COVERAGE ISSUES RAISED BY COMPARISONS BETWEEN CPS AND ESTABLISHMENT EMPLOYMENT

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Labor market analysts have long been puzzled by the fact that the Census Bureau's Current Population Survey (CPS) measure of nonagricultural wage and salary employment declines less in business contractions than does the Labor Department's payroll (or establishment) survey measure. 2/ In this paper, I will show that a major cause of the difference in the cyclical behavior of the two employment measures is the fact that the CPS does not cover about 6 percent of the population.

In chart 1, I have plotted the seasonally adjusted difference (DIFF) between the adjusted payroll and CPS measures of nonagricultural wage and salary employment, monthly for the 20-year period from 1956 to 1976. The two adjusted employment measures were derived as follows:

1. The adjusted payroll measure equals the published measure, minus a small number of employees in agricultural services.

2. The adjusted CPS measure equals the published CPS measure of nonagricultural wage and salary employment, plus 14- and 15-year-old nonagricultural wage and salary workers outside private households, minus those workers age 16 and over who either work in private households or are on leave from their jobs without pay.

DIFF -- Throughout the period between 1956 and 1976, the difference between the two adjusted employment measures averaged about 4 million. This is largely -- but not completely -- attributable to two factors.

1. The payroll measure is conceptually larger than the CPS one, because the payroll survey counts jobs, whereas the CPS counts workers. Thus, the payroll survey counts the second (and subsequent) jobs of multiple jobholders, and this factor contributed roughly 2 million to the average level of DIFF.

2. The CPS measure understates employment, because the independent population control totals understate the population age 14 and over by roughly 4 million -- due to census undercount. This statistical error in the CPS contributed roughly another 2 million to the average level of DIFF.

The focus of my work is not, however, on the average level of DIFF; it is, rather, on the variation in DIFF over time. For purposes of comparison with DIFF, I have plotted the adult male unemployment rate in chart 1, with a dashed line, on an upside-down scale. You can see in the chart that there is a cyclical pattern to DIFF: every time there is a business contraction and the unemployment rate rises, DIFF declines. Conversely, when business recovers and the unemployment rate falls, DIFF generally increases.

This cyclical pattern to DIFF reflects the following situation. In contractions, the adjusted payroll employment measure declines more than the adjusted CPS employment measure. For example, from August 1957 to April 1958, the adjusted payroll measure declined 2.1 million, while the adjusted CPS measure declined only 1.2 million; consequently, DIFF declined 0.9 million (all figures seasonally adjusted). In recoveries and expansions, the adjusted payroll measure generally increases more than the adjusted CPS measure.

In principle, the cyclical pattern to DIFF could be due to any of three causes, or to a combination of them:

1. Conceptual differences in the coverage of the two adjusted employment measures could be responsible.

2. Statistical error in the payroll survey might cause the adjusted payroll measure to exaggerate employment fluctuations.

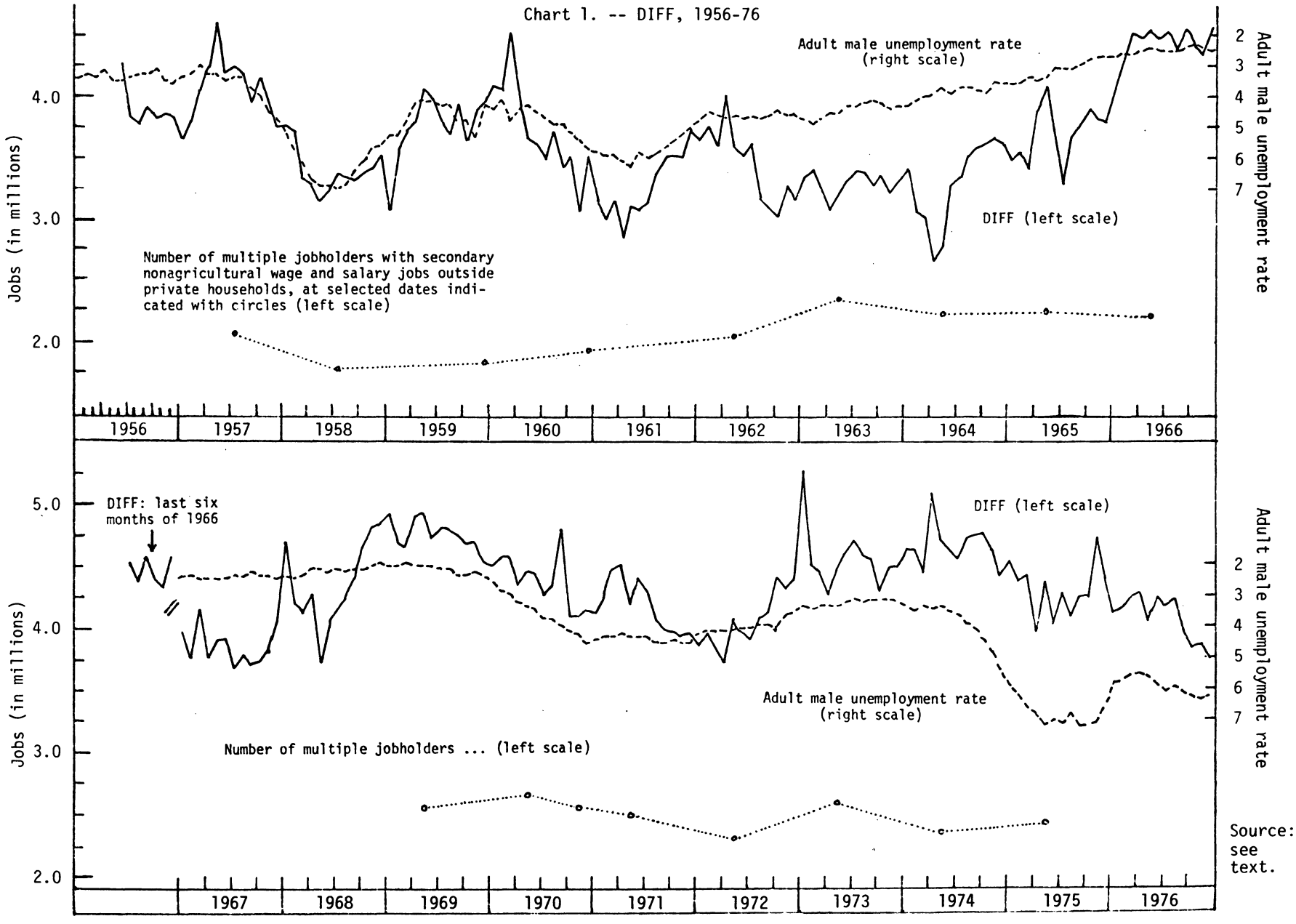
3. Statistical error in the CPS might cause the adjusted CPS measure to dampen employment fluctuations.

In my research, I have examined all of these possible explanations. In this report, I will deal very briefly with the first two, in order to concentrate on the third explanation -- statistical error, and specifically undercoverage, in the CPS -- which is the theme of this panel.

Multiple jobholding and job changing -- First, as previously mentioned, the major conceptual difference between the two adjusted employment measures is that the payroll survey counts jobs, whereas the CPS counts workers. This has two consequences.

1. The monthly CPS classifies multiple jobholders by the characteristics of their primary job -- that is, the job at which the largest number of hours were worked. Consequently, while the adjusted payroll measure counts secondary nonagricultural wage and salary jobs outside private households, the adjusted CPS measure omits such jobs. Intermittent CPS surveys of multiple jobholding indicate that the number of secondary nonagricultural wage and salary jobs outside private households -- plotted with small circles connected by a dotted line in chart 1 -- does not fluctuate sharply with the business cycle. Thus, multiple jobholding does not explain the cyclical pattern to DIFF, although it may contribute to the pattern in a minor way.

2. If a worker leaves a job in a pay period that includes the 12th of the month and starts another job in a pay period that



Source: see text.

includes the 12th of the same month, the payroll survey counts both jobs, whereas the CPS counts one worker. The Social Security Administration's Continuous Work History Sample (CWHS) provides clear evidence that job changing declines during contractions and increases during recoveries and expansions. An illustrative calculation suggests, however, that this factor accounts for cyclical fluctuations in DIFF that are on the order of 50,000-100,000, and are therefore much too small to explain fully the observed cyclical pattern to DIFF.

The payroll series -- Next, I will briefly discuss the hypothesis that the payroll series exaggerates cyclical employment fluctuations. The payroll series is benchmarked, usually once a year, to universe counts of employment based on administrative records. To benchmark employment in the private sector BLS has mainly used ES-202 reports. These are quarterly tax returns submitted by employers to State agencies in compliance with unemployment insurance (UI) laws. On the returns, employers state the number of persons who worked or received pay in the pay period that included the 12th of each month. I will first discuss the reliability of ES-202 data, then the payroll series as a whole.

The principal cause of inaccuracy in the ES-202 data is the attempt by some employers to evade UI taxes by either not filing returns or by concealing some workers. If tax evasion were to increase during business contractions, ES-202 tabulations would exaggerage cyclical employment declines. Realistically, tax evasion is feasible only for very small firms. Cyclical declines in payroll employment have been concentrated almost entirely in goods-producing industries -- manufacturing, construction and mining. If evasion does increase during contractions, the increase would have to be concentrated among small firms in goods-producing industries. But data for the most recent contraction indicate that the increase in evasion among these firms cannot have been very large. From March 1974 to March 1975, goods-producing firms with fewer than 20 workers reported an employment decline of only 5.0 percent, or 129,000 workers, on ES-202 returns; meanwhile, all firms in goods-production reported a decline of 11.4 percent, or 2.56 million workers. 3/

BLS supplements ES-202 reports with other data sources to benchmark employment, and uses data from a panel of 160,000 establishments to interpolate employment for months between benchmarks. There are problems with these procedures: Some of the other benchmark sources may be less reliable than ES-202 reports; and the panel, which is not a probability sample, may introduce bias into the inter-benchmark estimates. However, for the private sector, a comparison of the payroll series with ES-202 tabulations for the same months indicates that the two series have moved in parallel over the course of each business cycle. Therefore, if you accept the ES-202 tabulations as an accurate measure of cyclical changes in employment in firms covered by UI laws, it follows that the payroll series has not exaggerated cyclical employment fluctuations in the private sector. It is implausible that error in the payroll series for government employment has substantially exaggerated cyclical fluctuations in total payroll employment, because government employment continued to grow at all phases of the business cycle.

Error in the CPS

I come now to the central thesis of this report -- that statistical error in the CPS has dampened cyclical employment fluctuations. Much of the analysis will take place in terms of employment ratios. The aggregate employment ratio is the percentage of the civilian noninstitutional population (CNIP) age 16 and over that is employed; similarly, for any sex-race-age group, the employment ratio is the percentage of the CNIP in the group that is employed. 4/

Data from two independent sources underlie the monthly CPS employment estimate.

1. From the most recent decennial census, the Census Bureau extrapolates current population control totals for 96 separate sex-race-age groups.

2. From the monthly sample of 47,000 households, the Census Bureau ascertains employment ratios for each of the 96 sex-race-age groups.

To estimate employment, the Bureau blows the sample employment ratios up to the population control totals. There are two flaws that impair the accuracy of the CPS employment estimate.

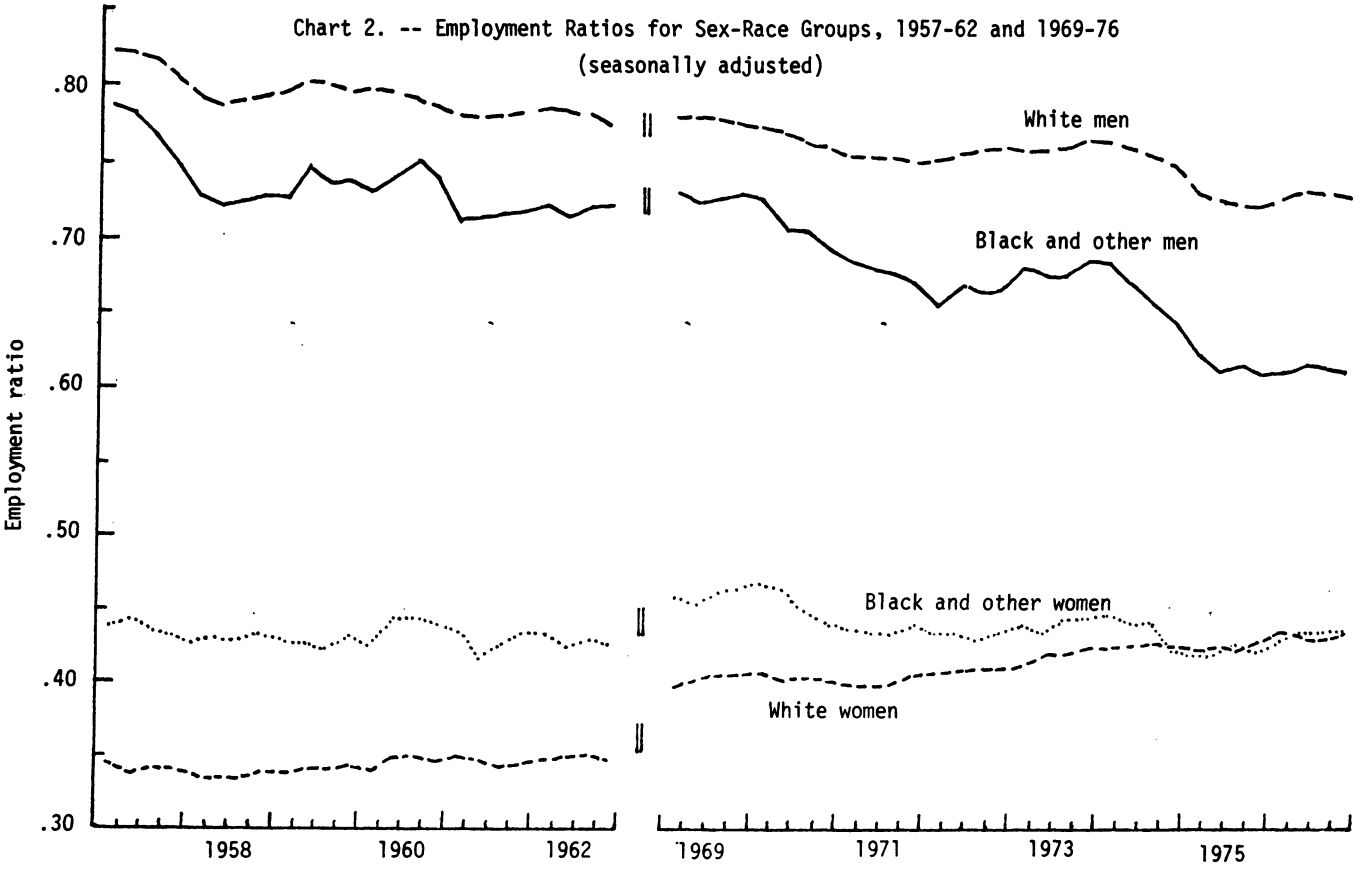
1. The population control totals are too low due to undercount in the decennial census, and the percentage error varies among sex-race-age groups.

2. Sample data for each sex-race-age group are probably biased, because the sample misses some of the persons it is designed to cover.

I will examine the effect of these flaws on cyclical changes in the CPS employment estimate in two steps. In the first step, I will examine the effect of error in the population control totals, on the assumption that the sample data are unbiased. In the second step, I will examine the effect of bias in the CPS sample, on the assumption that the population control totals have been corrected for census undercount.

Control total error -- Jacob Siegel has estimated that the 1970 Census undercounted the population by 2.5 percent. The undercount of the working-age population, 18 to 64, is of particular interest, because this group accounts for almost all employment. The overall undercount rate for this age group in 1970 was 2.8 percent; it was 4.1 percent for men and only 1.5 percent for women. Within each sex group, the rate was about

Chart 2. -- Employment Ratios for Sex-Race Groups, 1957-62 and 1969-76
(seasonally adjusted)



Source: Bureau of Labor Statistics, quarterly averages.

4 times as high for black and other races as for whites. 5/

I define the "control-total-corrected" employment estimate as the estimate that the Census Bureau would have made if it had blown CPS sample data up to control totals corrected for census undercount. In business contractions, control-total-corrected employment would have declined more than published CPS employment, for two reasons.

First, the population base would have been larger. Second, and analytically more interesting, the aggregate employment ratio would have declined more, because those sex-race groups with the largest census undercount rates have experienced the largest cyclical declines in the employment ratio (chart 2). In each contraction, the employment ratio declined far more for men than for women, and within each sex group, it declined far more for black and other races than for whites.

In business recoveries, control-total-corrected employment would have increased more than published CPS employment, but for only one reason -- the population base would have been larger. The aggregate employment ratio would not have increased more, because the sex-race groups with the largest census undercount rates did not (after 1959) experience above-average increases in the employment ratio in recoveries (chart 2).

Undercoverage -- Now I will assume that the Census Bureau has corrected the control totals for census undercount, and I will examine the effect of bias in the CPS sample on cyclical changes in the CPS control-total-corrected employment estimate.

The CPS sample is designed to include about 1 housing unit for every 1,400 in the country. At units designated for the sample, interviewers inquire about the employment activities of all household members age 14 and over, except Armed Forces members. To estimate the population actually covered by the sample, the Census Bureau multiplies the population in each sample household by the inverse of its probability of selection and adds the products. Subtracting the covered population from the best estimate of the population -- i.e., the population corrected for census undercount -- you have the uncovered population. Thus, in 1975, the covered population was 154.1 million and the uncovered population was 9.7 million (table 1, lines 4 and 5).

The undercoverage rate equals the uncovered population as a percentage of corrected CNIP. In 1975, the average undercoverage rate was 5.9 percent. The rate has always been much higher for men than for women, and for black and other races than for whites (chart 3). There are two groups in the uncovered population:

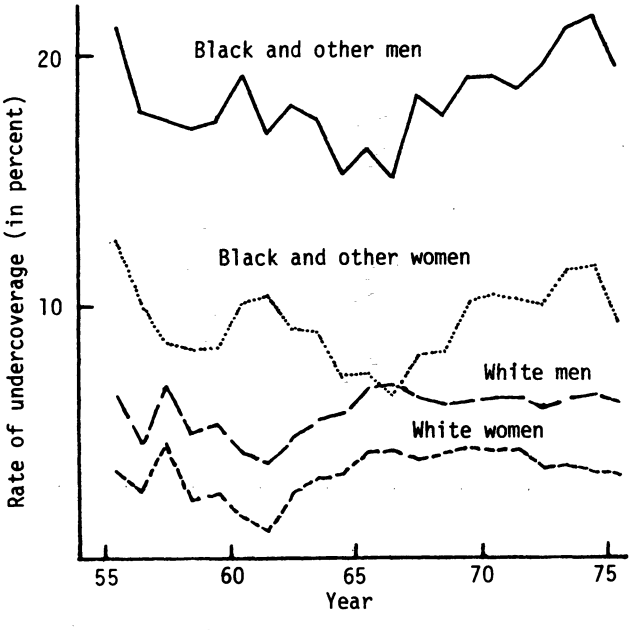
1. The population in housing units missed by the CPS has probably been the minority group at most times. David Bateman will later discuss how the CPS misses housing units. On the basis mainly of information

Table 1.--CPS Undercoverage, 1975 Annual Average (in thousands of persons)

Item	Total	Men			Women		
		Total	White	Black and other	Total	White	Black and other
1. Uncorrected (census-level) civilian noninstitutional population, age 14 and over	159.71	75.70	67.03	8.67	84.02	73.62	10.40
2. Plus: Adjustment for census undercount	4.09	2.73	1.82	.91	1.36	.95	.41
3. Equals: Corrected civilian noninstitutional population	163.80	78.43	68.85	9.58	85.37	74.56	10.81
4. Minus: Population covered by CPS	154.13	72.24	64.58	7.66	81.89	72.09	9.80
5. Equals: Uncovered population	9.67	6.18	4.27	1.91	3.48	2.47	1.01
6. Minus: Population in uncovered housing units	2.91	1.39	1.21	.18	1.52	1.31	.21
7. Equals: Residual uncovered population	6.76	4.79	3.06	1.73	1.97	1.16	.81
Notes (in percent):							
8. Rate of undercoverage -- (5) ÷ (3)	5.90	7.89	6.20	19.98	4.08	3.31	9.38
9. Population in uncovered housing units as a percent of corrected CNIP -- (6) ÷ (3)	1.78	1.78	1.76	1.93	1.78	1.76	1.93
10. Residual uncovered population as a percent of corrected CNIP -- (7) ÷ (3)	4.12	6.11	4.44	18.05	2.30	1.56	7.45
11. Adjustment for census undercount as a percent of corrected CNIP -- (2) ÷ (3)	2.55	3.48	2.71	10.52	1.59	1.29	3.94

Source: Census Bureau.

Chart 3. -- CPS Undercoverage of the Corrected Civilian Noninstitutional Population Age 14 and Over, by Sex and Race, 1956-75



Source: Census Bureau.

he supplied to me, I estimated that in 1975 the CPS missed 2.9 million persons age 14 and over in uncovered housing units, or 1.8 percent of the CNIP (table 1, lines 6 and 9). It is likely that men and women were missed at roughly equal rates in uncovered housing units.

2. The remaining group, what I call the "residual uncovered population," has probably been the majority group at most times. This group consists mainly of residents of covered housing units whom respondents fail to report, for various reasons. The size of this group can only be estimated residually. There were about 6.8 million persons on average in this group in 1975, or 4.1 percent of the CNIP (lines 7 and 10). The miss rate for men (6.1 percent) greatly exceeded that for women (2.3 percent), in this group.

Consequences of undercoverage -- Later, I will present evidence that -- within the uncovered population -- the residual group consists of persons who are poorer on average than their covered counterparts of the same sex, race, and age. For the moment, if you will allow me the assumption that this is the case, I will show that persons in the residual group experience larger cyclical fluctuations in their employment ratios than covered persons of the same sex, race, and age.

Labor economists have long contended that poor

persons suffer disproportionate employment losses during business contractions, and enjoy disproportionate employment gains when the labor market is tight. They believe this happens for two reasons.

1. Relatively few poor persons are in white collar occupations, which experience much smaller cyclical employment fluctuations than do other nonfarm occupations.

2. Employers are said to rank potential employees in a "labor queue." Those persons who lack characteristics that are desirable to employers -- high skill, high educational attainment, and steady work records -- stand at the end of the queue. Poor people lack skills, have low educational attainment, and checkered work records, and therefore stand toward the end of the queue. In contractions, employers are said to lay off disproportionate numbers of workers at the end of the queue; in tight labor markets, when workers toward the front of the queue are not available, employers are said to hire disproportionate numbers of workers at the end of the queue. To some extent, seniority rules reinforce this pattern.

Evidence that provides partial support for this picture of labor market behavior is contained in chart 4. Poverty and low educational attainment are known to be correlated. If poor persons do indeed experience disproportionately sharp cyclical employment fluctuations, I would expect persons with low educational attainment to experience disproportionately sharp employment fluctuations. Men with less than 12 years' schooling have in fact suffered much larger declines in their employment ratio in contractions than men with high school diplomas (chart 4). However, they have not enjoyed disproportionate gains in recoveries and expansions in the period 1964-75. For women -- who constitute a minority of residual missed persons and who are not represented in chart 4 -- the cyclical differentials are similar to those for men, but less pronounced.

For business contractions, the available evidence thus supports the hypothesis that residual missed persons suffered larger declines in their employment ratio than did covered persons of the same sex, race, and age. Consequently, the absence of these persons from the CPS sample dampened the decline in the control-total-corrected CPS employment estimate.

For recoveries and expansions, my findings are less clear-cut. The absence of residual missed persons from the CPS sample may have dampened the increase in the control-total-corrected CPS employment estimate, but this is not supported by the evidence in chart 4.

Illustrative calculation -- In sum, the CPS understates the employment decline in contractions for two reasons: first, because the population control totals are in error, due to census undercount; and second, because the CPS sample is biased, due to undercoverage. Let us return now to the problem I began with -- the cyclical pattern to DIFF. I will present an illustrative calculation of the impact of CPS error on DIFF in the 1974-75 business contraction. From the first 9 months of 1974 to the first 9 months of 1975, the adjusted payroll measure of nonagricultural wage and salary employment declined 1.67 million, while the adjusted CPS measure declined only 1.18 million (both figures are seasonally adjusted). The independently seasonally adjusted DIFF declined 425,000, and CPS error contributed to the decline of DIFF in two ways.

1. Control total error dampened the decline in adjusted CPS nonagricultural wage and salary employment by 105,000.

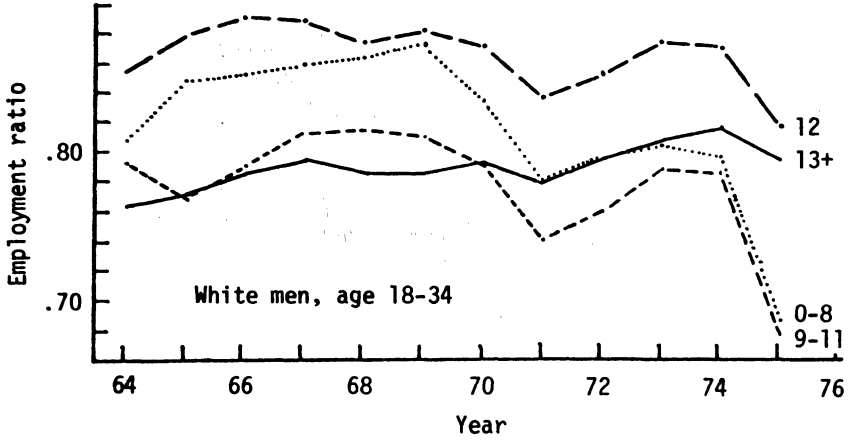
2. CPS undercoverage dampened the decline in control-total-corrected adjusted CPS nonagricultural wage and salary employment by a further amount. I will assume for the sake of argument that: Residual uncovered persons experienced declines in their employment ratios that were twice as large as the declines for covered persons of the same sex, race, and age; and persons in missed housing units experienced the same employment ratio declines as did covered persons of the same sex, race, and age. It follows that undercoverage dampened the decline in the control-total-corrected adjusted CPS measure of nonagricultural wage and salary employment by about 186,000. 6/

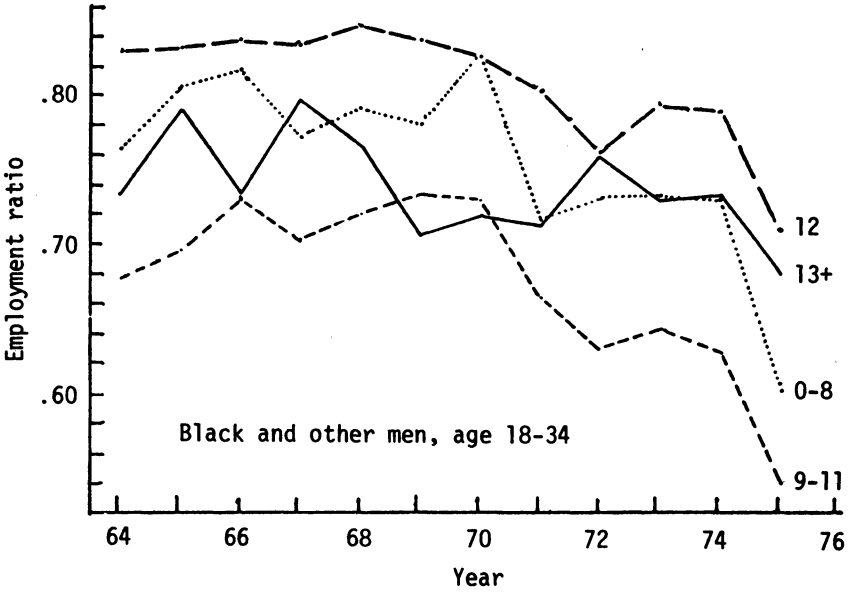
If my assumptions are correct, the two errors dampened the decline in the adjusted CPS measure of nonagricultural wage and salary employment by 291,000. This dampening would explain 68 percent of the decline in DIFF in the 1974-75 contraction.

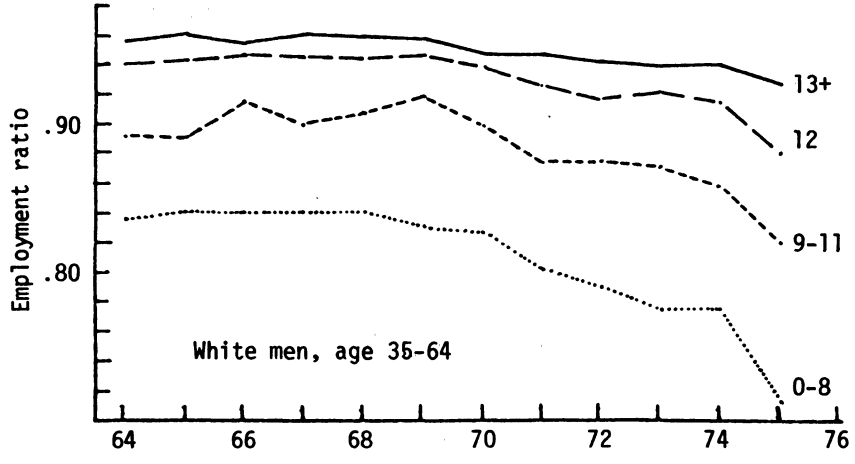
Illegal immigration -- An issue that complicates the analysis of control total error and undercoverage is the effect of illegal immigration on the accuracy of the CPS employment series. The population control totals corrected for census undercount ignore illegal immigrants, because Siegel's estimates of the 1970 undercount and the Census Bureau's estimates of the month-to-month change in the population do not take account of illegal immigration, a subject on which reliable data are altogether lacking. Consequently, the CPS control-total-corrected employment series -- and, ipso facto, the published series -- do not reflect changes in illegal alien employment. 7/ The payroll series, however, appears to count most of the illegal aliens who work in nonagricultural wage and salary jobs outside private households. 8/ DIFF is therefore sensitive to changes in the employment of illegal aliens in nonagricultural wage and salary jobs outside private households; however, DIFF is a somewhat ambiguous indicator of such changes, because it is also sensitive to other factors.

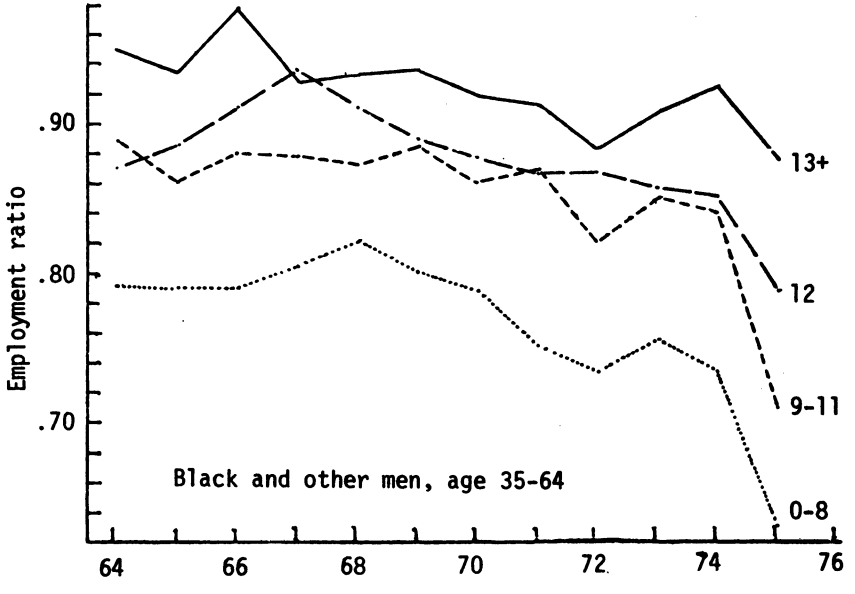
If allowance is made for a break in DIFF in January 1967, 9/ DIFF increased the record amount of 2.2 million from 1964 to 1969 (chart 1). The

Chart 4. -- Standardized Employment Ratio, by Educational Attainment in Years, 1964-75









Note -- Based on unpublished BLS tabulations from the March CPS. Original data for six age groups (18-19, 20-24, 25-34, 35-44, 45-54, and 55-64) were combined. To standardize for secular shifts in the age distribution within each educational group, the average March population for the 12-year period was used as a fixed weight for each sex-race-age-education group.

increase may reflect a sharp rise in illegal alien employment during the long business expansion of 1964-69. I have not been able to identify other factors that could account for the increase in DIFF in 1964-69.

There has been no sustained increase in DIFF since 1970, suggesting that the employment of illegal aliens in nonagricultural wage and salary jobs outside private households may not have increased substantially since 1970, and casting doubt on the widespread impression that illegal alien employment has grown rapidly since 1970. Of course, offsetting factors that tended to reduce DIFF could have masked growth in illegal alien employment, but I have not been able to identify any such factors.

Characteristics of Residual Uncovered Persons

Earlier, I asked you to allow me the assumption that residual uncovered persons are poorer on average than their covered counterparts of the same sex, race, and age. My argument that undercoverage is, in part, responsible for the cyclical behavior of DIFF hinged on this assumption; and it therefore remains for me to present evidence in support of the assumption.

Each month, the Census Bureau provides interviewers with lists of about 55,000 housing units designated for the CPS. There are three ways that interviewers miss persons while canvassing the designated housing units.

1. Interviewers find that an average of 7,500 housing units is vacant or otherwise ineligible for interview each month. Some of the units so classified are actually occupied; the residents of such "false vacancies" are missed.

2. At respondent households, interviewers ask a responsible household member to name all persons "who are living or staying here," including persons who are temporarily absent. Any persons whom the respondent omits from the roster of residents are missed by the CPS.

3. Persons with no usual residence are, of course, automatically missed.

There is evidence that persons missed in each of the three ways are more likely to be poor than covered persons of the same sex, race, and age. For brevity, I will discuss only the characteristics of persons omitted from household rosters. This is by far the largest group among residual uncovered persons.

Incomplete rosters -- Analysts of census undercount and ethnographers have identified two reasons why respondents give incomplete rosters to census enumerators and to CPS interviewers -- concealment and oversight. Both reasons apply with more force to men than to women, and to poor persons than to nonpoor persons.

1. Concealment -- Some respondents fear that information given to the Census Bureau will be used against them, and feel safer in withholding the names of some residents. Men are more likely to be concealed than women for two reasons: first, many of the motives for concealment apply particularly to men; and second, the majority of respondents are women. The motives for concealment are highly correlated with poverty.

Recipients of public assistance have, or may think they have, an incentive to conceal wage-earning or other income-receiving residents. Women receiving Aid to Families with Dependent Children (AFDC) have an incentive to conceal the natural father or adopting stepfather of their children, and may feel safer not reporting a husband or boyfriend even in cases where it would not affect AFDC eligibility. In many States, AFDC recipients also have an incentive to conceal nonearning residents not eligible for AFDC, because welfare officials prorate rent and utilities among all residents in computing AFDC grants.

In an ethnographic study of 35 Puerto Rican households in a poor New York neighborhood, Alan Harwood found the households had failed to report 15 of 52 resident men to a 1967 survey. Whereas the survey indicated that 67 percent of the households were female-headed, Harwood found that only 38 percent were actually female-headed. Fear of losing public assistance was the main motive for concealing the presence of male residents. 10/

Housing regulations create additional incentives for respondents to conceal residents. Fear of police or private retribution is another motive for concealment. Illegal immigrants, persons engaged in illegal activities, and persons wanted by the police have strong incentives to hide. 11/

2. Oversight -- Some respondents apparently "overlook" persons loosely attached to their household when asked to provide a roster of residents. The oversight may be entirely unintentional, or it may be linked to subjective motives such as a wish to protect privacy in regard to ambiguous matters, or a wish to keep interviews short. Ethnographic evidence for blacks indicates that loose attachment to households is more prevalent in poor neighborhoods than in more affluent neighborhoods, and that is more characteristic for men than for women. 12/

Driver's license study -- A Census Bureau study in connection with the 1970 Census provides insight into the circumstances under which respondents in poor neighborhoods omit men from rosters they provide to census enumerators, and, presumably, to CPS interviewers as well. From the rolls of the District of Columbia's Department of Motor Vehicles, the Bureau took a sample of 710 men, age 20-29, mostly black, with addresses in poor neighborhoods and with newly issued or renewed driver's licenses. In attempting to match the names with persons reported to the 1970 Census, the Bureau found that 23.5 percent of the men had been missed or probably missed by the census. There were two groups of missed men.

1. Twelve percent were misses that were confirmed by a resident at the man's address in reinterviews. Of these, 9.0 percent were in housing units that were enumerated and classified as occupied in the census. The investigators were generally unable to obtain clear explanations of why the men had not been reported to the census. Oversight may have been a major reason for this type of miss.

2. The other 11.5 percent were misses or probable misses that residents would not confirm in reinterviews, although the men had received their licenses by mail, and the investigators were frequently able to obtain corroborating evidence from the Post Office or Internal Revenue Service that the men received other mail at the address. Residents said they did not know the men, or said the men lived at other addresses that could not be confirmed in interviews at these addresses, or gave replies that appeared evasive or confused to the investigators. Deliberate concealment appears to have been a major reason for this type of miss. 13/

Sex ratios -- Thus far I have presented impressionistic evidence in support of my assumption that persons omitted from household rosters are poorer than their covered counterparts of the same sex, race, and age.

Table 2. -- Sex Ratios for Persons Age 18 to 64 by Residence in and Outside Metropolitan Poverty Areas, 1975 Annual Average

Race and area	Population age 18 to 64 (millions)	Sex ratio: men per 100 women		Note: percent with 1975 income below poverty level
		CPS sample	Estimated "true"	
	(1)	(2)	(3)	(4)
White.....	107.1	91.8	95.8	9.7
Metropolitan poverty...	4.7	89.1		25.0
Metropolitan nonpoverty	68.2	91.4		7.0
Nonmetropolitan.....	34.2	93.0		12.6
Black and other.....	14.6	75.0	89.2	29.3
Metropolitan poverty...	4.4	68.3		37.4
Metropolitan nonpoverty	7.0	78.3		17.6
Nonmetropolitan.....	3.1	77.8		41.6

Sources and notes:

1. BLS. Refers to census-level civilian noninstitutional population.
2. Estimated sex ratios in the population covered by the CPS, before blow-up of sample data to census-level population control totals. Underlying data from BLS and Census Bureau.
3. Census Bureau. Based on population corrected for census undercount.
4. Census Bureau. Refers to census-level civilian noninstitutional population, all ages, plus Armed Forces members living off base in the United States.

My assumption is also supported by a systematic comparison of sex ratios in the population covered by the CPS in and outside metropolitan poverty areas. The sex ratio is the ratio of men per 100 women. Metropolitan poverty areas are census tracts in which 20 percent or more of the population reported 1969 incomes below the poverty level in the 1970 Census.

Sex ratios in the population age 18-64 that was covered by CPS in 1975 are shown in column 2 of table 2 by race, for metropolitan poverty and nonpoverty areas. You can see that, in metropolitan areas:

1. For whites, the CPS found 2.3 fewer men per 100 women in poverty areas than in nonpoverty areas.

2. For black and other races, the CPS found 10.0 fewer men per 100 women in poverty areas than in nonpoverty areas.

There are two possible explanations for these differences.

1. They may reflect greater CPS undercoverage of men in poverty areas than in nonpoverty areas, due to incomplete rosters.

2. They may reflect lower true sex ratios in poverty areas than in nonpoverty areas.

Although data are lacking with which to settle the issue, the former explanation is more plausible. In defense of the latter explanation, it is sometimes argued that low sex ratios in poverty areas reflect a situation in which men have left their wives and children in poverty areas and gone to live elsewhere. This view is not persuasive, for two reasons.

1. It ignores the findings of ethnographers that many of the households that the CPS counts as female-headed are actually male-headed.

2. It begs the question of where the departed husbands and fathers went to live. Since ethnographers have found that the inability of men to earn steady incomes is a major cause of marital instability among poor persons, it would be surprising if the departed men were to resettle en masse in the more affluent sections of metropolitan areas.

FOOTNOTES

1/ The views expressed in this paper are not those of any Governmental agency. In writing this paper, I have benefited from the generous editorial assistance of Edward Steinberg, and from discussions with David Hirschberg and Fritz Scheuren. Don King and Tom Kraseman first got me interested in the topic. I have received data and other assistance from Paul Armknecht, Carol Utter, and Alan Harwood; and

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2/ See, for example, President's Committee to Appraise Employment and Unemployment Statistics, Measuring Employment and Unemployment, 1962, p. 113.

3/ Employment and Wages, first quarters of 1974 and 1975.

4/ This ratio is higher than the ratio published by the Bureau of Labor Statistics, because the former is based on the civilian noninstitutional population, the latter on the total noninstitutional population (including Armed Forces).

5/ Census Bureau, Estimates of Coverage of Population by Sex, Race, and Age: Demographic Analysis, PHC(E)-4, 1974.

6/ There are three steps to the illustrative estimate. First, residual uncovered persons accounted for 70 percent of the uncovered population in 1975 (table 1). I therefore assume that the employment ratios of uncovered persons declined 70 percent more than the employment ratios of covered persons of the same sex, race, and age. Second, there were an average of 10.1 million persons age 14 and over in the uncovered population in the two 9-month periods under consideration. If their employment experience had been the same as that of covered persons of the same sex, race, and age, their adjusted nonagricultural wage and salary employment would have declined 267,000. Third, under my assumption, the adjusted nonagricultural wage and salary employment of uncovered persons declined 70 percent more than 267,000, or an additional 186,000.

7/ The population control totals corrected for census undercount ignore most emigration as well as illegal immigration. Therefore, the controls implicitly allow for illegal immigration equal to uncounted emigration. There is some evidence that uncounted emigration during the decade 1960-70 was about 100,000 per year. Robert Warren and Jennifer Peck, "Emigration from the United States: 1960 to 1970," paper presented at the annual meetings of the Population Association of America, 1975; and Ada Finifter, "Emigration from the United States -- An Exploratory Analysis," paper prepared for the Conference on Public Support for the Political System at the University of Wisconsin-Madison, August 13-17, 1973.

8/ There is evidence that employers pay Social Security taxes for about 80 percent of their illegal alien nonagricultural wage and salary employees. Employers who pay Social Security taxes probably pay UI taxes for the same workers when they are covered by UI laws. Consequently, the ES-202 tabulations probably include most illegal alien nonagricultural wage and salary workers outside private households. David S. North and Marion F. Houstoun, The Characteristics and Role of Illegal Aliens in the U.S. Labor Market: An Exploratory Study, report to the Department of Labor, March 1976, p. 142.

9/ In January 1967, BLS reclassified about 750,000 nonagricultural workers from self-employment to wage and salary employment, thus reducing DIFF by the same amount. Note the break in DIFF in chart 1.

10/ Harwood and his associates observed the households for 12-14 months in 1968-69, and reconstructed their rosters as of the date of a 1967 survey conducted by a neighborhood health center. Alan Harwood, "Participant Observation and Census Data in Urban Research," paper delivered at the annual meeting of the American Anthropological Association, 1970; and personal communication to the author.

11/ Leon Pritzker and N. D. Rothwell, "Procedural Difficulties in Taking Past Censuses in Predominantly Negro, Puerto Rican, and Mexican Areas," in Social Statistics and the City, David M. Heer, editor, Report of a Conference held in Washington, D.C., June 22-23, 1967, Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University, 1968, pp. 72-73.

12/ Carol B. Stack, All Our Kin: Strategies for Survival in a Black Community, New York, 1975; and Elliot Liebow, Tally's Corner, Boston, 1967.

13/ Census Bureau, "1970 Census: Preliminary Evaluation Results Memorandum No. 21," prepared by Ralph Novoa, October 1971.