is observed as flow type i) for $i=1,\ldots,4$ and $j=1,\ldots,4$. Let $M=((m_{ij}))$ be a 4x4 matrix. It is then easy to show that $EY=MT$. Our general idea is to estimate M and then estimate the bias as

$$\hat{\text{bias}} = Y-\hat{M}^{-1}Y = (I-\hat{M}^{-1})Y, \qquad (1)$$

where I is the 4x4 identity matrix.

Of course, estimating M is extremely difficult. Furthermore, there is evidence that M varies strongly by characteristic and by whether the gross flows are observed within a period or between periods.[7] It is also possible that M depends on status with respect to the characteristic of interest at another point in time, or other characteristics such as sex, region, income, etc. However, there is some reason to hope that M is fairly stable by characteristic for gross flows observed between periods but over varying time periods. This hope is based on heuristic arguments. If M does vary over time (between periods), it could be due to changing error rates or changing correlations between the errors. While the error rates do probably fluctuate from period to period, there is little reason to think that a trend would exist. As for the correlations, any correlation is probably due more to having the same poorly informed proxy respondent, the same poorly performing interviewer, or the same respondent misunderstanding of concepts, rather than active memory of response from the prior period. Thus while the correlations probably do weaken with increased time, the weakening may be rather slow. If the correlations do in fact weaken, our assumptions generally lead to an underestimate of the bias.

So we assume that an estimate of M for a pair of consecutive months observed between periods is still a reasonable estimate for a pair of months, for example, separated by 11 months. (A great deal of interest focuses on gross flows from a month to a year later.) Fortunately, estimating M for a pair of consecutive months is easier.

Let $C_1,\ldots,C_4$ be error rates for the four flow types at time 1 and $C_5,\ldots,C_8$ be error rates for the four flow types at time 2. (C_1 and C_2 are false negative rates at time 1 for flow groups 1 and 2. They are allowed to be different since we think that stable units may have a different rate than those actually experiencing a transition. The overall false negative rate at time 1 is $(T_1C_1+T_2C_2)/(T_1+T_2)$. C_3 and C_4 are false positive rates at time 1, C_5 and C_7 are false negative rates at time 2, and C_6 and C_8 are false positive rates at time 2.) Also, let $C_9,\ldots,C_{12}$ be the conditional probabilities of error at time 2 given error at time 1 for the four flow types. It is then fairly easy to show that

$$M = \begin{bmatrix} 1-C_1-C_5+C_1C_9 & C_6-C_2C_{10} & C_3(1-C_{11}) & C_4C_{12} \\ C_5-C_1C_9 & 1-C_2-C_6+C_2C_{10} & C_3C_{11} & C_4(1-C_{12}) \\ C_1(1-C_9) & C_2C_{10} & 1-C_3-C_7+C_3C_{11} & C_8-C_4C_{12} \\ C_1C_9 & C_2(1-C_{10}) & C_7-C_3C_{11} & 1-C_4-C_8+C_4C_{12} \end{bmatrix}$$

Using the reinterview, C_1 through C_8 may be directly estimated. Also, the reinterview provides an improved estimate Y_R of the gross flows. The problem is thus reduced to finding C_9 through C_{12} such that

$$MY_R = Y_B, \qquad (2)$$

where Y_B is the vector of observed gross flows between periods for the same pair of consecutive months. Unfortunately, the existence of a solution to (2) is quite rare.

We only sketch the proof of this assertion, leaving the details to the reader.

Letting $X = [1, -1, -1, 1]^T$, we may write M as $M = X[C_1C_9 \quad -C_2C_{10} \quad -C_3C_{11} \quad C_4C_{12}] + A$, where A does not depend on C_9 through C_{12}. Then (2) has a solution if, and only if, Y_B-AY_R is a multiple of X. While least square solutions do exist, there is no unique solution. (Any $(C_9,\ldots,C_{12})$ such that $(M-A)Y_R$ is the projection of Y_B-AY_R onto X is a least squares solution.)

Thinking this over, we realized that we had insufficient data to estimate the error correlation for each flow type separately. Somehow, it was necessary to define a measure of association that would apply simultaneously to the four flow types. We came up with the idea that $(C_9,\ldots,C_{12})^T$ should lie on the line between the points $(1,0,0,1)^T$ and $(C_5,\ldots,C_8)^T$. We then defined the measure of association r to be the ratio of the Euclidean distance between $(C_9,\ldots,C_{12})^T$ and $(C_5,\ldots,C_8)^T$ to that between $(1,0,0,1)^T$ and $(C_5,\ldots,C_8)^T$. This has some intuitive appeal since if $r=0$, then $(C_9,\ldots,C_{12})^T = (C_5,\ldots,C_8)^T$, which implies that errors occur independently. On the other hand, if $r=1$, then $(C_9,\ldots,C_{12})^T = (1,0,0,1)^T$, which implies strong dependence on errors. For example, it implies all correlation of 1.0 among flow types 1 and 4 (the no change categories) provided that the error rates are equal at time 1 and time 2. In addition, it implies a strong negative correlation among flow types 2 and 3 (the with change categories). Another way of conceptualizing $r=1$ is: if an error is made at the first observation, then the same response will be obtained at the second observation regardless of the flow type of the unit. With some algebra, we obtain the value of r that minimizes $\|MY_R-Y_B\|^2$:

$$r = \frac{X^T(Y_B-AY_R) - 4\left(C_1C_5 \quad -C_2C_6 \quad -C_3C_7 \quad C_4C_8\right)Y_R}{4\left(C_1(1-C_5) \quad C_2C_6 \quad C_3C_7 \quad C_4(1-C_8)\right)Y_R} \qquad (3)$$

To summarize, our technique is to estimate C_1 through C_8 and Y_R from reinterview, then use these with Y_B to estimate r. Using r and linear interpolation, we can estimate C_9 through C_{12}. We can then compute an estimate of M, and apply $(I-\hat{M}^{-1})$ to any observed gross flows between periods to estimate the biases in the gross flows.

This technique also provides estimates of bias in transition rates, the percentages of those with an initial status who change status by the second time point. Let the elements of $\hat{M}^{-1}Y$ be denoted Z_1 through Z_4. Then the biases in the transition rates are

$$\frac{Y_2}{Y_1+Y_2} - \frac{Z_2}{Z_1+Z_2} \quad \text{and} \qquad (4)$$

$$\frac{Y_3}{Y_3+Y_4} - \frac{Z_3}{Z_3+Z_4} \qquad (5)$$

(4) and (5) are referred to as the bias in the exit and entrance rates, respectively.

V. SENSITIVITY ANALYSIS

Given the uncertainties in the estimation of the error rates and the improved estimate of gross flows discussed in Section II, we believed an appropriate approach to getting an idea of the magnitude of the bias in gross flow estimates from SIPP was to perform sensitivity analysis.

Due to the weakness of the data produced from the SIPP reinterview, we limited our analysis to the gross flow estimates of food stamp program participation. In particular, the unit of analysis was the authorized person of a food stamp unit. (A food stamp unit is all persons covered under an authorized person's allotment.) We focused on food stamps because their error rates seemed more plausible than those of other characteristics. The main reasons for presenting this analysis of food stamp gross flows are to provide some information on the probable magnitude of biases in gross flow estimates from SIPP and to illustrate the application of the technique. Another reason is to observe how sensitive the biases in gross flow estimates are to changes in the error rates, Y_R, and the year-to-year gross flow estimates. The greater the sensitivity, the less reliable the comparisons of gross flows across demographic groups or across time will be if we do not maintain a high degree of uniformity in SIPP data collection and processing procedures.

Our sensitivity analysis consists of varying the estimate of M for food stamps by varying the values of C_1 through C_8 and Y_R.

We then estimate biases by applying $(I-\hat{M}^{-1})$ to observed food stamp gross flows between periods and evaluate the sensitivity of these biases to the changes in C_1 through C_8 and Y_R. For this analysis, we studied observed <u>year-to-year</u> food stamp gross flows because of interest expressed in the production of statistics based on year-to-year gross flow estimates from SIPP. As an additional part of our sensitivity analysis, we varied the year-to-year gross flow estimates. The purpose was to study the reliability of comparisons of gross flow estimates across demographic groups or across time.

In our presentation of the sensitivity analysis of the bias in gross flow estimates for food stamps, we first describe the estimation of parameters needed to apply the technique. We then discuss how these parameters were varied to perform the sensitivity analysis. Finally, we present the results.

A. <u>Estimation of Parameters for Food Stamps</u>

Error rates, an improved estimate of consecutive month-to-month gross flows, and observed gross flows must be estimated to apply the technique. Observed food stamp gross flow estimates are readily available from SIPP data. However, the estimation of error rates and improved gross flow estimates for food stamps are much more subjective. The methodology used to estimate these parameters is discussed below.

1. Error Rates

Several assumptions are required in order to determine the error rates $(C_1,\ldots,C_8)$ from the SIPP reinterview. The SIPP reinterview references the entire period--not each month within the period. Thus, we are unable to differentiate time 1 and time 2 error rates based on length of recall. In addition, we are unable to differentiate error rates, for a specific time, based on the flow type. These two limitations forced us to assume $C_1 = C_2 = C_5 = C_7$ and $C_3 = C_4 = C_6 = C_8$. Therefore, the determination of the error rates is reduced to computing two error rates: the probability of falsely observing no food stamps (false negative) and the probability of falsely observing food stamps (false positive).

These error rates were actually computed for food stamps and several other characteristics from the SIPP reinterview. Upon examination of these error rates we immediately questioned their surprisingly small magnitude. We realized that error rates referencing the entire period would most likely be smaller than those that reference a single month, which we would have preferred. To estimate the magnitude of this underestimate we examined AFDC (Aid to Families with Dependent Children) data from ISDP (Income Survey Development Program).[8] The data indicated that the false negative error rate computed from administrative record checks was approximately three times larger than that computed from the SIPP reinterview. (False positive error rates were unavailable.) Believing the ISDP error rates to be more realistic, we applied a factor of 3 to the food stamp false negative error rate.

In considering the computation of the false positive error rate for food stamps, we realized that the false positive observations were in terms of food stamp units while the true negative observations were in terms of persons 18 and over. To adjust for this we applied a factor of 1.4 (average number of persons 18 and over in a food stamp unit) to the false positive error rate.

Thus, the above assumptions and adjustments provide us with the following estimates of the error rates:

False Negative $= C_1 = C_2 = C_5 = C_7 = 0.0597$
False Positive $= C_3 = C_4 = C_6 = C_8 = 0.0034$

2. Improved Estimate of Gross Flow for Food Stamps

Our intuition tells us that flow types 2 and 3 (the with change categories) are probably overestimated and underestimated by gross flows observed between and within periods, respectively. However, we thought we had a better understanding of the nature of the underestimates in flow types 2 and 3 observed within a period. We intuited that within a period flow types 2 and 3 may be observed as flow types 1 and 4, while flow types 1 and 4 are not as likely to be observed as flow types 2 and 3. This corresponds to r=1 with the error rates for flow types 1 and 4 equal at time 1 and time 2. Thus, an improved estimate of consecutive month-to-month gross flows for food stamps is computed as follows:

$$Y_R = \hat{M}^{-1}_{r=1} Y_W,$$

where Y_W is the vector of observed gross flows within a period. For food stamps, $Y_W = [.039867 \ .001287 \ .001645 \ .957202]^T$ which results in an improved estimate of consecutive month-to-month gross flows $Y_R = [.038923 \ .001374 \ .001756 \ .957948]^T$

B. Varying the Parameters for Food Stamps

Given the subjective nature of the estimation of C_1 through C_8 and Y_R we thought it necessary to study the robustness of the estimated biases to assess their usefulness. To accomplish this we arbitrarily decreased and increased the error rates. We also used different improved estimates, Y_R. One Y_R was a weighted average of the observed gross flows within and between periods. Another Y_R was somewhat arbitrarily computed, so as to have gross flows with change that were closer to the gross flows with change from between periods.

C. Results

It is our understanding that of central interest in the problem of biases in gross flow estimates is the production of transition rates (defined in Section IV). Consequently, our sensitivity analysis results are presented in terms of the biases in the transition rates.

To assess the seriousness of the magnitude of the bias in a transition rate, we compared it to an estimate of the standard error of the transition rate. The greater the absolute value of the ratio of bias to standard error is; the more serious the problem.

Using the observed year-to-year gross flows for food stamps we computed the ratio of bias to standard error of the transition rates for several combinations of error rates and Y_R (Table A).

The rows of Table A are the various error rates used. The first row (original) is the error rates estimated in Section V.A.1. Still concerned about the possible underestimation of the error rates, we used the remaining permutations of doubling the false negative and false positive error rates in rows two through four. Concerned with the assumption that, error rates are the same for all flow types, in particular, the with change categories versus the without change categories, we doubled the error rates for flow types 2 and 3 (the with change categories) in the fifth row. In the opposite direction of the top five rows, we used the unadjusted false negative error rate in the sixth row. (See Figure 2.)

Figure 2. Error Rates by Type of Error and Row

Row	False Negative ($C_1=C_2=C_5=C_7$)	False Positive ($C_3=C_4=C_6=C_8$)
1	.0597	.0034
2	.1194	.0034
3	.0597	.0068
4	.1194	.0068
5	$C_1=C_5=.0597, C_2=C_7=.1194$	$C_3=C_6=.0068, C_4=C_8=.0034$
6	.0199	.0034

The columns of Table A are the three values for Y_R. The first column is our intuited estimate of Y_R, as explained in Section V.A.2. Flow types 2 and 3 of our intuited Y_R are very close to those of the observed gross flows within a period Y_W. The middle column is a weighted average of Y_W (three fourths weight) and the observed gross flows between periods Y_B (one fourth weight), where $Y_B = [.036444 \ .005865 \ .004461 \ .953229]^T$ for food stamps. For the weighted average Y_R, flow types 2 and 3 are larger, but still closer to those of Y_W. Note, respectively, these two columns correspond to month-to-month over-reporting and equivalent-reporting of flow types 2 and 3. The last column corresponds to the other extreme of month-to-month under-reporting of flow types 2 and 3. For this column, flow types 2 and 3 are about in the middle of those for Y_W and Y_B. (See Figure 3.)

Figure 3. Gross Flows by Assumed Y_R and Flow Type

Flow Type	Intuition	Weighted Average	Upper Estimate
1	.038923	.039011	.037954
2	.001374	.002431	.003488
3	.001756	.002349	.002942
4	.957948	.956209	.955616

For each combination of error rate and Y_R in Table A, we computed the ratio of bias to standard error for exit (upper right) and entrance (lower left) rates. For example, the ratios for exits and entrances are 5.13 and 4.85, respectively, for the original error rates and the intuited Y_R (extreme upper left cell). (Detailed results along with a more detailed explanation of the application of the technique to compute these ratios are provided in Appendix A.) The reported year-to-year exit rate is 29.54%. Referring to Table A-11 in Appendix A, the technique estimated the "true" year-to-year exit rate to be 23.23% with a standard error (SE) of 1.23%. This results in a bias to SE ratio of 5.13 ((29.54%-23.23%)/1.23%) for exits. Similarly for entrances, the bias to SE ratio is 4.85 ((.978%-.667%)/.064%).

The implications of the magnitude of these ratios are evident. For most applications, a ratio less than .75 is not serious, while a ratio greater than 1.5 is cause for some concern.

However, as stated earlier, to assess the robustness of this result, we varied the error rates and Y_R. The results of each combination constitute the remainder of Table A.

In the first column, varying the error rates does affect the ratios to some extent. Still, the magnitude of the ratios is large, even when all the error rates are doubled (row 4): exit ratio=3.26 and entrance ratio=4.07. In the second column (Y_R=Weighted Average) the ratios are smaller than the corresponding ratios in the first column, but all are still large enough for concern. Even for the extreme assumption of Y_R in the third column, the ratios are large except for the exit ratio when the false negative error rate is doubled (Rows 3 and 4). So, for almost every combination of error rate and Y_R in Table A, the magnitude of the bias in the observed year-to-year transition rates relative to the standard error appears to be quite serious.

Another part of our sensitivity analysis was to assess the effect of varying the observed year-to-year gross flow estimates. To accomplish this, we decreased and increased flow types 2 and 3 by 30%. (Note, the sum of flow types 1 and 2 and the sum of flow types 3 and 4 were held constant.) Table B contains the results of the 30% decrease in flow types 2 and 3. (Detailed results are given in Appendix B.) Compared to Table A, all the ratios appear to have increased by at least 50%. Clearly, with these exit and entrance rates, the magnitude of the bias relative to the standard error is very serious for all combinations of error rates and Y_R. Table C contains the results of the 30% increase in flow types 2 and 3. (Detailed results are given in Appendix C.) Comparison of ratios to Table A vary by the assumed Y_R. For columns 1 and 2 of Table C, almost all of the ratios (except exit ratios for rows 3 and 4) decreased by about 30%, but are still greater than 1. However, in column 3, the absolute value of almost all of the ratios is at the most 1.5, with the smaller ratios coming from the rows with doubled error rates. This means that the magnitude of the bias relative to the standard error is generally not as serious for these certain combinations of increased error rates, Y_R, and year-to-year gross flow estimates. However, these combinations are rather extreme compared to our original combination of error rates, intuited Y_R and observed year-to-year gross flow estimates.

D. Summarization of Results

For the characteristic of food stamps, the ratio of bias to standard error was sensitive to the assumption of Y_R and the year-to-year gross flow estimates and, to a lesser extent, the error rates. The combinations of these variables covered a very large part of the realm of reasonable possibilities. In almost all cases, the magnitude of the ratio indicated a serious bias in observed transition rates. Yet, there were sufficient changes in the ratio to warrant concern about the reliability of comparisons between transition rates if a high degree of uniformity in SIPP data collection and processing procedures is not maintained.

FOOTNOTES

[1] For an excellent overview of the history of the problem, see the proceedings of the recent conference [8].

[2] Parnes [2] first formulated a type of uncertainty principle in this area. A good example is participation in government programs. Respondents may learn of these at the first contact and avail themselves of the benefit by the second contact.

[3] For an overview of SIPP, see [7].

[4] A general description of reinterview as conducted at the Bureau is given in [3]. An internal critique is given in [4]. The results of an experiment with independent reconciliation are given in [5]. Design modifications are given in [6].

[5] See, for example, Fuller and Chua in [8] pp. 65-77.

[6] Recommendation number 3 on page 135 of [8].

[7] See [1] for a comparison of within a period and between period gross flows.

[8] For a more detailed discussion of AFDC error rates in ISDP, see [9].

REFERENCES

[1] Burkhead D. and Coder J., "Gross Changes in Income Recipiency From the Survey of Income and Program Participation." 1985 Proceedings of the Section on Survey Research Methods. Washington, D.C.

[2] Parnes, Herbert A. "Longitudinal Surveys: Prospects and Problems." Monthly Labor Review, 95(1972).

[3] U.S. Bureau of the Census. The Current Population Survey Reinterview Program, January 1961 through December 1966, Technical Paper No. 19. Washington, D.C.: U.S. Government Printing Office, 1968.

[4] ________. "Problems in Procedure and Design of CPS-Reinterview." Memorandum from William Owens to Robert T. O'Reagan, January 9, 1975.

[5] ________. "Reinterview Results from the CPS Independent Reconciliation Experiment (Second Quarter 1978 through third Quarter 1979)." Memorandum by Irv Schreiner, May 7, 1980.

[6] ________. "Final Report of the Reinterview Work Group." Memorandum from Reinterview Work Group to SMD Operations Redesign Task Force, March 11, 1982.

[7] ________. An Overview of the Survey of Income and Program Participation. SIPP Working Paper Series No. 8401 prepared by Dawn Nelson, David McMillen, and Daniel Kasprzyk, 1984

[8] U.S. Bureau of the Census and U.S. Bureau of Labor Statistics. Proceedings of the Conference on Gross Flows in Labor Force Statistics. Washington, D.C.: June 1985.

[9] U.S. Department of Health and Human Services. Reports from the Site Research Test. Chapter 11. Washington, D.C.: December 1980.

TABLE A

Ratio of Bias to Standard Error for Observed Year-to-Year Transition Rates for Food Stamps

ERROR RATES	Assumed True Month-to-Month Gross Flows (Y_R): Intuition (Near Within) (1) — Ratio for Entrances	Intuition (Near Within) (1) — Ratio for Exits	Weighted Average (2) — Ratio for Entrances	Weighted Average (2) — Ratio for Exits	Upper Estimate (Near Between) (3) — Ratio for Entrances	Upper Estimate (Near Between) (3) — Ratio for Exits
Original (1)	4.85	5.13	3.38	3.46	2.14	1.95
Double False Negative (2)	4.15	5.33	2.73	3.71	2.30	1.55
Double False Positive (3)	4.94	3.26	3.47	1.55	2.20	-0.04
Double All (4)	4.07	3.26	2.70	1.67	1.54	0.23
Double Both for Flow Types 2 & 3 (5)	4.16	4.22	2.76	2.64	1.57	1.22
One Third of False Negative (6)	5.59	5.33	4.00	3.54	2.63	1.89

Key

ERROR RATE	Y_R
	Ratio for Entrances \ Ratio for Exits

Year-to-Year Gross Flow for Food Stamps = [2.90% 1.21% 0.94% 94.95%]

Exit Rate for Food Stamps = 29.54% Standard Error = 1.23%

Entrance Rate for Food Stamps = 0.978% Standard Error = 0.064%

TABLE B

Ratio of Bias to Standard Error for Decreased Observed Year-to-Year Transition Rates for Food Stamps

ERROR RATES	Assumed True Month-to-Month Gross Flows (Y_R): Intuition (Near Within) (1) — Ratio for Entrances	Intuition (Near Within) (1) — Ratio for Exits	Weighted Average (2) — Ratio for Entrances	Weighted Average (2) — Ratio for Exits	Upper Estimate (Near Between) (3) — Ratio for Entrances	Upper Estimate (Near Between) (3) — Ratio for Exits
Original (1)	7.64	7.67	5.16	5.27	3.27	3.26
Double False Negative (2)	7.50	8.23	4.95	5.82	3.08	3.85
Double False Positive (3)	7.59	6.05	5.17	3.70	3.28	1.66
Double All (4)	7.08	6.28	4.74	4.02	2.94	2.10
Double Both for Flow Types 2 & 3 (5)	7.23	6.98	4.83	4.70	3.00	2.79
One Third of False Negative (6)	8.12	7.66	5.54	5.18	3.54	3.04

Key

ERROR RATE	Y_R
	Ratio for Entrances \ Ratio for Exits

Year-to-Year Gross Flow for Food Stamps = [3.26% 0.85% 0.66% 95.23%]

Exit Rate for Food Stamps = 20.68% Standard Error = 0.98%

Entrance Rate for Food Stamps = 0.685% Standard Error = 0.046%

TABLE C

Ratio of Bias to Standard Error for Increased of Observed Year-to-Year Transition Rates for Food Stamps

ERROR RATES	Assumed True Month-to-Month Gross Flows (Y_R): Intuition (Near Within) (1) — Ratio for Entrances	Intuition (Near Within) (1) — Ratio for Exits	Weighted Average (2) — Ratio for Entrances	Weighted Average (2) — Ratio for Exits	Upper Estimate (Near Between) (3) — Ratio for Entrances	Upper Estimate (Near Between) (3) — Ratio for Exits
Original (1)	3.49	3.70	2.41	2.33	1.46	1.05
Double False Negative (2)	2.50	3.70	1.49	2.42	0.62	1.26
Double False Positive (3)	3.66	1.48	2.55	0.01	1.56	-1.39
Double All (4)	2.56	1.34	1.56	0.01	0.67	-1.24
Double Both for Flow Types 2 & 3 (5)	2.62	2.66	1.60	1.36	0.70	0.16
One Third of False Negative (6)	4.40	4.04	3.20	2.53	2.11	1.09

Key

ERROR RATE	Y_R
	Ratio for Entrances \ Ratio for Exits

Year-to-Year Gross Flow for Food Stamps = [2.53% 1.58% 1.22% 94.67%]

Exit Rate for Food Stamps = 38.40% Standard Error = 1.37%

Entrance Rate for Food Stamps = 1.272% Standard Error = 0.078%

Martin H. David
Department of Economics, University of Wisconsin-Madison

COMMENTS

The SIPP was conceived as an instrument for measuring transitions and change. The conception has yet to reach full term, and these papers indicate some of the major technical problems that must be solved to use the longitudinal aspects of the design successfully.

Estimation of person's characteristics from the panel is essential. The scheme proposed by Kobilarck and Singh has several strong points:

a. COHORT ORIENTATION. The representative sample is a sample of the household universe at the time of the first wave of the panel. The longitudinal change data are estimated with respect to that initial population.

b. DIFFERENTIAL TREATMENT OF WAVE 1 AND SUBSEQUENT NON-RESPONSE. The non-response at the first wave is a typical cross-sectional problem for which well established techniques already exist. Subsequent non-response can be conditioned on Wave 1 data; less is known about these conditional non-response rates.

c. WEIGHTING FOR MISSING WAVES. The absence of one or more waves can not yet be modelled with an appropriate imputation scheme for longitudinal estimation. Thus weighting should be used to deal with partial response over the period of interest.

Point (a) implies that births into the sample universe are ignored and data for persons who leave the universe must be measured or imputed. Ignoring births is not of great importance, since a new representative panel is available every 12 months, and provides a source of information on additions to the household universe. When (a) is followed to its logical conclusion, the implication is that, after 8 waves of measurement, data on change refer to the experience of the initial cohort, and can not easily be reconstructed into a measure of retrospective reports of change from a representative sample of the universe on the terminal date of the panel.

Point (b) implies that it is appropriate to use different variates to predict probability of "complete" response, given response to wave 1, than the variates used to predict probability of response to wave 1. My main quarrels with the proposed non-interview adjustment are threefold: evidence, variance estimates for the weighting procedure, and modelling. It is inexcusable that no empirical evidence is offered for the variates that are chosen to provide noninterview adjustment. Work by McArthur and Short [1] shows that at least two of the variates chosen are not significant in a study of attrition to the fifth wave, conditioned on response to the first. I would prefer no nonresponse adjustment to an adjustment that is not substantiated by strong, published, empirical evidence.

Second, I am concerned about mean square error of estimates. Nothing in the argument presented suggests that the weights calculated are stable, even if they are unbiased. Indeed, the collapsing procedure used by the Bureau is subject to unknown sampling variation so that one can not be assured that the technique for weighting is not distorting measures of change or transitions.

Third, the appropriate statistical tool for smoothing the weights and understanding the statistical properties of the procedure is a model of non-interview. Multi-variable probit or logit would appear to be the preferred technique. Such a technique would require the Bureau to structure the model according to some well-articulated hypotheses rather than jumping from one data-fishing excursion to another. I do not believe that adjustment by classification is a technique that will be acceptible to most analysts of the SIPP data. They will want instruction in general techniques from the Census.

Point (c) realistically declares that, given present technology, a complete observation must contain all waves for the period for which analysis is desired. The Bureau proposal is overly restrictive. Interviewed persons with complete data are rejected if they reside in households where one or more observations are missing. Those persons tend to live in unusual households (3 or more persons over the age of 18), and the non-interviewed persons tend to be younger persons with some economic independence from the others in the household. A preferable procedure is to include such persons in the interviewed population. Their data will be complete, except for variables that are defined on the household as a whole.

The proposal before us is not sufficiently general to meet present analysis needs. The concept of a longitudinal panel of waves 1-3 has already been made obsolete by Williams [2] who is linking waves 2-5 for analysis of annual poverty and work by McArthur [3] which links waves 1-5. Their studies use w_i from the first wave or no weights on samples that are not clearly described with respect to inclusion of imputed waves or entrants to the sample (Williams). It is extremely important that the Bureau issue a paradigm for constructing weights to include all of the available public use data, because analyses will be done on the longer panels. The Bureau has been negligent in failing to produce this document at the same time that it released the public use version of Wave 3.

Some other blemishes on the proposal need to be mentioned. The Bureau is discarding data for people who marry (or move in with) other members of the sample, because its data processing programs are inadequate. This is only worth noting because the Bureau intends to propagate this stupidity through the remainder of the 1984 panel, the 1985, panel, the 1986 panel, and the 1987 panel! Second, as I understand it, no weighting adjustment is made for sample loss due to movement out of the universe. Imputations are required for such cases. For the deceased, appropriate imputations to arrive at 12-month totals can be made by assuming zero income in the months after death. However, no such technique exists for persons who move out of the household universe. The proposal implied by using an indicator variable for months after leaving the universe, is that the Bureau wishes to censor data from such cases. I can not see a better alternative, but I think the problem should be explicitly described, and the distinction between deceased and others should be clearly drawn. Third, the raking of the SIPP to the CPS totals appears to be out of place.

Much is to learned from treating these two measures as independent, and I do not see the need for imposing sampling error from the CPS on the weights for SIPP.

This proposal does give a concrete framework for building longitudinal weights. It should be generalized immediately to assist those who are doing analyses on the 1984 calendar year and to provide the appropriate extensions for analyses of the full panel and year-to-year change.

HUBBLE AND JUDKINS

The model developed by Hubble and Judkins is exciting. The execution raises questions. First, nothing is done to establish that the reinterview observations are more valid than the originals. The references to unpublished memoranda and 1968 CPS material do not inform the reader. Lack of documentation makes it unclear why false negatives and false positives from the reinterview imply the conditions C1 = C2 and C3 = C4 that are asserted. Moreover, since the timing of the reinterviews is not described it is impossible to know why C2 = C5 and C4 = C6. Part of my confusion about this may have to do with the absence of a definition of period, which I have taken to mean the reference period for the SIPP (4 months prior to the month of interview). Without more information about the reinterview program for SIPP it is impossible to judge whether the approximations made late in the paper are appropriate.

I believe that attention to the role of errors in estimating annual gross flows is also misplaced. Considerable interest relates to the instantaneous probabilities that persons (households) will enter the Food Stamps program (or some other state) and the probabilities that they will leave the program (or state). Such probabilities have already been estimated by Carr, Lubitz, and Doyle (4), using a discrete time (monthly) Markov model and data from the Income Survey Development Program (ISDP). Such models may or may not include duration dependence of the transition probabilities. Whether or not there is duration dependency, the statistic of interest is not a year-to-year gross flow, the statistic of interest is the conditional probability of remaining in a given state given the date at which the individual entered the state. Year-to-year change will be net of all persons who make two transitions in that span of time, and will therefore not relate to the cumulative effect of a monthly Markov process on the distribution of the population by state.

If this opinion is correct, then the Hubble-Judkins model in equations (2)-(4) can usefully be applied to correct flows measured between waves for month-to-month change. Such flows will affect only one rotation group for every change to be estimated. However, application of the technique to this problem raises other questions. The measurement from month t+1 is four-months' recall; the measurement from month t is one-month's recall. Applicability of the proposed technique requires that such differences in recall are not associated with differences in error, or that the model be extended for systematic effects from recall that could conceivably be estimated from the other rotations.

An intuitive argument that leads to the same conclusion is that (2) will have different arguments for every choice of elapsed time for measurement of gross flows, implying different estimates for M. If the error process is incorporated into a discrete Markov model, the implications of parameters of the error process for measurement of change for different intervals can be computed.

While correlated errors over periods as long as one year appear quite plausible for those situations in which an episode has been forgotten (whatever the cause--length of recall, proxy, or interviewer errors), more specific models of the correlation structure might be appropriate to the problem of telescoping. For example, if my last spell of unemployment terminated less than four months ago, I should report unemployment in the current SIPP interview. If I fail to remember the one week of unemployment in the first reference month that is the only unemployment to be reported, I have telescoped the termination of unemployment back in time. We would expect the probability of such telescoping to be small and to diminish, the longer the period for which telescoping occurs. In the limit, we would expect the probability of telescoping yesterday's unemployment to be infinitesimal. This conceptualization leads to a rather different model of the correlation of errors at two points in time than the model proposed for estimation. Both need to be tested.

The authors are suitably sceptical of

their estimates. I would argue that the false negative probabilities estimated from the reinterview ought not to be arbitrarily increased from evidence in the ISDP validation. That validation was certainly encumbered with matching error that is not present in the present context. How one should discount the ISDP rates for matching error is any one's guess, but the ISDP error rate is not necessarily a better indication of truth than the reinterview. (Again, it is hard to appraise the author's judgement because no data about the reinterview are provided.)

The authors should be congratulated for a promising start on an important problem.

WEIDMAN

Weidman's paper underscores my comment about the importance of a Census document on the framework for longitudinal samples. It is not obvious that his selection of data represents any meaningful universe. Weidman professes to be interested in entry and exit probabilities for a number of different income types. However, analysis of the rate with respect to income type 1 is conditioned on receipt of income type 1 at some point in the sample or receipt of selected other income types. In addition, the sample is conditioned on complete data and continuing membership in the household population. These conditions vitiate meaning that might be assigned to demographic differences, per se. Weidman maintains that the comparison of such differences across rotation groups to examine the differences between a measurement of status change that is within a wave and a measurement of status change that is between waves will be informative. Given sampling error that attaches to any estimator from a rotation group and the fact that error processes may differ between the included and the excluded population, I find this argument doubtful. When the argument is coupled with a totally ad hoc fishing expedition and no statistics I am dismayed. One would hope to find behaviorally motivated hypotheses about response error and a thoughtful statistical structure to deal with the problem.

References

1. McArthur, Edith and Kathleen Short (1986). Life events and sample attrition in the SIPP. Proceedings of the Survey Research Methods Section, American Statistical Association, Washington, D.C.

2. Roberton Williams (1986). Poverty rates and program participation in the SIPP and CPS. Proceedings of the Social Statistics Section, American Statistical Association, Washington, D.C.

3. McArthur, Edith, Kathleen Short, and Suzanne Bianchi (1986). Following children in SIPP. SIPP Working Paper Series No. 8612, Washington, D.C.: U.S. Bureau of the Census.

4. Carr, Timothy, Irene Lubitz, and Pat Doyle (1984). Measuring entry and exit rates to the Food Stamp program (Food and Nutrition Service, USDA, Washington, D.C.)

SURVEY OF INCOME

AND

PROGRAM PARTICIPATION

AS A DATABASE

FOR

PUBLIC POLICY

SESSION IV

The two papers presented in this session were sponsored by the Social Statistics Section of the American Statistical Association.

Poverty Rates and Program Participation in the SIPP and the CPS

Roberton Williams, Congressional Budget Office[1]

Official poverty rates published by the Bureau of the Census are calculated by comparing annual income estimates derived from the Current Population Survey (CPS) against poverty thresholds indicating the amount of cash income needed during a year to be nonpoor. Data that have only recently become available from the Survey of Income and Program Participation (SIPP) allow calculation of alternative poverty rate estimates, based on time periods of different lengths and on different definitions of poverty. This paper compares some of the poverty measures that can be derived from SIPP data with CPS estimates for 1984 to determine the degree of variation among alternative measures. In addition, the poverty status of participants in various transfer programs is examined using alternative poverty estimates to show how the definition of poverty can affect evaluations of public programs.

Three basic questions are posed in the analysis. First, how much variation exists among poverty rates measured over different time periods? In particular, how different are poverty statistics based on monthly income and monthly poverty thresholds from those based on annual income and thresholds, when both estimates are made with the same survey data? Second, how different are poverty rates calculated from CPS and SIPP data when both use annual measures? Finally, do means-tested transfer programs appear to be better targeted on poor people when monthly rather than annual poverty definitions are used?

Measuring Poverty

The official definition of poverty determines whether a person is poor during a given year by comparing the total cash income of all family members living together with a poverty threshold based primarily on family size.[2] All members of the family are labeled "poor" if cash income is below the threshold, and "nonpoor" otherwise. Income is measured before taxes, no account is taken of income received in kind rather than as cash, and wealth is considered only to the extent that it produces cash income.[3]

The Census Bureau estimates poverty rates each year using data obtained in the March supplement to the CPS, which asks respondents their incomes from various sources during the previous calendar year. This income information is obtained only for people living in sample households at the time of the survey, and families are defined as those surveyed people who are related and living together at that time. Combining annual income with family composition in one month masks any month-to-month variations in either available resources or needs.

The SIPP Data

Alternative income data from the SIPP first became available in 1984.[4] While these new data are much more detailed in terms of sources of income, more important are the facts that data are reported for each month rather than for an entire year and that data collection occurs every four months rather than annually.

Poverty rates and program participation estimates obtained from the SIPP are likely to differ from those derived from the CPS for three reasons. First, the SIPP data allow the calculation of monthly estimates: poverty rates will tend to be higher than annual statistics, while the reverse will be true for program participation. Second, the SIPP may be subject to smaller recall error because there is a shorter time period between income and program events and the collection of data. Finally, because family composition is reported monthly, both incomes and needs can be determined more precisely, without the disparities of timing in the CPS data.

Differences Due to Time Period. Estimates of poverty rates and program participation will tend to differ depending on whether annual or monthly data are used in their calculation. In the case of poverty rates, this results from the distribution of incomes around the poverty level, while differences in the meaning of participation in a month or at any time during a year explain the divergence of program participation statistics.

During a given time period, members of a family are in poverty if family income is less than the relevant poverty threshold.[5] Over time, a family's income will vary, sometimes rising above its average and sometimes falling below. For families with annual incomes that are below the poverty level, monthly variations may occasionally cause their income to climb above poverty during a month; the reverse will be the case for families with annual incomes above poverty. Because more families have annual incomes in the range immediately above the poverty line than in the range just below, such monthly fluctuations in income might be expected to cause monthly poverty rates to exceed annual poverty rates. The number of families that are not poor on an annual basis but who experience an income decrease that drops them below the poverty line for a given month will likely be greater than the number of families that are poor for the year but whose incomes rise above the poverty line for that month. The number of poor families in any given month would thus be expected to exceed the number of families that are poor over a full year, and monthly poverty rates would be greater than annual poverty rates.[6]

It is worth noting that neither monthly nor annual poverty rates are necessarily superior as indicators of true need. Monthly rates may be more closely related to the eligibility criteria for transfer programs, but do not take account of the fact that families may well be able to defer expenditures during months with low incomes until incomes are higher in the future. Annual poverty rates, on the other hand, give less recognition to the fact that some needs simply cannot be postponed for long periods.

Monthly data can be used to construct alternative measures of poverty across a year that provide information about the movement of families into and out of poverty. The percentage of families with incomes below the poverty line in all 12 months of a year indicates how many families have consistently low incomes. This percentage of "always poor" can be no greater than the annual poverty rate discussed above, and will be as large as the latter rate only if no poor family has its income rise above the poverty threshold during any month of the year. Alternatively, the percentage of families with monthly income ever below poverty shows how many families experience some period of low income during a year. That percentage of "ever poor" is necessarily greater than the annual poverty rate if there is any movement of families into and out of poverty, and is almost certain to be greater than the percentage of families that are poor in any given month.

Annual program participation would be expected to exceed monthly program participation because of the way in which it is defined. If we consider a family to have participated in a given program during a particular period if it received any program benefits during that time, the number of families in the program at any time during a year must be at least as large as--and will almost certainly be greater than--the number in the program during any given month in that year. Only if all families receiving benefits at some time during the year also got benefits during a particular month would the monthly and annual participation rates be the same.

Differences Due to Recall Error. Recall error is likely to be greater in the CPS than in the SIPP, leading to more underreporting of both income and receipt of program benefits. One kind of recall error is simply forgetting about receipt of income or benefits. The CPS recall period ranges from three months to fifteen months, compared to between one month and four months for the SIPP. The longer time between events and reporting could lead to mistakes on the part of respondents, resulting in omission or understatement of both sources and amounts of income and program benefits.[7] Underreporting of cash income means that poverty rates will be overstated, and the CPS would be expected to show more poor people than would the SIPP. Conversely, underreporting of program benefits would generate participation estimates below true values.

A second kind of recall error involves mistakes in the timing of receipt of income or benefits. People may remember that they received food stamps in the past, for example, but may forget whether it was during the last year or the year before. Such errors yield inaccurate data, but the direction of error cannot be predicted. What can be said, however, is that shorter recall periods should result in less of this type of error, and thus data from the SIPP should be more accurate than those from the CPS.

Differences Due to Definition of Family Composition. Because poverty statistics are defined on the basis of family income rather than individual income, differences between the CPS and the SIPP in terms of when family composition is ascertained can generate differences in poverty estimates. In the CPS, incomes and benefits are those received during the preceding calendar year, but family composition is defined as related people living together in March, the time of the survey. Consequently, families may exclude people who had been household members but moved out prior to the survey, and include people who did not live in the household during the preceding year but who entered between January and March. In either case, incomes, program participation, and family composition may not be consistent in terms of estimating poverty rates or the characteristics of people receiving benefits. Data from the SIPP indicate that there is significant change in household composition over a year: about 17 percent of all households interviewed at the start of the SIPP changed in terms of either family type or size within 12 months.[8]

By contrast, the SIPP determines for each month both family composition and information about incomes and program benefits received by all people living in each sample household. These data allow more accurate and contemporaneous association of needs (based primarily on family size) and incomes of all family members. In addition to avoiding the possibility of assuming that some people were living together and sharing incomes when in fact they did neither (or vice versa), this means that income and program should better reflect the characteristics of participants when they are actually receiving benefits.

Poverty Rates in the SIPP and the CPS

Data from the SIPP and the CPS for 1984 were used to examine differences in poverty rates arising because of alternative definitions, data sources, and time periods. Statistics from the CPS were calculated using the methods of the Bureau of the Census discussed above, and are the same as Census estimates where comparisons can be made. The SIPP data required more manipulation and the results call for greater qualification.

The following SIPP estimates are based on Waves 2 through 5 of data from the 1984 SIPP panel, covering the period from October 1983 through March 1985. Because data from each wave were released separately, individual records for each of the five waves had to be linked together to create files that spanned the entire calendar year. In order to allow comparisons with CPS data for 1984, only records for those people who were in the SIPP sample for all of 1984 were used in the analysis; this required discarding about one-third of the more than 60,000 records in the matched file. The remaining records will be referred to as the 1984 SIPP file.

One potential problem with using the file of linked wave records involves continuity of the data across waves. In processing the raw SIPP data for release as public use files, the Bureau of the Census performed a wide range of editing and imputation activities to ensure that the data are complete and internally consistent. All edits and imputations were done on each wave of data individually, without reference to data from other waves, however; there was no attempt to insure continuity across waves, either in terms of information reported by sample households or that imputed by the Census Bureau. Preliminary analysis of linked data files indicates that there is much greater variation in incomes across waves than within waves, in terms of both kinds and amounts of income received. This may arise from inaccurate reporting by respondents or from the fact that the imputation methods used by the

Census Bureau do not look across data waves. Further work is needed to determine the cause of the uneven temporal variations in income data and to devise methods of correcting them.

Because the SIPP contains only a sample of the entire population, the Census assigns a weight to each sample person to allow estimates of national values.[9] In this analysis, these weights were not adjusted to take account of the observations discarded in creating the 1984 SIPP file. Therefore no absolute population counts are offered in the following; results are limited to percentages calculated from populations from the 1984 SIPP file using the unadjusted weights. To the extent that attrition from or entry into the SIPP sample was not randomly distributed across demographic groups, this use of unadjusted weights will result in biased results not representative of the true population. Without further analysis, no conclusions can be drawn about the effects of using these weights. It may be the case, however, that people moving into and out of the SIPP sample are more likely to have low incomes than those remaining in the sample. If so, excluding those people not in the sample for all of 1984 would yield estimated poverty rates below true rates.

Four sets of poverty rates were calculated from the 1984 SIPP file. First, poverty rates for each of the 12 months of 1984 were estimated by comparing total family cash incomes for the month against monthly poverty thresholds, defined as one-twelfth of annual poverty thresholds applicable for that month. Because family composition was assumed to be fixed during a given month, there was no confusion in selecting thresholds for the appropriate family make-up.

Second, an annual poverty rate was estimated by comparing, for each person, the sum of family cash incomes for each of the 12 months of 1984 against the sum of the monthly poverty thresholds. Because family composition could, and often did, change over the year, neither the annual income nor the annual threshold need apply to any fixed group of people. Instead, the two sums represent aggregates of monthly incomes and needs across the different family groups with whom an individual lived. For families whose composition was constant, this was identical to the Census definition of poverty. For others, however, this approach assessed whether they were poor in terms of a threshold-weighted average of monthly incomes and needs.[10]

The final two poverty indicators offer information about the movement into and out of poverty. The first, termed "always poor," assesses the number of people with consistently low incomes by reporting the percentage of people poor on a monthly basis in all 12 months of 1984. This measure is a lower bound on the annual poverty rate, since all people who were poor in every month were necessarily poor on an annual basis. The second indicator, called "ever poor," is the percentage of people who were poor during any month in 1984, a measure of how many people were poor at least sometime during the year. This group obviously includes all people who were poor for the entire year, but may also include many who were not. The "ever poor" thus provide an upper bound on the annual poverty rate.

Poverty rates under each of these alternative definitions were estimated for each of five population subgroups and for the population as a whole. The subgroups were:[11]

<u>Married Couples with Children</u>: all people living in families headed by married couples with own children under age 18 in the household.

<u>Single Parents with Children</u>: all people living in families headed by single parents with own children under age 18 in the household.

<u>Unrelated Individuals</u>: all people not living with relatives.

<u>Other</u>: all people living in families not headed by parents of children under age 18 living in the household. This group is comprised of all people not part of the first three subgroups described above, and includes married couples without children and other groups of related people living together without their own children.

<u>Persons 65 Years and Over</u>: all people at least 65 years of age, regardless of whether they are living with other relatives.

In combination, the first four subgroups are both mutually exclusive and exhaustive of the entire population. Members of the last subgroup are also members of one of the other four groups.

Table 1 presents monthly and annual poverty rates for each of the subgroups and for the entire population. As expected, more people are poor on a monthly basis than on an annual basis. The difference between monthly and annual poverty rates varies across population subgroups as indicated by the ratios of annual to average monthly poverty rates given in the last line of the table. People in family types with relatively stable incomes, such as the elderly who tend to have pensions and Social Security, have annual poverty rates more nearly equal to monthly rates than do people in other family types, such as married couples with children, that may be more reliant on employment income.

Equally interesting is the amount of movement into and out of poverty from month to month (see Table 2). While the SIPP data show an annual poverty rate of 11 percent and monthly poverty rates averaging about 14 percent over the year, only 6 percent of people were poor in every month and over one-fourth were poor in at least one month. There appears to be a significant degree of monthly variation in income, at least for people in families with incomes near the poverty line.

Table 2 also shows the marked differences between annual poverty rates calculated from the SIPP data and those derived from the CPS. For every family type, the SIPP results are at least two percentage points lower than the CPS estimates, ranging from about 70 percent as large for members of married couple families with children to nearly 90 percent as large for people in single-parent families with children. The lower poverty estimates from the SIPP data may well result from the reduced underreporting of income, relative to the CPS.

Curiously, for all family types except "other," the CPS poverty rates are much closer to the average of the monthly rates estimated from the SIPP than they are to the annual rates from the SIPP. This may indicate that respondents to the CPS calculate their answers to survey questions about income on the basis of income for a given month rather than income for the entire year. Alternatively, this result could derive from the differences in the definitions of family composition used in the SIPP and the CPS. Both the CPS and the monthly SIPP poverty statistics use income and poverty threshold values for given and unchanging family compositions, while the annual SIPP numbers are based on family compositions that can vary from month to month but are reported under the most frequently occurring family make-up. In the aggregate, however, there are only minor differences between the CPS and the SIPP results in the distribution of the population among family types, and virtually no differences between annual and monthly values from the SIPP. (See Table 3.)[12]

Poverty and Program Participation

Because means-tested transfer programs are designed to assist the low-income population, one measure of their effectiveness is the fraction of beneficiaries that are poor. There are different ways, however, to assess the poverty status of program participants. On the one hand, since eligibility for many means-tested federal transfer programs is based to a large degree on monthly income, it can be argued that poverty status of recipients should be determined only for those months in which benefits are gotten. This avoids classifying as nonpoor those people who get benefits during months when their incomes are low but whose annual incomes are above the poverty thresholds. On the other hand, it may be that annual incomes are in fact a better indicator of true need, and therefore that annual poverty statistics offer a better measure of the target efficiency of transfer programs, regardless of actual program eligibility criteria.[13]

Comparison of the poverty status of means-tested transfer program recipients assessed for each month and that for an entire year reveals some marked differences. Table 4 shows the percentage of all program participants with family incomes below poverty using alternative time periods, alternative measures of annual poverty, and data from both the SIPP and the CPS, for four programs -- Aid to Families with Dependent Children (AFDC), food stamps, Medicaid, and Supplemental Security Income (SSI). Separate panels present the findings for all people, those in married couple families, those in single parent families, and the elderly. It is worth noting that the poverty rates given below are based on incomes that include cash transfer payments, while income limits for eligibility in a particular program do not consider benefits received from that program. The poverty rates reported here thus understate the pretransfer needs of recipients.

The fraction of program beneficiaries in poverty is almost always greater when measured across months than when measured over the entire year. On average, over four-fifths of food stamp recipients are poor during the months in which they receive aid, but fewer than two-thirds are poor for the year. About 70 percent of AFDC recipients are poor on an annual basis, but another 10 percent are poor during the months they get benefits. For SSI, on the other hand, just under half of beneficiaries are poor under either measure.[14] While results thus vary across programs, it is clear that some recipients who are not poor on an annual basis are in fact in poverty during those months when they receive aid.

To some extent, the differences between monthly and annual poverty rates of program participants may derive from the same sources as those differences for population subgroups. Monthly and annual rates are further apart for recipients in married couple families than for elderly recipients or for recipients in single-parent families; this is similar to the relative differences found above for the three subgroups. Again, the fact that members of the first group may have more variable sources of income than those in the other groups may explain the differences.

Annual poverty rates based on data from the SIPP and from the CPS also show the same relationship for transfer program participants as for the entire population. For all four programs examined, a larger fraction of recipients are poor using the CPS data than using the SIPP data; a similar pattern occurs in almost all cases within population subgroups. The reduced underreporting of income in the SIPP may again explain the differences.

Conclusions and Future Work

The above analysis offers provisional answers to the questions posed, but further work is needed before firm conclusions can be drawn. The provisional answers are:

- More people are poor when poverty is measured on the basis of monthly income than when annual income is used. In other words, temporary income fluctuations cause more people who are nonpoor on an annual basis to fall below the poverty line in one or more months than people who are poor based on annual income to move above the line in some months.

- Annual poverty rates are lower when measured with SIPP data than when CPS data are used. This may be due in part to more complete reporting of income in the SIPP and in part to the fact that the SIPP determines family composition contemporaneously with income data while the CPS matches family composition in March with incomes for the previous calendar year.

- More recipients of means-tested transfers are poor when poverty is evaluated with monthly data than when annual data are used. Eligibility criteria work to deny aid to people whose monthly incomes are not low, but do not ensure that benefits go only to those with low incomes over the entire year.

Because of limits of this analysis and because of potential problems with the SIPP data, these conclusions can only be considered provisional until further work is done. Concerns about these results and the additional work needed to resolve them include the following:

- Throughout the analysis, poverty status was based on total family cash income, including that coming from means-tested transfer programs. While this makes sense in looking at the well-being of individuals, it is less meaningful in assessing the effectiveness of the transfer programs themselves in reaching their intended recipients. For this purpose, the poverty status of beneficiaries might be measured using cash incomes before receipt of benefits from the program. This would make no difference for in-kind transfers such as food stamps and Medicaid, but could have major effects on analyses of cash transfers such as AFDC and SSI.

- The entire analysis used only a subset of the SIPP sample, those people who were surveyed in all twelve months of 1984. While it includes about two-thirds of all SIPP records, this subset may not be representative of the entire population. Comparison of the part-year population with the full-year group would help to determine whether significant bias is introduced by omitting the former.

- Weights used in this analysis were not adjusted to account for the exclusion of the part-year population. Calculation of appropriate weights would not only allow estimation of population counts, but could also correct for any bias introduced by using only data for people in the SIPP sample for all of 1984.

- Data from individual waves of the SIPP were linked without any attempt to provide cross-wave editing or imputations. The fact that there is greater variation in incomes across waves than within waves makes it likely that errors are introduced if uncorrected data spanning waves are used.[15] To the extent that the cross-wave variation is due to Census imputation methods, this difficulty might be resolved by limiting the analysis to those records in which all data items are obtained from respondents. Comparison of records containing imputed data with those that do not would indicate the extent of the difficulty.

  Some of the variation in incomes is probably due to inconsistent reporting by respondents. This may be much more difficult to correct, however, since any attempts to smooth income variations across waves would undoubtedly remove some of the actual variations that are important in looking at patterns of income receipt. Much careful work is needed to address this problem.

Despite these needs for further analysis, it is clear that the SIPP data provide an alternative to the CPS as a means of measuring poverty and program participation. To the extent that they are more like program eligibility criteria, monthly poverty rates might be preferred over annual rates in assessing program targeting. Use of the full range of poverty measures discussed here, however, gives a more complete picture of the low-income population.

BIBLIOGRAPHY

Bureau of the Census, Characteristics of Households and Persons Receiving Selected Noncash Benefits: 1984, Current Population Reports, Series P-60, No. 150, November 1985.

---, Economic Characteristics of Households in the United States: Third Quarter 1984, Current Population Reports, Series P-70, No. 5, October 1985.

Citro, Constance F., Donald Hernandez, and Roger Herriot, "Longitudinal Household Concepts in SIPP: Preliminary Results," presented at the Census Bureau Second Annual Research Conference, March 26, 1986.

Congressional Budget Office, Reducing Poverty Among Children, May 1985.

Nelson, Dawn, David McMillen, and Daniel Kasprzyk, "An Overview of the Survey of Income and Program Participation," Bureau of the Census SIPP Working Paper, No. 8401, June 1984.

ENDNOTES

1. This paper does not constitute an official Congressional Budget Office (CBO) document as it has not been reviewed by the Director of the CBO. The analysis and conclusions expressed should therefore not be construed as representing those of the CBO.

2. Thresholds also vary by whether the householder is age 65 or older (in the case of one- and two-person families) and by the number of children under age 18 (in families with two or more members). A family consists of all people related by blood or marriage living together in the same household.

3. These and other limitations of the official poverty measure are discussed in "Measuring Poverty," Appendix A of Congressional Budget Office, Reducing Poverty Among Children, May 1985.

4. For a detailed discussion of the SIPP, see Dawn Nelson, David McMillen, and Daniel Kasprzyk, "An Overview of the Survey of Income and Program

Participation," Bureau of the Census SIPP Working Paper, No. 8401, June 1984.

5. For the purposes of this paper, the poverty threshold for a month is defined to be one-twelfth of the annual poverty threshold. Arguments could be made for using other thresholds to calculate monthly poverty rates.

6. This is consistent with the finding from the Panel Study of Income Dynamics (PSID) that more families are poor in a year than are poor over a series of consecutive years.

7. Recall error might result in overreporting of amounts of income, particularly if respondents have experienced general increases in income over time and use their current incomes to remember what their incomes were in the past.

8. See Table 4 in Constance F. Citro, Donald Hernandez, and Roger Herriot, "Longitudinal Household Concepts in SIPP: Preliminary Results," presented at the Census Bureau Second Annual Research Conference, March 26, 1986.

9. The Census actually assigns a different weight to each sample person for each month. People in the 1984 SIPP file thus have 12 weights for the year. This analysis used the average of the 12 weights in calculating the population percentages reported in the tables.

10. In particular, if income and the poverty threshold in month i are denoted Y_i and P_i, respectively, and $r_i = Y_i/P_i$, then a person is poor in month i if $r_i < 1$, and poor for the year if

$$(Y_1 + \dots + Y_{12})/(P_1 + \dots + P_{12}) < 1, \text{ or}$$
$$Y_T/P_T < 1,$$

where $Y_T = Y_1 + Y_2 + \dots + Y_{12}$ and
$P_T = P_1 + P_2 + \dots + P_{12}$.

The annual poverty condition is equivalent to

$$(P_1r_1 + P_2r_2 + \dots + P_{12}r_{12})/P_T < 1,$$

or

$$(P_1/P_T)r_1 + (P_2/P_T)r_2 + \dots + (P_{12}/P_T)r_{12} < 1;$$

that is, a threshold-weighted average of income-threshold ratios is less than one. If the P's are constant, this is identical to the Census definition of poverty.

11. Population subgroups are determined for each month based on the individual's living arrangements or age. Over a year, however, these characteristics can change. Therefore, an individual's subgroup on an annual basis was defined to be that subgroup of which he or she was a part for the most months during the year. Individuals who were in two different subgroups for the same number of months during the year were arbitrarily assigned to the subgroup in which they were later in the year. Thus, for example, a person who spent the first six months in a "married couple with children" family and the last six months in a "single parent with children" family would be considered to be in the latter family type for the year as a whole. Note that this affects only the assignment of people to population subgroups for the reporting of poverty rates, and not the calculation of individual poverty status.

12. Note that there would be no difference between the distributions by family type using CPS data for March and the average of SIPP monthly values _if_ the actual distributions are constant across months and not changing over time and _if_ the CPS and SIPP obtain comparable responses.

The differences in these distributions reported in Table 3 may be deceptive in that they represent net rather than gross changes in family types over time. It is the movement of individuals among family types, which would be measured by gross changes, that would lead to differences among the various poverty rates given in Table 2. Measures of the gross changes are not yet available, however.

13. Some people argue that program benefits would be better targeted if recipients who have "high" annual incomes but some months with incomes low enough to qualify for aid were required to repay their benefits through the tax system. In essence, this view asserts that one year is a more appropriate period for measuring need than one month. Note that the asset tests that are generally part of the eligibility criteria for these programs may often serve to screen out people who are only temporarily poor and thus less needy.

14. To some extent, this result for SSI is due to the relative generosity of the SSI program in some states. California, for example, with 15 percent of all SSI beneficiaries, provides benefits that are above the poverty level for both single recipients and married couples. In addition, poverty measures are based on the total income of all related people living together, while eligibility for SSI may be based on less than total family income.

15. It appears that respondents are more likely to report changes in income sources and amounts between four-month interview waves than during those periods. This probably means that within-wave variation is understated and between-wave variation is overstated, leading to incorrect measurement of income fluctuations, and thus to errors in assessing monthly poverty.

TABLE 1: Monthly and Annual Poverty Rates of Individuals by Population Subgroup, 1984 a/ (in percent of all people in the subgroup)

Accounting Period	Population Subgroup: Married Couples w/Child	Single Parents w/Child	Unrelated Individuals	Other b/	All	Persons 65 Years & Over
Monthly						
January	12.3	43.9	23.0	6.8	15.1	12.1
February	11.1	43.1	22.4	6.8	14.4	12.2
March	10.6	42.3	21.9	6.3	13.9	11.8
April	10.9	43.7	21.6	6.4	14.1	11.6
May	10.0	42.0	21.6	5.9	13.4	11.6
June	9.6	41.9	21.4	6.0	13.2	11.8
July	9.8	42.1	21.3	5.9	13.3	11.8
August	8.8	41.4	21.8	5.7	12.7	12.3
September	9.3	43.3	22.1	6.2	13.4	12.6
October	9.3	43.4	21.7	6.4	13.4	12.6
November	9.8	42.4	22.0	6.6	13.6	12.3
December	10.6	42.5	22.0	6.5	14.0	12.0
Annual	7.4	39.9	17.7	4.5	11.0	10.3
Simple Average of Monthly Poverty Rates	10.2	42.7	21.9	6.3	13.7	12.1
Ratio of Annual to Average Monthly Poverty Rates	0.73	0.93	0.81	0.72	0.81	0.85

SOURCE: Tabulations of Survey of Income and Program Participation data.

a. Poverty rates are calculated on the basis of total cash income; no adjustment has been made for in-kind income. See text for discussion of methodology.

b. Other Persons include married couples without children and other groups of related people living together without their own children.

TABLE 2: Alternative Poverty Rates by Family Type, 1984 (in percent)

Family Type	Survey of Income and Program Participation: Annual Rate *	Poor All 12 Months	Poor In Any Month	Average of Monthly Rates *	Current Population Survey: 1984 Annual Rate
All Persons	11.0	5.9	26.2	13.7	14.4
Married Couples with Children	7.4	2.8	24.3	10.2	10.5
Single Parents with Children	39.9	25.8	60.8	42.7	44.7
Unrelated Individuals	17.7	11.0	35.9	21.9	21.8
Other Persons	4.5	2.0	14.3	6.3	5.3
Elderly Persons	10.3	6.8	18.5	12.1	12.4

SOURCE: Tabulations of data from the Survey of Income and Program Participation and the Current Population Survey.

* The SIPP annual poverty rates and the averages of monthly poverty rates are taken from Table 1.

TABLE 3: Distribution of Population Among Family Types Under Alternative Definitions, 1984 (in percent)

Family Type	SIPP Monthly a/	SIPP Annual b/	CPS Annual c/
Married Couples with Children	45.4	45.5	44.6
Single Parents with Children	10.4	10.3	11.8
Unrelated Individuals	12.0	11.8	13.0
Other	32.2	32.4	30.7
All	100.0	100.0	100.0
Elderly	11.7	11.8	11.5

SOURCE: Tabulations of the Survey of Income and Program Participation and the March 1985 Current Population Survey.

a. The average of monthly distributions of people among family types, based on SIPP data for 1984

b. The distribution of people among family types based on the type occurring in the greatest number of months according to SIPP data for 1984.

c. The distribution of people among family types as of March 1985, based on the CPS.

TABLE 4: Percentage of Program Beneficiaries* with Total Family Cash Incomes Below the Poverty Threshold, Under Alternative Definitions of Poverty, By Family Type and Transfer Program, 1984 (in percent)

Program	Based on SIPP Data: Annually Poor	Always Poor	Ever Poor	Monthly Average	Based on CPS Data
			ALL PEOPLE		
AFDC	70.1	49.3	88.6	80.4	76.0
Food Stamps	63.9	39.5	88.5	80.1	72.5
Medicaid	59.8	40.2	78.8	68.7	69.7
SSI	47.2	33.5	63.5	49.7	51.1
			MARRIED COUPLE FAMILIES WITH CHILDREN UNDER AGE 18		
AFDC	56.7	30.3	84.5	71.9	65.7
Food Stamps	56.4	25.8	88.1	76.0	64.1
Medicaid	50.6	24.6	76.8	63.0	65.3
SSI	36.0	9.0	55.7	28.3	32.8
			SINGLE PARENT FAMILIES WITH CHILDREN UNDER AGE 18		
AFDC	77.2	58.7	92.2	84.1	83.4
Food Stamps	73.4	52.6	93.7	85.7	84.9
Medicaid	73.4	54.1	90.4	80.3	84.0
SSI	50.7	32.7	77.4	56.1	59.2
			ELDERLY		
AFDC	**	**	**	**	**
Food Stamps	58.8	43.0	73.4	67.5	64.2
Medicaid	43.9	31.8	57.2	48.3	41.9
SSI	50.5	37.9	64.1	52.3	54.3

SOURCE: Tabulations of Survey of Income and Program Participation data and the March 1985 Current Population Survey.

* Except for the monthly average values, program beneficiaries are defined as people in families that received benefits from the relevant program at any time during the year. For the monthly average values, program beneficiaries are people in families that received program benefits during the relevant month.

** There were too few AFDC recipients among the elderly to obtain reliable estimates of the fraction of program participants in poverty.

Factors Affecting the Earnings & Welfare Income of Unmarried Mothers

Thomas P. Gabe, Jeanne E. Griffith, and Richard V. Rimkunas

ABSTRACT

Many studies have found differences in the labor force participation, earnings, and reliance upon welfare of unmarried black and white women. The reasons for these differences have not been fully explored. We employ a multivariate logistic regression model to identify the factors that determine whether a woman will have earnings that exceed any welfare income she may have. The empirical results suggest that, among unmarried mothers, a woman's specific marital status, age, and sources of income other than earnings and welfare have a greater effect on whether she will have earnings that exceed welfare income than does race, which loses significance when these variables are introduced into the equation. We find that income from sources other than earnings may have important effects on unmarried mothers' earnings and reliance on welfare.

INTRODUCTION

The economic and social plight of unmarried mothers has been at the center of the argument for the existing welfare network in the United States for some time. Looking at the sociodemographic characteristics of these mothers, the economic environments in which they find themselves, and their reliance on sources of income other than welfare or earned income, it becomes apparent that much variation within this group exists. While over 40 percent of unmarried mothers in the fall of 1983 received some type of welfare income, almost 60 percent received some form of earned income. Some unmarried mothers rely on earned income as their primary source of family income, others may rely upon some form of deferred compensation like a deceased husband's life insurance or pension, others may receive interfamily transfers such as child support or alimony, while still others may rely on AFDC or other forms of welfare.

Since the circumstances surrounding a mother's decision about work and welfare may be complex, it is important to determine if there are some systematic relationships among the characteristics of an unmarried mother, her economic and social environment, and her reliance on earned income or welfare income. Labor force participation rates have been shown to vary substantially by race. However, other characteristics that also vary with race and marital status are similarly associated with factors that may influence employment and earnings. Are particular socio-demographic or economic characteristics other than race likely to predict an increased probability of a mother's having earnings in excess of welfare income?

This paper attempts to address that question. An underlying assumption of our work is that the economic circumstances of an unmarried mother and her children may differ substantially depending upon the prior earnings history of the absent or deceased husband or father and that this effect should partly be captured by the amount of income from sources other than earnings or welfare that are available to the mother. For some mothers, an absent or deceased husband's present or prior earnings may contribute to her present economic circumstances, by making available other income such as social security survivor's benefits, child support or alimony, pension income, or life insurance annuities. For others, such support may not be available. The amount of income other than earnings available to unmarried mothers may have a direct bearing on their labor force attachment or their reliance on welfare.

In the first section of the paper, we present simple bivariate descriptions of unmarried mothers, to show that there are substantial differences among unmarried mothers. These descriptions do not control for any other factors; they are included to indicate possible relationships between variables.

In the second section of the paper, we present a multivariate analysis to control for the effects of a number of different variables, including but not limited to those addressed in the bivariate descriptions. Since the circumstances associated with work and welfare are complex, the increased probability that earnings will exceed welfare income cannot be adequately described by looking individually at specific characteristics of unmarried mothers. To better understand the complex relationships among earnings, welfare, and important explanatory variables, a multivariate model which simultaneously controls for numerous factors is employed.

THE DATA

The data source used in this analysis is the first wave of the Survey of Income and Program Participation, conducted by the U.S. Bureau of the Census. This is a multistage sample of 20,897 housing units, representative of the total resident population of the United States, excluding persons living in institutions and military barracks. The overall nonresponse rate in this wave of the survey was 4.9 percent. The first wave of the survey was initiated in October 1983;

interviewing extended through January 1984. (Each household is interviewed once every 4 months for approximately two and one-half years, so that as data are produced, longitudinal analyses will be possible.)

In the first wave of SIPP, households were asked detailed questions about demographic characteristics, living arrangements, labor force participation, amounts and types of income received, and participation in Federally-sponsored (and some other) programs. Information obtained in this wave covers the four months preceding the interview. As a result, the months covered in the recall period depend on the month in which a particular household was interviewed. (That is, households interviewed in October were asked about labor force participation and income recipiency over the period of June through September, whereas those interviewed in January were asked about September through December.) A more detailed description of the survey is available in "An Overview of the Survey of Income and Program Participation" (Nelson, McMillen, and Kasprzyk).

The sample yielded 1,475 unmarried women with children for the analysis, a representative sample of such women in the noninstitutionalized population at that time. Of these women, 861 (58.4 percent) were white, non-Hispanic (this figure includes a small proportion of women of other races who are neither black nor Hispanic, i.e. "other, non-Hispanic"); 479 (32.5 percent) were black, non-Hispanic; and 135 (9.2 percent) were Hispanic. Other characteristics of the mothers in the sample are also of interest for this analysis. On average, the mothers were 33.8 years of age and had 1.9 children; the youngest child, on average, was 7.8 years old. The mothers had attained an average education of 11.7 years.

EMPLOYMENT STATUS AND INCOME RECIPIENCY

To provide background for the model development in the paper, this section explores whether observed differences in employment, income recipiency by selected sources of income, and welfare participation among unmarried mothers of different race and ethnic groups can be at least partially explained by differences in marital status. This section presents only simple cross-tabulations that do not control for additional factors. The major purpose is to illustrate the observed differences among mothers by race/ethnicity in the relative reliance on earnings rather than welfare.

The discussion and data suggest that these differences are more likely attributable to differences in the unearned income that unmarried women in different marital statuses (i.e. widowed, separated, divorced, or never married) can amass. Since the composition of the race/ethnic groups varies substantially by marital status, on average the alternative resources available in each group also vary. This, in turn, may lead to the observed differences in relative reliance on earnings.

This section shows that the employment of unmarried mother varies with two factors -- marital status and race. Initial findings show that never married women are less likely to be employed than any other marital category. Black unemployment rates are also lower, on average, than the rates among white unmarried women. Furthermore, the distribution of women in each marital category varies among the race/ethnic groups. This is an indication that perhaps the influence of the distribution of the marital status within a race/ethnic group will affect the aggregate employment and earnings statistics of the group. For example, when marital status and race are controlled for simultaneously, the employment differences between black and white unmarried women are greatly reduced.

When we examine income recipiency according to race/ethnicity and marital status, we find that the recipiency of different sources of income also varies by each of these characteristics, suggesting that women in the different groups do not have the same resources available to them. Finally, we observe that within each race group, there is a similar pattern of reliance on earnings and welfare among unmarried women of the different marital statuses.

Employment and Marital Status Composition

Labor force participation rates. Numerous studies have noted that white and black women have different rates of labor force participation, and that participation rates vary by marital status as well. For the most part, studies compare married and unmarried women, but a more detailed look at unmarried women reveals some important differences in labor force participation according to their marital statuses. The findings among unmarried women from SIPP support this assertion, as shown in Table 1.[1] Divorced women have the highest rates of employment followed by separated and widowed women. Never married women have the lowest employment rates.

Among unmarried mothers (who are heads of their own families) white and other non-Hispanic[2] women have the highest employment rates, followed by black non-Hispanics. Hispanic mothers have the lowest employment rates, but the sample size for Hispanics is sufficiently small as to call into question the validity of distinctions within the group.[3] Unemployment rates are highest among Hispanic mothers, followed by blacks then whites.[4] The figures for women not in the labor force represent the reverse picture of the employment data.

There is reason to believe that the level of employment varies as these two factors, marital status and race/ethnicity interact. Black wives have been shown to be more likely to work than white wives (Bell, Landry and Jendrek, Leuthold). Among unmarried women in general, however, white women are more likely to work than black women (Bureau of Labor Statistics).

Employment by race and marital status. Does marital status affect the employment status of unmarried women in the different race groups in a different manner? Table 2 provides some information about this issue. This table shows the employment rates for mothers,according to their race/ethnicity and marital status. It appears from this table that divorced women are the most likely to work, and never married women are the least likely to do so. However, among blacks, widowed mothers are about as likely to work as divorced mothers, and among whites, widowed and never married women show about the same employment rates.

Marital status composition. Overall employment rates in the different race and ethnic groups can be considered as a function of the composition and employment rates of each of the race/ethnic groups according to marital status. As shown in Table 3, within each race group, the unmarried women show a very different distribution according to marital status. Among white unmarried mothers, a substantial majority are divorced and the next largest group are separated; a smaller percentage of these women are never married, and even fewer are widowed. The distribution among black unmarried mothers is quite different. The largest group are the never marrieds, followed by separated and then divorced mothers. A very small share of black unmarried mothers are widowed.
Summarizing this distributional data on marital status and employment rates, we can conclude that: 1) the relatively low employment rates of black unmarried mothers appear to be related to the sizable minority of these mothers in a group (never married) with a low employment rate, while 2) for whites the overall employment rate is relatively high since a larger share of these women are in categories (divorced and separated) with higher employment rates. This is simply a description of the employment situation of unmarried mothers. It is probable that marital status varies with some other determinants of labor force participation as well.

Sources of Income

What do these initial findings suggest? Is the importance of these differences in employment by race and ethnicity according to marital status the result of the different types of income these women may obtain and the consequences of that income for labor force participation, earnings, and welfare recipiency? Much higher poverty rates among unmarried mothers (compared to married mothers) have been widely documented in recent years (U.S. House of Representatives, Bane, Pearce and McAdoo, Ehrenreich and Piven, Vickery, Ford Foundation). Some evidence exists that the types of income available to women affect their labor force participation. For example, Bowen and Finegan (1969) found that other income is an important variable in accounting for the labor force participation of both married and unmarried women. They found that the labor force participation of both married and unmarried women tends to diminish as the amount of other income increases. However, Bowen and Finegan include welfare income as part of their other income variable; and welfare income, in itself, may be related to low levels of labor force participation and low earnings. More recently, Grossman and Hayghe (1982) found that mothers who receive child support or alimony may be more likely to work than those who do not. At first blush, Grossman and Hayghe's results seem to run counter to the expectation that receipt of other income should result in lower rates of labor force participation. However, the effect of other income on the labor force participation and earnings of unmarried mothers may be complicated by the existence of welfare.

Among others, there are three important reasons to expect that unmarried mothers who receive income from sources other than earnings or welfare may be more likely to have earnings than women who have no such other income. The first is that other income will be used in determining her welfare eligibility. If it is sufficiently high, she is unlikely to be eligible for any means-tested income programs.

Second, even if an unmarried mother's income is not high enough to disqualify her for welfare eligibility, the high implicit tax on a mother's welfare benefit from earned or unearned sources may substantially reduce the amount of welfare income for which she could qualify. Given the comparatively high welfare tax rate, at times exceeding 100 percent when the entire welfare package is taken into account, a mother with some other income might be better off working than receiving a reduced welfare benefit.

Third, if a woman receiving some other source of income has previously had a living standard somewhat higher than that which the other income would afford her, she may decide to work to maintain that previous standard. In this case, the other income is serving, in some aspect, as a proxy variable for the previous living standard of the woman.

To the extent that sources of income affect work behavior, and that sources of income vary by marital status, the marital status composition of the different race and ethnic groups should affect their relative labor force participation rates. The rationale for believing that different income sources would affect work behavior stems from the assumption that most unmarried women supporting families rely on one of two major sources of income: earnings or welfare; a minority of these women receive some income from both sources. The majority of persons, of course, rely on earnings, but when a woman has some non-negligible probability of relying on welfare, whether she does so or works may be influenced by a number of factors. Many background factors, such as her education, work history, skills and abilities, fertility and marital history, age and the local unemployment levels may affect this outcome. In addition, what other sources of income are available and at what level may affect this outcome. Again, if a woman heading a family receives income other than earnings or welfare, there may be a greater incentive for her to work because, even at a low salary, she is likely to receive more total income than she could if she turned to welfare to supplement that other income. A woman who has no other source of income, however, may be substantially less likely to work, because she may lose some level of benefits with each additional dollar earned. If she cannot command a high salary to begin with, the tax effect of her earnings on her welfare income will be very high, and she may end up no better or only marginally better off than if she did not work.

Tables 4 and 5 begin to shed some light on the recipiency of various sources of income by each of the race and ethnic groups and according to marital status.

Income recipiency by race. The percentage of families headed by unmarried women with income from different broad income categories varies considerably according to the race and ethnic category of the head, as Table 4 shows. Families headed by white mothers are much more likely than those of black mothers to receive interfamily transfers. (This category includes income from child support, alimony, and relatives or friends.) In addition, white women are much more likely to have income from various property-related sources than are black women. Black women, on the other hand, are much more likely to receive welfare income from any of a variety of programs. When all sources of income other than earnings or welfare are examined together, white unmarried women are considerably more likely to have some other source of income than are blacks.

Income recipiency by marital status. Table 5 shows that the income sources available to widowed, divorced, separated and never married women differ considerably. This finding serves as initial evidence that there may be income source and marital status effects on receipt of earnings versus welfare; these effects may exaggerate simple race effects if they are not accounted for. Nearly half of divorced mothers receive some sort of property income, and nearly as many receive some sort of interfamily transfers. Only about one-third of these mothers receive public welfare. Nearly three-fourths of divorced mothers have income from some source other than earnings or welfare. Among never married mothers, however, a very different picture emerges. Two-thirds receive welfare, but far fewer have some property income. Among never married mothers, very few receive interfamily transfers. Only a little more than a third of these mothers receive any income other than earnings or welfare. Separated mothers have income recipiency patterns generally between those of divorced and never married mothers. Widowed mothers show the least dependency on welfare. These mothers are much more likely to receive some source of income security or property income. Very few receive interfamily transfers. Over 90 percent of these mothers receive some form of income other than earnings or welfare.

Earnings. Almost 60 percent of all unmarried mothers have some earned income. Whites are more likely to earn income than black unmarried mothers. But, like the other sources of income this distinction may be confounded by marital status. Almost three-fourths of divorced mothers have earnings compared with just over 40 percent of never married women. Separated and widowed mothers fall in between.

Likelihood of earnings exceeding welfare

All of these statistics focus on recipiency and do not reflect the relative size of each type of income. To determine a woman's likelihood of depending relatively more on earnings than on welfare, the dependent variable constructed for the remainder of the analysis compared the relative levels of these two sources of income. This dichotomous variable identifies whether earnings constitute half or more of a woman's combined income from earnings and welfare. Table 6 shows the percentage of women who eceived more earnings than welfare income, according to race/ethnicity and marital status. The table provides initial evidence that marital status plays a very substantial role in determining whether earnings exceed welfare. In each race and ethnic group, divorced and widowed women were much more likely to receive a greater share of income from earnings than welfare than were never married women. Separated women fell in between these categories. Among black women, the figures for never married and widowed women were very similar to those of white women, while the data for divorced and separated women still show a slightly greater reliance on welfare for black women.

In this preliminary analysis we have explored the relationships among race, marital status, employment, earnings, welfare, and other income. We first sought to explore whether observed differences in labor force participation and welfare recipiency among unmarried mothers of different race and ethnic groups is associated with differences in marital status. We then determined that the receipt of other income by these women is associated, in turn, with marital status. Finally, we identified some similarities in the tendency for women of different race groups but of the same marital status to receive earnings that exceed welfare.

METHODS AND MODEL

The preceding discussion examined some of the simple bivariate relationships of unmarried women's race and marital status with their employment situation and earnings patterns. A multivariate approach is necessary, however, to more fully control for these and other relevant independent variables.

Methods

A logistic regression model is used to estimate the probability that a woman is likely to depend relatively more on earnings than on welfare as a major source of income. Although the dependent variable could have been defined as continuous (to identify the share that a woman's earnings represented of the total of her earnings and welfare income) we chose to define it as a dichotomous variable. It indicated either: 1) that she earned half or more or the total of her earnings and welfare, or 2) that she earned less than half of that total. The reason the continuous variable was converted into a dichotomous one was that the distribution on the variable was strongly bimodal, indicating that it would not be appropriate to use ordinary least squares regression for the model estimation. Chart 1 demonstrates that bimodality. Although the proportion of families receiving only earnings and no welfare varies greatly by marital status, within each marital status there are very few cases of families that receive some income from both earnings and welfare.

-Insert Chart 1 here -

Less than 12 percent of women receive a mix of these income types.

One feature of logistic regression is that the effect of an independent variable varies across the distribution of the probability of the outcome variable. Independent variables have a larger impact at the middle of the predicted probability range on the outcome variable than at the extremes. This feature of logistic regression is particularly appealing for this analysis because there is intuitive reason to expect that if a woman is either very likely or unlikely to have a greater share of her income from earnings, the effects of the independent variables would be less. If, however, she is in the middle ground on that outcome measure-- in terms of the combined effects of observed independent variables -- then a change in an independent variable of interest is likely to carry a greater effect.

Model

We examine whether among unmarried women, the level of income received from sources other than earnings and welfare has a strong effect on whether a woman will work or turn to welfare as a source of support. A woman's ability to provide adequately for her family often depends on her ability to obtain resources from a variety of sources. If she has some income available from other sources, she is unlikely to qualify for substantial welfare income, and consequently may be able to add a marginally greater amount to her total income through earnings. However, if she has little or no outside income, she may qualify for a relatively greater share of welfare and the implicit tax effect on any additional income she may gain from earnings will be high. Consequently, we expect that such a woman is less likely to work and have earnings than a woman with some outside income.

We further examine the importance of the other sources of income relative to the importance of race/ethnicity and the marital status of these unmarried women. Since we know that income from outside sources is strongly related to marital status, and that marital status, in turn, is related to race/ethnicity, we examine whether these latter two variables retain significance in a model where other income is introduced. The different compositions of race and ethnic groups, according to marital status, make each group, in the aggregate, appear as if it had a different propensity to obtain relatively more of their income from earnings. However, introduction of other income allows us to test whether women within each race/ethnic group with similar recipiency of other income behave the same. Relevant background variables are controlled for in the analysis to the extent the data permit.

Variables

Dependent Variable. As noted above, the dependent variable used in our model (ERNVSWLF) is a dichotomous variable to indicate whether the share of a woman's income from welfare and earnings combined was predominantly from earnings or welfare. The variable assumed a value of "0" if this part of a woman's income was primarily from welfare and "1" if it was 50 percent or more from earnings. The dependent variable was defined for income received in the month preceding the interview.

Sociodemographic. A number of sociodemographic variables are typically included in models estimating women's earnings and labor force participation; these were included in the equation to control for differential likelihoods of working for women with different demographic characteristics. Age and the square of age of the mother (AGE AND AGESQ) were both included, to control for the effect of increasing likelihood of working associated with increasing age, but also the fact that this effect is not linear, and the rate of increase declines with increasing age.

A woman's family type was included as a dummy variable, to indicate whether she was a member of a primary family or a related or unrelated subfamily. A dummy variable was created to account for family type, with the reference category being a primary family, to indicate whether a woman was a member of a related or unrelated subfamily (SUBFAM). This variable was included because it seemed that the support systems (both financial and social, in terms of providing a source of child care and assistance) available to a woman who headed a subfamily would be greater than those of a woman in a primary family. As a result, these mothers should show a greater propensity to work, controlling for all other factors.

Educational status of a woman has been shown in numerous studies to play an important role in determining labor force participation and affecting earnings. Two continuous variables were introduced to control for increasing labor force participation at higher levels of educational attainment: 1) years of school completed (HIGRADE) and 2) the square of years of school completed (HIGRADSQ). The variables were introduced simultaneously for the same reason as the age variable above, that the effect is not expected to be linear.

The number and age of a woman's children have been shown repeatedly to affect her labor force participation. Women with young children and with larger numbers of children are less likely to work, as are women who have relatively more children. Two variables were included in the model to account for these effects of children. The first indicates the total number of children a woman has (NUMKIDS), and the second indicates the age of the youngest child (YOUNGAGE).

The next two sets of sociodemographic variables are related to one another and are of primary interest. The first is race, which was coded as a pair of dummy variables to define a trichotomous variable with white, non-Hispanic as the reference category. Black, non-Hispanic (BLKNOHIS) and Hispanic (HISPANIC) are the categories shown. Race is known to be highly associated with labor force participation and receipt of earnings, although that effect is expected to be reduced with the inclusion of marital status and other income in the equation.

The second is marital status, which has four categories and was coded as three dummy variables, with divorced as the reference category. The first category shown is never married (NEVMARRY), the second is separated (SEPARATE), and the third is widowed (WIDOWED). This variable is apparently closely related to the recipiency of income from sources other than earnings and welfare, and, consequently, is expected to affect the dependent variable. Unmarried mothers who are divorced, separated, or widowed are expected to have a higher probability of having a greater share of earnings than are unmarried mothers.

Environmental. Two variables that are more descriptive of the environment in which a woman lives were included to account for external effects on the relative proportions of earnings and welfare. The first is the maximum benefit level of the payment under the Aid for Dependent Children (AFDC) program in the woman's state. All other things being equal, mothers in high paying AFDC states would be more likely to have welfare that exceeds earnings whereas in low paying AFDC states, there is a greater probability that a mother's earnings would exceed her AFDC payments. This was coded as a trichotomous variable dividing the states into three categories (with equal numbers of states), with the middle level used as the reference category. States that fell into the third with the highest AFDC benefits (AFDCHIGH) and the third with the lowest AFDC benefits (AFDCLOW) are the two categories shown.

The second environmental variable included was the unemployment rate of the state in which the woman lived, during the fourth quarter of 1983. That was the period in which most of the interviews were conducted and the variable serves as an average unemployment rate to introduce into the equation the effects of high unemployment, which may affect the ability of mothers to secure employment.

Economic. The last variable included in the model was the independent variable of primary interest, related to the sources of income a woman received other than earnings and welfare. The variable was the total amount of other income (OTHINC). This variable was defined for the three months preceding the month before the interview. This timing was determined because we believed that this variable should be considered as a precursor to a woman's later work behavior. With the data base available, we could not define a longer period preceding the observation of the dependent variable (which would have been preferable), but this accommodation should account to a degree for the antecedence of other income to current earnings and labor force behavior. We included in this income all cash sources of income as well as food stamps valued at their reported face value. In addition, all such income available within the immediate family was included. This income either directly or indirectly could influence a woman's decision to work or rely on welfare. The primary sources of other income available to unmarried mothers were those shown in Tables 4 and 5: property income, income security, and interfamily transfers.

FINDINGS

The next section discusses the selection of a multivariate model; the following section more fully examines the individual variables within the final model.

Model Selection

The first step in the analysis involved testing a series of "nested" models; these models were sequential in that each subsequent model included all the variables from the preceding model, plus one or more additional variables. The significance of the combined effects of added variables is estimated by comparing likelihood ratio statistics of two nested models. The estimation produces a maximum likelihood estimate, $L(\theta_2)$ (with the additional variables included) or $L(\theta_1)$ (with the specified set of variables excluded). These log-likelihood estimates can be compared, in a manner comparable to the F-test used with OLS regressions, by computing the test statistic:

$$-2 \ln \frac{L(\theta_2)}{L(\theta_1)}.$$

This statistic is distributed as a chi-square, with degrees of freedom equal to the number of variables excluded in the second (nested) model. The test is whether the model including those variables produces a maximum likelihood estimate significantly better than the model which excludes them. The probability of achieving a value at least as high as the test statistic is computed (Harrell, 1983; Takai, 1981).

A series of four nested models were compared to determine the improvement in the model gained by adding variables representing race, marital status, and income other than earnings or welfare to a set of background variables (all others discussed above) in estimating the probability that an unmarried mother's earnings will exceed her welfare income. In the first model, all the independent variables except these three were included. In the second, race (using two dummy variables) was included in addition. In the third, marital status was added to the second model (using three dummy variables); and in the fourth model, other income was added. In each case, the variables added were significant; the improvements to the overall fit of the model, as represented by the figure -2 log likelihood, are shown in Table 7. (The model results are shown in Appendix A.)

Race, Marital Status, and Other Income

These summary statistics show that in each case, these three variables significantly improve the model beyond that including only the preceding variables. This finding indicates that income from sources other than earnings and welfare plays a significant role in determining whether a woman will receive a greater share of income from earnings than from welfare. Although race and marital status are significant when added alone or as a pair, the model is significantly enhanced by adding the income variable.

In support of our second expectation, as will be discussed further below, when the income variable (OTHINC) is included, the race variables are greatly reduced in significance and their coefficients are much less influential. This result indicates that a combination of marital status and other income operate together to influence the dependent variable. When race alone is included in the model (without marital status and other income, as shown in model II in Table 7), both blacks and Hispanics are significantly different from whites, and the magnitude of the race effects is substantial. The coefficient for black unmarried women is nearly as large as that for Hispanic women, indicating a large effect in the aggregate of being black. However, when marital status and other income are controlled for, blacks are no longer significantly different from whites, but Hispanics remain so. This indicates that, although in the aggregate with respect to our dependent variable blacks behave very differently from whites, within each marital status category and at similar income levels, the likelihood of earnings exceeding welfare among black women is similar to that of whites. Among Hispanic women, however, differences remain, although a significant portion of the difference between Hispanics and whites is explained when marital status and other income are controlled.

The remaining discussion examines the findings from our model IV, selected as the most appropriate for estimating the dependent variable.

Final Model

The variables included in the final model are:

Dependent Variable:

ERNVSWLF: earnings as a proportion of earnings and welfare (dummy: 1 if >= .5, 0 if < .5)

Independent Variables:

AFDCLOW : in State with low AFDC maximum payment standard (dummy: 1=yes, 0=no)

AFDCMID : in State with middle AFDC maximum payment standard (dummy: 1=yes, 0=no);

AFDCMID is omitted from the equation, with its effect showing up in the intercept

AFDCHIGH: in State with high AFDC maximum payment standard (dummy: 1=yes, 0=no)

UNRAT: State unemployment rate

YOUNGAGE: age of youngest child

NUMKIDS : number of children in the family

SUBFAM: mother is head of related or unrelated subfamily (dummy: 1=yes, 0=no)

HIGRADE : highest grade of school completed

HIGRADSQ: HIGRADE squared

AGE: mother's age

AGESQ: mother's age squared

NEVMARRY: dummy: 1 = never married, 0 = other;

SEPARATE: dummy: 1 = separated, 0 = other;

DIVORCED: dummy: 1 = divorced, 0 = other; DIVORCED is omitted from the equation, with its effect showing up in the intercept;

WIDOWED : dummy: 1 = widowed, 0 = other)

BLKNOHIS: black, non-Hispanic (dummy:1=yes,0=no)

HISPANIC: Hispanic (dummy: 1=yes, 0=no)

WHTOTHNH: white and other races, non-Hispanic (dummy: 1=yes, 0=no); WHTOTHNH is omitted from the equation, with its effect showing up in the intercept

OTHINC : other family income: pensions, insurance and annuities, social security and railroad retirement, child support, alimony, and other miscellaneous sources of income.

The basic results from the logistic regression model are depicted in Table 8. The first column shows the coefficient; the second, the chi-square associated with the coefficient; and the third, the level of significance of the chi-square. The R^2 for the regression model is 0.27.[5] Somer's D_{yx}, a rank order correlation measure of the predicted versus the observed values on the dependent variable, is .678.

The beta coefficients for logistic regression models are difficult to interpret directly, as they represent the change in the log odds ratio associated with a unit change in the independent variable (Hanushek and Jackson, 1977). Converting the log odds ratio into a probability results in a more accessible interpretation. Each coefficient can be converted to indicate the effect of that independent variable on the probability that the dependent variable will assume a value of "1" (indicating that earnings exceed welfare), holding constant for all other independent variables in the model. In this case, the conversion represents the effect, all other things being equal, of a unit change in an independent variable on the probability that a mother's income from earnings exceeds that derived from welfare.

As mentioned above, however, given the functional form of the logit, the effect of a unit change in the independent variable upon the probability of occurrence associated with the dependent variable is not uniform over the entire range over which the independent variable may be evaluated. A marginal change in an independent variable is greater when the regression equation is assessed at the middle of the distribution on the dependent variable (meaning that someone has approximately a 50 percent likelihood that earnings will exceed welfare), when the probability associated with an occurrence is 0.5, for example, than at the tails of the distribution, at 0.1 or 0.9. Thus, it is useful to assess the marginal effect of an independent variable at several levels of probability associated with the dependent variable.[6] (The several levels of probability of the dependent variable are associated with the effects of different, but unspecified, combinations of the remaining independent variables.) The resultant estimated changes in the probabilities associated with a unit change in the independent variable are shown in Table 9.

Effects of Background Variables. We begin by examining the effect of environmental factors on the predicted probability that a mother's earnings exceed the family's welfare income. While a State's maximum AFDC benefit level was found to be significant, the State's unemployment rate (UNRAT) was not. As was expected, mothers in low paying AFDC States are more likely to have earnings that exceed welfare than mothers in high paying AFDC states. These estimates show the effect of living in either high or low paying AFDC States in contrast to a mid-level State. Compared to a mother in a mid-level State with a .50 probability of having earnings that exceed welfare, a mother living in a State with a low maximum benefit level (but with all other characteristics the same) is estimated to have a likelihood that her earnings will exceed welfare income of .64. In contrast, living in a high paying AFDC State is estimated to decrease that to a .41 likelihood. It should be noted, however, that while mothers in low paying AFDC states are more likely to have earnings that exceed welfare, they may or may not be as well off financially as mothers whose earnings do not exceed welfare, but who have a larger welfare benefit.

Next we turn to examining the background sociodemographic variables in the model. Consistent with other findings relating to women's labor force participation, both the age of the youngest child (YOUNGAGE) and number of children in the family (NUMKIDS) have a significant impact upon whether a mother's earnings are expected to exceed her welfare income. The signs of the coefficients are in the expected direction, with lower age of the youngest child and the number of children reducing the likelihood that earnings will exceed welfare. An additional child would reduce the probability that earnings exceed welfare to .41 from .50 for a woman with the same characteristics but one less child. On the basis of other characteristics, a mother with a youngest child of age six may have a .50 probability that her earnings exceed her welfare income. In contrast, a woman with all the same characteristics but a youngest child of age one would have only a .38 predicted probability.

Mothers who lived as subfamily heads were no more or less likely to have earnings that exceed their welfare income than mothers who headed primary families.

Educational level, measured by highest grade completed (HIGRADE) and its square (HIGRADSQ), were found to be significant at the .10 and .001 levels, respectively.

Evaluated at the mean level on all other variables in the model[7], unmarried mothers with 11 years of education are expected to have a predicted probability that their earnings exceed welfare of .57, compared to .65 for mothers who complete 12 years of schooling; an estimated .12 (14 percent) increase in probability attributable to completion of high school. Holding all other variables constant at their mean levels, completion of college results in a predicted probability that an unmarried mother's earnings will exceed welfare of 0.93; a 0.28 (43 percent) increase over that of mothers who only completed high school.

A mother's age is an important predictor of whether her earned income will exceed that derived from welfare. Chart 2 shows the predicted probability that an unmarried mother's earnings exceed her welfare income, by the mother's age and marital status. The probabilities are assessed for mothers of each marital status at their mean values on the other independent variables in the model. The estimated probability that a mother has earnings that exceed welfare increases but at a diminishing rate up to about age 35, at which point the probability decreases as the mother gets older. At age 20 an unmarried mother is predicted to have a 0.55 chance that her earnings exceed welfare, compared to a 0.72 chance at age 35 (a 31 percent increase); at age 45 the predicted probability declines to 0.64. The marginal effect of age on the dependent variable differs for mothers of different marital status as a result of the probability level associated with the mean level on the other independent variables.

The main question of interest in this analysis is whether race, marital status, or the amount of other income are more important in predicting whether unmarried mothers are more likely to depend upon earnings or welfare.

Marital Status. Marital status is an important predictor in the model as to whether a mother's earnings are likely to exceed her welfare income. Dummy variables were included for whether the mother was never married, separated, or widowed. The coefficients in Table 8 represent the effects of these statuses in comparison to being divorced. In combination, the marital status variables represent a significant contribution to the model.[8] Consider a divorced mother who, on the basis of other characteristics, is at the .50 probability that earnings will exceed welfare. A never married mother with the same set of other characteristics would have only .31 probability, and a separated mother, a .39 probability of having earnings that exceed welfare income. The coefficient for widowed mothers indicated that their probability was not significantly different from that of divorced mothers.

Chart 3 shows the effects of the background characteristics on each marital status' predicted probability that earnings will exceed welfare. One set of columns shows the expected probability that earnings exceed welfare for the average mother in each marital status. The other set of columns (shaded) shows the same expected probability, but assumes that all the women share the same overall average set of background characteristics. Comparison of the two columns in each marital status group shows the effect of the differences in average background characteristics between that group and unmarried women in general.[9] As shown in the chart, on average, divorced mothers are the most likely to have earnings in excess of welfare. However, they would have a subsfantially lower probability that their earnings would exceed welfare if they had the average characteristics of all unmarried mothers.

The chart also shows that if mothers in all marital status groups shared the same average background characteristics, there would be substantially less difference in the predicted probability that earnings exceed welfare between the different marital status groups. The chart shows, for example, that the average never married mother is estimated to have only a 28 percent chance that her earnings will exceed welfare.

- Chart 3 here -

However, if never married mothers had the average background characteristics of all unmarried mothers, the estimated probability that their earnings would exceed welfare would increase to 45 percent. If all unmarried mothers had the same average background characteristics, separated mothers would have somewhat higher probabilities and widowed mothers, somewhat lower.

Other Income. Not surprisingly, a mother's other income (OTHINC) (pensions, annuities, child support, alimony, social security, and the like) has a significant effect on the probability that she will have earnings in excess of welfare. The amount of other income directly offsets the amount of welfare for which a mother and her children would otherwise be eligible. Also, the amount of other income may negatively affect a mother's work effort, with other income in excess of some level being expected to reduce a mother's work effort, although this would be expected only at much higher income levels than are of interest here.[10] For a mother otherwise at the .50 level of probability, the marginal effect of $100 in other income per month is to increase the estimated probability that a mother's earnings will exceed welfare to .56, a 12 percent increase.

The amount of other income an unmarried mother receives can have an important effect upon whether the mother is likely to rely more upon earnings or welfare for her family's income support. The effect is expected to differ depending upon the expected probability associated with the mother's background characteristics.

Chart 4 shows the predicted probability that a mother's earnings exceed welfare, based on the mother's marital status and the amount of other income she receives. Each curve is defined by relating the predicted probability of the dependent variable to the mean level of the independent variables for each marital status group and allowing other income to vary. With no other income, never married mothers are predicted to have more income from welfare than from earnings, and separated mothers are predicted to have about an even chance of having earnings that exceed welfare. Divorced and widowed mothers, on the other hand, are more likely to have income from earnings than from welfare. As other income increases, the probability that earnings will exceed welfare increases for each group.

Comparing the curves for divorced and widowed mothers shows that without any income from other sources, divorced mothers are more likely to have earnings that exceed welfare than widowed mothers (74 percent and 63 percent, respectively). However, widowed mothers on average receive substantially more other income than do divorced mothers ($596 compared to $185, as shown by the points marked on the curves).

- Chart 4 here -

This mean level of other income markedly increases the probability that a widowed mother will have earnings in excess of welfare, from 63 to 88 percent; surpassing the expected probability of divorced mothers (81 percent) when their own mean level of other income is considered.

Again there are several possible explanations for this result. First, other income directly reduces the amount of welfare which a mother might otherwise receive. Second, other income may act a proxy for other background characteristics. For example, higher other income may occur in families where the mother has an established work history and is therefore more easily able to obtain employment. Or, other income may reflect a prior standard of living that can only be maintained by the mother's work effort. Third, other income may make work and earnings more attractive than welfare, due to the lower implicit and explicit tax rates associated with earnings as opposed to welfare.

Never married mothers are at the other extreme. They are more likely to have income from welfare that exceeds their earnings based on their background characteristics and other income received. Chart 4 shows that a substantial amount of other income, about $400 per month, would be required to offset the background characteristics of never married mothers so that half all such mothers would be expected to have earnings in excess of welfare. However, these mothers receive only $30 per month in other income on average. We have not explored whether there are other untapped income sources available to these women, such as the earnings of the children's father or income from the mother's or father's parents. Given the young age and other background characteristics of many of these mothers, it is unlikely that other income sources from the absent father would be sufficient to improve the prospects that these mothers will likely rely more upon income from work than from welfare. While other income may help reduce the amount which the government pays out in welfare, or marginally improve the economic well being of the mother, the amounts typically received do not greatly improve the prospects that these mothers will rely more upon their own earnings than upon welfare.

Race/Ethnicity. Comparison of the reduced form and final model results show that when both marital status and other income are included in the model, there is no longer a significant difference between whites and blacks on the probability that a mother's earnings will exceed the family's income from welfare. There is a significant difference, however, between Hispanic and white mothers. As noted above in reduced form models, the race/ethnicity variables were significant: 1) when neither other income sources (OTHINC) nor marital status variables (NEVMARRY, SEPARATE, WIDOWED) were included or 2) when the marital status variables alone were included. The effect of being black on the dependent variable was no longer significant when other income was included, however. This sequencing seems to suggest that it is not race, per se, which accounts for whether a mother's earnings are likely to exceed welfare income, but rather whether unmarried mothers are likely to have claims to other income, often associated with differences in marital status.

DISCUSSION

To some extent, the amount of other income that may be available to an unmarried mother is related to the earnings capacity and/or earnings history of the absent father of the mother's children as well as to the marital status of the mother. Unmarried mother's claims to other income are likely to vary considerably with, among other factors, the age, marital status, and race of the mother.

Life cycle income effects improve the likelihood that, relative to younger mothers, unmarried mothers who are older will have income from sources other than earnings. The implication that the children of older mothers are also likely to have older fathers, means that the fathers of these children are likely to be better able to support their children, and that mothers are likely to be less reliant upon welfare.

A mother's marital status also affects the likelihood she will receive other income. In the case of widowed mothers, claims to benefits relating to the husband's past earnings are relatively well institutionalized. Social security survivors benefits, for example, are directly related to the earnings history of the deceased husband. Widows' claims to pensions, life insurance annuities, and other liquid assets also improve their income from sources other than earnings or welfare relative to other unmarried mothers. For other mothers, it is much less likely that they will receive substantial amounts of other income. For divorced mothers, property rights are less well established, and often are adjudicated. In the absence of legal agreements, provisions by which separated women receive income support may be even more tenuous. In either case, an absent father's payment of support may or may not be forthcoming. For never married mothers, the probability of receiving other income is greatly diminished since legal claims for never married mothers often rest with the mother's ability to establish paternity. In addition, never married mothers tend to be younger than other unmarried mothers. As a result, the earnings capacity of the young absent fathers is likely to be low, even if claims on their income are made. Relatively older previously married women may be more likely to have enjoyed a higher average standard of living that they may seek to maintain through earnings in addition to any other income that is available.

The amount of other income a mother receives also varies with her race; however, the observed differences appear to be mitigated by the effects of age and marital status. Black mothers are less likely to have income from sources other than earnings and welfare than white mothers; they are more likely to have income from welfare, and less likely to have earnings. In the aggregate, some of the differences between black and white unmarried mothers are related to differences in marital status; over two fifths of black unmarried mothers were never married compared to about one in ten white mothers. Given the preponderance of black never married mothers, black unmarried mothers, as a group, are likely to be younger than white unmarried mothers as a group. As shown in reduced form models, above, when age and marital status are taken into account, differences in the probability that a mother's earnings will exceed welfare persist between black and white unmarried mothers. However, once other income is included, the difference vanishes. This would seem to imply that racial differences in whether earnings are likely to exceed welfare stem at least in part from differences in the amount of other income available to the mother.

LIMITATIONS OF THE STUDY

There are a number of limitations to this study, some of which lend themselves as subjects for further analysis. The limitations stem either from the internal workings of the model itself, from the broader definitions of the issues addressed, or the shortness of the data series.

To first address the broad issues, an important aspect of this analysis that must be kept in mind is that it examines factors associated with an excess of earnings over welfare. It does not delve into the adequacy of income associated with different patterns in the independent and dependent variables. Thus, the results of this analysis suggest that a mother's background, race, marital status and other sources of income are important determinants in increasing the probability that her earned income exceeds her welfare income. What this analysis does not explore is the actual level of this earned income. When earned income exceeds welfare income, does it meet a family's needs as defined by some objective standard?

This point can be clarified with an example. Assume that two hypothetical families with unmarried mothers, one living in a high paying AFDC state, the other living in a low paying AFDC state, have the same total family income. The model results would suggest that while the total income for these families is constant, the probability that earned income exceeds welfare income is greater in the low paying AFDC state. The model does not explore whether greater relative earnings necessarily lead to an unmarried mother's being better off financially. Clearly, this is an important aspect of the work-welfare issue and needs to be better understood.

Perhaps one way to understand better the relationship between earned income, welfare income and family needs is to look at these families across time. The current analysis, relying on the first wave of SIPP, provides a static comparison of unmarried mothers. Ideally, we would like to know what changes in circumstances lead to increased earning or reduced welfare income of mothers. Linking a number of waves of the SIPP might provide the facility for cross-temporal comparisons.

Relying on multiple waves of SIPP would provide us with another advantage. As the model is currently specified, a number of important background factors have not been included. For example, information on how long an unmarried mother has been unmarried, what her work history is, how long she has received welfare, and a history of the variability in her income might be important explanatory variables in the model. Unfortunately, this information was not available on the first wave. Subsequent waves and SIPP topical modules will provide an opportunity to explore these avenues of research.

There were also limitations deriving from the structure of the model that were not entirely controllable at this stage of analysis. Our hypothesis was developed with the expectation that there would be interactions among sets of independent variables in the model, particularly in the case of marital status. That is, if separate models were run for each of the marital status groups, we would have expected to find differences in the operation of some of the independent variables in the different marital status equations. Particular variables that we identified as of interest for interaction effects were race, age, subfamily status, and education. However, because of limited sample sizes in some of the marital status groups, we were unable to develop the separate models. To include all these interaction effects in the single model would have led to an excessively complicated model. This problem, too, may be solved in future years if the sample of SIPP is increased sufficiently to allow for the specification of separate models.

Another issue that bears further exploration is the effect of subfamily status on the outcomes. The reasons that an unmarried woman with children decides to establish a separate household or to live with another family (either related or unrelated) must vary with age, marital status, and level of need. Although our model did not indicate that subfamily status had a significant effect on whether earnings exceed welfare, it is quite possible that, under specific circumstances, it does have an effect. The subfamily can provide important financial, emotional, and social supports, and these could alter labor force participation. In some situations, for example, a young mother may be living with a subfamily so that she may complete her education; this clearly would reduce her likelihood of labor force participation.

BIBLIOGRAPHY

Bane, Mary Jo. Household Composition and Poverty. IRP Conference Paper, Poverty and Policy: Retrospect and Prospects. University of Wisconsin-Madison. November 1984.

Bowen, William G. and T. Aldrich Finegan. The Economics of Labor Force Participation. Princeton University Press. 1969.

Ehrenreich, Barbara and Frances Fox Piven. The Feminization of Poverty. The Persistence of Poverty. Dissent. Vol. 31, Spring, 1984.

Ford Foundation. Women, Children, and Poverty in America. Working Paper. Ford Foundation. New York. January 1985.

Grossman, Allyson Sherman and Howard Hayghe. Labor Force Activity of Women Receiving Child Support or Alimony. Monthly Labor Review. November 1982.

Harrell, Frank E. The Logist Procedure. SAS Institute Inc. SUGI Supplemental Library User's Guide, 1983 Edition. Cary, NC. SAS Institute Inc. 1983.

Nelson, Dawn, David McMillen, and Daniel Kasprzyk. An Overview of the Survey of Income and Program Participation. Survey of Income and Program Participation, Working Paper Series No. 8401. U.S. Bureau of the Census. September, 1984.

Pearce, Diana and Hariette McAdoo. Women and Children: Alone and in Poverty. National Advisory Council on Economic Opportunity. Washington D.C. September, 1981.

Takai, Ricky T. Marital Separation in First Marriages of Women: An Examination of Divergent Patterns, Baltimore MD. Unpublished Ph.D. Dissertation. Johns Hopkins University. 1981.

U.S. House of Representatives, Committee on Ways and Means. Congressional Research Service. Children in Poverty. 99th Cong. 1st Session. Washington, D.C. May 22, 1985.

Vickery, Claire. The Changing Household: Implications for Devising an Income Support Program. Public Policy. 26 (2). Spring, 1978.

1. The labor force concepts used in the following tables do not match the official concepts used with the Current Population Survey. We define as employed any person with a job at least one or more weeks during the last month of the recall period who spent no time looking for work. Persons identified as unemployed spent one or more weeks during the last month of the recall period looking for work. Persons identified as not in the labor force had no job during the last month of the recall period and spent no time looking for work.

2. White non-Hispanic and other non-Hispanic women were combined into a single category in these analyses because of the small number of "other non-Hispanic" women in the sample. This combination caused only a very slight change in these percentages in comparison to those only for "white non-Hispanic" women.

3. For the remainder of this paper, the terms white and black will be used to simplify the language, but each group has been consistently analyzed exclusive of Hispanics to control for the possible biasing effects of including Hispanics.

4. Because of their small sample size, the remainder of this part of the analysis will not draw attention to these data. However, figures for Hispanic women will be shown in the cross-tabulations, to identify when patterns are similar to those of whites and blacks.

5. The R^2 reported here is analogous to an R^2 in an ordinary least squares regression with a correction for the number of parameters estimated. Here it represents the proportion of log-likelihood explained by the model.

6. The equation used to convert the coefficient into an indicator of the effect of a unit change in a given independent variable, X_i, on the probability, P, of the dependent variable is: $dP/dX_i = B_iP(1-P)$.

7. See Appendix B for the mean values on the independent variables for mothers of different marital status.

8. As shown in Table 8, the combined chi-square associated with the marital status variables is 19.68, with 3 degrees of freedom, significant with $p < .001$.

9. Other income (OTHINC) has been set to zero in order to show the effect of differences in background characteristics alone on the probability that a mothers earnings exceed welfare.

10. It seems likely, although here untested, that other income would not reduce work effort over the range at which a mother would be eligible for welfare. Because the dependent variable is coded in terms of earnings as the proportion of earnings and welfare income, the dependent variable is not sensitive to the suspected work disincentive effect entailed in receipt of other income because such an effect would be expected to occur at levels well above those in which a mother would receive welfare.

TABLE 1: Percent Distribution of Employment Status of Unmarried Female Heads of Families with Children Under 18 by Race/Ethnicity and Marital Status

	Employment Status			
	Total	Employed[1]	Unemployed[2]	Not in labor force[3]
Race/ethnicity				
White and other non-Hispanic	100.0	62.1	10.5	27.4
Black non-Hispanic	100.0	48.8	14.6	36.7
Hispanic	100.0	33.4	16.6	50.0
Total	100.0	55.2	12.4	32.4
Marital status				
Widowed	100.0	44.2	11.4	44.4
Divorced	100.0	70.4	9.5	20.1
Separated	100.0	49.1	14.5	36.4
Never married	100.0	35.3	16.3	48.4
Total	100.0	55.2	12.4	32.4

1 Includes persons with a job at least one or more weeks during the last month of the recall period who spent no time looking for work.

2 Includes persons who spent one or more weeks during the last month of the recall period looking for work.

3 Includes persons who had no job during the last month of the recall period who spent no time looking for work.

TABLE 2: Employment Rates by Marital Status and Race and Ethnicity

	Employment Rates by Race and Ethnicity			
Marital status	White and other non-Hispanic	Black non-Hispanic	Hispanic	Total
Widowed	40.2	61.3	24.3	44.2
Divorced	73.3	61.1	59.4	70.4
Separated	51.2	54.0	28.0	49.1
Never married	40.3	36.8	17.2	35.3
Total	62.1	48.8	33.4	55.2

TABLE 3: Marital Status Distribution of Unmarried Female Heads of Families with Children Under 18, by Race and Ethnicity

Marital status	Race and Ethnicity			
	White and other non-Hispanic	Black non-Hispanic	Hispanic	Total
Widowed	10.5	6.7	6.2	8.9
Divorced	60.2	23.9	28.9	45.8
Separated	18.3	26.0	32.7	22.1
Never married	11.1	43.3	32.2	23.2
Total	100.0	100.0	100.0	100.0

TABLE 4: Percent of Families with Children Under 18 Headed by Unmarried Women with Income From Specified Sources[1], by Race and Ethnicity

Income source	Race and Ethnicity			
	White and other non-Hispanic	Black non-Hispanic	Hispanic	Total
Property[2]	52.0	25.9	24.6	41.2
Interfamily transfer[3]	40.7	14.1	15.6	29.9
Social insurance[4]	13.4	9.8	8.8	11.8
-All "other" income[5]	71.9	45.3	44.1	60.9
Public welfare[6]	34.1	54.8	62.2	43.3
Earnings[7]	66.8	52.8	37.0	59.6

1 Includes income received by all family members (or subfamily members if unmarried female is head of a subfamily) during first three months of recall period.

2 Includes income from life insurance; estates and trusts; other retirement, disability, or survivor payments; lump sum payments; roomers or boarders; reinvested dividends; rental property; mortgages; royalties or other financial investments; other cash income; interest; and dividends.

3 Includes income from child support, alimony, and relatives or friends.

4 Includes income from social security, social security for children, railroad retirement, and railroad retirement for children.

5 Includes property income, interfamily transfers, social insurance, and other miscellaneous income, excluding income from earnings or public welfare.

6 Includes income from Federal SSI, AFDC, general assistance, foster child care, other welfare, charitable groups, and food stamps.

7 Includes wage and salary income, income from self-employment, and incidental earnings.

TABLE 5: Percent of Families with Children Under 18 Headed by Unmarried Women with Income From Specified Sources[1], by Marital Status

Income source	Marital Status: Widowed	Divorced	Separated	Never married	Total
Property[2]	59.8	49.3	35.5	23.5	41.2
Interfamily transfers[3]	6.0	46.4	28.5	8.8	29.9
Social insurance[4]	75.1	6.3	7.4	2.5	11.8
All "other" income[5]	90.7	72.2	52.9	34.5	60.9
Public welfare[6]	17.9	33.6	49.2	66.5	43.3
Earnings[7]	46.8	73.6	55.2	41.0	59.6

1 Includes income received by all family members (or subfamily members if unmarried female is head of a subfamily) during first three months of recall period.

2 Includes income from life insurance; estates and trusts; other retirement, disability, or survivor payments; lump sum payments; roomers or boarders; reinvested dividends; rental property; mortgages; royalties or other financial investments; other cash income; interest; and dividends.

3 Includes income from child support, alimony, and relatives or friends.

4 Includes income from social security, social security for children, railroad retirement, and railroad retirement for children.

5 Includes property income, interfamily transfers, social insurance, and other miscellaneous income, excluding income from earnings or public welfare.

6 Includes income from Federal SSI, AFDC, general assistance, foster child care, other welfare, charitable groups, and food stamps.

7 Includes wage and salary income, income from self-employment, and incidental earnings.

TABLE 6: Percent of Families with Unmarried Mothers Relying 50 Percent or More on Earnings in Comparison to Welfare, by Race/Ethnicity and Marital Status

Marital status	Percent of Families Relying 50 Percent or More on Earnings, by Race/Ethnicity: White and other non-Hispanic	Black non-Hispanic	Hispanic	Total
Widowed	75.3	78.8	35.3	74.6
Divorced	77.1	63.6	58.8	73.8
Separated	61.0	52.8	30.6	53.4
Never married	34.9	35.7	18.5	33.3
Total	69.1	49.0	34.9	59.3

TABLE 7: Comparison of Models Estimating the Probability That Earnings Exceed Welfare

Model	Variables Included	-2 log likelihood	d.f.	Diff. in d.f.	Diff. in -2 log like.	Signif.
I.	Background only[1]	1360.32	10			
II.	Background & race[2]	1336.87	12	2	23.45	<.001
III.	Background, race, and marital status[3]	1315.06	15	3	21.81	<.001
IV.	Background, race, marital status, and income[4]	1272.87	16	1	42.19	<.001

1 Background variables: AGE, AGESQ, HIGRADE, HIGRADSQ, SUBFAM, YOUNGAGE, NUMKIDS, AFDCLOW, AFDCHIGH, UNRAT.

2 Race variables: BLKNOHIS, HISPANIC.

3 Marital status variables: NEVMARRY, SEPARATE, WIDOWED.

4 Income variable: OTHINC.

TABLE 8: Logistic Regression Results Predicting Earnings in Excess of Welfare for Single Mothers

	BETA	CHI SQUARE	PROB.
INTERCEPT	-3.875470	8.21	0.0042
AFDCLOW	0.556305	9.96	0.0016
AFDCHIGH	-0.343764	3.92	0.0477
UNRAT	-0.039634	1.55	0.2137
YOUNGAGE	0.099406	19.79	0.0001
NUMKIDS	-0.345635	19.66	0.0001
SUBFAM	0.118587	0.23	0.6321
HIGRADE	-0.247858	3.86	0.0496
HIGRADSQ	0.026205	16.46	0.0001
AGE	0.241007	13.48	0.0002
AGESQ	-0.003457	16.20	0.0001
NEVMARRY	-0.768518	13.92	0.0002
SEPARATE	-0.426583	5.36	0.0206
WIDOWED	-0.238083	0.40	0.5258
BLKNOHIS	-0.182668	1.20	0.2734
HISPANIC	-0.533499	4.46	0.0351
OTHINC	0.002384	33.13	0.0001

R^2 = .269
Somer's D_{yx} = .678
-2 Log Likelihood = 1272.87

TABLE 9: Effect of Individual Independent Variables on The Probability That Mother's Earnings Exceed Welfare Assessed at Three Probability Levels

VARIABLE	p=.10	p=.25	p=.50
AFDCLOW	0.050067 **	0.104307 **	0.139076 **
AFDCHIGH	-0.030939 *	-0.064456 *	-0.085941 *
UNEAT	-0.003567	-0.007431	-0.009908
YOUNGAGE	0.008947 ***	0.018639 ***	0.024852 ***
NUMKIDS	-0.031109 ***	-0.064810 ***	-0.086414 ***
SUBFAM	0.010673	0.022235	0.029647
HIGRADE	-0.022307 *	-0.046473 *	-0.061965 *
HIGRADSQ	0.002358 ***	0.004913 ***	0.006551 ***
AGE	0.021691 ***	0.045189 ***	0.060252 ***
AGESQ	-0.000311 ***	-0.000648 ***	-0.000864 ***
NEVMARRY	-0.069167 ***	-0.144097 ***	-0.192130 ***
SEPARATE	-0.038392 **	-0.079984 **	-0.106646 **
WIDOWED	-0.021427	-0.044641	-0.059521
BLKNOHIS	-0.016440	-0.034250	-0.045667
HISPANIC	-0.048015 *	-0.100031 *	-0.133375 *
OTHINC	0.000215 ***	0.000447 ***	0.000596 ***

Note: *** p <= .001
** p <= .01
* p <= .05

Chart 1
Percent of Unmarried Mothers with Income from Earnings and/or Welfare

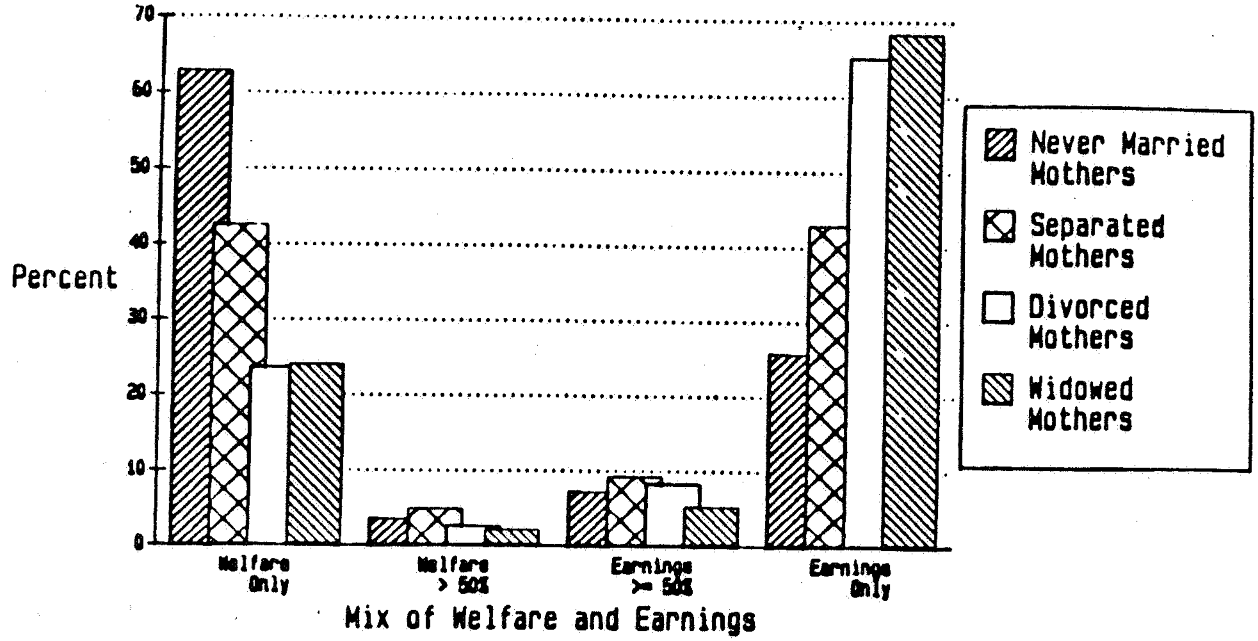


Chart 2
Predicted Probability that Unmarried Mothers' Earnings Exceed Welfare by Mothers' Age and Marital Status

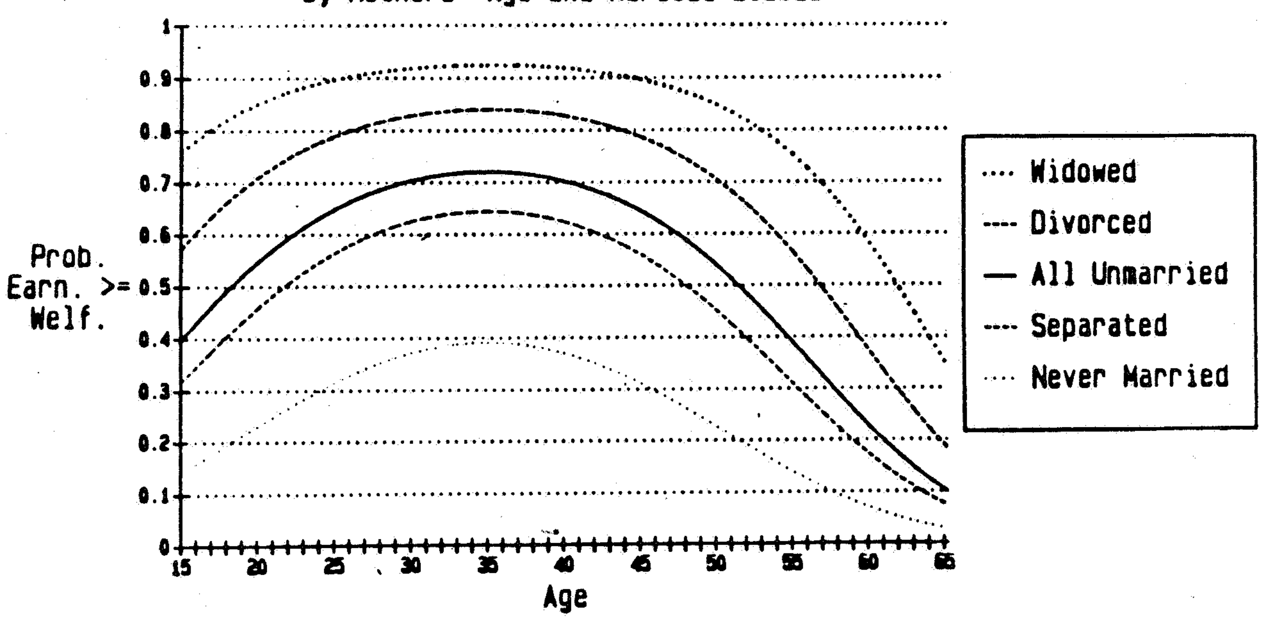


Chart 3
Effect of Background Characteristics on Predicted
Probability that Earnings Exceed Welfare
Comparison using Mean Values for Marital Status Groups
vs. Mean Values for All Unmarried Mothers *

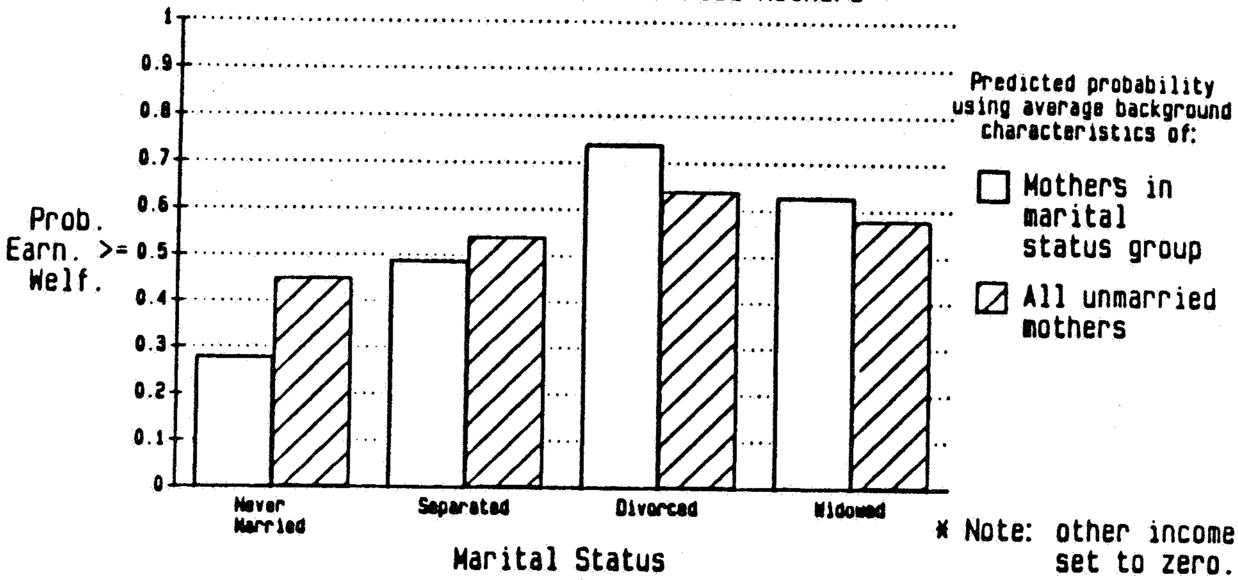
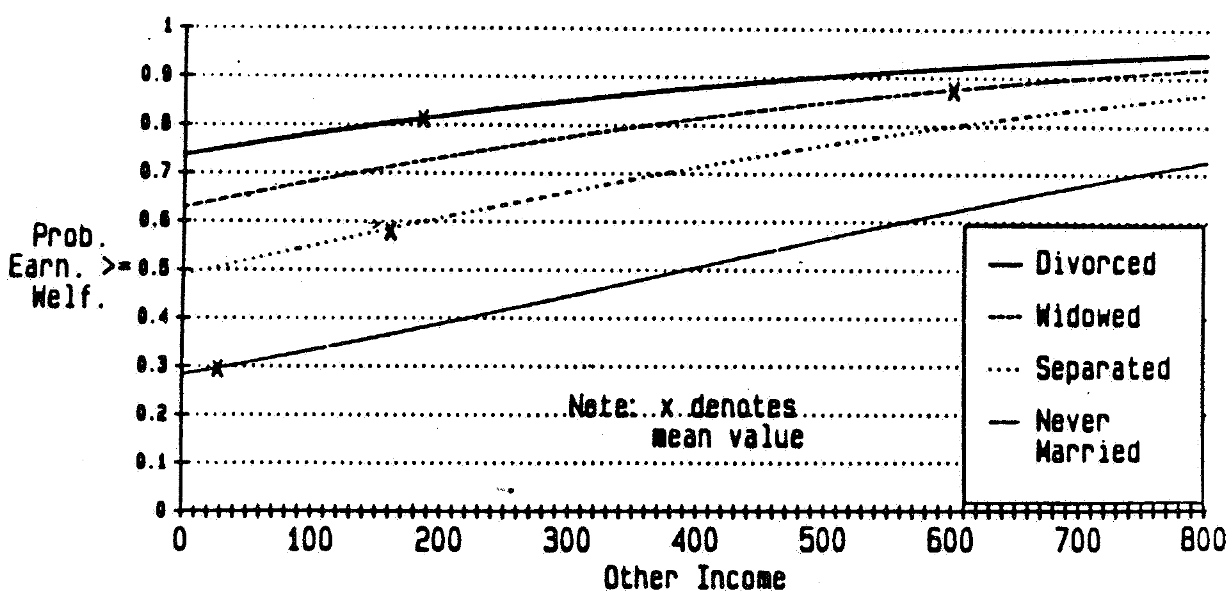


Chart 4
Predicted Probability that Earnings Exceed Welfare
by Amount of Other Income and Marital Status



APPENDIX A - Estimated Coefficients Under Four Models Probability That Mother's Earnings Exceed Welfare

	Coefficients by Model			
Variable	I	II	III	IV
AGE	.310722***	.290802***	.237385***	.241007***
AGESQ	-.004052***	-.003832***	-.003306***	-.003457***
HIGRADE	-.169166	-.213100	-.247167*	-.247858*
HIGRADESQ	.024562***	.026202***	.027485***	.026205***
SUBFAM	.063959	.091227	.088181	.118587
YOUNGAGE	.108472***	.110323***	.102469***	.099406***
NUMKIDS	-.307143***	-.255534***	-.281225***	-.345655***
AFDCLOW	.618789***	.660467***	.592098***	.556305**
AFDCHIGH	-.332386*	-.349525*	-.344143*	-.343764*
UNRAT	-.018013	-.031468	-.042109	-.039634
BLKNOHIS	--	-.619221***	-.337686*	-.182668
HISPANIC	--	-.813794***	-.614833*	-.533499*
NEVMARRY	--	--	-.859771***	-.768518***
SEPARATE	--	--	-.465519**	-.426583*
WIDOWED	--	--	.349846	-.238083
OTHINC	--	--	--	.002384***
R^2	.228	.238	.248	.269
Somer's D_{yx}	.624	.642	.655	.678
-2 Log Like.	1360.32	1336.87	1315.06	1272.87

*** p <- .001
** p <- .01
* p <- .05

Model I: Includes only background variables.
Model II: Includes background variables plus dummy variables to describe race.
Model III: Includes background variables plus dummy variables to describe race and marital status.
Model IV: Includes background variables plus total other income and dummy variables to describe race and marital status.

APPENDIX B - Mean Values of Unmarried Mothers on Selected Variables by by Marital Status

	ALL UNMARRIED	NEVER MARRIED	SEPARATED	DIVORCED	WIDOWED
ERNVSWLF (p-1)	0.593	0.333	0.534	0.738	0.746
AGE	33.4	26.8	33.7	35.4	44.7
AGESQ	1194.0	772.7	1201.2	1305.7	2065.4
BLKNOHIS (p-1)	0.332	0.595	0.401	0.168	0.282
HISPANIC (p-1)	0.091	0.124	0.143	0.056	0.042
HIGRADE	11.7	11.0	11.5	12.1	11.3
HIGRADSQ	142.7	127.6	139.4	152.6	139.5
NEVMARRY (p-1)	0.252	1	0	0	0
SEPARATE (p-1)	0.215	0	1	0	0
WIDOWED (p-1)	0.059	0	0	0	1
SUBFAM (p-1)	0.098	0.214	0.052	0.065	0.040
YOUNGAGE	7.7	4.5	7.0	9.1	12.4
ALLKIDS	1.9	1.8	2.2	1.8	2.0
AFDCLOW (p-1)	0.346	0.308	0.361	0.354	0.395
AFDCHIGH (p-1)	0.364	0.380	0.369	0.351	0.379
UNRAT	9.9	9.9	9.4	10.1	10.1
OTHINC	$166	$29	$166	$186	$597
n-	1318	332	286	621	79

DISCUSSION

John Fitzgerald, Bowdoin College and
Eugene Smolensky, University of Wisconsin-Madison

The three papers raise two types of issues. The first is the substantive contribution to our understanding of poverty in the U.S. and our policies for dealing with it. Second, they tell us about the usefulness of SIPP. Let us begin with the usefulness of SIPP.

SIPP offers several advantages over currently available data sets. It is monthly, longitudinal, has good detail on program participation and income sources, and enhanced data quality due to a shorter recall period. How do these papers exploit these advantages? Both Weinburg and Gabe, Griffiths and Rimkunas, hereafter GGR, are cross-sectional studies; that is, they use only one wave of SIPP. Weinburg makes use of the monthly nature of the data and the programmatic detail. GGR make use of the programmatic detail as well, but for them the advantage of SIPP over cross sectional surveys such as the Current Population Survey (CPS) is not large. In addition to using the monthly data, Williams's excellent paper does exploit the longitudinal nature of the data. He gives some indication of the large amount of movement into and out of poverty on a monthly basis when he presents results tabulated by "ever poor" in twelve months, "always poor" for twelve months, and so forth. This shows the richness that longitudinal data can provide, although we must be cautious. Williams reports that restricting his sample to those interviewed in each of the first five waves of SIPP caused a loss of one third of the sample to attrition. Is this reasonable and acceptable for longitudinal work? Williams admits that this high attrition rate may bias his results, presumably poor are more likely to leave the sample, but we would like to know by how much.

The papers demonstrate the usefulness of SIPP as a cross sectional survey, as well as its greater usefulness as a longitudinal sample. One of us questions whether longitudinal analysis using SIPP is too complicated for all but a few to use. We both do not share that view, particularly given the efforts of the Census Bureau and availability of a network such as Martin David's NSF supported SIPP ACCESS to facilitate working with data and training more users.

Two of the papers provide comparisons of SIPP and other data sets. Weinberg and Williams offer reasons to expect differences between the poverty rates calculated from SIPP and those from the CPS. Williams makes a good contribution toward quantifying some of the important differences, such as the effect monthly versus annual accounting, and the effect of measuring household composition at the same time as income. This takes us a step toward the time when we have enough studies to systematically distangle the effects of these influences as well as the influence of more full reporting of income with SIPP. Also, as Williams points out, SIPP includes imputed income values for certain individuals. These imputations are made cross-sectionally—that is, they ignore income information that the person may have supplied in previous interviews. These "hot deck" imputations will add variance to the monthly income numbers and presumably increase the measured monthly poverty rate. How much of an effect might this have?

Weinberg also offers another data set comparison: he compares April 1984 in the SIPP to that month five years earlier in the ISDP survey, the forerunner of SIPP. Most of the numbers compare reasonably, which is reassuring. But what are we to make of the apparent large difference in the reporting of AFDC benefits between the two surveys? Weinberg cites a Census report that 86 percent of AFDC recipients report receiving benefits in SIPP while the figure from the ISDP is 62 percent. Weinberg apparently tries a correction for this problem for AFDC, but the question for us remains: from 1979 to 1984, to what degree did better reporting of all types of benefits in the SIPP raise participation rates. Did this offset administrative cuts in program benefits over the period? We next turn to the poverty policy questions.

In our view, these papers share a common weakness: they miss interesting questions of the economic efficiency of transfer programs. We first deal with Williams and Weinberg together, and then address the point to GGR.

Both Williams and Weinberg address questions of target efficiency; that is, what proportion of program benefits go to the poor, or what proportion of program beneficiaries are poor. Weinberg finds that from 1979 to 1984 target efficiency has improved for means tested programs. What should we expect over the period? Given the Reagan administration's cuts in program benefits, particularly to the working poor, the "improvement" does not seem surprising. The worse off the eligible poor before transfers, the more target efficient transfers are likely to be. Williams finds that target efficiency improves when one uses a monthly rather than an annual accounting period to measure poverty. It improves even more when poverty is defined as "ever poor" in any one of the twelve previous months. Does that mean that programs are actually better targeted than we used to think that they were? Perhaps, but with given level of transfer programs, target efficiency will always improve when we use a poverty concept which classifies more people as poor. If we used a weekly accounting period to determine poverty by classifying anyone with low income for that week as poor, we would have a much larger proportion of the population classified as poor. The percentage of program benefits going to the poor would rise dramatically and so would target efficiency. This obviously does not represent an improvement in the programs themselves.

The more general point is why do we emphasize target efficiency? It is perhaps useful simply because it is used by others, but does it measure social welfare? As Sadka, Garfinkel and Moreland

(1982) have pointed out, maximizing social welfare subject to a budget constraint is unlikely to be the same as target efficiency. For example, if our goal is to transfer some level of income to the poor at least cost, that is to be economically efficient, we almost surely will want to preserve some work incentive for the poor by letting them keep a portion of any earned income. By so doing, some people above the poverty line are likely to continue to receive some benefits. This causes a loss of target efficiency, but may represent an improvement in economic efficiency. The emphasis on target efficiency, particulary for policy, is misplaced.

A similar, but less serious problem attends the emphasis on program participation and on the poverty gap. Here too there is constancy over time which seems surprising and pleasing to Weinberg. Why? If the eligible pre-transfer poor are worse off now, shouldn't both participation and the filling of the poverty gap be higher now? If so, why should we exult in the fact that participation and the filling of poverty gap have not grown? Perhaps we should be dismayed.

We now turn to GGR. The dependent variable in GGR is the "probability that a woman is likely to depend relatively more on earnings than on welfare as the major source of income". In a structural model predicting behavior for all women this variable might be of some interest. That is, we would like to know, based on labor market characteristics, program characteristics, and the personal characteristics of women just which women, and in what proportions, would, ex ante, earn more in the market than they would receive in welfare, or what proportion would earn more than their potential welfare. We could then ask, does that predict behavior well, or do other variables intervene; how might policy help. Estimation here is after the fact, however. After the game is played, and presumably each woman has revealed the best she can do, we find that some get more income from work and some welfare. In fact, in the short space of time of a quarter, the time interval of this data set, women either work or on welfare—very few get income from both sources at the same time. On page 21 of this paper, in fact, we are finally told that this dichotomy in the data is the real reason for choosing this odd dependent variable. Despite the argument the authors offer in defense of this dichotomous logit, we remain skeptical. First of all, we do not have an intrinsic interest in 50 percent, expost. We are much more interested in the natural binary dependent variable: work or not work. Second, the logit has all its mass in the two tails, and, as the authors indicate, all its policy relevance at the mean: that is, presumably the people certainly on the margin—the twelve percent who are already both working and receiving welfare. How confident can we be that the logit describes the data, never mind the complex set of decisions that produced these observed data, when there are very few cases around the mean?

Let us set that aside. The key substantive point is that unearned income is quite important in determining whether a woman earns more than her welfare payment. This is largely due, so they say, to the unearned income lowering the amount of welfare for which she qualifies, and thus making her more likely to have earnings. However, their conceptual model does not really allow a woman to make a marginal decision on the number of hours of work offered. For some women after tax income from work plus welfare plus other income (call it alimony) will be less than that from work plus alimony, while for others it is less than that from welfare plus alimony. Fine. But as long as the net wage from earnings is always lower when on welfare (due to the high implicit tax rate on earnings), no woman will both work and receive welfare. But many women do both—if not in a quarter then in a year—and we need to explain why.

When the choice is dichotomous, then we should not be surprised that other income is associated with the decision to work, since it may proxy for unmeasured components of earnings capacity. We know that husband's and wife's incomes tend to be positively correlated, and that never marrieds have lower earnings as a group. But the question of a marginal increase in earnings or hours of work is a different one.

Perhaps modelling the problem in a more traditional way by carefully modelling the budget constraint faced by women and allowing utility from leisure would solve this problem. Since the questions are important, an explicit labor supply model, or job search model, would be worth developing for future work.

We close this part of the discussion by emphasizing that it is terribly important to untangle the causal relationships in GGRs useful tables. The direct policy implication of GGR's way of putting things is a better system of child support from absent spouses. In their view, that would both raise income available to children directly and through an increase in women's labor supply. We are for that. If what is driving the result is earnings capacity, however, we should be concentrating on adding to the human capital of women. We are for that. But what if we have to chose? WHICH DO WE PREFER, EXPENDITURES HELD CONSTANT? It would be nice if GGR could tell us where the bang for the buck would be greatest, that is, the method that is more economically efficient.

In conclusion, we enjoyed reading the papers and hope that further policy work using SIPP is forthcoming.

References

Sadka, Efriam, Irwin Garfinkel and Kemper Moreland. "Income Testing and Social Welfare: An Optional Tax-Transfer Model," in Garfinkel, ed., Income Tested Transfer Programs. (Academic Press: New York) 1982.

NOTE: The Weinberg paper mentioned in the discussion is available from the author and is entitled "Filling the 'Poverty Gap', 1979-1984: Multiple Transfer Program Participation".

The author is Daniel Weinberg an Economist with the Office of Income and Security Policy, U.S. Department of Health and Human Services, Washington, D.C.