THE SURVEY OF INCOME AND
PROGRAM PARTICIPATION

THE IMPACT OF SURVEY AND QUESTIONNAIRE DESIGN ON LONGITUDINAL LABOR FORCE MEASURES

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The Impact of Survey and Questionnaire Design on Longitudinal Labor Force Measures

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ABSTRACT

In this paper we examine the tradeoffs between two methods of collecting labor force data, *retrospective questions* versus *repeated interviews*. In the case of gross change measures, such as month-to-month flows between labor force states, retrospective measures tend to understate change, while repeated interviews seem to overstate change because of response variability. In the case of measures of experience--the amount of time spent in a labor force state within a given time frame--retrospective measures tend to be affected by recall bias, which lead to the undercounting of short periods spent in that state. On the other hand, experience measures constructed from repeated interviews can be affected by selective attrition, since only observations with a complete set of interviews can be used.

KEYWORDS

retrospective questions, panel data, measurement error, gross flows

I. INTRODUCTION

In this paper, we examine how survey and questionnaire design affect labor force measures. Our objective is to clarify the tradeoffs between alternative ways of measuring the same labor market phenomenon, not that of indicating which questions or designs produce the "best" estimates. Whenever possible, we attempt to identify the presence and direction of the biases associated with a particular method of data collection. We focus on two types of longitudinal measures, namely the estimates of the number of individuals changing labor force state between two time periods (gross flows), and the estimates of the time spent in a given labor force state.

The last two decades have witnessed a rapid development in the availability of statistical methods to analyze longitudinal data. Such rapid development has not been matched by a similar improvement in the quality of longitudinal data. For example, the duration of unemployment has received a great deal of attention in terms of analytical modelling, but how to measure the time people spend looking for work is still not well understood.

A broader motivation for the type of analysis conducted in this paper stems from the fact that employment and welfare policies are typically targeted to persons with very unstable labor market histories. One of the things needed to design effective policies is a good knowledge of some of the most complex aspects of labor market behavior, in particular employment instability, incidence and duration of unemployment, withdrawals from the labor force, and wage changes associated with transitions (such as wage losses following unemployment). For those involved in the collection of labor force data this represents a quandary: the high turnover segment of the work force is of utmost interest for public policy, but this segment represents a major challenge in terms of survey data collection.

Possible Approaches

At the cost of some simplification, we identify three general methodological approaches to study the effect of survey and questionnaire design on labor force measures: (i) *laboratory techniques*, where the effects of alternative questionnaire designs are tested with experimental methods (Dippo 1989); (ii) *validation studies*, where the information provided by the survey is compared

with administrative data--such as the Panel Study of Income Dynamics (PSID) validation study (Duncan and Mathiowetz 1985); and (iii) *direct comparison of estimates* obtained from distinct surveys which measure the same phenomenon but use a different design or different questions (Ryscavage and Bregger 1985).

The main advantage of the first two approaches--laboratory techniques and direct validation--is that they can isolate specific aspects of the problem by focusing on single questions or single estimates. Therefore, they can often reach convincing conclusions. The third approach--comparing estimates from existing surveys, the approach used in this paper--is necessarily less robust, because it compares two or more surveys that differ along many dimensions, some of which might not be of interest for the analysis that is being conducted. In many instances this approach allows the analyst to offer only speculations, rather than firm conclusions. On the other hand, this method uses existing data sources, available at relatively low cost, while both laboratory techniques and validation studies involve either generation of new data or expensive matching between data sources. We view the three approaches as complements, rather than substitutes: the survey comparison approach, being less expensive and more easily implemented, can point to specific problems that can be further investigated with the more analytic techniques.

A Taxonomy of Labor Force Measures

In order to put our analysis into a broader context, we find it useful to distinguish between four broad classes of labor force measures: point-in-time, gross change, duration, and experience measures. We discuss them in turn:

(1) point-in-time measures--where the reference period is defined in such a way that changes in labor force status during the period are either impossible or irrelevant. The number of persons found employed in a given week is an example of a point-in-time measure.

(2) measures of gross change--defined as change in labor force status between two successive discrete time periods. The number of persons unemployed in a month who are no longer unemployed the following month is an example of a gross change measure.

(3) measures of duration--defined as the amount of time spent in a labor force state without any intervening change. The number of weeks a person looks for a job is a measure of duration.

(4) measures of experience--the amount of time spent in a labor force state within a given time frame, such as a year or a person's lifetime, but ignoring whether the time is spent consecutively or in separate spells. The number of weeks a person is unemployed during a year is an experience measure.

One methodological issue spans all four types of labor force measures, and represents the main theme of this paper: the distinction between measures constructed using *repeated observations* on the same individual and measures constructed using *retrospective information* collected on one occasion. The distinction applies to all four types of measures.

Point-in-time measures are typically collected with separate interviews having the same frequency as the measures to be constructed. However, it is possible to implement a data collection plan whereby several point-in-time measures are collected with one retrospective survey. The retrospective approach is likely to lead to some loss of information, due to the longer recall period, but such loss should be balanced against the possible reductions in survey costs and respondent burden due to the less frequent interview schedule.

Measures of gross change can be obtained from a one-time retrospective survey; from repeated surveys having the same frequency as the measure of change that is being constructed; or from repeated surveys that contain retrospective questions covering several time periods. Our *a priori* expectation is that retrospective questions tend to underestimate the amount of mobility between

labor force states--we hypothesize that respondents have an incentive to "simplify" their labor force experience during the reference period in order to shorten the interview. On the other hand, we expect that estimates of gross change based on repeated interviews overestimate mobility, because of response variability and classification errors.

Measures of (spell) duration and *measures of experience* are often collected by asking respondents at a point in time to summarize their labor force history during a certain period in the past. Empirical evidence collected in a variety of settings suggests that a substantial fraction of short spells go unreported, thus biasing upward the estimates of average duration and average experience. Duration and experience measures can also be constructed by following respondents over time and linking longitudinally portions of the same spell, thus shortening the recall period. However, measures constructed from repeated interviews can be affected by selective attrition, since only observations with a complete set of interviews can be used.

Plan of the Paper

This paper focuses on comparisons of labor force measures from the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS): although these surveys do not produce all possible types of labor force measures, their richness and complexity allow us to shed some light on a number of issues. The plan of the paper is as follows. Section II describes the features of SIPP and CPS that are relevant for the analysis. In Section III we compare estimates of monthly stocks of persons employed and unemployed based on SIPP to the official estimates based on the CPS. Section IV focuses on measures of gross change, namely the monthly labor force gross flows. Section V considers two types of experience measures, namely annual weeks worked and annual weeks unemployed.[1]

II. AN OVERVIEW OF LABOR FORCE MEASURES BASED ON SIPP AND CPS[2]

In this section we present a brief overview of the surveys used in the analysis. The SIPP is a multipanel longitudinal survey of the U.S. noninstitutional population aimed at collecting sub-annual information on income, program participation, labor force activity, and household composition. Each year, since 1984, the Census Bureau has begun a new SIPP panel of respondents to be interviewed every four months for approximately two and one-half years. At the start of each panel, SIPP is a sample of addresses, but subsequently to the first interview it becomes a sample of persons, in the sense that all members of the original sample are followed when they change address, move into an existing household or form a new household. Panels are divided into four rotation groups of equal size and each month one rotation group is interviewed resulting in a staggered sample design. The reference period for each interview is the previous four months. Although the 1984 Panel contained 20,000 households, panel sizes in subsequent years were considerably smaller. The 1986 Panel, which is the source of the data examined in this paper, consisted of approximately 11,500 households interviewed at the time of the first interview. For more information on the content and design of SIPP, see the *SIPP Quality Profile* (Jabine et al. 1990).

The monthly CPS is a rotating panel cross-sectional survey of the U.S. noninstitutional population designed to collect information on the labor force activities of persons as of the week containing the 12th of the month. It has been in operation every month since the 1940s. The CPS is a sample of households (or addresses) rather than persons. It consists of approximately 59,500 households divided into eight rotation groups. Households come into the sample for four months, are rotated out for eight months, and then return for

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[1]Measures of duration will be analyzed in future work.

[2]The reader familiar with the design and content of SIPP and CPS can skip to Section III.

four months. It is in this sense that the CPS contains a longitudinal component. For more information on the monthly CPS, see *The Current Population Survey: Design and Methodology* (U.S. Bureau of the Census 1978).

Every March a special supplement of the CPS is administered with the purpose of collecting information about the labor force activity and sources and amounts of income during the previous calendar year. These data are used in measuring the work experience of the population, the distribution of income, and the proportion of the population living below the Federal government's poverty thresholds. Since these data are collected each March, but relate to the previous calendar year, the recall period (especially for work experience questions) extends from 3 to 15 months. For a general description of the March CPS see the *Current Population Reports*, P-60 Series (U.S. Bureau of the Census 1989a).

The next two sections present a more in depth description of the questions on which the labor force measures analyzed in the paper are based. Because in the analysis SIPP data will be compared first to data from the monthly CPS and then to data from the March CPS, we organize the discussion of labor force questions along these lines. Section A compares SIPP to the monthly CPS, while Section B compares SIPP and the March CPS.

SIPP and Monthly CPS Labor Force Questions

The sequence of questions on labor force status in the monthly CPS results in the classification of respondents into three mutually exclusive labor force states--employment, unemployment, or not in the labor force. Classification is on a priority basis, that is, employment during the week containing the 12th takes precedence over unemployment, and unemployment takes precedence over not being in the labor force. Respondent are first asked about what they were doing "...most of last week..." (the week containing the 12th). If the reply is working, they are then asked a series of questions about the number of hours they worked and if they lost any hours due to illness, holidays, or slack work. If the reply is something other than working, additional questions attempt to find out if the person was not working because of a temporary absence from a job for personal reasons or on layoff. If they were absent because of personal reasons, they would be counted as employed. If they replied they were laid off then they would be counted as unemployed.

For those who did not work in the reference week nor were temporarily absent from a job or on layoff, questions are then asked if the respondent had been looking for work at any time during the past four weeks. If the answer is yes, a series of questions are asked about the job search and their availability for work. A negative response to the job seeking question triggers additional questions about past jobs and job seeking intentions.

The SIPP sequence of labor force questions is quite different than the CPS in that they do not funnel respondents into three mutually exclusive categories. Indeed, during the SIPP four month reference period, respondents could have been in all three states. Respondents are first asked whether they had a job or business at any point during the preceding four months. A *negative* answer triggers a question on whether they spent any time looking for work or on layoff during the preceding 18 weeks, and, if so, in which weeks. A *positive* answer to the first question on *any* job or business initiates a more complex series of questions. Respondents are first asked whether they worked for the entire four-month period (*always* worked question). If the answer is *no*, they are asked which weeks were spent looking for work or on layoff among the weeks they did not have a job or business. Finally, respondents are asked whether in any of the weeks with a job or business they were absent without pay. If the answer is positive, they are asked to identify exactly such weeks and to report the reason for the absence (layoff being one of the possible answers).[3]

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[3]Later in the questionnaire, persons reporting a wage and salary job are asked whether they worked at this job for the entire four-month period. If they did (continued...)

The responses to the SIPP labor force questions can be used to classify respondents in *each* week of the reference period into the three mutually exclusive CPS-type labor force states, employed, unemployed, or not in the labor force. Moreover, by linking successive interviews, "strings" of weekly labor force data can be constructed for a period of over two years.

We want to stress the following three major differences between SIPP and monthly CPS labor force questions (among others):

- In SIPP questions refer retrospectively to a relatively long period of time (18 weeks), rather than to the immediately preceding week--therefore the extent of recall is very different.
- The reporting of job search is not probed as it is in the CPS with questions about specific actions undertaken to find a job: only the question on availability for work is asked.
- CPS respondents are interviewed for at most four consecutive months before being dropped from the survey (either for eight months or permanently). Therefore, at most four consecutive monthly observations (with three transitions) are available for each individual. By contrast, SIPP produces strings of weekly observations that can span a two-year period.

SIPP and March CPS Labor Force Questions

The March CPS sequence of questions on the labor force experience in the previous year is similar to the SIPP questionnaire. Respondents are first asked whether they did work at any job or business during the previous calendar year. A negative answer triggers a question on the *number* of weeks spent looking for work or on layoff, while a positive answer triggers a similar question on the *number* of weeks in which the respondent worked (including paid vacation and sick leave). The similarity between the two questionnaires ends at this point, because the CPS takes a much shorter route. If the CPS respondents report 50 or 51 weeks of employment, they are asked whether they lost any full week of work because of layoff or job loss. If they report less than 50 weeks of employment, they are asked how many of the *remaining* weeks were they looking for work or on layoff from a job. As was mentioned, in SIPP an explicit accounting of labor force activity is made for *each* week in the reference period and a calendar is used as an aid in this process. In the March CPS, an attempt is made to account for *all* weeks in the reference period.

The methodological implications of survey design and questionnaire differences for CPS and SIPP experience measures are fairly evident. The cognitive task imposed on a respondent with an unstable work history is greater when confronted with the March CPS questions than with the SIPP questions. To answer questions about weeks worked and weeks unemployed, a CPS respondent must recall events that have occurred anywhere from 3 to 15 months earlier and then possibly perform a number of arithmetic operations. In SIPP the recall period is much shorter and the respondent is not burdened with any possible calculations.

III. MEASURES OF LABOR FORCE STOCKS FROM THE SIPP AND THE MONTHLY CPS

In the previous section we discussed the SIPP and monthly CPS labor force *questions* and concluded that there are many and important differences, pertaining both to the wording and structure of the questions and to the length

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[3](...continued)
not, they are asked to report the exact start and end dates for the job. This information can be converted into counts of weeks employed and weeks not employed. We analyzed this type of information and found that it replicates very closely the weeks with a job reported in the part of the questionnaire described in the text.

of the recall. In this Section we compare the monthly *estimates* of employment and unemployment for 1986 based on those questions.

Before we discuss these estimates in detail, we need to describe how the "strings" of weekly labor force data collected by SIPP are converted into monthly data that can be compared to the CPS data. We "superimpose" on the SIPP weekly strings a CPS-like data collection plan. More specifically:

- Respondents are classified as employed during a given month if they report being employed during the week containing the 12th of that month. (We do not utilize the information collected by SIPP regarding the remaining three or four weeks in each month, since it would not be collected by the monthly CPS.)

- Respondents are classified as unemployed if they are not employed during the week containing the 12th, but report being on layoff or looking for work during the week of the 12th or *any* of the preceding three weeks. Moreover, in order to be classified as unemployed, they must declare being available to take a job.

- The individual is residually classified as not in the labor force if none of the above situations applies.

We believe that, at least conceptually, this is the best way to treat SIPP labor force information in order to derive estimates comparable to the CPS.[4] However, we do not claim that the SIPP estimates obtained with this method are in any way *equivalent* to CPS estimates. We examine by how much they differ and offer some speculation on why differences exists.

Figure 1 offers a first glance at how SIPP-based estimates of the monthly *employment and unemployment rates* reproduce CPS estimates (not seasonally adjusted). The upper part of the chart compares the civilian SIPP and CPS employment rates (values on the left vertical axis), while the bottom part compares the two unemployment rates (values on the right vertical axis).

SIPP and CPS employment rates are on average less than one-half of a percentage point apart, and the difference is *not* statistically significant for any of the 12 months (at the 90 percent confidence level). Despite the lack of statistical significance of the monthly differences, it remains useful to compare the overall *time profile* of the two series. Two facts are easily noticeable from Figure 1: (i) with the exception of the three summer months, the SIPP point estimates always lie *at or above* the CPS estimates; (ii) the time profile of the SIPP estimates seems "smoother" than that produced by the CPS--in particular, the SIPP seems to be missing part of the large employment increase taking place during the summer months.

The picture is somewhat different when unemployment rates are considered. The estimates of the unemployment rates are significantly different in three of the thirteen months, while the SIPP point estimates always lie above the CPS estimates, with an average difference of one-half of a percentage point. The difference between SIPP and CPS unemployment estimates goes in the expected direction, since in SIPP no questions are asked about specific actions undertaken to find a job.

Besides producing a benchmarking of the SIPP estimates, these comparisons have some implications for the broader issue of the quality of monthly estimates obtained from surveys, such as SIPP, that collect monthly data with less than a monthly frequency. On the data quality side, there are two main problems:

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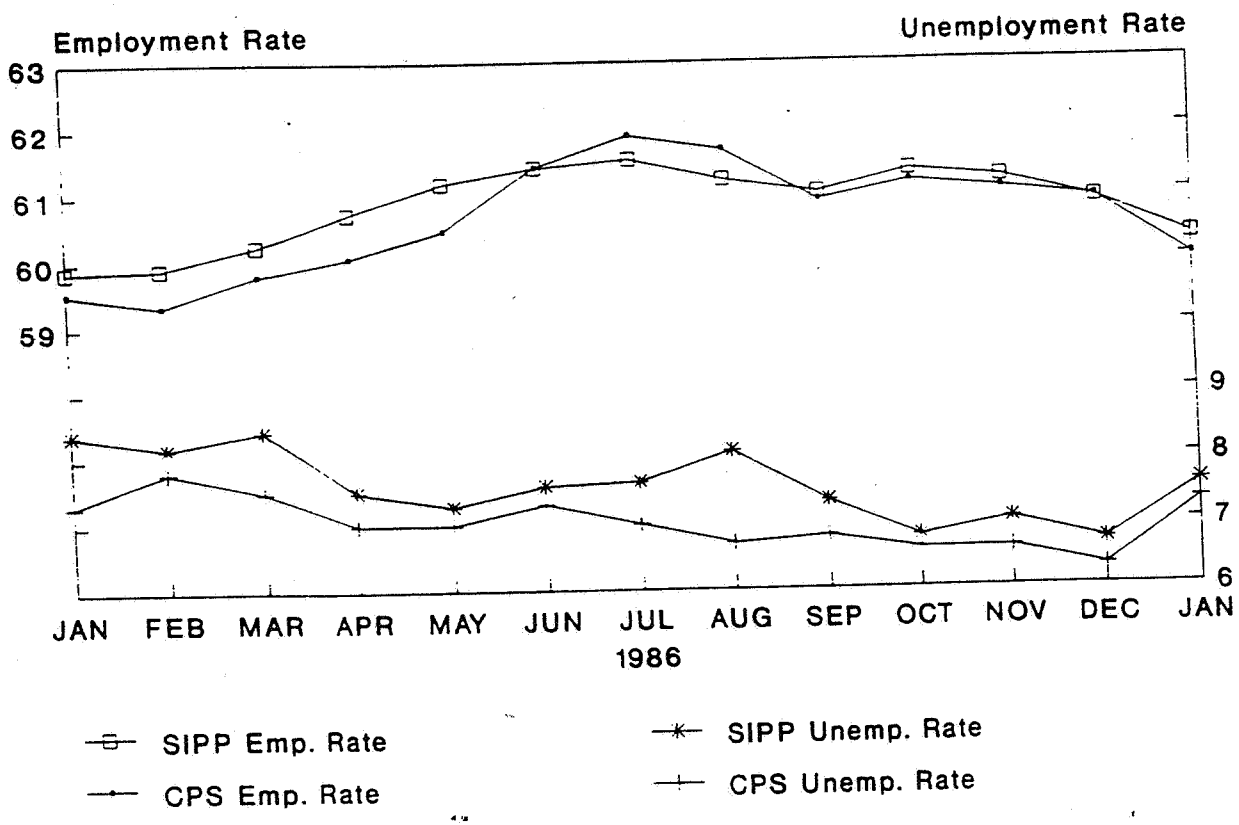
[4] It should be stressed that this in not the only way to use the SIPP labor force questions to recode monthly labor force status. SIPP contains enough information to support a variety of alternative classifications. We chose the method that is more comparable, at least in principle, to the CPS.

- The length of recall between the interview and the month for which the estimates are made is not equal for all months: the information concerning some months is more "recent" than that concerning other months.

- The reporting about more distant months might be *contaminated* by the reporting about more recent months: at the extreme, the labor force status during the most recent month could be mechanically reported for all the months in the reference period.

In the case of SIPP, because of its staggered sample design, the estimate obtained for each calendar month is an average of four monthly estimates, produced with one, two, three, and four months of recall, respectively. If SIPP respondents tend to report their current (or most recent) labor force status for all four months, the effect of the staggered design should be similar to that of a (non-centered) moving average. This hypothesis is broadly consistent with the time profile of the SIPP employment rate in Figure 1: SIPP seems to dampen the seasonal variation in the CPS series. This smoothing effect does not apply to the SIPP unemployment pattern.

FIGURE 1
SIPP and CPS Estimates of
Employment and Unemployment Rates



IV. MEASURES OF GROSS CHANGE: MONTHLY FLOWS

Both SIPP and the CPS allow the construction of monthly labor force gross flows--that is, estimates of the number of persons changing labor force status between two calendar months. As one would expect, the differences between the two surveys generate large differences between the size of these flows--larger than those observed between the stocks.

SIPP is still largely unexplored as a source of labor force gross flows estimates, and not much is known about the reliability of these estimates. Early analyses of SIPP gross flows include Ryscavage and Feldman-Harkins (1988) and Martini (1989). By contrast, attempts to construct gross flows from the monthly CPS have been made for several decades. Hogue (1985) reviews the history of the problems encountered in estimating CPS gross flows. Although not explicitly designed as a longitudinal survey, the monthly CPS allows the construction of gross flows because of its rotating sample design, which implies that three quarters of the sample are interviewed in two consecutive months. However, serious doubts among researchers about the quality of the gross flow estimates have hindered their use, and kept the Bureau of Labor Statistics (BLS) from publishing the gross flows estimates for many years. In 1984, a special Conference on Gross Flows Estimation was called to discuss and to provide solutions to these problems (the findings of the conference are summarized in Hogue and Flaim 1986.) Most contributions presented at the conference agreed on the fact that there are two broad classes of problems affecting the CPS gross flows: *missing observations* and *classification errors*. In the following two sections we discuss how these two types of problems arise in the context of both SIPP and CPS gross flows estimates.

The Impact of Missing Observations on the Estimates of Gross Flow

This problem can be simply stated: to compute the flows, only observations common to two consecutive months can be used. For a number of reasons, some sample members present in a given month are no longer present the following month, and they must be excluded from the computation of the flows. If these missing observations are not missing at random, then the observed gross flows could be biased estimates of the true gross flows. This problem is far from unique to the CPS--it is inherent to all panel surveys[5]--but because of its design, in the CPS the problem assumes a particular connotation.

The total attrition between any pair of monthly CPS's can be broken down into three components: (i) attrition due to the two *outgoing rotation groups* (a quarter of the sample each month)--a characteristic specific to the CPS survey design; (ii) attrition due to *residential mobility*--which is largely avoided by surveys that follow movers; and (iii) attrition due to *refusals* or to *persons leaving the sample* for other reasons (e.g., death, joining the Armed Forces)--a problem common to all panel surveys.

The most visible consequence of the problem of missing observations is that the flow estimates are not consistent with the monthly stock estimates based on the *entire* CPS sample. More precisely, the *net change* between the monthly stock estimates of a given labor force state does not match the *net inflow* into that state--that is, the difference between gross inflow and gross outflow. The recognition of this inconsistency motivated the BLS to first discontinue the publication of the gross flows during the 1950s. Subsequent research (Hilaski 1968, Hogue and Flaim 1986) documented that *non-identicals*--that is, those persons who are not common to two subsequent interviews, excluding the non-matches due to the outgoing rotation groups--have a different labor force profile than *identicals*--persons with two consecutive interviews. On average, non-identicals are more likely to be unemployed, and also more likely to be in the labor force, than identicals.

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[5]Only surveys that measure change *exclusively* with retrospective questions are able to avoid this problem: however, they run into other problems of a different nature, discussed later in the paper.

SIPP is affected by the above problems, but to a lesser extent. With few exceptions, a sample of SIPP is carried to completion--that is, observations are not periodically dropped from the survey. SIPP attempts to follow persons who change address during the time period covered by the survey.[6] Although this attempt might fail in some cases, a large number of movers are indeed successfully reinterviewed after they move. However, attrition due to non-interviews still remains (because of refusals and persons leaving the non-institutional population), and it is substantial: over the course of the 1984 panel, about 30 percent of original sample members did not participate in all interviews (Jabine et al. 1990). If such attrition is non-random we would expect some of the problems observed with the CPS gross flows to be found with SIPP gross flows.

SIPP and CPS gross flows are presented in Figures 2a through 2d. Each graph presents four quantities: gross inflows, gross outflows, net inflows (the difference between inflows and outflows), and net change between the monthly stocks. The first two graphs refer to employment estimates, the latter two to unemployment estimates.

Figure 2a depicts *employment* flows in 1986 based on the CPS. The size of the flows is striking: every month between 4 and 6 million persons enter, and between 4 and 7 million leave employment. The net inflow is much smaller, between 1.0 million and minus 1.5 million. What is more important, the net inflow is always smaller than the net change between monthly stocks, sometimes by almost 1 million. If we cumulate over a year the net changes, we obtain an *increase* in employment of 2.1 million, while when we cumulate the net inflows we obtain a *decrease* in employment of 2 million. In other words, while employment, as measured by the entire CPS sample, grew from January 1986 and January 1987, according to the flow estimates it shrunk by about the same amount.

Figure 2b depicts *employment* flows in 1986 based on the SIPP. The size of the gross flows is significantly smaller than in CPS: inflows into and outflows from employment are always between 2.5 and 4.5 million. Moreover, net inflows and net changes are substantially more in agreement in SIPP than they were in the CPS. Their difference never exceeds 0.4 million; the cumulative net change between January 1986 and January 1987 shows an increase in employment of about 1.5 million, while the net inflows imply a smaller (but still positive) increase of 0.4 million.

Figures 2c and 2d replicate this analysis for unemployment, and present analogous findings. CPS unemployment flows are significantly larger than their SIPP counterparts. The discrepancy between net change in stocks and net inflows is almost nil in the SIPP (the cumulative net change is negative 1 million, the cumulative net inflow is negative .9 million). By contrast, this discrepancy is still substantial in the CPS (positive cumulative net change of .1 million, versus a negative cumulative net inflow of .9 million).

One possible explanation for the relatively better performance of SIPP in aligning net changes and net inflows is the fact that SIPP follows movers. We can explore this hypothesis by looking at the behavior of movers and non-movers in SIPP (this analysis can be conducted with SIPP alone because in the CPS movers cannot be separately identified from the other non-interviews.) Figure 2.e replicates Figure 2.b (employment flows in SIPP), with the only exception that the sample is restricted to persons who do *not* change address between two

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[6]This rule does not apply uniformly to all SIPP respondents. Persons who are not original sample members (i.e., did not reside in a sample household at the time of the first interview) are not followed, unless they move together with an original sample member. Persons who move more than 100 miles from a SIPP Primary Sample Unit are interviewed by telephone.

FIGURE 2a
CPS Employment Gross Flows, Net Inflow
and Net Change Between Monthly Stocks

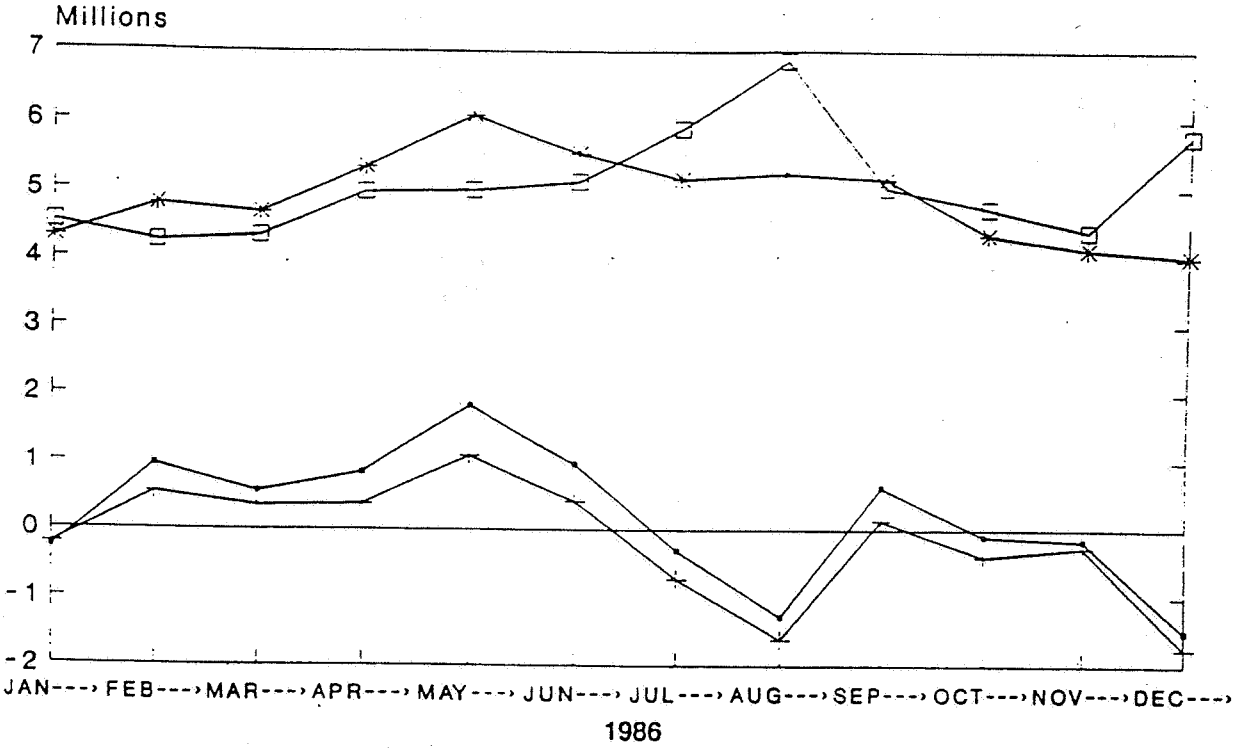

FIGURE 2b

SIPP Employment Gross Flows, New Inflow
and Net Change Between Monthly Stocks

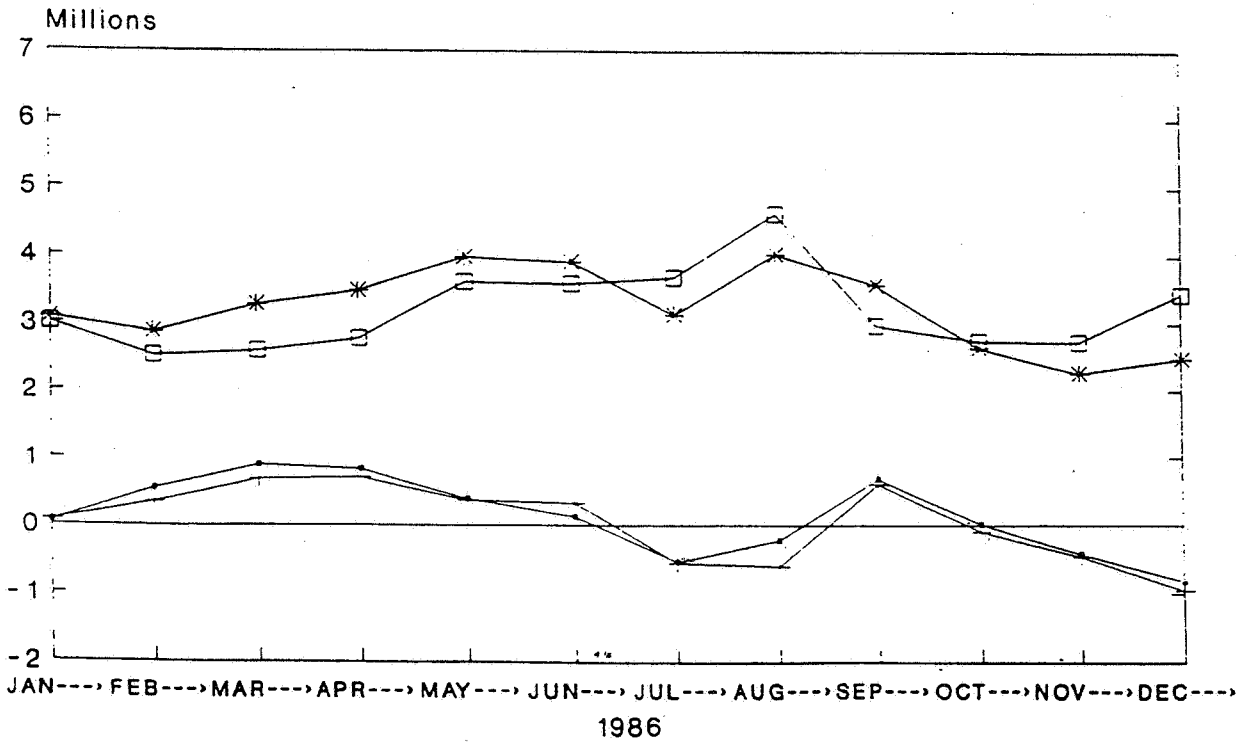

FIGURE 2c

CPS Unemployment Gross Flows, Net Inflow and Net Change Between Monthly Stocks

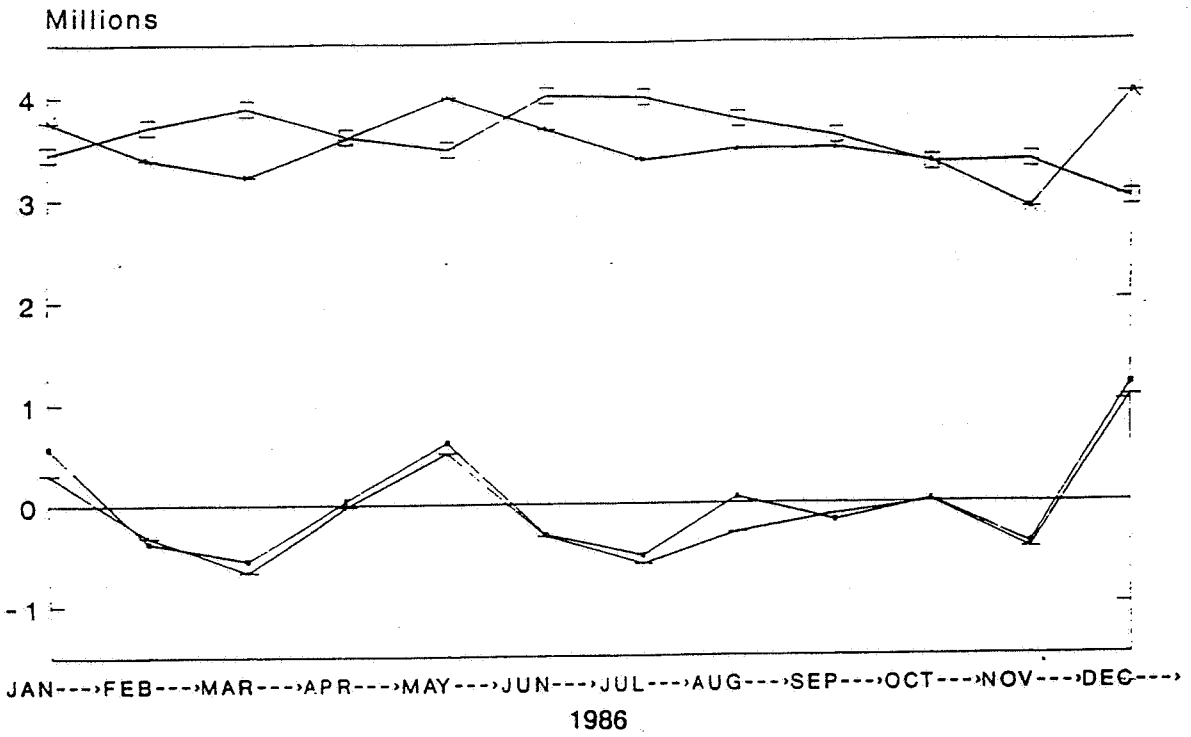

FIGURE 2d

SIPP Unemployment Gross Flows, New Inflow and Net Change Between Monthly Stocks

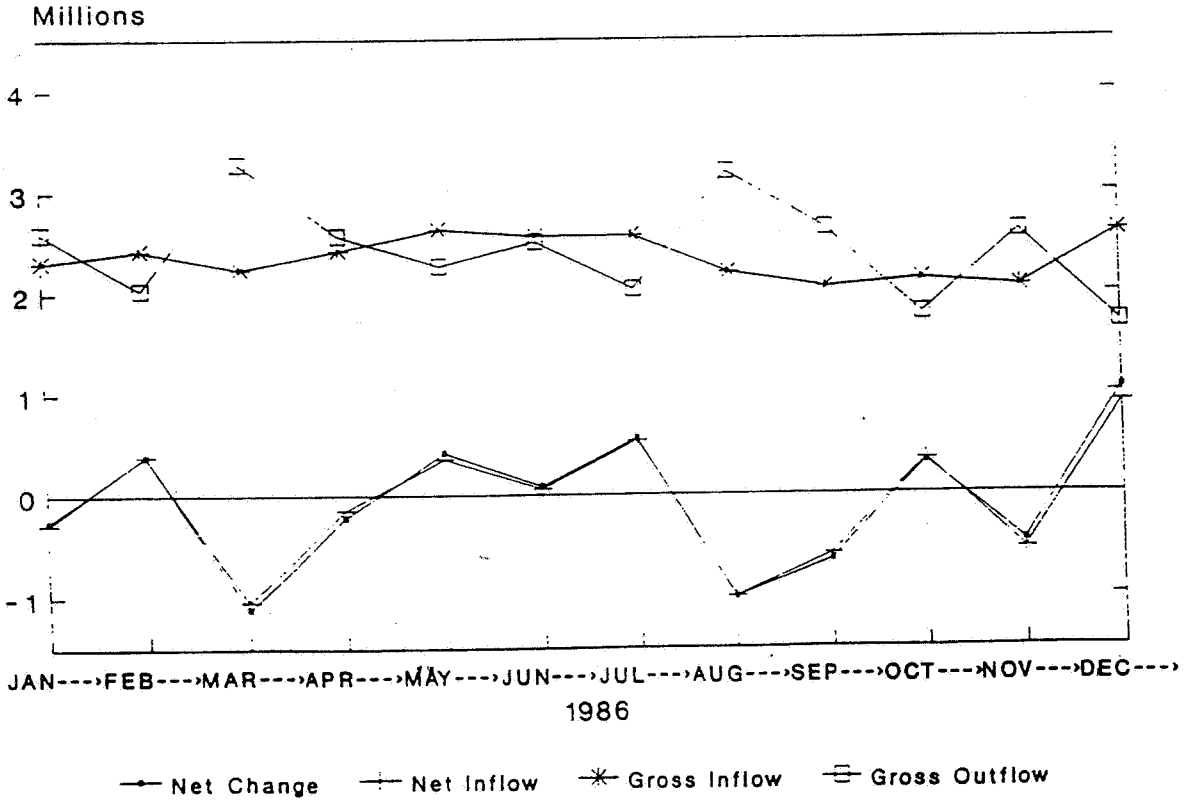

FIGURE 2e
SIPP Nonmovers - Employment Gross Flows, Net Inflow and Net Change Between Stocks

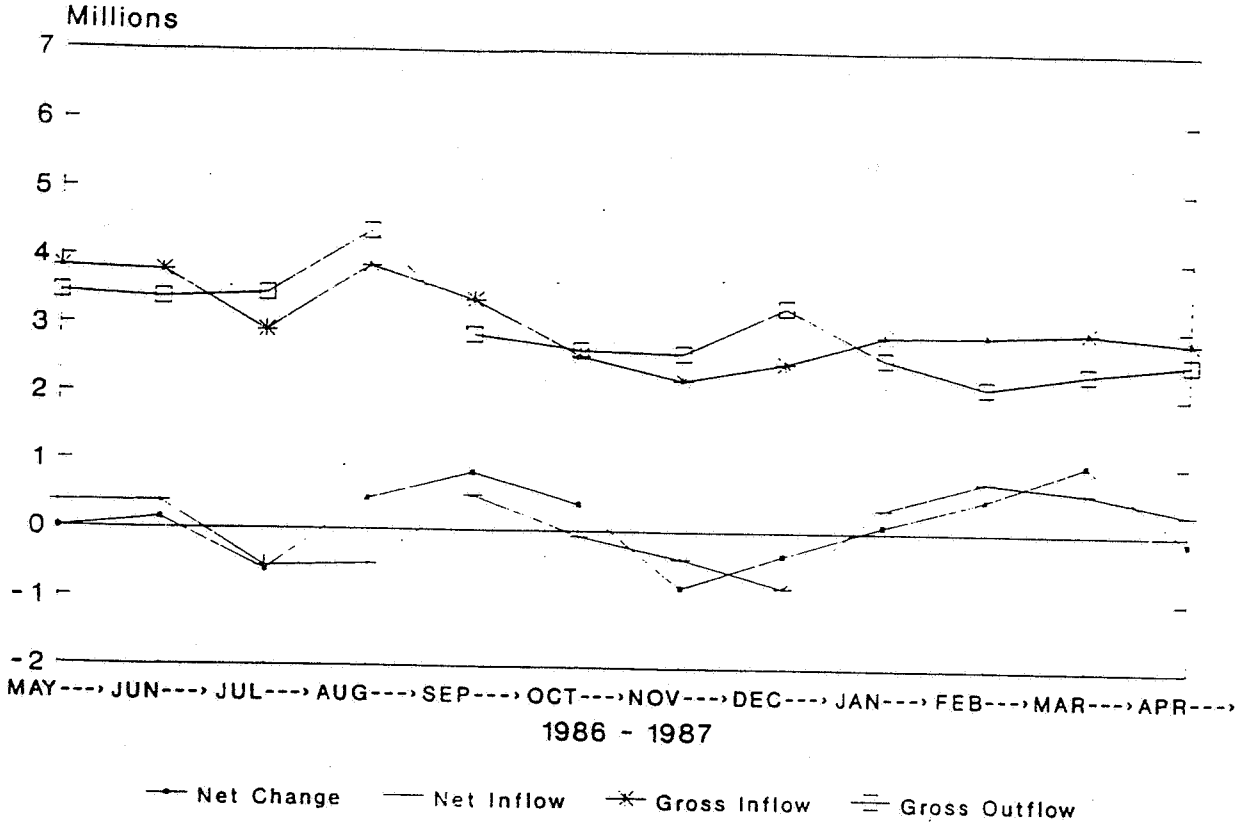

TABLE 1

EMPLOYMENT AND UNEMPLOYMENT RATES IN SIPP
ANNUAL AVERAGES
MAY 1986 - APRIL 1987

	Employment Rate	Unemployment Rate	Percentage of Total
All Persons	61.07	7.19	100.00
Non-movers	61.01	7.08	97.01
Movers	67.95	11.02	1.58
Non-identicals	57.92	10.00	1.41

SOURCE: SIPP 1986 Panel, persons 16 years of age or older.

NOTE: Non-identicals are persons in the sample in one month and no longer in the sample the following month, because of non-interview, death, or institutionalization.

adjacent months.[7] We immediately see that the exclusion of movers substantially worsen the "fit" between the net change and the net inflow profile, with respect with what we saw in Figure 2.b. This suggests that the exclusion of movers from the CPS gross flows is indeed partially responsible for the inconsistency between stocks and flows.

Table 1 provides a different perspective on this issue by comparing annual average employment and unemployment rates for SIPP movers, non-movers, and non-interviews. The comparison of the second and third row in the table (movers and non-movers) shows that movers (in the month *before* they move) have on average both higher employment and higher unemployment rates than non-movers (the differences are statistically significant at the 90 percent confidence level). The fourth row in the table shows rates for non-identicals (averages across the months immediately before these individuals drop from the survey). Non-identicals exhibit a rate of unemployment similar to that of movers, but a significantly lower employment rate.

These findings add to the evidence produced by Hogue and Flaim (1986), who showed that CPS non-identicals (movers plus refusals) have in the month before attrition much higher unemployment than identicals (between 2 and 3 percentage points, on average, from 1978 to 1980). They also showed that non-identicals have only marginally higher employment than identicals, which is consistent with our finding of a very different employment rate between movers and non-interviews in SIPP (the weighted average of movers and non-interviews employment rate is 63.2 percent, compared to a 61.0 percent for non-movers).

Overall, this evidence shows that movers are particularly different in terms of labor force behavior than the rest of the population. It should be emphasized that this does not imply a causal link between residential mobility and labor force status. Part of the difference in labor force status between movers and non-movers could be due to different demographic characteristics. Movers are certainly not a random sample of the population: for example, U.S. Bureau of the Census (1989b) shows that movers tend to be disproportionally represented among persons 20 to 29 years of age.

The data from SIPP can be used to compare the degree of labor force mobility among movers and non-movers. Table 2 presents transition rates separately for the two groups. These transition rates are annual averages (from May 1986 to May 1987) of monthly transition rates--computed by dividing the number of persons making the *ij* transition between month *t* and month *t+1* by the number of persons in state *i* in month *t*.[8] We use transition rates rather than gross flows because the former are more easily comparable across different type of transitions.

When tested one at a time, none of the differences between mover and non-mover transition rates is statistically significant. However, the fact that movers have rates that are two to three times larger than non-movers in five of the six transitions between labor force states should not be overlooked. Such systematic patterns go in the expected direction, and should be taken at least as some evidence that higher than average labor force mobility is associated with residential mobility. At the same time, movers have a very small impact on the *overall* transition rates, because they represent only about 1.6 percent of the population present in any given month (third column in Table 1).

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[7]One caveat applies: SIPP does not obtain information on residential mobility that takes place during the first wave. Therefore any analysis of residential mobility with the 1986 SIPP can only be conducted starting from the month of May of 1986. This is explicitly indicated by the labels in Figure 2.e and Table 1.

[8]Only observations in common to month *t* and month *t+1* are used.

TABLE 2

LABOR FORCE TRANSITION RATES
FOR MOVERS AND NON-MOVERS
ANNUAL AVERAGES
MAY 1986 - MAY 1987

	Type of Transition								
	EE	EU	EN	UE	UU	UN	NE	NU	NN
All Persons	97.03	1.26	1.72	17.78	72.19	10.03	2.78	1.68	95.53
Movers	92.97	3.28	3.75	26.90	66.07	7.03	6.75	5.26	87.99
Non-movers	97.19	1.16	1.64	17.99	71.96	10.04	2.68	1.50	95.82

SOURCE: SIPP 1986 Panel, persons 16 years of age or older

NOTE: E = Employed; U = Unemployed; N = Not in the Labor Force

In conclusion, the evidence from SIPP suggests that the exclusion of movers from the CPS contributes substantially to the mismatch between net inflows and changes between stocks. However, because movers comprise only a small fraction of the overall population, the impact of their exclusion on measures of turnover (such as transitions rates) is rather small. This is consistent with the findings of Abowd and Zellner (1985). The two authors adjust the CPS gross flows for both missing observations and for classification errors, and find that the adjustment for missing data changes the flows between 12 and 15 percent, while the adjustment for classification errors reduces the flows by nearly 50 percent in some cases. We turn now to the issue of classification error.

Classification Errors and CPS Gross Flows

Errors in classifying a sample member's true labor force state in any given month can arise for a number of reasons: incorrect interpretation of the question; faulty knowledge of the sample member's true situation in the case of proxy respondents; unwillingness to cooperate with the survey on the part of the respondent; errors in coding the answer or in matching individuals between two interviews. Evidence from the CPS Reinterview Program suggests that a sizeable fraction of individuals are misclassified every month. The largest incidence of misclassifications occurs between the unemployed and not in the labor force states (Fuller and Chua 1985), which is not surprising, given the possible ambiguities in defining unemployment.

The presence of classification errors at each interview tends to bias *upward* the estimated flows between labor force states only if the following conditions hold: (i) the diagonal elements of the true matrix of flows are larger than the off-diagonal elements--that is, there are more persons remaining in their labor force state than there are leaving; (ii) classification errors are *not* perfectly correlated over time--that is, the respondent does not make the same error repeatedly month after month. Both assumptions are likely to hold in the case of monthly labor force data. The great majority of persons tend to remain in their labor force state, certainly in the case of persons employed or not in the labor force. Regarding the second condition, we do not have much empirical evidence about the serial correlation of classification errors. However, if the assumption of perfectly uncorrelated errors is probably too extreme, the opposite assumption of perfectly correlated errors seems even less likely to hold.

The biasing effect of classification errors on CPS gross flows is just a special case of a more general problem that arises in the context of repeated surveys: the observed change between successive interviews is the sum of the *true change* and the *spurious change* induced by the survey process itself, and in general the two components cannot be separated.

A possible alternative to measuring change through repeated interviews is that of *measuring change retrospectively*. In this case, respondents are asked to report their labor force status over multiple time periods. The measures of gross change are computed by comparing responses given in the same interview with reference to distinct periods of time--rather than comparing responses given in separate interviews. Measures of change collected retrospectively are likely to suffer from a *downward bias*: rather than generating spurious change, retrospective questions might suppress true change. Respondents have an incentive to underreport transitions in order to shorten the interview; and transitions might simply be forgotten, or misplaced in time.

SIPP Gross Flows and the Seam Effect

SIPP data provides an example in which gross change is measured both retrospectively (change within a reference period) and with repeated interviews (change between reference periods). Previous research has widely documented the fact that in SIPP a disproportionate number of month-to-month transitions are reported at the seam between two interviews. This has been defined as the *seam effect*.[9]

Table 3 presents annual averages of labor force transition rates based on CPS and SIPP (first two rows).[10] Given the estimates of gross flows presented in the previous section, large differences between the SIPP and CPS transition rates should not come as a surprise. The CPS off-diagonal rates are between 32 percent (EU transitions) and 107 percent (UN transitions) larger than the corresponding SIPP rates. (All differences are statistically significant at the 90 percent confidence level).

As discussed in the previous section, the exclusion of movers from the CPS does not explain these differences. Movers seems to have higher transition rates and their inclusion in the CPS would further increase the differences with SIPP. Also, we do not believe that the presence of other types of non-response could explain a significant portion of those differences, since non-response occurs in both surveys. We believe that the reason for the SIPP-CPS differences is to be found in the combined effect of the upward bias induced by classification errors in the CPS and the downward bias induced by the retrospective reporting in SIPP. If this is true, SIPP and CPS transition rates could be seen as, respectively, lower and upper bounds on the true amount of turnover. Unfortunately, in several cases these bounds are too far apart to be analytically useful.

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[9]For evidence on the seam problem in the context of SIPP, see Marquis and Moore (1990) for transitions in the receipt of transfer program, Coder et al. (1988) for transitions in the receipt of various income sources, and Martini (1989) for labor force. In the context of the PSID, see Hill (1988).

[10]SIPP transition rates are annual averages from January 1986 to January 1987, while CPS rates are averages between December 1985 and December 1986. The discrepancy induced by the slightly different reference periods should contribute only very marginally to the total discrepancy between the two surveys. We were forced to choose the January-January period for SIPP because data for December 1985 are not available for one rotation group. On the other hand, annual averages for the CPS gross flows are available only for the December-December period.

TABLE 3

CPS AND SIPP LABOR FORCE TRANSITION RATES
ANNUAL AVERAGES
JANUARY 1986 - JANUARY 1987

	Type of Transition								
	EE	EU	EN	UE	UU	UN	NE	NU	NN
				Overall Transition Rates:					
CPS	95.35	1.67	2.97	25.52	53.71	20.75	4.61	2.69	92.73
SIPP	97.03	1.26	1.72	17.78	72.19	10.03	2.78	1.68	95.53
				SIPP Within-Wave Transitions:					
Mo.1-Mo.2	98.22	.95	.83	13.03	79.13	7.84	1.55	.91	97.54
Mo.2-Mo.3	97.84	1.00	1.15	15.45	79.87	4.68	1.81	1.18	97.00
Mo.3-Mo.4	97.89	1.01	1.11	16.66	78.18	5.16	1.95	1.15	96.90
				SIPP Between-Wave Transitions:					
Mo.4-Mo.1	94.02	2.10	3.88	26.90	50.05	23.05	6.02	3.62	90.36

SOURCE: SIPP 1986 Panel and tabulations of CPS gross flows produced by BLS, persons 16 years of age or older

NOTE: E = Employed; U = Unemployed; N = Not in the Labor Force

Further insights on the tradeoff between repeated and retrospective measures of gross change can be gained by examining SIPP transition rates disaggregated by whether the transitions occur at the seam between two reference periods (hereafter, for simplicity, "between-wave" transitions), or within the reference period ("within-wave" transitions). In the latter case, rates are further disaggregated on the basis of the length of time between the transition and the interview (bottom part of Table 3). For example, month 1 to month 2 transitions are those occurring between the first two months of the reference period.

The off-diagonal SIPP between-wave rates are several times larger than within-wave rates, and also larger than CPS rates (however, in the case of exits from unemployment, the difference between SIPP between-wave and CPS rates is not statistically significant). Moreover, most SIPP within-wave transition rates tend to increase as they get closer to the time of the interview: this trend is stronger in the case of transitions *into* employment (both UE and NE). The latter result might have two very different explanations: (i) the start of employment spells is systematically "telescoped" forward closer to the time of the interview; (ii) short employment spells occurring at the beginning of the reference period are more likely to be forgotten than those occurring closer to the interview.

This discussion highlights an important fact: because of the complexity of the SIPP design, there is more than one type of measurement error process that might bias the transition rates. We find it useful to distinguish between *three types of measurement error*. To do this, we must think in terms of the underlying labor force spells (measured in weeks). In fact, when asked about

their labor force activities during the preceding 13 weeks, SIPP respondents are (implicitly) asked to recall all the labor force spells that took place during that period. Some respondents are able to recall and willing to report without error all spells and all the information needed to classify them correctly, while others might report their labor force experience incorrectly. Conceptually, we distinguish between the following types of error: (1) *failures to report* short spells--namely spells that are completely contained within the reference period; (2) errors in reporting the *timing* of the spells--that is, their position within the reference period; and (3) errors in reporting some relevant facts, with the consequence that some spells are classified in the *wrong labor force state*. We discuss each type of error separately, focussing on whether the error biases the estimates of turnover and whether it contributes to the seam effect.

- *Omitted spells*. Some spells are forgotten or simply go unreported. The omission of spells unequivocally *biases downward* the estimates of turnover, since for every omitted spell two transitions are suppressed. However, the omission of spells by itself causes *no seam effect*, as long as the spells are completely contained within the reference period. The prevalence of these omissions might decrease with the duration of the spell and increase with the length of the recall period. The patterns observed in Table 3 among SIPP within-wave transitions are consistent with the latter hypothesis.

- *Errors in the timing of the spell*. Some respondents might report all spells, but they might incorrectly report their "position" in the reference period. This type of error does *not* reduce the amount of turnover, since the number of transitions is reported correctly. These timing errors contribute to the seam problem only when the onset of the spell, which truly began *during* the reference period, is "pushed back" to the beginning of the period (backward telescoping). In this case the transition is recorded at the seam between two waves.

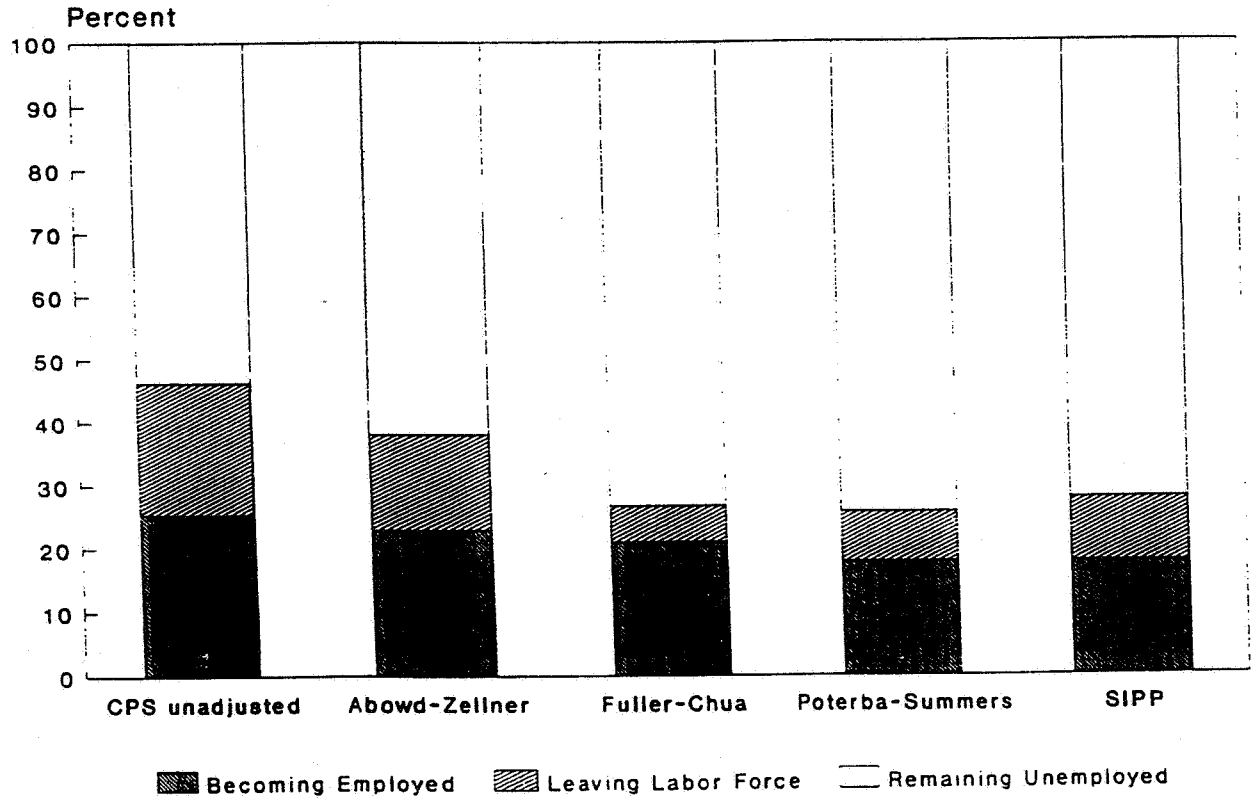
- *Errors in the classification of the spell*. This occurs when the respondent provides erroneous information that leads to the misclassification of a spell--for example, a spell of job search is classified as non-participation, or a period on layoff is reported as a period of unpaid vacation. For each misclassified spell, a pair of transitions are recorded incorrectly. It is important to distinguish two different situations: (i) when the "true" spell is completely contained in the reference period, no seam effect occurs because of the misclassification, and the misclassified transitions might tend to cancel out the same way errors of classification tend to cancel out in the estimates of labor force stocks; (ii) when the "true" spell spans two (or more) reference periods and only the portion in one reference period is misclassified, the consequences are more serious: the misclassification contributes to the seam effect and also artificially increases the amount of turnover, since two spurious transitions are created.

The bottom line of this discussion is that "seam effect" and "biased transition rates" are by no means synonymous. A dramatic seam effect can coexist with unbiased average transitions, as long as the only error process taking place is of the second type--errors on the timing of transitions. The first type of error (omitted spells) and third type of error (misclassification of portions of spells) bias the amount of turnover, although in opposite directions. The overall SIPP transitions rates presented in the Table 3 are therefore unbiased *only if* type 1 and type 3 errors do not take place (which seems very unlikely), or if they tend to cancel each other out--that is, for every spell that is lost

because of omission another of the same type is gained because of misclassification.

Unfortunately, we are unable to present any convincing evidence that type 1 and type 3 errors offset each other. What we can do is to compare transition rates based on SIPP to CPS transitions rates corrected with the methods proposed by participants at the 1984 Census-BLS Conference on Gross Flows in Labor Force Statistics.[11] We focus on transitions out of unemployment. Figure 3 reports unadjusted CPS transition rates, adjusted CPS rates based on the method proposed by Abowd and Zellner, Fuller and Chua, and Poterba and Summers (See Hogue and Flaim (1986) for details on these methods), and SIPP transition rates (from the second row in Table 3). SIPP unemployment to employment rate is very similar to the rate obtained by Poterba and Summers, whose method only corrects for classification errors using CPS Reinterview data. The SIPP unemployment to not in the labor force rate is between the rates obtained by the three methods. While these results do not constitute definitive evidence that SIPP transition rates are unbiased, they at least suggest that SIPP labor force data should be seriously considered as a source of gross change measures.

FIGURE 3
Flows Out of Unemployment:
CPS Unadjusted, CPS Adjusted, and SIPP



[11]We were unable to apply all the methods proposed at the conference. Instead, we applied the percentage reductions of the unemployment flows reported in Hogue and Flaim (1986) to 1986 CPS flows. We believe that for illustrative purposes this method is adequate.

V. MEASURES OF EXPERIENCE

The following discussion focuses on two experience measures--weeks worked and weeks unemployed during a calendar year. Just as survey design and questionnaire differences between the CPS and SIPP influence gross change measures, so they affect these experience measures. The methods by which the experience measures are derived reflect the overall design of the two surveys.

In the March CPS they are obtained from one retrospective interview covering the previous calendar year. In SIPP they are constructed from repeated observations on the same individual--each observation covering at most four calendar months. In a sense this is a reversal of the methods in which gross flow data from the CPS and SIPP are developed--that is, the CPS flows are based on repeated interviews while the SIPP flows are based on a mixture of retrospective and repeated interviews.

The obvious consequence is that the same tradeoffs between recall bias and biases associated with repeated interviews emerge in the context of the experience measures. But now recall bias is of more importance in the CPS and possible biases associated with attrition are more important to SIPP. In particular, in order to construct annual experience measures from SIPP, one must drop the observations with an incomplete set of interviews during the calendar year.[12] The weights of the sample members with complete interviews are inflated so that they compensate for attritors having similar characteristics. However, if such noninterview adjustment procedure fails to completely compensate for the attrition, the estimates of annual experience measures obtained from the complete sample could be biased.[13]

A comparison of weeks worked and weeks unemployed estimates from the CPS and SIPP provides an opportunity to investigate four specific questions.

(1) Do the survey design and questionnaire differences in the CPS and SIPP produce different estimates of weeks worked?

(2) Do the CPS questions about unemployment among part-year workers induce a significant distortion in the CPS distribution of weeks unemployed vis-a-vis the SIPP distribution?

(3) Does the longer recall period in CPS result in more short-term unemployment to be forgotten?

(4) What is the effect of attrition in SIPP on its estimates of very long-term unemployment?

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[12]There are other problems associated with the repeated interview process that are not examined in this paper. For example, information on the same sample member at successive interviews might be provided by different respondents, because of changes between self and proxy responses, or because of changes in the proxy respondent. Moreover, repeated interviews might have a conditioning effect on the respondent, so that responses given at later interviews tend to differ from those provided in earlier interviews.

[13]One additional caveat applies to the use of longitudinal weights created by the Bureau of the Census to estimate experience measures. Persons who die or move to an ineligible address--such as an institution--during the course of the calendar year are given a positive longitudinal weight. However, we were forced to drop these observations from the analysis, because they do not have a complete set of interviews for the calendar year. As a consequence, the weighted population count we use falls short of the CPS population by about four million persons. Therefore, the comparisons between SIPP and CPS should be done in terms of rates rather than weighted counts.

Measures of Weeks Worked

Table 4 presents various descriptive statistics of persons who worked at some time in 1986 according to the CPS and SIPP. The CPS measure of weeks worked is based on the simple question, "During 1986 in how many weeks did ... work even for a few hours? Include paid vacation and sick leave as work."[14] Three alternative measures of weeks worked based on SIPP data are presented: weeks "paid", weeks "employed", and weeks "with a job". Weeks with a job are constructed from the answers--given in four successive interviews[15]--to the question "In which weeks did ... have a job or business?". Weeks employed equals weeks with a job minus the weeks specifically identified by the respondent as weeks absent from work *without pay because of layoff*. Weeks paid is constructed from weeks employed by further subtracting weeks absent from work without pay because of other reasons (illness, vacation, bad whether, or labor dispute).[16] From the wording of the CPS question, one might infer that weeks unpaid (for whatever reason) should be *excluded from the count of weeks worked*. If this is the case, "weeks paid" should be the SIPP measure most comparable to the CPS measure.

TABLE 4

SUMMARY STATISTICS ON WORK EXPERIENCE
ACCORDING TO SIPP AND CPS

	CPS	SIPP		
	Week Worked	Weeks Paid	Weeks Employed	Weeks With Job
Ever Worked (,000)	126,723	127,106	127,226	127,232
Percent of Population	68.90	71.30	71.36	71.37
Worked 52 Weeks (,000)	80,798	75,358	85,020	86,942
Percent of Ever Worked	63.7	59.28	66.82	68.33
Worked 50+ weeks (,000)	84,937	82,025	87,857	89,038
Percent of Ever Worked	67.0	64.53	69.05	69.98
Average Weeks Worked:				
Among Those Who Ever Worked	43.90	43.86	44.48	44.61
Among Those Who Worked Less Than 52 Weeks	29.25	32.00	29.33	28.68

SOURCE: SIPP 1986 Panel and 1987 March CPS, population 16 years of age or older

[14]In 1986, this question was asked only of persons 16 years of age or over at the time of the March interview who were not currently members of the Armed Forces.

[15]For one of the SIPP rotation groups, the 1986 calendar year is covered by exactly three interviews, while for the other rotations groups four rounds of interviews are needed.

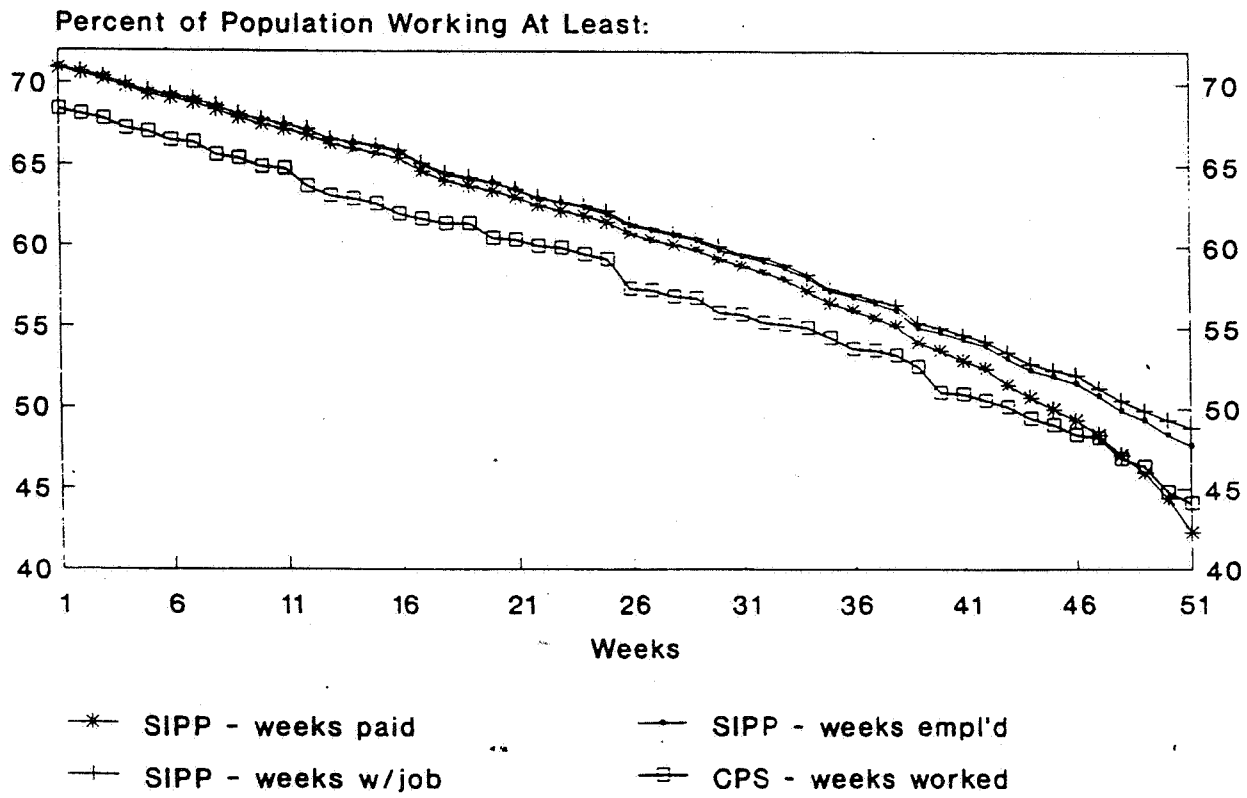
[16]A fourth measure of weeks worked was created from SIPP, using the start and end dates of jobs reported in the Wage and Salary Section of the questionnaire. The estimates based on this measure were always almost identical to those based on the weeks with job measure. This indicates that when employment is measured by asking respondents about dates of jobs, periods of unpaid leave or temporary layoff go completely unreported.

Table 4 begins by comparing counts of persons who had *any employment* during 1986. The SIPP places this number at around 127 million persons, or about 71.3 percent of the population. The differences among the three SIPP definitions are negligible. The CPS estimate is 126.7 million, or 68.9 percent of the population, which is 2.4 percentage points below the SIPP rate. The differences between the CPS and SIPP rates are statistically significant.[17] The longer recall period in the CPS might account for this lower percentage of persons with any employment during the year.

The picture changes completely when estimates of *year-round work* are compared. Much larger differences emerge, both among SIPP measures and between SIPP and CPS. Using a definition of year round work of 52 weeks finds the SIPP estimates ranging from about 75.4 million for weeks paid to 86.9 million weeks with a job. The CPS estimate of 80.8 million persons falls between the weeks paid and weeks employed definitions in SIPP, and the corresponding rate, 63.7 percent (computed as percentage of all persons who ever worked) is statistically different from the corresponding SIPP rates, which are 59.3 and 66.8, respectively. Previous research has shown that full-time, year-round employment (35 hours or more a week for 50 or more weeks a year) as measured in SIPP is lower than comparable CPS estimates (Ryscavage and Coder 1989), and the weeks paid estimate confirms that finding.

The complete distribution of weeks worked in CPS and SIPP can be seen in Figure 4. The figure shows the *percentage* of the population age 16 and over working *at least* the number of weeks indicated on the horizontal axis. In other words, the intercept on the left vertical axis represents the proportion of the population with at least one week of employment, while the intercept on the right vertical axis represents the proportion of the population working 52 weeks.

FIGURE 4
Distribution of Weeks Worked
According to SIPP and CPS



[17]The weighted counts of persons employed are not strictly comparable.

Apart from the sharp difference at the right tail between the SIPP measures, and the fact that the CPS distribution is uniformly about 2 percentage points lower, all of the measures have a similar distribution. This is reflected in the small and statistically insignificant differences between the four estimates of average weeks worked, reported at the bottom of Table 4. These means, of course, are dominated by those persons working 52 weeks a year. But even when the average weeks worked are calculated only for persons with less than 52 weeks of work (last row), the estimates fall within a narrow range of 29 to 32 weeks.[18]

The findings presented in this section have two broad implications for studies that use measures of weeks worked. For studies of labor supply or labor force participation, it may not make much difference as to which SIPP experience measure of weeks worked is used, or whether the CPS measure is used, because the overall distributions are fairly similar. On the other hand, for analyses focusing on year-round workers--such as studies on earnings inequality--the choice of the CPS or SIPP work experience measure may be critical, since the estimates diverge by 11 million persons (when a less stringent version of year-round work is used, 50 weeks or more, the estimates still diverge by as much as 7 million persons.)

Measures of Weeks Unemployed

Previous research has shown that considerably more persons are found unemployed during a calendar year in SIPP than is the case with CPS (Ryscavage and Martini 1990). In addition, it has been shown that the average weeks of unemployment experienced by individuals is considerably shorter when measured in SIPP than in the CPS. In this section, we further explore the reasons for these differences, with particular emphasis on the possible distorting effects of the March CPS question's on the distribution of weeks unemployed.

Table 5 shows the summary statistics relating to weeks unemployed as measured in SIPP and CPS for 1986. SIPP's estimate is almost 50 percent higher than CPS's--30.1 million versus 20.7 million. Almost 23 percent of persons ever in the labor force experience some unemployment according to SIPP, about 16 percent according to the CPS.

TABLE 5

SUMMARY STATISTICS ON UNEMPLOYMENT EXPERIENCE ACCORDING TO SIPP AND CPS

	CPS	SIPP
Ever Unemployed (,000)	20,704	30,084
Percent of Population	11.2	16.9
Percent of Ever in Labor Force	16.2	22.6
Unemployed and Worked (,000)	18,324	23,959
Unemployed, Never Worked (,000)	2,380	6,125
Average Weeks Unemployed	17.73	13.83

SOURCE: SIPP 1986 Panel and 1987 March CPS, population 16 years of age or older

[18]Only the SIPP weeks paid and CPS measure differ significantly from one another.

In Table 5, we also present a simple categorization of unemployed persons by their employment experience--those who worked and were unemployed and those who never worked but were unemployed at some point during the year. In both categories, SIPP counts considerably more unemployed persons than in the CPS. However, for persons with some employment the SIPP estimate is 32 percent higher than the CPS, while for persons who never worked the SIPP estimate is 158 percent higher. And last, the average weeks of unemployment according to SIPP was only 13.8 weeks as compared to 17.7 weeks in the CPS, a statistically significant difference.

In Table 6 the CPS and SIPP estimates of unemployment are decomposed on a more refined employment-unemployment experience basis. Persons who were both employed and unemployed are split into two groups: those whose weeks of employment and unemployment add up to 52 weeks and those whose weeks do not. The differences here are even more startling. According to the CPS, close to 2 out of every 3 persons with some unemployment in 1986 were *full year participants* in the work force. In contrast, the comparable estimate from SIPP is slightly less than 1 out of 3. Thus, the two surveys present very different profiles of the labor force commitment of the unemployed.

TABLE 6

DECOMPOSING UNEMPLOYMENT BY THE TYPE
OF EMPLOYMENT/UNEMPLOYMENT EXPERIENCE

	Percent of Ever Unemployed		Average Weeks Unemployed	
	CPS	SIPP	CPS	SIPP
Ever Unemployed (,000)	20,704	30,084	17.7	13.8
	100.0	100.0		
In Labor Force 52 Weeks	62.3	31.8	19.7	13.5
In Labor Force < 52 Weeks	26.2	47.9	9.0	11.3
Never Employed	11.5	20.3	24.9	20.2

SOURCE: SIPP 1986 Panel and 1987 March CPS, population 16 years of age or older

One likely cause for these dramatic differences may be found in the March CPS questionnaire. For persons who reported working less than 50 weeks in the year, an additional question is asked about how many of the *remaining* weeks of the year were spent looking for work or on layoff. In other words, the CPS interviewer provides the respondent with an upper bound, that is, 52 weeks minus the weeks spent employed. How many respondents consciously begin the recall process and engage in the necessary computations is not known, but clearly the temptation is present to say "all of them." In SIPP, such a possible biasing affect is not present since respondents must identify the specific weeks in which they were looking for work or on layoff.

Additional evidence on this issue is on the right side of Table 6, which shows the average weeks of unemployment of persons in these employment-unemployment categories. CPS full-year labor force participants with some unemployment experienced an average of 19.7 weeks of unemployment in 1986, compared to 13.5 weeks for SIPP full-year participants, a large and statistically significant difference. One would indeed expect the average weeks unemployed for persons who erroneously report full-year attachment to the labor force to be biased upward. The combination of the high concentration of full-year participants

among the total unemployed in the CPS *and* their longer reported duration of unemployment has obviously a large impact on the overall average duration.

The Distribution of Weeks Unemployed

Recalling short spells of job search or layoff may be more difficult for a respondent if the reference period extends back three to fifteen months rather than four months. Table 7 shows the distribution of weeks unemployed by number of weeks. As can be seen, the estimate of very short periods of unemployment of 1 to 2 weeks is 111 percent higher in SIPP than in the CPS, and the estimate of 5 to 8 weeks of unemployment is 75 percent higher. When the first three intervals--unemployment periods ranging from 1 to 8 weeks--are combined, the SIPP estimate is approximately 70 percent higher than the CPS estimate (13.1 million vs. 7.7 million). These findings tend to confirm other research (Mathiowetz and Duncan 1988) which has shown that short spells of unemployment go largely unreported in retrospective surveys. Even the SIPP estimates of unemployment ranging from 9 to 39 weeks were significantly higher--from 35 to 50 percent higher--than in the CPS. But periods of unemployment of 40 weeks or more were significantly higher in CPS than in SIPP.

TABLE 7

DISTRIBUTION OF WEEKS UNEMPLOYED
ACCORDING TO SIPP AND CPS

	CPS		SIPP	
	Persons (,000)	Percent of Persons Ever Unemployed	Persons (,000)	Percent of Persons Ever Unemployed
Persons Ever Unemployed	20,703	100.0	30,084	100.0
1-2 weeks	2,286	11.0	4,754	15.8
3-4 weeks	2,727	13.2	3,491	11.6
5-8 weeks	2,736	13.2	4,776	15.9
9-13 weeks	3,562	17.2	4,777	15.9
14-26 weeks	5,110	24.7	7,676	25.5
27-39 weeks	2,164	10.5	3,161	10.5
40-51 weeks	2,117	10.2	891	3.0
52 weeks	712	3.4	555	1.8

SOURCE: SIPP 1986 Panel and 1987 March CPS, population 16 years of age or older

Table 8 shows the distributions of weeks unemployed disaggregated by the three employment-unemployment categories discussed earlier. In SIPP, persons with and without year-round labor force participation have very similar distributions of weeks unemployed (fourth and fifth columns). By contrast, the two corresponding CPS groups (first and second columns) have very different distributions, with the majority of year-round participants reporting more than 14 weeks of unemployment.

Among those never employed, longer unemployment is common place according to both surveys: 54 percent in the CPS and almost 60 percent in SIPP report more then 14 weeks of unemployment. This similarity is surprising given that the

two surveys produce a dramatically different count of the unemployed with no work experience: 6.1 million in SIPP compared to 2.4 million in the CPS.

TABLE 8

DISTRIBUTION OF WEEKS UNEMPLOYED BY TYPE OF EMPLOYMENT/UNEMPLOYMENT EXPERIENCE

	CPS			SIPP		
	Labor Force=52	Labor Force<52	Never Employed	Labor Force=52	Labor Force<52	Never Employed
Persons Unemployed (,000)	12,908	5,416	2,380	9,502	14,457	6,124
	100.0	100.0	100.0	100.0	100.0	100.0
1-2 weeks	7.4	20.1	10.0	15.9	18.4	9.2
3-4 weeks	10.1	20.1	13.3	12.5	12.7	7.5
5-8 weeks	11.8	19.7	10.5	16.4	16.8	12.7
9-13 weeks	18.5	16.5	11.9	16.4	17.5	11.0
14-26 weeks	28.7	19.0	15.7	23.4	24.9	30.0
27-52 weeks	23.5	6.3	38.5	15.1	9.4	29.3

SOURCE: SIPP 1986 Panel and 1987 March CPS, population 16 years of age or older

The Effect of Attrition on Experience Measures

The SIPP estimates discussed so far are based on sample members who have a complete set of interviews covering the 1986 calendar year. These estimates have been weighted using longitudinal weights computed by the Census Bureau. These weights are based on a set of demographic and economic characteristics, which include labor force status and the receipt of Unemployment Insurance (UI) during the first reference period. If the labor force experiences of sample members who leave the survey during the year are systematically different from those who stay in the survey, the longitudinal weights might not be able to compensate for this heterogeneity, and thereby the SIPP measures of experience presented above might be biased.[19]

In Table 7 we saw that SIPP counts more persons with unemployment in all duration categories, with the only exception of persons reporting 40 or more weeks. If persons who experience long periods of unemployment during the year are particularly likely to leave the survey, and the control for receipt of UI[20] and other relevant characteristics observed in the first wave does not completely compensate for this phenomenon, then the estimates of long-term unemployment based on the SIPP longitudinal sample are biased *downward*. In

[19]Estimates from retrospective surveys, such as the March CPS, may also be affected by a similar bias. If sample members' likelihood of not responding to the retrospective survey is related to their labor force behavior in the reference period, biased estimates of experience measures are possible.

[20]The point of exhaustion of UI benefits is below 40 weeks in all States.

other words, selective attrition from the SIPP survey may prevent persons with very long spells of unemployment from being adequately represented.

Detecting the presence and size of this type of bias is difficult. We suggest a rather heuristic method to ascertain the existence of bias in the SIPP estimates weighted longitudinally. We computed three indicators of labor force activity--the employment and unemployment rates in January 1986, and the proportion of weeks unemployed during the time a person is in the survey. In Table 9 estimates of these indicators are presented for: (1) SIPP sample members who were in the survey in January 1986 but left the survey or refused an interview sometimes during the year (hereafter, defined as "incompletes"); their estimates are computed using January cross-sectional weights; (2) SIPP sample members who were present in the survey for the entire 1986 (hereafter, defined as "completes"); their estimates are computed using both cross-sectional and longitudinal weights.

TABLE 9

ATTRITION AND LABOR FORCE MEASURES

	Weighted Count (,000)	Proportion of Weeks Unemployed When on the Survey	Employment Rate in Jan.'86	Unemployment Rate in Jan.'86
Incomplete Respondents (January 86 x-sect. weights)	32,958	6.72	55.74	11.37
Complete Respondents (January 86 x-sect. weights)	149,539	4.25	59.56	7.85
Complete Respondents (longitudinal weights)	178,265	4.49	60.02	8.25

SOURCE: SIPP 1986 Panel, population 16 years of age or older

Respondents who left the survey had significantly lower employment rates, higher unemployment rates, and more weeks spent unemployed than those who remained in the survey (comparing first and second rows). Moreover, the longitudinal weights seem to have made only a minor compensation for the missing sample respondents, as far as labor force attributes are concerned. In fact, the *longitudinally weighted* estimates based on persons always in the survey are only slightly different from the *cross-sectionally weighted* estimates based on the same group of individuals (comparing third and second rows). The tentative conclusion emerging from this analysis is that selective sample attrition in SIPP may explain why fewer persons in SIPP are observed to experience very long-term unemployment than in the CPS.

VI. SUMMARY AND CONCLUSIONS

We devoted the first part of the paper to a comparison of gross change measures based on the SIPP and the monthly CPS. Such comparison confirmed our a priori expectation that retrospective questions (such as in SIPP) yield a significantly lower estimate of the extent of mobility between labor force states than repeated interviews (used in the CPS). We also presented some evidence that the omission of movers from the CPS does not affect the estimates of CPS gross flows in any substantial way, at least for the overall adult population.

All in all, the evidence presented in the first part of the paper is not sufficient to conclude whether the retrospective measures of change based on SIPP represent a better estimate of the true amount of turnover than CPS-based measures. However, SIPP transition rates turn out to be very close to the rates obtained by correcting the CPS gross flows for classification errors. We believe that this result should stimulate further investigation into the usefulness of SIPP as a source of data on labor force transitions.

The second part of the paper consisted of a comparative analysis of two experience measures--weeks worked and weeks unemployed--obtained in the CPS and SIPP. The analysis showed that a retrospective survey with an annual reference period--such as the March CPS--is very likely to yield estimates of unemployment which do not include many short spells of unemployment. In fact, estimates of the prevalence of periods of unemployment up to 8 weeks are about 70 percent higher in SIPP than in the CPS. Overall, the March CPS counts about 21 million persons with some unemployment during the year, compared to over 30 million counted by SIPP.

We showed that experience measures from repeated interviews conducted on a subannual basis (such as is done in SIPP), while mitigating recall bias, introduces the possibility of attrition bias. Evidence indicates that those who leave the survey tend to have different labor force attributes than those who stay. This may help to explain why estimates of very long term unemployment are higher in the CPS than in SIPP. Consequently, the use of repeated interviews to collect and then construct labor force experience measures will involve a tradeoff between more comprehensive and accurate counts of unemployment and biases caused by sample attrition.

We also presented some compelling evidence that the wording of the March CPS questions on weeks unemployed might induce a rather substantial bias in the estimates of how many unemployed persons have a year-round commitment to the labor force, and more generally in the estimate of the distribution of weeks of unemployment.

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