Job Instability and Insecurity for Males and Females in the 1980's and 1990's

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Introduction

This paper has two objectives. The first is to measure changes in job *instability* over the 1980's and 1990's. We provide evidence on changes in short term job turnover using a previously underutilized data source, the Survey of Income and Program Participation (SIPP) that provides monthly information on the respondent's employer.[2] The results from the SIPP are contrasted with results from the Panel Study of Income Dynamics (PSID), a more widely used data set. The second objective focuses on changes in what has been labeled job *insecurity*. The duration of jobs may not have changed but turnover may have been accompanied by less desirable outcomes. Turnover may be more likely to be "involuntary" or turnover may lead to worse outcomes, such as an increase in the probability of an intervening spell of non-employment or a decrease in the wage gains from changing employers. We therefore also examine several of these outcomes to see if the perception of greater insecurity reflects changes in these events.

Job Stability

Review of the Literature

There is now a sizable literature on changes in job separation rates in the United States[3]. As Table 1 shows the conclusions differ widely across studies. Since these studies use different data sets, samples and measures of turnover it is sometimes difficult to determine the underlying causes for these differences.

Almost all studies based on the various CPS supplements (Farber, 1997a. 1997b; Diebold, Neumark and Polsky 1996, 1997a, 1997b; Jaeger and Stevens, 1998) show little change in overall separation rates through the early 1990's[4]. The exception is Swinnerton and Wial (1995) which shows substantial increases in separation rates. However, their revised estimates in Swinnerton and Wial (1996) show much smaller increases, bringing their results closer to other CPS based studies.

While overall separation rates in the CPS may not have increased through the 1980's, there is a fairly consistent pattern across studies that shows increases in separation rates for some sub-populations. Men show greater changes than women, and groups that were experiencing greater declines in earnings, including the young and less educated, were also somewhat more likely to experience greater job instability through the 1980's[5]. This pattern seems to have been reversed in the 1990-91 recession. Farber (1997) and Diebold, Neumark and Polsky (1997b) find that separation rates for more educated workers started increasing in the first half of the 1990's. Since these workers were experiencing increases in relative wages, this would seem to break any simple relationship between changes in the wage distribution and changes in job separation rates.

The studies based on the PSID give a much less consistent picture than those based on the CPS. Rose (1995), Boisjoly, Duncan and Smeeding (1998) and Marcotte (1995) find rather sharp increases in job instability while Polsky (forthcoming) and Jaeger and Stevens (1997) find no change.[6] Differences between the CPS and PSID could reflect constraints imposed by the two data sets. For example, the PSID questions are asked only of heads of households and wives. Unless the separation rates of heads and wives are representative of the full population this selection will affect the level of separation rates and if the composition of the population changes over time this selection may also affect trends. Not only is the analysis in most PSID studies limited to heads but it is further restricted to male heads. Another inherent difference between the CPS and PSID is that the former does not provide tenure information on respondents who are not employed at the time of the interview while the event history data in the PSID allows the full population to be analyzed. Unfortunately neither Marcotte (1995) nor Jaeger and Stevens (1997) include persons who were not employed at the time of the interview.

These differences cannot be the full story since there are still major differences among PSID studies. Furthermore, differences in variables and measures used in PSID studies may be more important than differences between the PSID and CPS since Jaeger and Stevens (1997) find similar patterns when the PSID is used as a series of cross sections to replicate the CPS.

The National Longitudinal studies (NLS-YM and the NLSY) provide other data sets with which to measure separation rates. Monks and Pizer (1997) and Bernhardt et al. (1997) both find increases in separation rates for the young. The fact that these two studies give very similar results does not tell us very much about the robustness of these data since the two studies use very similar samples and measures. While the increase in separation rates for the young are substantially larger than those found in CPS data, the qualitative conclusion that turnover increased for the young is at least the same in these two data sets.

We are left with mixed evidence from these different data sets. A more direct comparison, where samples and definitions are made as similar as possible would improve our understanding of the contradictory conclusions in the literature. More work needs to be done to identify the sources of the discrepancies between the CPS, PSID and NLSY.

Our primary contribution is to provide evidence using a new data set (the SIPP). However, in order not to introduce more non-comparabilities, we use the PSID extensively to benchmark our results against this alternative longitudinal data set that has been used extensively in the literature. We start by comparing turnover measures from our PSID sample to previous studies using both the PSID and CPS. By showing that our PSID sample gives similar results when similar measures are used, we eliminate one potential source of discrepancy. We then use the SIPP to construct yearly separation rates that can be compared directly with those from our PSID sample. Having shown that the SIPP and PSID give similar results we then turn to the SIPP to measure monthly turnover. This allows us to examine whether there has been an increase in short-term turnover, which is particularly important since a high proportion of jobs are of short duration.[7] Furthermore, short-term turnover may have increased, even if yearly turnover in the PSID did not.

The evidence on turnover addresses the question of job instability. A related issue is job insecurity, which has sometimes been associated with involuntary separations. While the longitudinal SIPP files we use do not differentiate between voluntary and involuntary terminations, they do provide information on events accompanying the turnover[8]. Were job leavers more likely

to go through a spell of non-employment before moving to a new job? Did the duration of intervening spells of unemployment increase? Did recent job changers experience smaller wage increases? These attributes of job exits can be used to explore whether insecurity increased, even if instability (i.e., turnover) did not increase.

Changes in Turnover

Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) is a large nationally representative longitudinal data set that has been used extensively to study changes in job turnover. The primary advantage of the PSID is that it covers a sufficiently long period to track long term changes in turnover. The PSID, however, has several disadvantages. First, the tenure questions have changed over time.[9] This is particularly important for changes prior to 1984. Second, answers to questions about tenure are sometimes inconsistent. For example, cumulative tenure with an employer sometimes increases by more than the difference between interview dates. Without outside information it is impossible to determine whether this reflects measurement error or the respondent returning to a previous employer for which cumulative tenure is greater than tenure on the job he left[10]. Third, the questions are only asked of heads and wives. This is a particularly important drawback when examining transition rates of the young. Fourth, the job tenure questions are not asked of self-employed heads, so it is impossible to estimate turnover for the self-employed, a group of significant size who may have substantially different dynamics[11]. Fifth, the PSID does not differentiate between first and second jobs. This has two consequences. First, as we discuss below, this will result in some miscoding of job changes. Second, we cannot differentiate between exits to a new job and movement between simultaneously held jobs. Finally, for the period we study, the PSID identifies only changes between jobs held roughly one year apart[12]. And as we will show, there is considerable job turnover within a year.

Definition of Separation in the PSID

We identify a separation in the PSID if the "months with current employer" at the time of interview is less than the difference between interview dates[13]. A separation is also identified if a person makes a transition from being employed to not being employed in the following interview.

There are three potential difficulties with our definition. The first is the lack of consistent work histories for the period we study[14]. A person may cycle through several jobs during the year. Not only will durations less than a year be missed, but in some cases no separation will be recorded. For example, a person may be unemployed at both interviews but may have held a short-term job in the interim. Second, some longer-term spells are miscoded[15]. Alternatively, a job switch to a job previously held will be missed if the cumulative prior tenure on this new job is greater than the difference in interview dates[16]. The third potential problem involves an ambiguity in the question asked prior to 1984. Prior to that year the question on "months with current employer" did not specify whether the respondent should give total months across all spells with the same employer or only the most recent spell. As long as respondents answered the question consistently in all years prior to 1984, this ambiguity would not affect measures of transitions during that sub-period (cumulative as well as current tenure would increase in each year until a transition occurred). The change in the question in 1984 would also not lead to misclassification for persons who previously interpreted the question as referring to cumulative tenure. However, the change in wording may have led to misclassifications for respondents who had interpreted the previous question as referring to tenure in the current spell.

Findings

Before comparing job exit rates in the PSID and SIPP we build bridges to the previous literature by making sure that our PSID sample yields measures that are similar to those coming from PSID/ CPS comparisons. In the process we make the important distinction between longitudinal and cross-sectional measures of job exits. Since both the SIPP and PSID offer longitudinal data, we will focus on longitudinal measures. For completeness we, however, start by constructing cross-sectional measures that have been used by Jaeger and Stevens (1998) to benchmark the PSID.

The CPS is the primary data source that has been used to benchmark the PSID. Since it is a cross-sectional data set, its measure of job turnover is based on whether tenure with the current employer is less than a year. In contrast, the PSID can provide both cross-sectional measures, also based on tenure at time of interview, and longitudinal measures discussed earlier. As we will show, these two approaches to measuring job separations do not necessarily give the same picture.

The tenure supplements and the benefit supplements of the CPS ask employed respondents how many years they have worked for their current employer.[17] The separation rate is given by the proportion of respondents who report being with their current employer for one year or less. The PSID can also be used to generate cross-sectional measures based on reported monthly tenure at the time of interview. One key issue, addressed by Jaeger and Stevens (1998), is how to translate the *monthly* tenure in the PSID to the *yearly* tenure reported in the CPS. Jaeger and Stevens assume that respondents in the CPS round the number of months they have worked for their current employer to the nearest whole year. Following their lead, we classify any job tenure reported as 18 months or less in the PSID as equivalent to tenure of one year or less in the CPS.

The four points in Figure 1, labeled CPS, show the yearly separation rates in the CPS as measured by Jaeger and Stevens. The solid line labeled, PSID Cross-sectional identifies a transition if reported tenure is less than 18 months. While our sample differs slightly from that used by Jaeger and Stevens, our series is very similar to theirs[18]. In both cases the PSID and CPS series show no upward trend in separation rates.

We contrast these cross-sectional measures with the longitudinal measure that we use in our comparison with the SIPP. A job change is assumed to have occurred if tenure with the current employer is less than the difference in interview dates. This longitudinal measure will give a different classification than the cross-sectional measure based on a single interview when reported tenure is less than or equal to 18 months but is greater than the difference in interview dates[19]. For example, if tenure is reported as 1 month in the first interview and 13 months in the following interview 12 months later, then the cross-sectional measure would classify this as a job change but the longitudinal measure would not. Both measures will, however, miss job changes that occur

when a person returns to a previously held job and reports cumulative tenure greater than 18 months.[20] Without a full enumeration of employers, as in the SIPP, both measures will miss this transition[21].

The line labeled PSID Longitudinal (employed) identifies a transition when a person is employed in both interviews and tenure is less than the difference in interview dates. This definition leads to substantially lower job ending probabilities but similar patterns over time. The shift down in the function reflects the fact that roughly 30 percent of reports of tenure less than 18 months in the PSID were preceded by even lower reported tenure in the prior year. Thus, an 18-month definition misclassifies some spells with short tenure as job changes rather than a continuation of a spell that began in the previous year. The time-series patterns in the series discussed thus far, however, are very similar, confirming the conclusion that overall job separation rates did not increase.

One consequence of any procedure based on tenure with current employer is that it excludes persons who were not employed at the time of the interview (because they were not employed, out of the labor force or self-employed). If the non-employed have different separation probabilities then inferences cannot be made to the full population. Since the risk set for separation rates is all persons who were employed in the base period, one should include both persons who were employed and those who are not employed in the subsequent period. This can be done with the SIPP and PSID but not the CPS since it does not ask questions about tenure to persons not employed[22]. The line labeled PSID-Longitudinal (All) in Figure 1 plots the proportion of persons who held jobs in the previous interview and were either employed in another job (i.e., their tenure with the current employer is less than the difference in interview dates) or not employed (i.e., unemployed, out of the labor force or self-employed) in the subsequent interview. The large upward shift in the function indicates that excluding jobs that end in a spell of non-employment seriously understates the amount of job transitions. The fact that this series is roughly as large as the cross-sectional measure indicates that the two previously discussed misclassifications roughly offset each other. However, this series also shows no upward trend.

In summary, comparable data from the CPS and PSID lead to the similar conclusion that separation rates did not increase. This holds even after using the longitudinal aspects of the PSID and including persons who were not employed at the time of the interview.

While there does not seem to be an upward trend in any of the series in Figure 1, it is possible that some demographic groups did experience an increase. To explore the possibility that the aggregate trends mask demographic-specific trends in exit rates we estimate Cox proportional hazard models by gender, race and education group[23]. Similar to all the multivariate models in this paper we include year, a quadratic in age, and an interaction of year and age to capture differences in trends by age within a demographic group. We do not condition on measures of labor market tightness since we want to include the effects of secular changes in labor market tightness in the overall trend. We believe that these are the results relevant to the debate, which focuses on the gross change in instability, including instability associated with cyclical factors. However, for completeness we also estimated all models including the de-trended gender-specific employment rate[24]. Results in this and the following tables are not affected by including this cyclical variable.

The sample in Tables 2a and 2b includes all job beginnings during the panel for males and females respectively. We show the exponentiated coefficients along with tests for the joint significance of the coefficients on year and the year-age interaction.[25] Separate results are shown for persons disaggregated by race and education.

The estimated equations for white males in Table 2a indicate a statistically significant *decline* in the hazard of a job ending, with the largest decline for older workers. For white males the coefficients indicate a decline in the hazard for all education groups, though the age interaction is significant only for college-educated workers. For non-white males the coefficients on year and the year-age interaction are also jointly significant at conventional levels, though the coefficients on year and the age interaction indicate that the hazard was at first increasing with age and then decreasing. The bottom row of Table 2a, which shows the age at which the time derivative is equal to zero, however, indicates that the hazard starts to decline well before working age for all but persons with at least some college. And for them the decline starts at age 20. The estimates for

females in Table 2b are remarkably similar to those for white males, indicating that they also experienced a decline in the hazard of leaving their jobs. From this we conclude that the duration of jobs was increasing in the PSID even within demographic groups.

The perception that jobs had become less stable may be based on the characteristics of the exits rather than their frequency. To explore this possibility Figures 2a and 2b disaggregate the overall probability of a job separation into exits to three possible exit states: (1) exits to another non-self employment job[26], (2) exits to self-employment and (3) exits to unemployment or out of the labor market.[27] Each series is bracketed by the 95 percent confidence interval for these estimated proportions.[28] The probability of exiting to another job increased from a low of .045 in 1982 to a high of .090 in 1987 and then declined steadily through the early 1990's[29]. In contrast, transitions to non-employment declined during the early 1980's and then stabilized during the late 1980's and early 1990's. Females experienced very similar patterns in exit states. Thus, there is no evidence of a secular increase in the probability that a job ending would be followed by a spell of non-employment.

While exits to non-employment did not increase, it is possible that a greater *proportion* of exits were to non-employment. This would occur if exits to employment (i.e., job to job transitions) declined faster than exits to non-employment. To explore this possibility Tables 3a and 3b show the results of estimating trends in the conditional probability of non-employment, given that an exit occurred[30]. Again year is interacted with age to allow trends to vary by age as well as race and education. If a case is to be made that exits were more likely to result in non-employment, then the case can only be made for older workers since the significant trends are all negative except for males and females in their 40's[31]. And for many groups the trend coefficients are not significant. Thus, if there was an increase in the probability that a job exit was followed by a spell of non-employment, it was limited to a subset of older workers.

Finally, we explore the possibility, suggested by Boisjoly et al. (1998), that involuntary terminations may have risen, even if overall job exit rates did not. In order to make our work as comparable as possible to theirs, we restrict our sample to their age range (25 to 59) and use the

same variable to determine whether a termination was involuntary[32]. Our replication of their work also shows a significant increase in the probability of an involuntary termination in the post-1968 period that they study. However, when we limit the period to the post-1980 period, which is the focus of our study, we do not find a significant trend either for males (figure 3a) or females (figure 3b). We, therefore, conclude that their finding of an upward trend in involuntary terminations is driven by increases in involuntary terminations from the 1970's to the 1980's, rather than by a continued increase during the 1980's [33].

We also estimate Cox proportional hazard models of the hazard of being involuntarily terminated (where voluntary terminations are treated as censored spells) to see if the hazard increased for some subgroups. Tables 4a and 4b indicate that over the 1980's and 1990's the hazard of involuntary terminations either did not change over time or declined for persons of working age in all demographic groups. We therefore conclude that rising involuntary terminations cannot be used to explain the perception of rising insecurity during the 1980's and early 1990's.

While the probability of an involuntary termination may not have increased during the period we study, it is possible that among the exits, which did take place, more exits were involuntary. This would happen if the decline in the overall hazard of a job exit (documented in Tables 2a and 2b) declined more quickly than the hazard of involuntary terminations (as shown in Tables 4a and 4b). To explore this possibility Tables 5a and 5b show probit estimates of the trend in the conditional probability that an exit is involuntary. Again it is only older workers who show a statistically significant increase in the proportion of exits that are involuntary. Thus, there is some evidence of increased insecurity for older workers in the limited sense that turnover was more likely to be the result of an involuntary termination, not that turnover increased for this group.

In summary, the overall picture emerging out of our analysis of the PSID is of greater job stability, with some changes in the composition of these exits. The overall probability of a job ending did not increase during the 1980's and early 1990's. This holds whether we focus on all

job endings or on involuntary terminations. If a case is to be made that insecurity increased then it has to be based on the changing composition of exits for older workers. There is some evidence that exits among older workers were more likely to be the result of involuntary terminations and that exits were more likely to be followed by non-employment. This should, however, not obscure our main finding, consistent with findings from the CPS, that job exit rates declined, both overall and across a wide variety of demographic groups.

Survey of Income and Program Participation

We now turn to the Survey of Income and Program Participation. The availability of monthly data from this survey allows us to study short-term dynamics, as well as year to year turnover, as in the PSID. Within year turnover may have changed in ways different from yearly turnover. The SIPP, therefore, adds an important dimension to the study of job instability and insecurity. The availability of job specific monthly wage data also allows us to examine whether the wage gains (or losses) associated with job turnover changed during the 1980's and 1990's. Since these wage changes may be a more relevant measure of the consequences of the job changes, they offer a useful indicator of changes in job insecurity.

The Survey of Income and Program Participation consists of a series of nationally representative longitudinal surveys of nearly 30,000 individuals who are followed for roughly two and a half years. A new panel has been started in every year (other than 1989) since 1983. With reoccurring two and a half-year panels, there is substantial overlap across panels. Individuals within each panel are interviewed every four months. These interviews, called waves, include retrospective questions on job and earnings histories that cover the previous four months.

The SIPP offers several important advantages over the PSID. First, it includes information on job histories that assign unique identifiers throughout the panel to each employer for which the respondent worked either in a primary or secondary job[34]. The availability of full job histories for the 32 months covered by the typical panel is a clear advantage over the tenure questions asked in the PSID[35]. With job histories it is possible to identify transitions without having to rely on reported measures of tenure. A second, and related, advantage of the SIPP is that it can be used to

estimate the distribution of monthly duration starting in the early 1980's. Third, the SIPP includes job histories for secondary jobs, making it possible to identify transitions in which a secondary job becomes a primary job. Finally, SIPP includes self-employment histories for all persons. This is a distinct advantage over the PSID where one cannot follow non-heads or even heads that are self-employed, since the self-employed do not report tenure.

Structure of the SIPP

An important feature of the SIPP is that the sample is phased in over time. The sample is divided into four rotation groups, with one group being started in each of the first four months of the panel. The four rotation groups are asked retrospective questions covering the last four months. Since the questions cover the previous four months, each month is covered by each rotation group (other than the months at the beginning and the end of the panel). For example, the first wave of the 1990 panel was first interviewed in February 1990. Job histories and earnings histories were asked for October through January (wave 1 of rotation group 1). This first rotation group was re-interviewed in June when it provided information for February through May. The fourth rotation group, which was started in April, reported information on January through April. There is further overlap in the SIPP since new two and a half-year panels are started every year (other than 1989). Therefore, information is gathered from respondents in up to three overlapping panels at any one time.

One well known problem with retrospective questions is that changes in status are considerably more likely to occur between interviews than within the period covered by the interview. This is known as the "seam bias" problem. For example, respondents are more likely to report the same employer in the four months covered by the survey than between surveys. This results in higher job change probabilities at the seams than between seams. Since the seams occur in different months for people in different waves, monthly job change probabilities are mixtures of the low transition rates reported between seams and the high rates at the seams. If respondents are correctly reporting the number of job changes but are reallocating the timing of the change to the seams, then this mixture will yield unbiased estimates of job change probabilities as long as each

month has an equal probability of being at a seam[36]. We, however, also take account of seams in our multivariate estimates.

Definition of Separation in the SIPP

Respondents in the SIPP are asked for the name of the employer in each primary job (i.e., Job 1) and secondary job (i.e., Job 2). Identification numbers are assigned to each employer so it is possible to determine when a secondary job becomes a primary job and when an individual returns to a previously-reported employer. Each individual is also asked to list self-employment businesses in which he participated in each month. These are also given unique identification numbers.

We identify transitions when the identification number of the primary employer changes[37]. This includes transitions to other employers, to self-employment or to non-employment. For the self-employed we identify a transition when the individual becomes employed in a primary job or becomes unemployed or out of the labor force. Changes among self-employment businesses are not classified as transitions since the person continues to work for the same employer, namely himself.

Composition of the SIPP Sample

Our SIPP sample includes persons 20 to 62 with valid data on job and self-employment histories.[38] Table 6 shows the distribution of the sample in each year between 1983 and 1995 according to four mutually exclusive categories: the person (1) has a primary job but no secondary job; (2) has both a primary and secondary job; (3) is self-employed (and not employed by someone else) or (4) is not employed (i.e., either unemployed or out of the labor force).[39]

In 1983 only 70.2 percent of males matched the stereotype of having only one outside employer. An additional 2.8 percent had a second job and 8.3 percent were solely self-employed. The remaining 18.7 percent were either unemployed or out of the labor force. Consistent with CPS data, the SIPP shows an increase during the second half of the 1980's in the proportion of males and females with multiple jobs and the proportion self-employed.[40] This increase in non-

traditional employment was, however, reversed during the early 1990's, leaving the proportions about where they had been in the early 1980's.

Comparison of Separation Rates in the SIPP and the PSID

Before using the SIPP to explore monthly transitions we benchmark this data set against the PSID. The two data sets differ in the period and sample covered, as well as the measures of job separations. The PSID can be used to determine whether a person was in the same job in successive interviews roughly one year apart. We, therefore, use the data in SIPP to measure changes in employers one year apart. Since most interviews in the PSID occur between March and May we compare the reported jobs in the SIPP for these same months[41]. Because SIPP does not include a 1989 panel it is not possible to calculate yearly transition rates between March through May of 1989 and 1990.[42]

To make the two data sets as comparable as possible, we also restrict the samples to employed married males. The restriction to persons employed by others is dictated by the fact that the PSID does not ask tenure questions to the solely self-employed. Since the PSID only asks tenure questions to heads of household we must also restrict the SIPP sample in the same way. However, since the SIPP does not identify heads of household we must use other variables to make the two samples comparable. By restricting the SIPP sample to married males and the PSID to married male heads we achieve roughly the same coverage.[43]

While restricting the analysis to transitions between jobs a year apart for married males makes the SIPP more closely comparable to the PSID, the two data sets still differ in the underlying questions used to identify transitions. The PSID transitions are based on reported tenure, which is not asked directly in the SIPP, and the SIPP transitions are based on changes in employer identification numbers. Any differences between estimates of transitions may, therefore, reflect this area of continued non-comparability.

Figure 4 plots the estimated probability that a sample member in the PSID or the SIPP was in a different job (or had become self-employed, unemployed, or had left the labor force) roughly one year after the interview date.[44] The PSID shows transitions rates that fluctuated around 18

percent. Yearly separation rates for married males in the SIPP are somewhat higher, hovering around 22 percent and, if anything, they show a downward trend[45]. Whether these differences reflect the remaining non-comparability of definitions and samples or differences in reporting is an open question. While the levels are different neither data set shows an increase in exit rates.[46]

We conclude that while there are differences in these two data sets, neither shows an increase in instability. Separation rates for married males do not increase secularly in either data set; if anything they decrease in the mid 1980's.

Monthly Transition Rates from the SIPP

Thus far we have used the SIPP to calculate the probability that a sample member would still be working for the same employer one year later. This restriction and the restriction of the sample to married males were imposed to compare the SIPP with the PSID. Having shown that the trends in yearly measures are similar in these two data sets, we now exploit the unique advantages of the SIPP by examining monthly rather than yearly transitions for persons who were self-employed as well as employed by others. The SIPP also allows us to include females and males that were not heads of households.

We determine whether each employed (or self-employed) respondent separated from his/her employer in each month (i.e., had a different employer, became self-employed or did not work in the following month).[47] Likewise, we determine whether each person who was self-employed in each month changed employment status (i.e., became employed by someone else or did not work) in the following month.

Figures 5a and 5b show the time series patterns in the monthly separation rates for employed and self-employed males and females, disaggregated by race. Since the separation rates in each month have large sampling variability we show the average separation rates of all person months falling in the calendar year[48]. These data again do not show a secular increase in monthly separation rates. If anything, there was a secular decline in job exit rates between the mid-1980's and the early 1990's. While exit rates did increase sharply for most groups in 1994, this was followed by an equally sharp decline in 1995, leaving exit rates at roughly the same level as a

decade earlier. Thus, expanding the sample to include females and males who are not heads of households and using monthly separation rates (instead of separation rates based on jobs held a year apart) gives further evidence that separation rates did not increase. In fact, separation rates declined modestly for most groups between 1985 and 1993.

We again explore whether these declines were specific to certain demographic groups by estimating Cox proportional hazard models separately by race and three education groups. Trends are again captured by year (measured in terms of months) with a time trend entered separately and interacted with age.[49] In order to control for the lumping of job exits at the end of an interview period we enter a dummy variable equal to one if the risk of exit is measured in the month prior to a seam. With nine panels we end up with 176,648 non-left censored jobs for males and 154,845 for females.

The results in Tables 7a and 7b indicate that the trend in exit rates were either not statistically different from zero or, when they were positive, the trends were not quantitatively important.[50] For non-white males and females the coefficients on the trend terms are not significantly different from zero, indicating that the hazard of leaving a job was constant. For white males with less than a college degree the trend coefficients are significant and indicate a mildly increasing hazard. But this is largely driven by the spike in 1994. When a dummy variable is included for this year, the trend is again insignificant. For white females the trend in the hazard is positive for all but the middle education group. The largest trend (for college educated white females) is, however, only .9 percentage per year. We conclude that job separation rates were constant or where positive trends appear they largely reflect a one-time increase in 1994.

Outcomes Accompanying Exits

Again we explore the possibility that the perception of increased insecurity is more a reflection of deterioration in outcomes that accompany job endings than a reflection of an increase in the probability of a separation. To explore this we examine whether (1) there was an increase in the probability that a job ending was followed by a spell of non-employment, (2) that spell a of

non-employment following a job loss increased in duration or (3) that job changes were accompanied by smaller wage gains (or larger losses).

Figures 6a and 6b show the time trends in the probability that a job exit was followed by a non-employment spell.[52] If anything, these probabilities decline, indicating that transitions to unemployment or out of the labor force became less common. Tables 8a and 8b explore whether the lack of a positive trend holds when we control for our standard set of characteristics. For non-white males and females the probability that a job was followed by a period of non-employment decreased over time for the young and increased for older workers, but the trends are small, even for workers in their late 50's (as indicated by the partial derivative on the age interaction). The pattern for whites is less consistent but the derivatives on year and the year-age interactions are small for all groups except white females with less than a high school degree. We conclude that (except for young females with a high school degree or less) there is little evidence that job endings were increasingly likely to be followed by a non-employment spell.

Although the prevalence of non-employment spells was not increasing for the vast majority of the population, the duration of these spells may have increased, leading to a perception that the consequences of job endings had worsened. Tables 9a and 9b indicate that there is some support for this perception. While most groups show no trend in the duration of non-employment, when hazards of job re-entry changed, they declined. White and non-white males with a high school degree or less had a significant decline in the hazard of re-entry. For females three out of the six race and education groups have significant declines in hazards throughout most of their working lives. Thus, there is evidence that while the prevalence of exits to non-employment did not increase substantially, the duration of such spells did increase for some groups.

Finally, we turn to the wage changes that accompanied job changes[54]. It is well known that much of the life cycle increases in wages occur when a person changes jobs[55]. In this section we explore whether the resulting wage changes declined. While we recognize that wage changes and job dynamics are clearly jointly determined, we make no attempt to model the complex causal mechanism generating these outcomes. Consistent with our general approach throughout this

paper we provide reduced form estimates of the net outcome of this process. However, even within the context of our descriptive approach we must deal with the question of the appropriate reference group against which to contrast the wage changes of job switchers. Even if job exits were associated with increasingly small wage gains (or larger wage losses) this would not indicate a deterioration in outcomes associated with turnover if this change reflected a general decline in wages.

This can be seen by considering a very simple error components model of the association between job change and wage change. In the past literature the major issue in estimating the effect of job change on wage change has been the possible selection bias in who moves and who does not. We do not seek to provide a new method of avoiding this bias but instead use some simple comparison groups that have been used previously but which avoid bias completely only under strong assumptions.

We assume that the wage of individual i in job j with experience t is:

$$W_{ijt} = \mu_i + \alpha_{ij} + \beta_i t_i + \varepsilon_{ijt} \qquad (1)$$

where μ_i is an individual fixed effect, α_{ij} is a fixed effect unique to an individual-job combination, β_i is a random wage growth parameter, which allows heterogeneity in age-earnings profiles across individuals but is common across jobs, and ε_{ijt} is random error for which we assume $E\left(\varepsilon_{ijt}\middle|\mu_i,\alpha_{ij},\beta_i\right) = 0$ in the population.[56] We will add a vector of observed covariates below. Our object of interest in this model is the mean value of $\alpha_{ik} - \alpha_{ij}$ for $j \neq k$ (i.e., the change in intercepts for those who change jobs between t and t+1). The parameter α_{ij} is a measure of the permanent wage of a job and, hence, we seek to determine the change in that wage for those who change jobs.[57]

Let k denote the individual's job at time t+1 and let D_{it} be a dummy variable indicating a job change (i.e., $D_{it} = 1$ if $j \neq k$ and $D_{it} = 0$ if j=k). Then

$$W_{ik,t+1} - W_{ijt} = \alpha D_{it} + \beta_i + \Delta\varepsilon_{it} \qquad (2)$$

where $\alpha = \alpha_{ik} - \alpha_{ij}$, which is the object of interest, and $\Delta\varepsilon_{it} = \varepsilon_{ik,t+1} - \varepsilon_{ijt}$.

We estimate (2) in two ways by making two different identifying assumptions. The first assumption is that $\beta_i = \beta$ (no heterogeneity in slopes) and $E\left(\Delta\varepsilon_{it} \middle| D_{it}\right) = 0$. The former rules out bias arising from a differential selection of movers and non-movers on the basis of the value of β_i. The latter rules out differential selection of movers and non-movers on the basis of transitory wage shocks. Under these assumptions a simple regression of wage change on the mover dummy yields an estimate of mean α. As a sensitivity test of the potential bias associated with violation of these assumptions, we define the wage change of the comparison group (i.e., the $D_{it} = 0$ group) in one of two ways: (1) the average wage change of the group of individuals who never moved in any of the periods we observe in our panel and (2) the average wage change of the group of individuals who moved during the panel, but including only wage changes in periods in which they did not change jobs. The latter comparison group is due to Mincer (1986) and is based on the notion that the distribution of unobservables of those who move at different periods may be closer to the counterfactual distribution of movers than that of never-movers.

Our second approach to the problem of inferring the wages the person would have received had they not changed jobs drops the restriction of a homogeneous β_i but requires the use of more data. We allow individual-specific β_i but we eliminate this component of heterogeneity by double-differencing. Let l be the job held at time t=1 and let D_{it-1} be a dummy for whether the individual changed jobs between t-1 and t. Then

$$W_{ijt} - W_{il,t-1} = \alpha' D_{i,t-1} + \beta_i + \Delta\varepsilon_{i,t-1} \tag{3}$$

Where $\alpha' = \alpha_{ij} - \alpha_{il}$ and $\Delta\varepsilon_{i,t-1} = \varepsilon_{ijt} - \varepsilon_{il,t-1}$. Then subtracting (3) from (2) for those who did not change jobs from t-1 to t (i.e., j=l), we have

$$\left(W_{ik,t+1} - W_{ijt}\right) - \left(W_{ijt} - W_{ij,t-1}\right) = \alpha D_{it} + \left(\Delta\varepsilon_{it} - \Delta\varepsilon_{i,t-1}\right) \tag{4}$$

The assumption we need for an unbiased estimate of α in this model is that $E\left(\Delta\varepsilon_{it} - \Delta\varepsilon_{i,t-1} \middle| D_{it}, D_{i,t-1}\right) = 0$. This model simply uses the wage data from t-1 to t to estimate

the individual wage growth for each individual i, and then identifies α as the deviation from that wage growth between t and t+1 for those who move.

We implement this second strategy in the following way. First, since (4) does not have an intercept, those who do not move from t to t+1 are not needed for the estimation; the mean of the double-differenced wage of movers estimates α by itself.[58] Second, we use all wage data available for the individual's job at time t to estimate wage growth on the previous job. Specifically, in place of $\left(W_{ijt} - W_{ij,t-1}\right)$ we estimate a job-specific slope, based on all years observed for the individual in that job. Third, we allow α to be a function of a vector of covariates, one important covariate being a time trend to allow us to determine whether wage gains have changed over time.

We start by showing the results of following our first approach. Figures 7a and 7b show our estimates based on the wage changes of movers and the two comparison groups described above: persons who changed jobs but in a different period, and persons who did not change jobs at any point in the panel[59]. Since there is substantial month to month variability in wage changes we show the annual averages of the monthly changes. The first thing to notice is that the two control groups have very similar time-series patterns in wage growth. Our conclusion are, therefore, not sensitive to the choice between the two. Second, the time series data do not indicate a secular decline in the gains from job changes[60]. The average wage gains for movers are generally greater than for either of the control groups, though there is substantial year to year variability. The series for movers, however, shows no downward drift over the full period. There is, however, some downward drift after 1991 for whites, which may point to a secular trend but only in the very recent period.

Finally, we show the results of following our second approach in which we use the job changer's own prior wage growth to adjust the observed wage change for the wage growth the person would have experienced had he or she not changed jobs (as shown in equation 4).[61] The resulting net wage changes are the dependent variables in the descriptive linear regressions shown in Tables 10a and 10b.[62] The top panel shows estimates for all transitions while the bottom panel

includes only transitions with no intervening spell of non-employment. These regressions likewise provide little evidence that job changes have been accompanied by smaller wage gains. Tests on the joint significance of the coefficients on year and the year age interaction indicate that only one trend is significant at the 5 percent level (for older non-white males with some college)[63].

Conclusions

This paper has provided evidence on changes in both job instability and job insecurity using two large data sets, the SIPP and PSID. On the question of instability, we find that neither data set provides evidence that yearly exit rates increased during the 1980's and 1990's. This evidence is consistent with much of the recent literature that finds little change in overall job exit rates during the period we cover. While the evidence on earlier changes is mixed, we believe that the evidence is now strong that any increase in instability between the 1970's and 1980's that may have existed did not persist into the more recent period.

The primary contribution of this paper is to provide evidence on changes in monthly transition rates using the SIPP. These higher frequency data also indicate no increase in short-term job turnover. The fact that yearly and monthly measures give similar patterns suggests that the need to rely on yearly measures in previous studies has not masked offsetting changes in short-term job holding.

The second objective of this paper has been to explore whether job insecurity has increased. The claim has been made that, even if job exits did not increase, exits were more likely to have adverse consequences. Examples of insecure jobs are those that are more likely to end involuntarily or to be followed by a spell of non-employment or employment at a lower wage. Our evidence does not support this claim. We find no evidence of an increase in involuntary terminations during the period we study. Furthermore, we find little evidence of a greater likelihood of a job ending in a spell of non-employment or of job changes being accompanied by wage declines.

While there are still substantial differences across studies in results for sub-populations, we believe that a consistent picture is emerging on changes in job stability and job security in the

1980's and 1990's. Job instability does not seem to have increased and the consequences of separating from an employer do not seem to have worsened. This holds whether one looks at yearly or monthly transitions.

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Footnotes

[1] Gottschalk is Professor of Economics at Boston College and Moffitt is Professor of Economics at the John Hopkins University. Partial funding for this project was provided by the Henry J. Kaiser Family Foundation. Helen Connolly, Michael Hansen and Kelly Haverstick provided outstanding assistance on this data intensive project.

[2] Fitzgerald (1998) has recently used the SIPP to examine job turnover of less educated workers. His focus is on 12 month transitions while we exploit the SIPP to estimate monthly dynamics.

[3] For measures of job instability in OECD countries, see OECD (1997).

[4] The following CPS Supplements give information on job tenure:

- Displaced worker Supplement (DWS)--Jan 84, 86, 88, 90, 92, and February 94,96.
- Job Tenure (or Mobility) Supplements -- Jan 1973, 78, 81, 83,87,91.
- Contingent Work Supplement-- February 1995.
- Pension and Benefit Supplements--May 79,83,88 and April 1993.

[5] Marcotte (1995) provides a useful comparison of studies. See OECD (1997) for a comparisons of studies across OECD countries which also show greater decrease in stability among the young and less educated.

[6] An early version of Gottschalk and Moffitt (1994) presented separation rates based on the PSID. While these results are sometimes cited as showing increased instability, they were considered sufficiently unreliable because of changes in the wording of questions that they were dropped in the published version of the paper. For a summary of changes in the wording of question in the PSID see Polsky (Table A1, forthcoming).

[7] See Topel and Ward (1992).

[8] These attributes of changes may, in fact, be more informative than information on whether the respondent views the separation as involuntary.

[9] The appendix in Polsky (forthcoming) provides a useful summary of these changes.

[10] Brown and Light (1992) examine this issue. They conclude that if persons do not return to prior employers then there is substantial inconsistency in the data. Our tabulations of the SIPP, however, indicate that many respondents return to previous employers.

[11] The question asked is "How many years experience do you (HEAD) have altogether with your present employer?" Self-employed individuals are coded as missing.

[12] The Work History files provide monthly measures of changes in employers starting in 1988, which is too late for our purposes (monthly changes in position are available for 1984 to 1987).

[13] An alternative would be to use “months in current position”, rather than “months with current employer”. This would, however, include changes in positions with the same employer. Polsky (forthcoming, footnote 12) uses “months in current position” rather than “months with current employer” since the latter was not asked in every year he covers in his study. Since we focus on a shorter time period, this cost is lower for us and does not warrant mixing changes across employers with changes in positions while working for the same employer.

[14] Since we are interested in changes in job instability during the 1980’s we use the earliest consistent data series on tenure with current employer, which is available from 1982 onwards. The PSID work histories start in 1984 but there were major changes between 1987 and 1988. (See PSID documentation, 1992 interview year, Volume I, Section I, Part 7 p 42.)

[15] Even the work history files do not identify the return to an employer in a previous year since the job histories only identify specific employers within the year. See Brown and Light (1992, footnote 3).

[16] If tenure were reported without error we could identify a transition when the increase in tenure is larger than the months between interviews. Inspection of the data, however, indicates that the reporting error is too great to warrant this refinement.

[17] See Farber (1997). Note that the CPS Displaced Worker Surveys capture only involuntary separations since questions about tenure are only asked to workers who were displaced from their jobs in the prior five years (starting in 1994 this is changed to three years). See Farber (1997a).

[18] Jaeger and Stevens exclude respondents who are neither black nor white, while we include all races. They include persons 20 to 59 while our sample includes persons 20 to 62.

[19] Another minor difference involves the appropriate weight. If the measure of a job exit in t (i.e., a change in jobs between t and t+1) were based on truly cross-sectional data such as in the CPS, then the question on tenure would come from the interview in t+1 so the only available weight would be for the year after the exit. Longitudinal data do not impose this restriction so the cross-sectional measure is based on weights in t+1 while the longitudinal measure is based on the weights in t. Weights, however, change sufficiently little to make this difference quantitatively unimportant.

[20] This assumes that the difference in interview dates is less than the accumulated tenure.

[21] The SIPP attaches a unique identifier to each employer the respondent works for during the panel.

[22] Note that there is nothing inherent in the CPS that would preclude adding persons who had been unemployed for less than a year to both the numerator and denominator of the separation rate when estimating one year separation rates. Since persons who were unemployed for more than a year were not in the risk set, they should not enter either the denominator or the numerator.

[23] The sample includes all job beginnings during the panel.

[24] These results are available from the authors.

[25] For ease of interpretation we report $e^{\hat{\beta}}$. A value below one on year implies that the hazard declines over time.

[26] For convenience we use the term exit to a job and exit to a non-self employment job interchangeably.

[27] Exits to non-employment include both persons who were not employed at the time of the interview and persons whose tenure with the current employer is less than the difference in interview dates and was not employed or out of the labor force during the year.

[28] Note that the statistical significance of changes in exit rates cannot be read from the graph since the variance of a change includes a covariance between outcomes in the two periods being compared.

[29] We refer to exits between the job held at the interview in year t and the job held at the interview in t+1 as an exit in t.

[30] The sample is all job exits. The indicator variable is equal to one if the exit is to non-employment.

[31] The trend is zero for persons in their late 30's or early 40's and gradually becomes positive for older workers.

[32] We follow Boisjoly et al. by defining separate samples for each pair of years. To be included in the sample of persons who were at risk of being involuntarily terminated from the job they held at the interview in t the person had to satisfy the following criteria: (1) head of household or wife 25 to 59 at the interview in t; (2) not self employed with at least twelve months of tenure in the job held at t (3) worked at least 1000 hours in the year prior to t. Involuntary terminations are based on the answer to the question "What happened with that job--did the company go out of business, were you laid of, did you quit , or what?" The major difference between our replication and their series is that they exclude firings from involuntary terminations. We would not have been able to make this exclusion, even if we had wanted to, since they had to go back to the original questionnaires to identify firings. This however does not seem to affect the time-series pattern of the two series since our replication is very similar to their series. The correlation in the two series is .85.

[33] A simple linear trend fit to the series in figure 1 of their paper indicates a significant positive trend over the whole period (1968 to 1991) but no significant change in the period we study. Polsky (forthcoming) and Valletta (1997) also find increases in involuntary terminations in the PSID but their sample periods again start earlier than ours.

[34] Codes are specific to each individual and are consistent over all interviews in the panel. For

example, an employer identification number of 3 identifies the third employer observed for that individual. If the person returns to that employer later in the panel the employer will have the same code of 3. The SIPP also provides unique identifiers on each of the respondent's self-employment businesses in each month. The PSID Work History file also provides employer id's that can be used to track job changes within the year. However, these cannot be used to link employers across years since employer id's are initialized each year.

[35] The NLSY also includes job histories but the design of the panel which follows persons 14 to 21 in 1979 makes it difficult to separate age from period effects. Monks and Pizer (1997) and Bernhardt et al. (1998) attempt to overcome this drawback by contrasting the job histories in the NLSY with those from a previous cohort covered by the National Longitudinal Survey of Young Men.

[36] A quarter of the observations are at seams wherever four rotation groups overlap (i.e., except at the beginning and ends of the panels).

[37] We ignore transitions between secondary jobs since the sample sizes are too small (less than 4 percent of the sample holds two jobs simultaneously).

[38] All valid person months are included. We do not require valid data for all months in the panel.

[39] Since there are a large number of possible combinations of primary, secondary and self-employment, many of which would have very few observations, throughout this paper we classify persons as self-employed if this was the only source of employment in that month. Persons with a primary job and a self-employed business are classified as having an employer and are not counted among the purely self-employed. The yearly proportions shown are the average of all person months in each year, including persons in all panels covering the given year.

[40] Abraham et al. (1997) report that multiple job holdings in the May CPS peaked at 6.2 percent in 1989. The peak for males in the SIPP occurs a few years earlier and is somewhat lower, though the levels are more similar when the proportion of secondary job holders in SIPP is calculated as in the CPS.

[41] A person in SIPP with valid data in each of these three months in the two years will provide three observations on yearly separations (i.e., March to March, April to April and May to May). The resulting equally weighted average of the separation rates for each person in the three months is an unbiased estimator of the expected separation rates if the interview month is random. Standard errors are adjusted for repeated observations across individuals.

[42] The 1988 panel covers March through May of 1988 and 1989, but not 1990, while the 1990 panel covers March through May of 1990 and 1991, but not 1989.

[43] These restrictions are imposed only for this comparison.

[44] PSID interviews may not be exactly one year apart.

[45] SIPP standard errors take account of the clustered nature of the data caused by our inclusion of data for up to three months in each year for each individual. The narrower confidence intervals in the SIPP than the PSID reflect the larger number of persons (or clusters).

[46] Including males who were not married increases the exit rates but does not change the patterns over time.

[47] The sample includes all persons 20 to 62.

[48] Overlapping panels that cover the same year, therefore, contribute to the separation rates shown in Figure 5a and 5b.

[49] Year is measured in terms of months, for example, March 1990 is 90.25. Left censored spells are excluded.

[50] The large and significant coefficients on the seam variable illustrate the degree to which job turnover tends to be reported between interview periods.

[51] Tests are on the joint significance of the trend and age interaction.

[52] The sample includes all observed exits from jobs.

[53] See the coefficients on the age interactions.

[54] We do not examine this question with the PSID since the availability of information only on annual earnings severely limits the usefulness of this data set for this question. One would have to

compare earnings in the year before and the year after the job change (since earnings in the year of job change is a mixture of earnings in the two jobs).

[55] For example, Topel and Ward (1992) find that roughly a third of early career wage growth occurs at job changes.

[56] Allowing β to vary across jobs as well as individuals does not alter our conclusions.

[57] This statistical model is consistent with a job search model in which a set of homogeneous individuals initially located in different jobs receive draws of α_{ij} each period and accept those which are above the reservation value.

[58] Put differently, the model in (4) assumes that the double-differenced wage change for non-movers has mean zero. This could be relaxed by adding a quadratic in t to equation (1).

[59] Since the SIPP asks respondents who are paid by the hour their "regular hourly pay rate" we use this variable whenever it is available. For others we use earnings divided by hours. We calculate wage changes between successive jobs, whether or not there was an intervening spell of non-employment.

[60] Polsky (forthcoming) also examines the wage consequences of job separation using the PSID. While he finds larger wage losses in the late 1970's than in the late 1980's his results are not comparable to ours which focus on changes during a more recent period (the 1980's and early 1990's) and which use monthly data rather than annual earnings divided by hours.

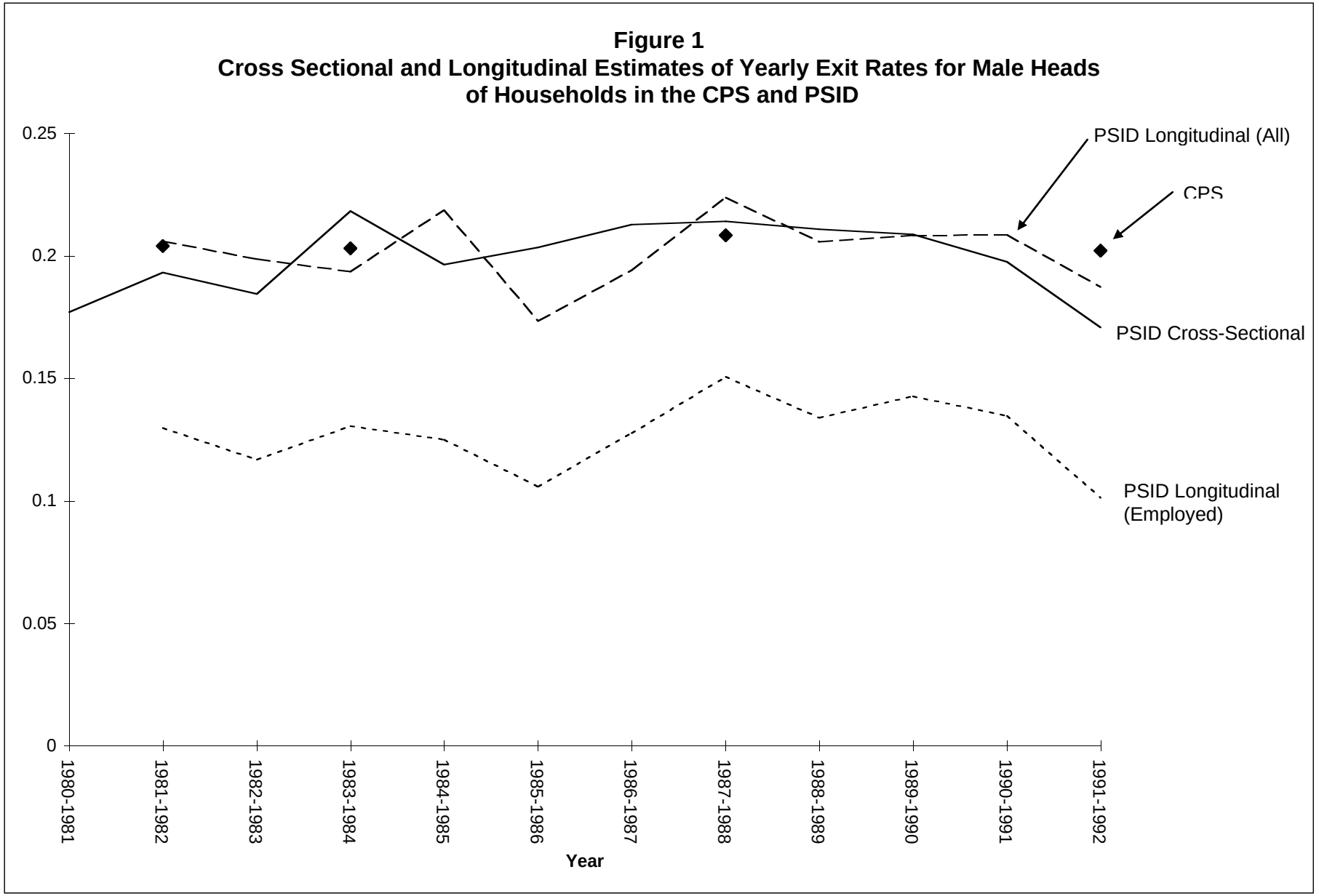
[61] The net wage change is the actual change in wages between jobs, ΔW, minus $\hat{\beta}_j(1+m)$, where $\hat{\beta}_j$ is the estimated growth rate in the job the person left and m is number of intervening months of non-employment. Thus, for persons who went directly to a new job the net wage change is $\Delta W - \hat{\beta}_j$.

[62] Since job leavers must have obtained a new job in order for us to observe their wage change, the sample includes all exits followed either immediately by a new job or with a completed intervening

spell of non-employment. Each observation is weighted by the inverse of the sampling variability of $\hat{\beta}_j$.

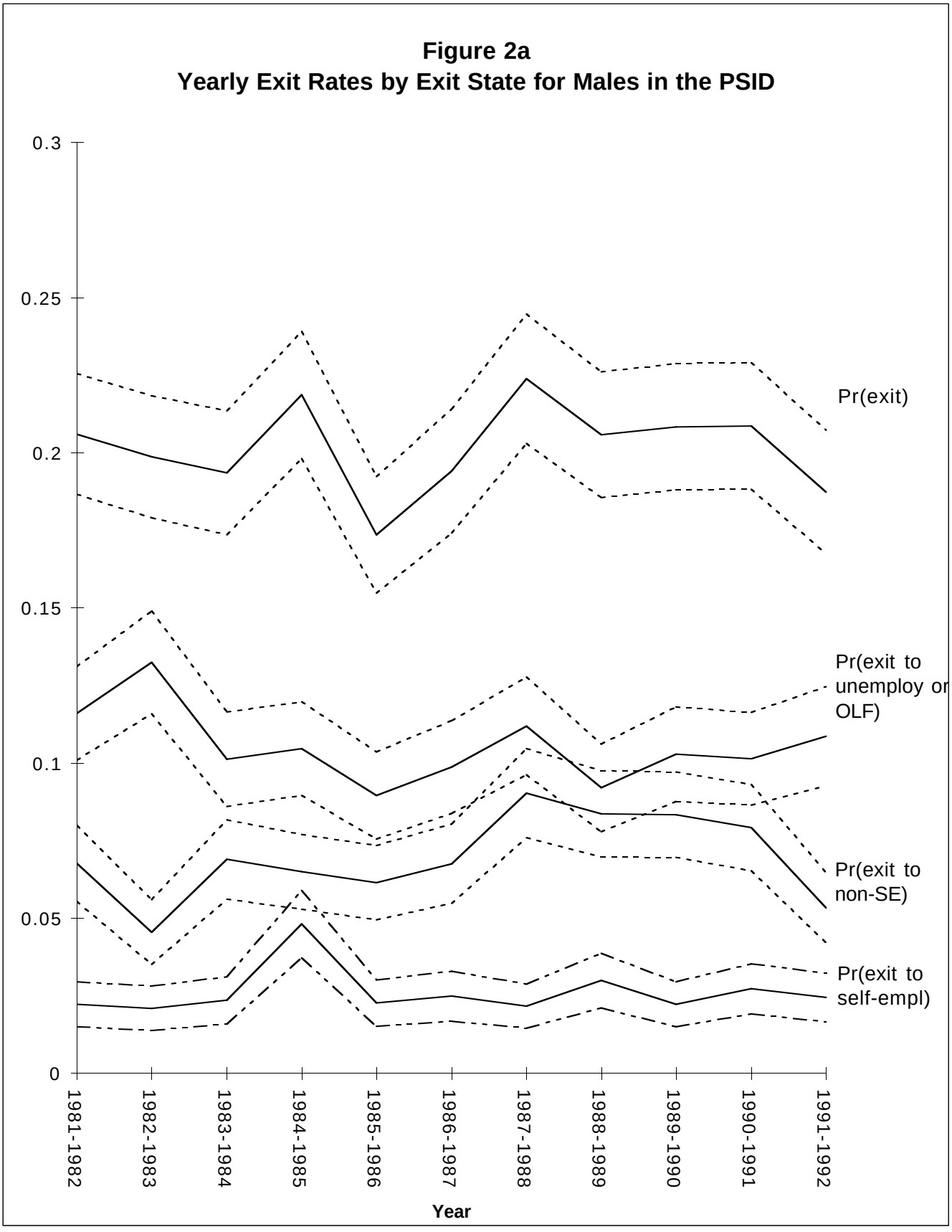
[63] Again year is shorthand for time measured in months.

Figure 1
Cross Sectional and Longitudinal Estimates of Yearly Exit Rates for Male Heads of Households in the CPS and PSID



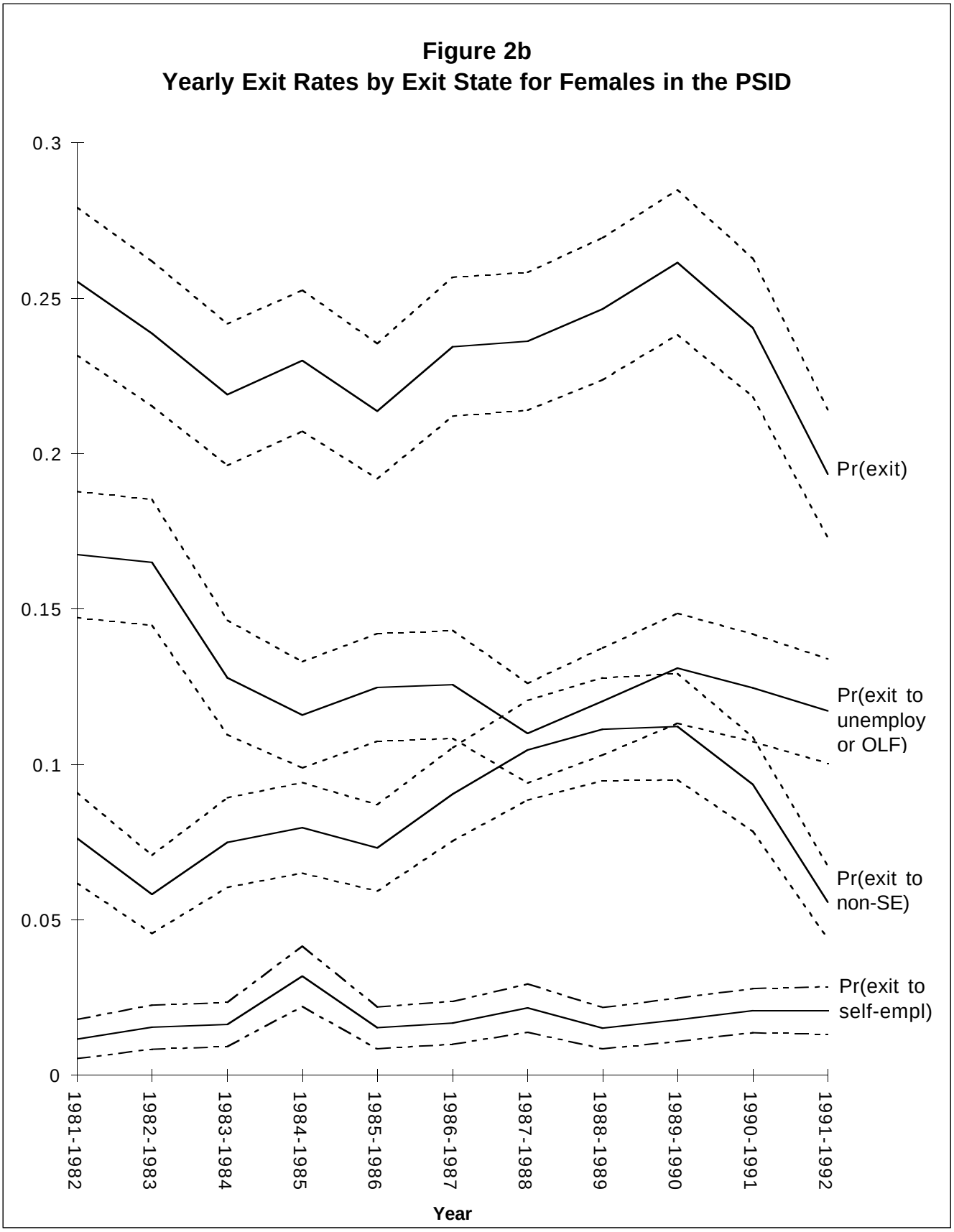
Source: CPS measures from Jaeger and Stevens (1997). PSID measures from author's tabulation of PSID. For each year, the sample consists of employed male heads of households, 20-62.

Figure 2a
Yearly Exit Rates by Exit State for Males in the PSID



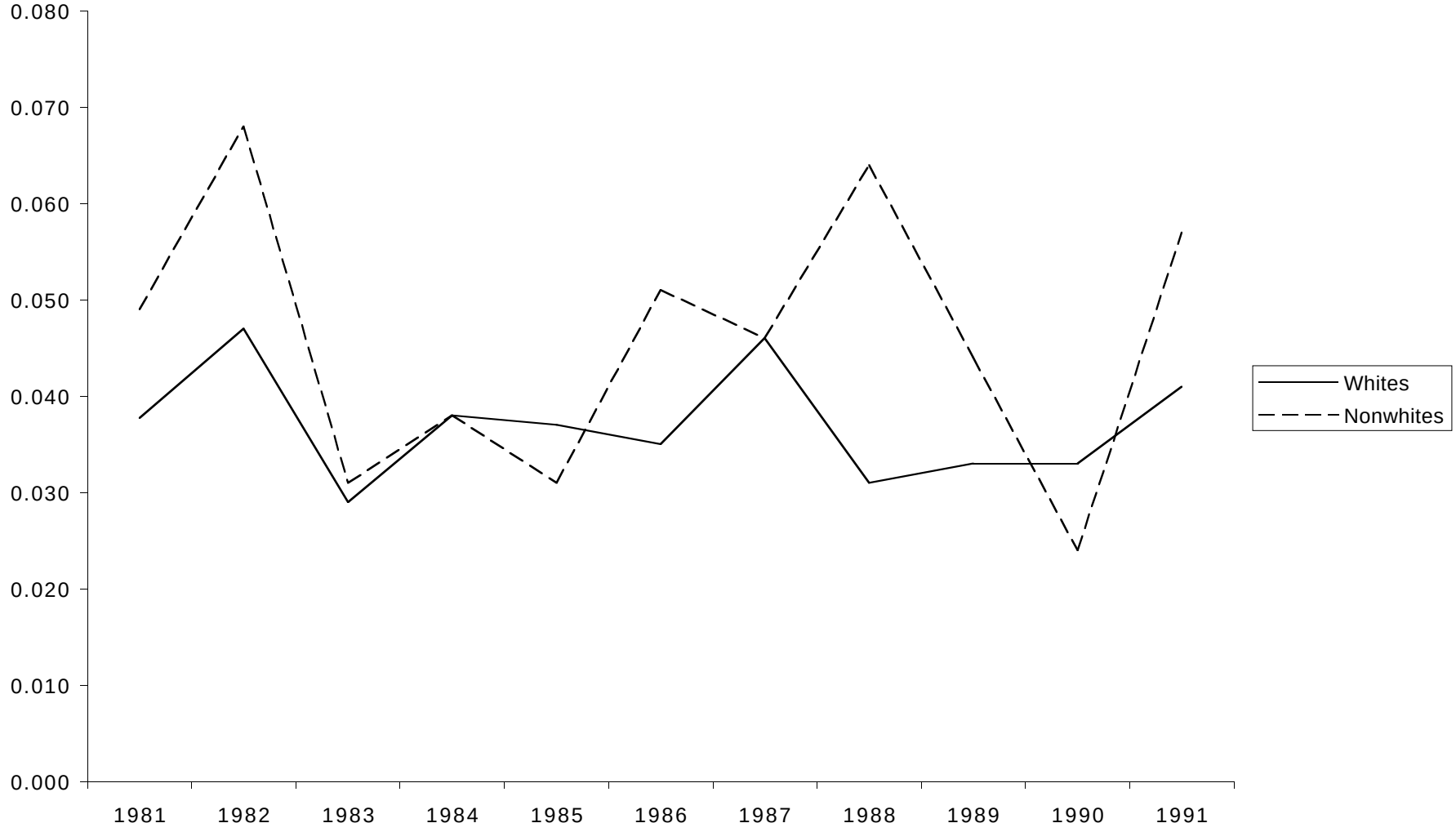
Source: Author's tabulation of PSID. Sample includes males 20-62. Confidence bounds are based on robust standard errors.

Figure 2b
Yearly Exit Rates by Exit State for Females in the PSID



Source: Author's tabulation of PSID. Sample includes females 20-62. Confidence bounds are based on robust standard errors.

Figure 3a
Yearly Involuntary Exit Rates by Race for Males in the PSID



Note: Sample includes male heads of households 25-59 with strong labor force attachment (see text).

Figure 3b
Yearly Involuntary Exit Rates by Race for Females in the PSID

Note: Sample includes female heads and wives of heads of households 25-59 with strong labor force attachment (see text).

Figure 4
Yearly Employment Exit Rates for Employed Married Males 20-62
PSID and SIPP

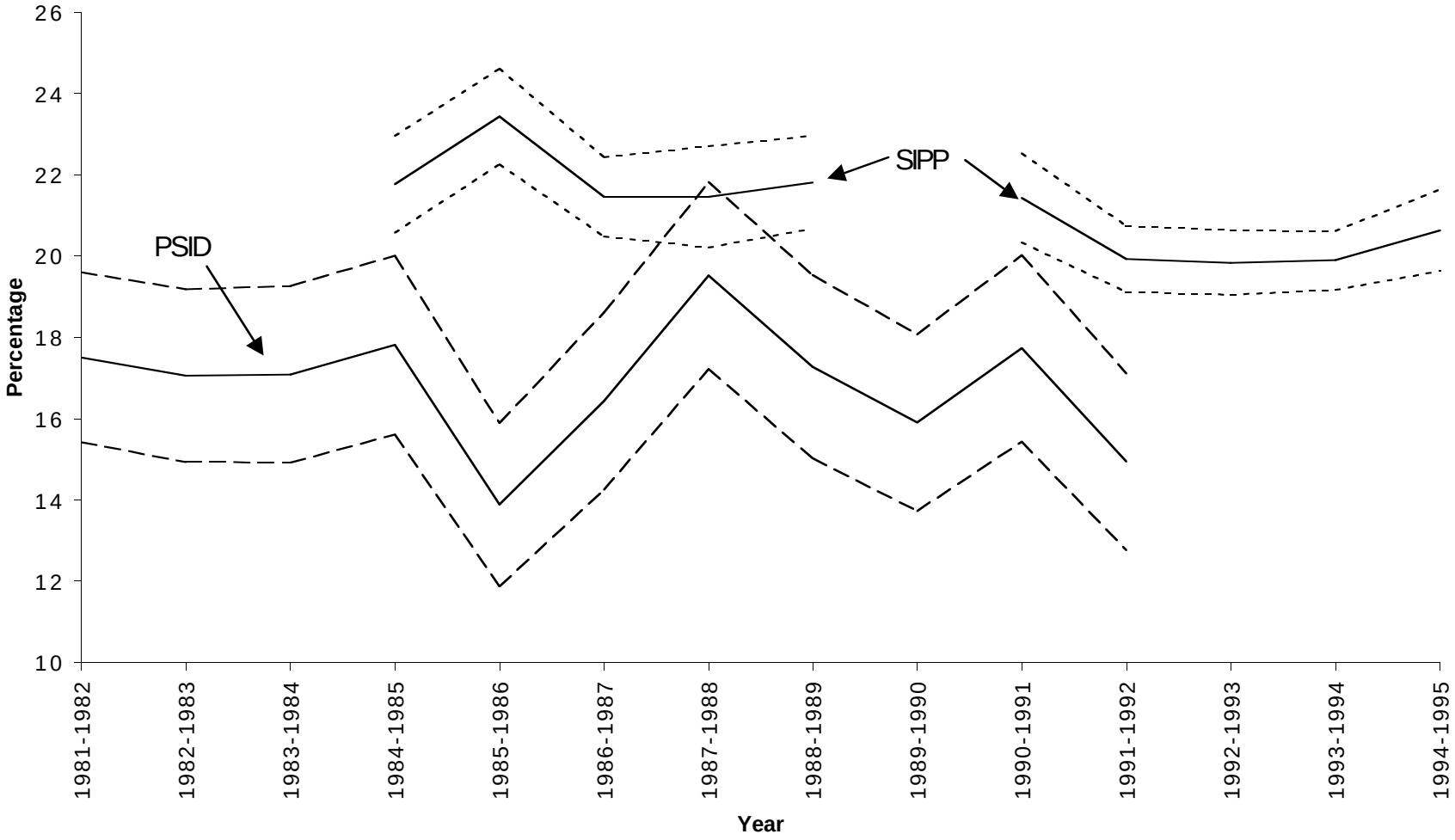
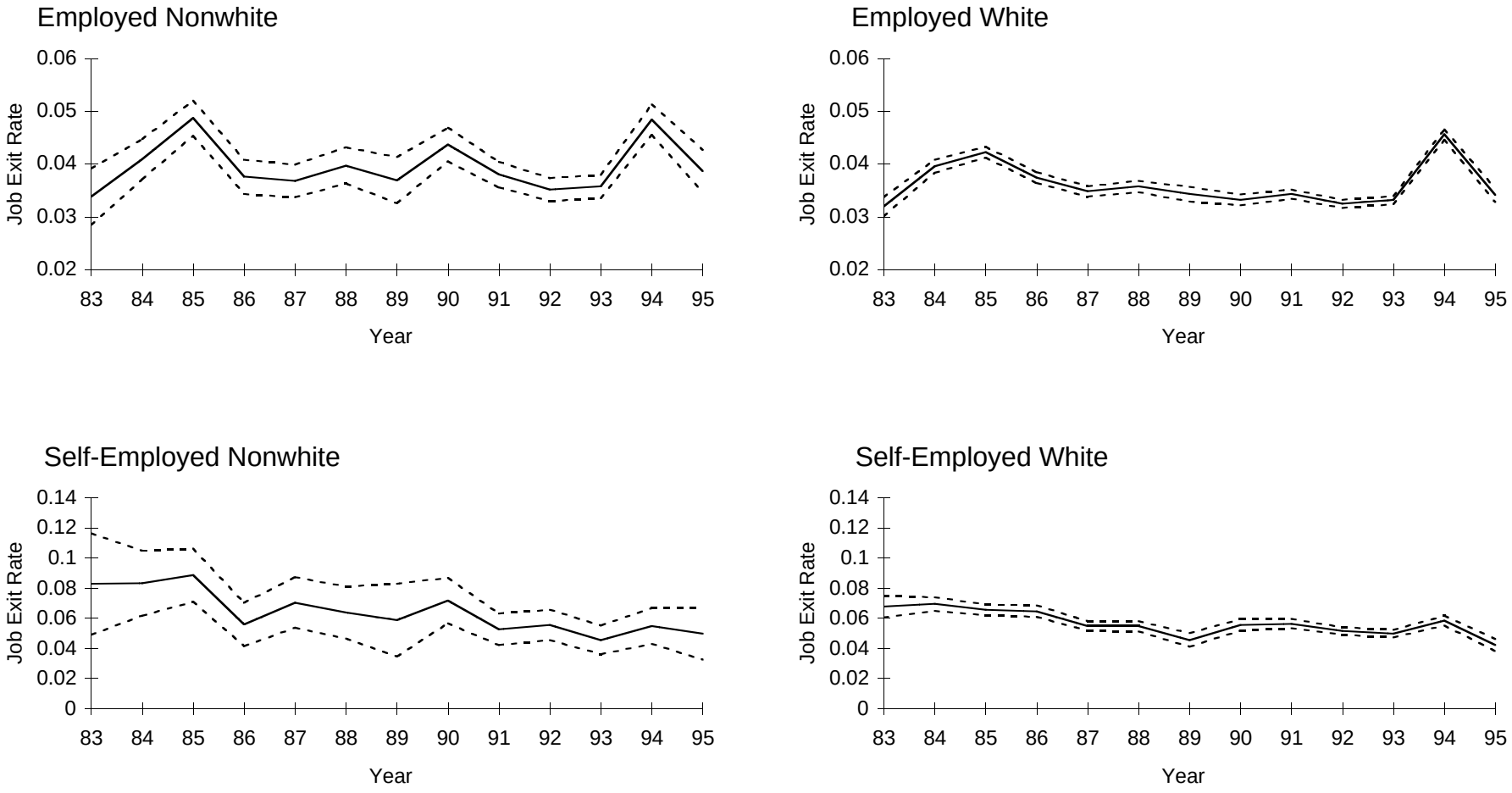
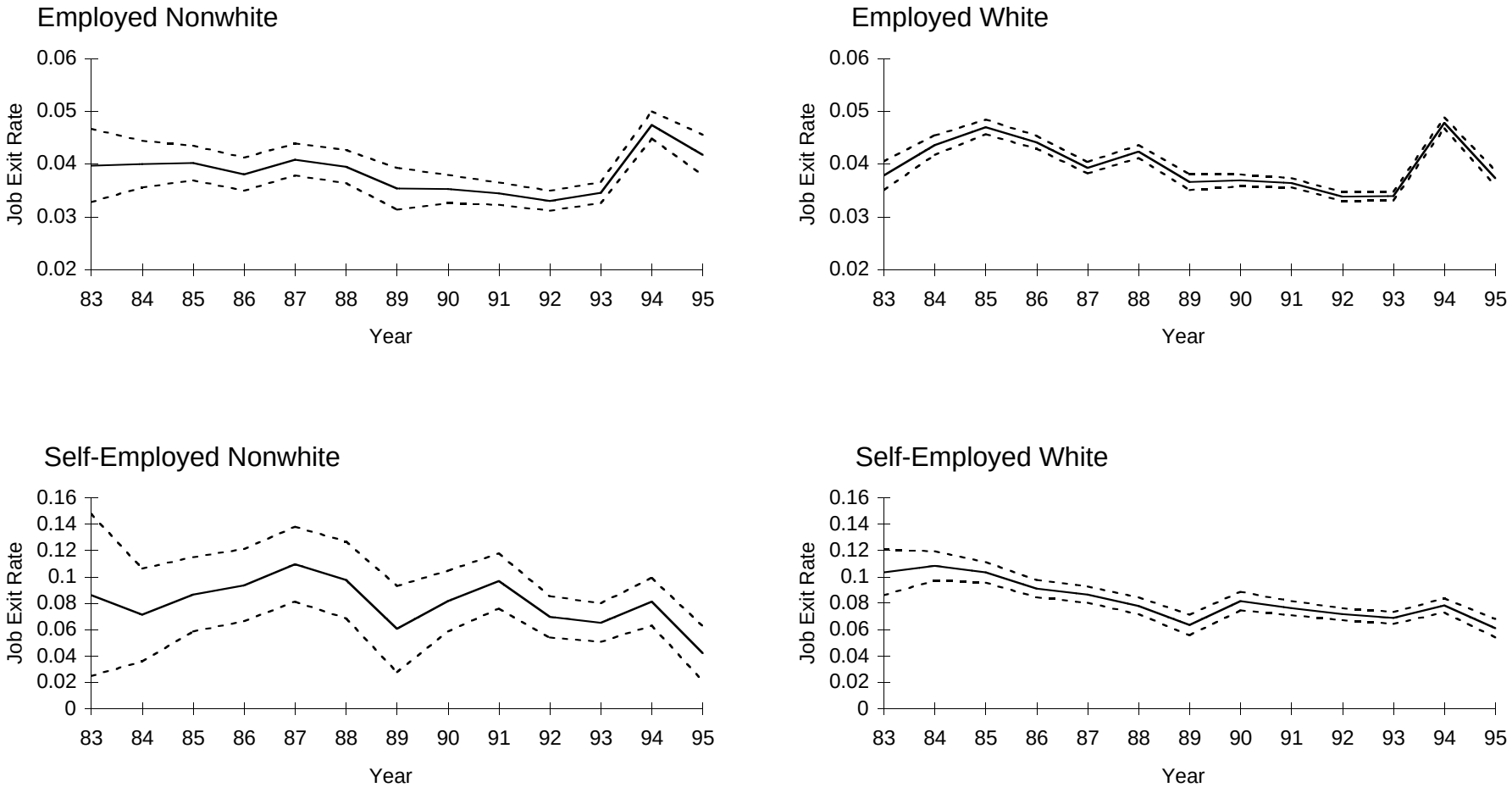


Figure 5a
Monthly Job Exit Rates
for Employed and Self-Employed by Race
SIPP Males



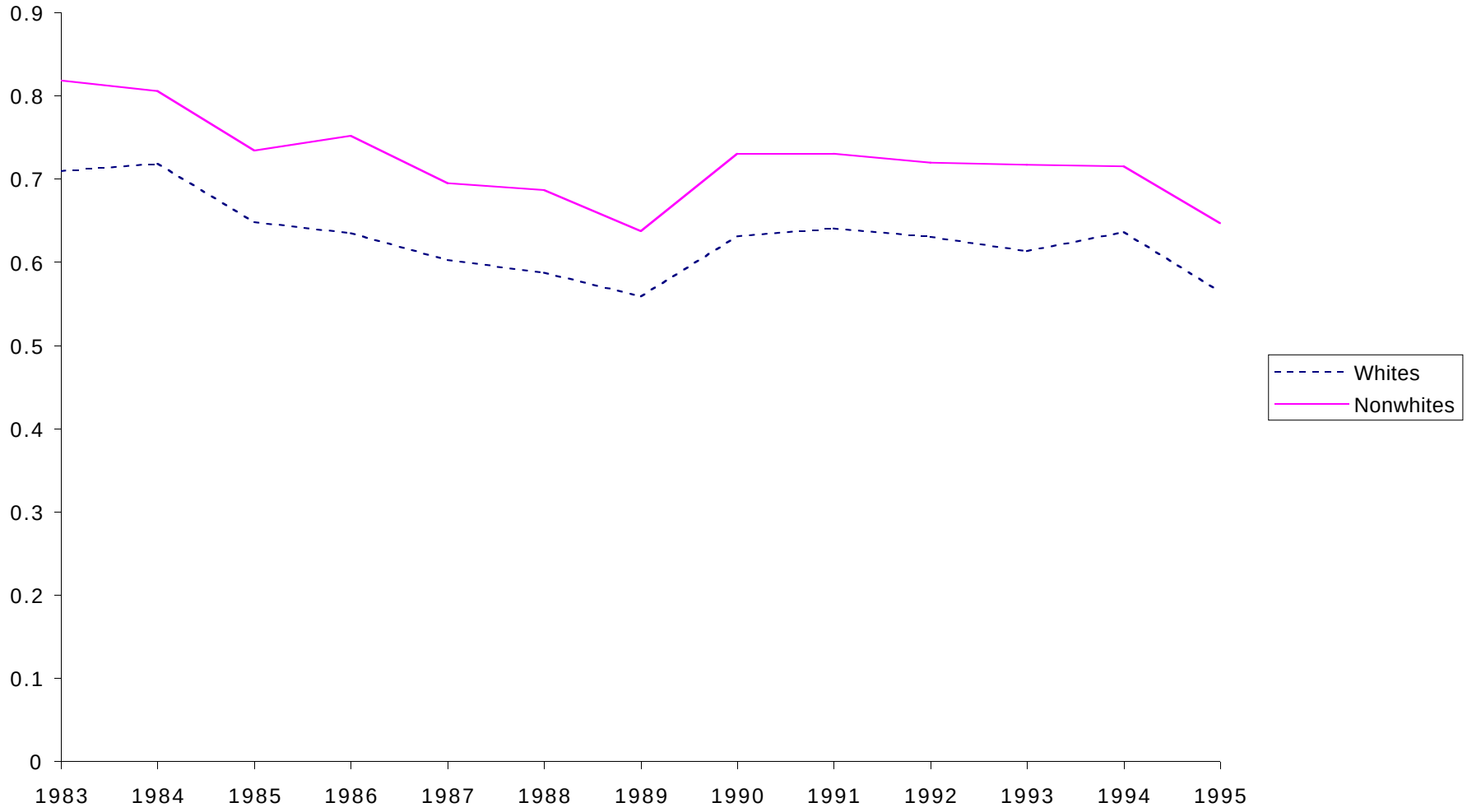
Note: Yearly mean and standard errors of monthly job exit rates.

Figure 5b
Monthly Job Exit Rates
for Employed and Self-Employed by Race
SIPP Females



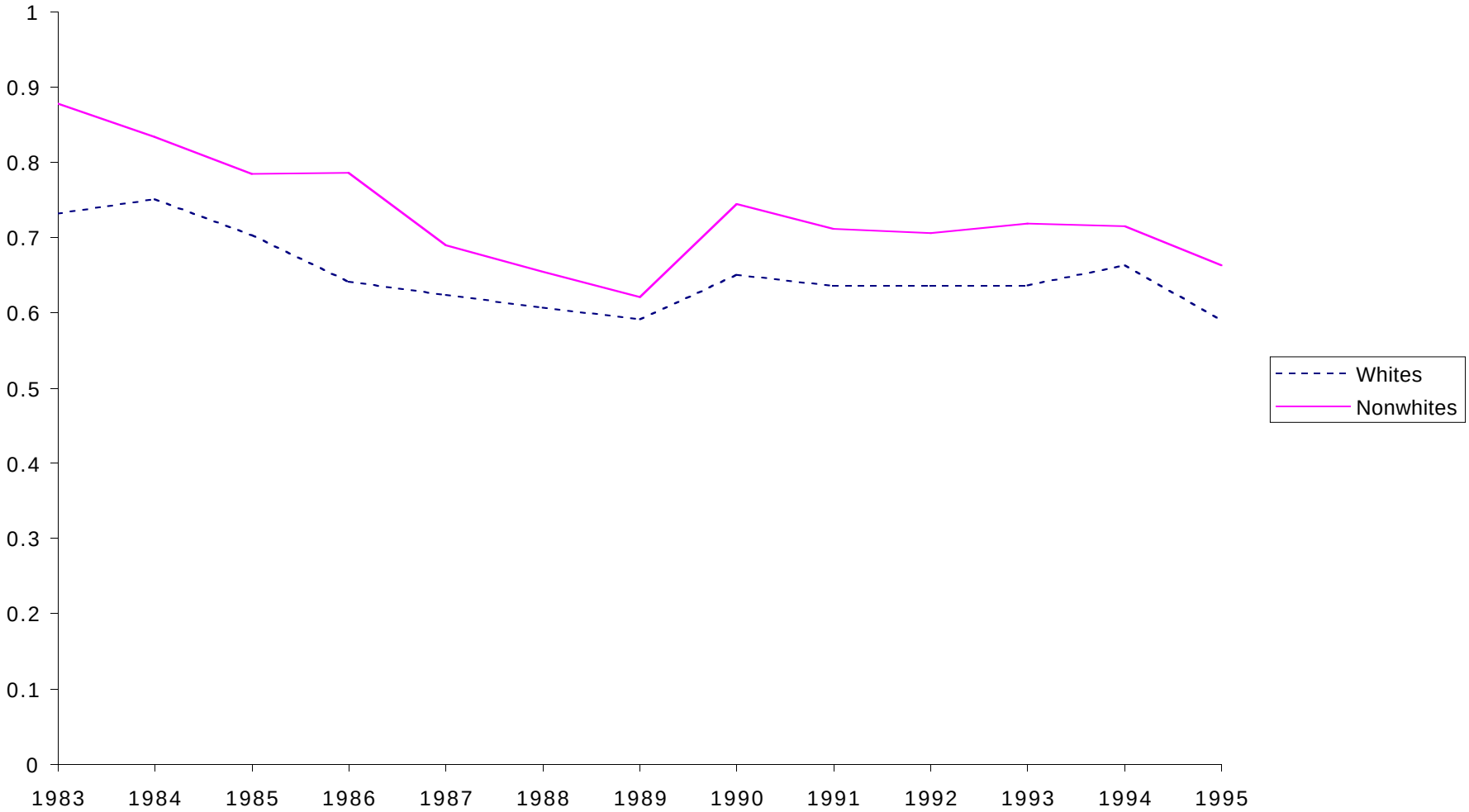
Note: Yearly mean and standard errors of monthly job exit rates.

Figure 6a
Probability that a Job Ending is Followed by a Spell of Non-employment -- SIPP Males



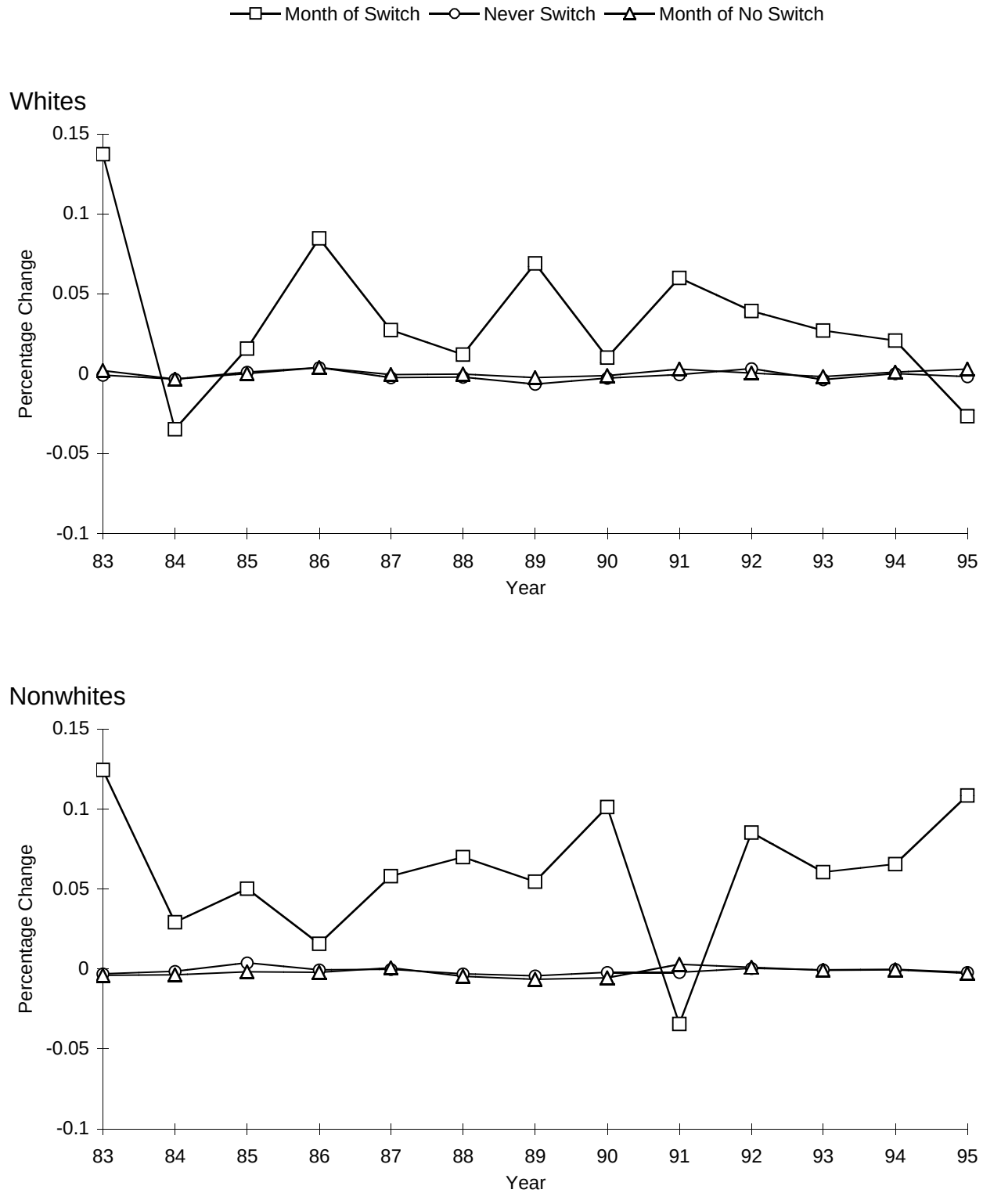
Note: Sample includes all job endings. Yearly probabilities are averages of monthly probabilities.

Figure 6b
Probability that a Job Ending is Followed by a Spell of Non-employment -- SIPP Females



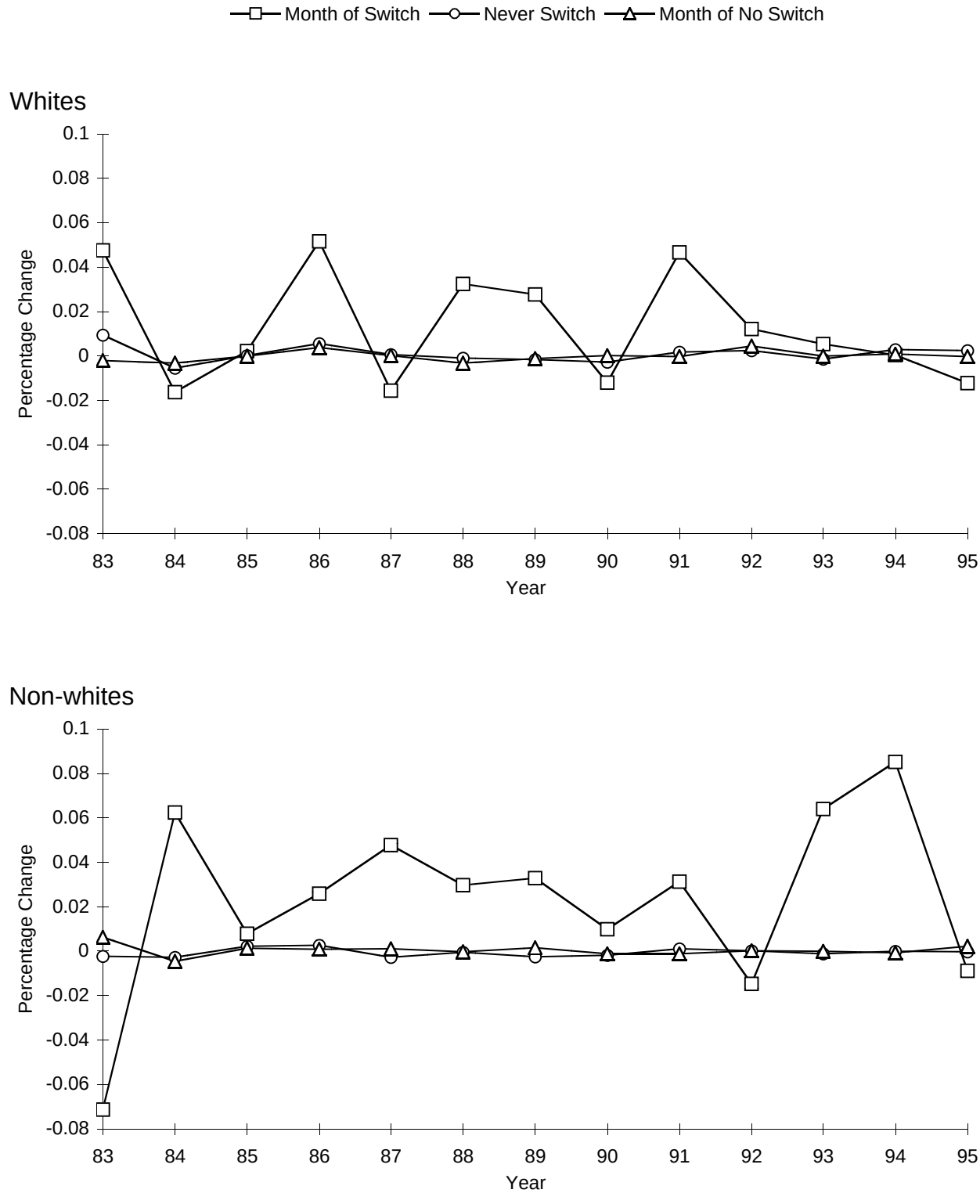
Note: Sample includes all job endings. Yearly probabilites are average of monthly probabilities.

Figure 7a
Percentage Change in Monthly Earnings of Males 20 to 62
by Job Change Status and Race--SIPP Males



Note: Monthly earnings adjusted for age.

Figure 7b
Percentage Change in Monthly Earnings of Females 20 to 62
by Job Change Status and Race--SIPP Females



Note: Monthly earnings adjusted for age.

Table 1
Comparison of Studies

Study	Data Set	Sample Composition				Measure	Includes Transition to Unemployment	Findings
		Sex	Age	Self Employ	Other			
CPS Studies								
Farber (1997a)	Mobility Supplement 79-96	Both	35-64			Pr(tenure>10); Pr(tenure>20)		Decline in the proportion of males with ten or more years tenure, especial after 1993. Increase in long-term employment of females.
Farber (1997b)	DWS 84-96	Both	20-64			Pr(displaced in last three years)		Increase in the displacement of males with high education. No change in the probability of displacement due to plant closing.
Swinnerton and Wial (1995)	Job Tenure Supplement 83, 87, 91	Both	16+	excluded		Pr(not exit in last four years)		Increase, and then decrease, in retention rates for low-seniority workers.
Diebold, Neumark and Polsky (1996)	Job Tenure Supplement 83, 87, 91	Both	16+	excluded	Non-agri	Four year separation rates		Swinnerton and Wial (1995) overstate increase in job instability.
Diebold, Neumark and Polsky (1997a)	Job Tenure Supplement 83, 87, 91	Both	16+	excluded	Non-agri	Four and ten year separation rates		No overall change. Some decline in stability for less educated workers.
Diebold, Neumark and Polsky (1997b)	Job Tenure Supplement 83, 87, 91 Contingent Worker Supp 1995	Both	16+	excluded	Non-agri	Four and eight year separation rates		Some decline in retention in the 1991-95 period, especially for more educated workers.
Jaeger and Stevens (1997)	Job Tenure Supplement 73,78,81, 83,87,91	Male reference persons	20-59	excluded		Pr(tenure<18 months)		Stability in job tenure. Blacks and low educated workers have greater instability.
PSID Studies								
Polsky (forthcoming)	PSID 76-81; 86-91	Male heads	25-54	excluded	Non-agri Full-time	Exit if not employed or tenure in current position declines	Yes	No change in separation probabilities. Increase in involuntary separations. Lower re-employment probabilities. Larger wage loss associated with job switches.
Jaeger and Stevens (1997)	PSID 76-92	Male heads	20-59	excluded		Tenure with employer <18 months		Stability in job tenure. Greater instability for blacks and less educated workers.
Boisjoly, Duncan and Smeeding (1998)	PSID 68-92	Male heads	25-59	excluded	Hours> 1000	Pr (involuntary termination)--including plant closing and layoffs but not firings.	Yes	Increase in involuntary terminations.
Rose (1995)	PSID 1970-79; 1980-89	Male heads	24-48	unknown		Pr(had other main employer in previous 12 months)	No	Increase in number of jobs.
Marcotte (1995)	PSID 76-78; 85-88	Male heads	18-44	excluded		Exit if tenure declines	No	Increase in the probability of a job change, especially for blacks and less educated workers.

NLSY Studies

Monks and Pizer (1996)	NLS-YM 71-78 NLSY 84-90	Males	19-36	Full-time	Two year separation rates	Increase in separation rates especially less educated workers. Increase in voluntary and involuntary separations.
Bernhardt et al (1997)	NLS-YM 71-78 NLSY 84-92	Males	14-37	Non-Hisp, Whites	Two year separation rates	Increase in separation rates.

CPS Supplements that give tenure information

1. Displaced Worker Supplements (DWS)--Jan 84,86,88,90,92,and February 94,96
2. Job Tenure (or Mobility) Supplements -- Jan 1973, 78, 81, 83,87,91
3. Contingent Work Supplement-- February 1995
4. Pension and Benefit Supplements--May 79,83,88 and April 1993

Table 2a
Cox Proportional Hazard
Estimates of Job Ending for Males in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.938***	0.926***	0.937	0.991	1.034	1.021	1.105	1.148
	(0.017)	(0.020)	(0.039)	(0.045)	(0.030)	(0.033)	(0.082)	(0.149)
Year*age	0.999	0.999	1.000	0.998**	0.996***	0.997***	0.995**	0.993*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
Age	1.034	1.016	1.013	1.147	1.352***	1.311***	1.624**	1.730
	(0.048)	(0.058)	(0.111)	(0.127)	(0.116)	(0.128)	(0.355)	(0.668)
Age squared	1.000**	1.000	1.000	1.000**	1.000	1.000	0.999	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Some college	0.945				0.993			
	(0.041)				(0.060)			
College +	0.799***				0.880			
	(0.035)				(0.072)			
Number of jobs	4685	2627	1079	979	2414	1721	501	192
Number of exits	3093	1759	731	603	1598	1152	318	129
Chi2(2)	284.79	182.11	45.37	62.17	114.57	93.10	14.09	8.20
Prob > chi2	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.017
Age at which ∂h/∂yr=0	n.a	n.a.	n.a.	n.a.	8	7	20	20

Notes:
(1) Exponentiated coefficients are shown.
(2) Robust standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, ** 5%, *** 1% level.
(4) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant
(5) Chi square test of joint significance of coefficients on year and year*age

Table 2b
Cox Proportional Hazard
Estimates of Job Ending for Females in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.958**	0.983	0.908**	0.988	0.921***	0.926***	0.960	1.206
	(0.019)	(0.025)	(0.037)	(0.056)	(0.024)	(0.027)	(0.068)	(0.174)
Year*age	0.999**	0.998**	1.000	0.997*	1.000	1.000	0.997	0.989**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.005)
Age	1.106*	1.143**	0.992	1.306*	0.986	0.959	1.322	2.159*
	(0.058)	(0.075)	(0.104)	(0.206)	(0.067)	(0.071)	(0.299)	(0.941)
Age squared	1.000	1.000	1.000	1.000	1.001**	1.001***	0.999	1.002
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Some college	0.963				0.902*			
	(0.039)				(0.051)			
College +	0.813***				0.774***			
	(0.038)				(0.071)			
Number of jobs	4676	2619	1220	837	2411	1563	672	176
Number of exits	3020	1704	790	530	1537	1031	406	101
Chi2(2)	236.25	105.07	78.79	70.84	152.26	83.80	56.90	19.30
Prob > chi2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Age at which ∂h/∂yr=0	n.a	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	17

Notes:
(1) Exponentiated coefficients are shown.
(2) Robust standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, ** 5%, *** 1% level.
(4) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant.
(5) Chi square test of joint significance of coefficients on year and year*age.

Table 3a
Probit Estimates of Probability
Exit is to Nonemployment for Males in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
All Exits								
Year	-0.039	-0.060*	-0.033	0.068	-0.122*	-0.101	-0.145	-0.370
	(0.025)	(0.032)	(0.055)	(0.063)	(0.069)	(0.081)	(0.191)	(0.245)
	[-0.015]	[-0.021]	[-0.012]	[0.027]	[-0.042]	[-0.030]	[-0.055]	[-0.147]
Year*age	0.001	0.001	0.001	-0.002	0.003	0.001	0.006	0.012*
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.006)	(0.007)
	[0.000]	[0.000]	[0.000]	[-0.001]	[0.001]	[0.000]	[0.002]	[0.005]
Age	-0.127**	-0.166**	-0.081	0.096	-0.387**	-0.234	-0.812	-0.982*
	(0.061)	(0.076)	(0.141)	(0.149)	(0.188)	(0.218)	(0.566)	(0.558)
	[-0.047]	[-0.057]	[-0.030]	[0.038]	[-0.132]	[-0.069]	[-0.309]	[-0.391]
Age squared	0.001***	0.001***	0.001	0.001***	0.002***	0.002***	0.005***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.002]	[0.000]
Some college	-0.132**				-0.388***			
	(0.060)				(0.151)			
	[-0.049]				[-0.132]			
College +	-0.517***				-0.681***			
	(0.060)				(0.193)			
	[-0.193]				[-0.232]			
Chi2(2)	3.70	4.85	0.61	1.40	13.76	24.75	5.47	6.74
Prob>chi2	0.158	0.088	0.736	0.497	0.001	0.000	0.065	0.034
Number of observations	3263	1763	757	736	1798	1334	317	141
Age at which ∂Pr/∂yr=0	39	60	33	34	41	101	24	31

Notes:
(1) Sample includes all exits.
(2) Robust standard error in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant
(6) Chi square test of joint significance of coefficients on year and year*age

Table 3b
Probit Estimates of Probability
Exit is to Nonemployment for Females in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
All Exits								
Year	-0.092***	-0.110***	-0.029	-0.150**	-0.073	-0.034	-0.224	0.005
	(0.026)	(0.033)	(0.053)	(0.065)	(0.058)	(0.070)	(0.147)	(0.305)
	[-0.035]	[-0.040]	[-0.012]	[-0.059]	[-0.026]	[-0.012]	[-0.082]	[0.002]
Year*age	0.002**	0.002**	0.000	0.004**	0.002	0.001	0.005	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.009)
	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.000]	[0.002]	[0.001]
Age	-0.193***	-0.227***	-0.056	-0.299*	-0.143	-0.023	-0.553	-0.431
	(0.062)	(0.078)	(0.137)	(0.157)	(0.139)	(0.161)	(0.362)	(0.924)
	[-0.074]	[-0.083]	[-0.022]	[-0.118]	[-0.050]	[-0.008]	[-0.202]	[-0.145]
Age squared	0.001***	0.001***	0.001	0.000	0.000	0.000	0.001	0.005
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]
Some college	-0.224***				-0.132			
	(0.058)				(0.135)			
	[-0.085]				[-0.046]			
College +	-0.248***				-0.073			
	(0.063)				(0.203)			
	[-0.095]				[-0.026]			
Chi2(2)	26.22	23.44	2.50	6.25	4.76	0.76	9.99	1.53
Prob>chi2	0.000	0.000	0.287	0.044	0.093	0.683	0.007	0.466
Number of observations	3178	1822	773	582	1920	1333	472	111
Age at which ∂Pr/∂yr=0	46	55	174	38	37	34	45	n.a.

Notes:
(1) Sample includes all exits.
(2) Robust standard error in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant.
(6) Chi square test of joint significance of coefficients on year and year*age.

Table 4a
Cox Proportional Hazard
Estimates of Involuntary Job Ending for Males in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.821***	0.806***	0.886	0.897	0.995	1.041	0.624**	1.028
	(0.029)	(0.033)	(0.070)	(0.114)	(0.054)	(0.063)	(0.121)	(0.272)
Year*age	1.003***	1.003**	1.001	1.000	0.997**	0.995**	1.011*	0.997
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.006)	(0.007)
Age	0.795***	0.771***	0.896	1.074	1.432**	1.588**	0.565	1.444
	(0.067)	(0.077)	(0.175)	(0.288)	(0.225)	(0.290)	(0.271)	(0.914)
Age squared	1.000	1.000	1.000	0.999	0.999**	0.999	0.994***	0.998
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Some college	0.729***				0.794**			
	(0.061)				(0.794)			
College +	0.392***				0.484***			
	(0.042)				(0.096)			
Number of jobs	4685	2627	1079	979	2414	1721	501	192
Number of exits	850	567	191	92	545	424	94	27
Chi2(2)	116.12	98.22	12.58	8.24	76.11	62.74	17.43	1.03
Prob > chi2	0.000	0.000	0.002	0.016	0.000	0.000	0.000	0.597
Age at which ∂h/∂yr=0	66	72	121	238	n.a.	8	43	n.a.

Notes:
(1) Exponentiated coefficients are shown.
(2) Robust standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, ** 5%, *** 1% level.
(4) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant
(5) Chi square test of joint significance of coefficients on year and year*age

Table 4b
Cox Proportional Hazard
Estimates of Involuntary Job Ending for Females in the PSID

	Whites				Nonwhites			
	All	<= HS	Some college	College +	All	<= HS	Some college	College +
Year	0.968	0.964	1.048	0.779	0.858***	0.917	0.759*	0.541
	(0.052)	(0.062)	(0.117)	(0.170)	(0.047)	(0.057)	(0.114)	(0.214)
Year*age	0.999	0.999	0.995	1.006	1.001	0.999	1.003	1.017
	(0.001)	(0.002)	(0.003)	(0.007)	(0.002)	(0.002)	(0.005)	(0.013)
Age	1.224	1.196	1.584*	0.655	0.917	1.052	0.755	0.226
	(0.166)	(0.193)	(0.432)	(0.390)	(0.130)	(0.166)	(0.333)	(0.234)
Age squared	0.999*	0.999	0.999	0.998	1.000	1.000	1.000	1.000
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.004)
Some college	0.788**				0.813			
	(0.090)				(0.106)			
College +	0.483***				0.553**			
	(0.074)				(0.140)			
Number of jobs	4676	2619	1220	837	2411	1563	672	176
Number of exits	431	279	101	51	328	232	80	16
Chi2(2)	33.13	20.94	13.39	2.02	55.41	29.14	25.28	3.53
Prob > chi2	0.000	0.000	0.001	0.364	0.000	0.000	0.000	0.171
Age at which ∂h/∂yr=0	n.a.	n.a.	9	n.a.	153	n.a.	92	n.a.

Notes:
(1) Exponentiated coefficients are shown.
(2) Robust standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, ** 5%, *** 1% level.
(4) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant.
(5) Chi square test of joint significance of coefficients on year and year*age.

Table 5a
Probit Estimates of the Probability
Exit is Involuntary for Males in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	-0.078***	-0.079**	-0.023	-0.120	-0.157**	-0.099	-0.704***	-0.126
	(0.027)	(0.032)	(0.058)	(0.087)	(0.061)	(0.069)	(0.242)	(0.224)
	[-0.025]	[-0.028]	[-0.007]	[-0.029]	[-0.056]	[-0.038]	[-0.179]	[-0.032]
Year*age	0.002**	0.001	0.000	0.004*	0.004**	0.002	0.023***	0.004
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.008)	(0.006)
	[0.001]	[0.000]	[0.000]	[0.001]	[0.002]	[0.001]	[0.006]	[0.001]
Age	-0.082	-0.051	0.024	-0.197	-0.286*	-0.147	-1.783**	0.041
	(0.063)	(0.076)	(0.147)	(0.189)	(0.158)	(0.180)	(0.716)	(0.538)
	[-0.027]	[-0.018]	[0.008]	[-0.047]	[-0.101]	[-0.056]	[-0.454]	[0.010]
Age squared	-0.001***	-0.001***	0.000	-0.002***	-0.001***	-0.001	-0.003	-0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
	[0.000]	[0.000]	[0.000]	[-0.001]	[-0.001]	[0.000]	[-0.001]	[-0.001]
Some college	-0.241***				-0.530***			
	(0.063)				(0.149)			
	[-0.079]				[-0.188]			
College +	-0.561***				-0.617***			
	(0.069)				(0.202)			
	[-0.184]				[-0.219]			
Chi2(2)	10.58	12.07	1.95	8.34	17.86	9.27	21.26	0.41
Prob > chi2	0.005	0.002	0.376	0.016	0.000	0.010	0.000	0.816
Number of observations	3164	1724	748	686	1758	1305	311	137
Age at which ∂Pr/∂yr=0	39	79	n.a.	30	39	50	31	31

Notes:
(1) Sample includes all exits.
(2) Robust standard errors in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant
(6) Chi square test of joint significance of coefficients on year and year*age

Table 5b
Probit Estimates of Probability
Exit is Involuntary for Women in the PSID

	Whites				Nonwhites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	-0.016	-0.022	0.042	-0.202	-0.049	-0.042	-0.036	-1.021*
	(0.033)	(0.041)	(0.069)	(0.140)	(0.061)	(0.071)	(0.149)	(0.586)
	[-0.004]	[-0.006]	[0.009]	[-0.029]	[-0.015]	[-0.013]	[-0.012]	[-0.115]
Year*age	0.001	0.001	-0.001	0.006	0.001	0.001	-0.001	0.038*
	(0.001)	(0.001)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.021)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.004]
Age	0.005	-0.015	0.117	-0.419	-0.009	-0.002	0.143	-2.741*
	(0.078)	(0.095)	(0.166)	(0.359)	(0.140)	(0.160)	(0.369)	(1.646)
	[0.001]	[-0.004]	[0.025]	[-0.060]	[-0.003]	[-0.001]	[0.047]	[-0.309]
Age squared	-0.001**	-0.001**	0.000	-0.002**	-0.001	-0.001	0.000	-0.009
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.006)
	[0.000]	[0.000]	[0.000]	[0.000]	[-0.000]	[0.000]	[0.000]	[-0.001]
Some college	-0.155**				0.090			
	(0.071)				(0.142)			
	[-0.035]				[0.028]			
College +	-0.395***				-0.358			
	(0.083)				(0.254)			
	[-0.088]				[-0.110]			
Chi2(2)	0.58	0.87	0.44	3.43	7.67	3.04	13.15	8.26
Prob > chi2	0.750	0.647	0.803	0.180	0.022	0.218	0.001	0.016
Number of observations	3191	1827	777	586	1923	1335	473	111
Age at which ∂Pr/∂yr=0	16	22	42	34	49	42	n.a.	27

Notes:
(1) Sample includes all exits.
(2) Robust standard errors in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) n.a. indicates that function is monotonic in year or that the coefficients on year are not jointly significant.
(6) Chi square test of joint significance of coefficients on year and year*age.

Table 6
Distribution of SIPP Sample
by Employment Status
1983-1995

Year	Primary Job Only	Primary and Secondary Jobs	Self-Employed Only	Unemployed or Out of Labor Force
Males				
1983	70.2	2.8	8.3	18.7
1984	71.4	2.8	8.5	17.3
1985	71.5	3.1	8.8	16.6
1986	71.1	3.2	9.0	16.7
1987	71.1	3.1	9.3	16.6
1988	72.0	3.0	9.6	15.5
1989	69.4	2.8	9.2	18.6
1990	72.1	2.8	8.6	16.4
1991	70.8	2.8	8.4	18.1
1992	69.5	2.7	8.2	19.7
1993	69.5	2.7	7.9	19.9
1994	68.9	2.6	7.9	20.7
1995	62.5	2.2	7.1	28.2
Females				
1983	54.4	2.5	3.1	40.0
1984	55.7	2.4	3.2	38.8
1985	57.1	2.7	3.4	36.9
1986	58.0	2.8	3.6	35.7
1987	58.9	2.8	3.6	34.7
1988	60.2	2.9	3.7	33.2
1989	59.3	2.8	3.6	34.2
1990	60.2	3.2	3.4	33.2
1991	60.4	3.1	3.4	33.1
1992	59.7	3.1	3.5	33.7
1993	59.4	3.0	3.5	34.1
1994	59.3	3.0	3.5	34.3
1995	53.9	2.6	3.2	40.3

Averages of monthly proportions

Table 7a
Cox Proportional Hazard
Estimates of Job Ending
SIPP Males

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.998	1.007	0.988	0.965	0.999	0.998	.990*	1.002
	(0.008)	(0.010)	(0.016)	(0.021)	(0.003)	(0.004)	(0.006)	(0.007)
Year*age	1.000	1.000	1.000	1.001	1.000**	1.000**	1.000**	1.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.922***	.955*	.876***	.827***	.894***	.915***	.859***	.848***
	(0.019)	(0.025)	(0.043)	(0.046)	(0.007)	(0.009)	(0.014)	(0.015)
Age squared	1.001***	1.000***	1.001***	1.001***	1.001***	1.001***	1.001***	1.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	7.816***	7.236***	7.755***	10.980***	6.282***	5.974***	5.892***	7.836***
	(0.192)	(0.226)	(0.390)	(0.716)	(0.057)	(0.072)	(0.106)	(0.162)
Some college	0.811***				.835***			
	(0.017)				(0.007)			
College +	0.727***				.742***			
	(0.017)				(0.006)			
Number of jobs	21963	12734	5039	4190	154685	79545	37782	37358
Number of exits	14632	8812	3341	2479	95211	51473	23426	20322
Chi2(2)	1.23	2.21	0.58	2.68	27.21	34.79	5.04	0.85
Prob > chi2	0.540	0.331	0.749	0.263	0.000	0.000	0.081	0.653
Age at which ∂h/∂yr=0	na	na	na	na	8	8	27	na

Notes (1) Exponentiated coefficients are shown.
(2) Standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, **5%, ***1% level.
(4) Year measured as year plus month divided by 12.

Table 7b
Cox Proportional Hazard
Estimates of Job Ending
SIPP Females

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.998	1.004	0.983	0.999	1.003	1.003	1.001	1.009
	(0.008)	(0.011)	(0.017)	(0.022)	(0.003)	(0.005)	(0.006)	(0.008)
Year*age	1.000	1.000	1.001	1.000	1.000	1.000	1.000	1.000**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age	.919***	.936**	.887**	.886**	.930***	.943***	0.922***	.902***
	(0.021)	(0.027)	(0.044)	(0.052)	(0.009)	(0.011)	(0.016)	(0.019)
Age squared	1.001***	1.001***	1.001***	1.002***	1.001***	1.000***	1.001***	1.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	8.002***	7.714***	8.079***	8.917***	6.017***	5.895***	5.885***	6.533***
	(0.198)	(0.252)	(0.382)	(0.560)	(0.058)	(0.079)	(0.107)	(0.138)
Some college	.858***				.863***			
	(0.017)				(0.007)			
College +	.776***				0.815***			
	(0.018)				(0.007)			
Number of jobs	22530	12214	6001	4315	132315	65954	35581	30780
Number of exits	14426	7980	3896	2550	81658	41740	22145	17773
Chi2(2)	0.98	3.07	1.15	3.18	2.01	12.84	1.05	10.71
Prob > chi2	0.614	0.216	0.563	0.204	0.365	0.002	0.592	0.005
Age at which ∂h/∂yr=0	na	na	n.a.	na	na	na	na	na

Notes: (1) Exponentiated coefficients are shown.
(2) Standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, **5%, ***1% level.
(4) Year measured as year plus month divided by 12.

Table 8a
Probit Estimates of Probability of
Exit to Nonemployment
SIPP Males

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	-.030**	-0.029	-.051*	-0.037	-0.001	.013*	-0.010	-.020*
	(0.015)	(0.019)	(0.030)	(0.038)	(0.005)	(0.007)	(0.009)	(0.011)
	[-0.011]	[-0.011]	[-0.020]	[-0.015]	[-0.000]	[0.005]	[-0.004]	[-0.008]
Year*age	.001***	.001*	.002**	0.002	.000***	0.000	.001**	.001***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
Age	-.183***	-.159***	-.268***	-.252**	-.078***	-0.028	-.110***	-.150***
	(0.041)	(0.053)	(0.092)	(0.100)	(0.013)	(0.018)	(0.026)	(0.029)
	[-0.070]	[-0.059]	[-0.105]	[-0.100]	[-0.031]	[-0.011]	[-0.044]	[-0.060]
Age squared	.001***	.001***	.001***	.002***	.001***	0.000***	.001***	.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
Seam	-.187***	-.268***	-130**	0.011	-.030***	-0.082***	-0.027	.090***
	(0.031)	(0.041)	(0.062)	(0.071)	(0.011)	(0.015)	(0.020)	(0.022)
	[-0.072]	[-0.099]	[-0.051]	[0.004]	[-0.012]	[-0.032]	[-0.011]	[0.036]
Some college	-.183***				-.151***			
	(0.036)				(0.013)			
	[-0.070]				[-0.060]			
College +	-.245***				-.164***			
	(0.040)				(0.013)			
	[-0.093]				[-0.066]			
Chi2(2)	16.29	4.70	11.78	4.88	95.89	60.34	17.10	23.79
Prob>chi2	0.000	0.095	0.003	0.087	0.000	0.000	0.000	0.000
Number of observations	7655	4367	1813	1475	58056	28704	15677	13675
Age at which ∂Pr/∂yr=0	23	27	21	23	2	0	15	21

Notes (1) Sample includes all exits.
(2) Standard errors are in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) Year measured as year plus month divided by 12.

Table 8b
Probit Estimates of Probability of
Exit to Nonemployment
SIPP Females

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	-.031**	-0.018	-0.044	-.062*	.016***	.033***	.019**	-0.016
	(0.014)	(0.020)	(0.027)	(0.036)	(0.005)	(0.008)	(0.009)	(0.011)
	[-0.012]	[-0.006]	[-0.017]	[-0.025]	[0.006]	[0.013]	[0.008]	[-0.006]
Year*age	.001**	0.000	.002**	0.001	.000**	-.001***	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.001]	[0.000]	[-0.000]	[-0.000]	[-0.000]	[0.000]
Age	-.091**	-0.033	-.198**	-0.147	.030**	.059***	0.031	-0.022
	(0.039)	(0.051)	(0.080)	(0.100)	(0.014)	(0.020)	(0.026)	(0.031)
	[-0.035]	[-0.012]	[-0.078]	[-0.058]	[0.012]	[0.023]	[0.012]	[-0.009]
Age squared	.000***	0.000	.001**	.001***	.000***	.000***	.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Seam	-.103***	-.130***	-0.117**	-0.003	-.0812***	-.084***	-.073***	-.090***
	(0.029)	(0.041)	(0.055)	(0.067)	(0.011)	(0.016)	(0.020)	(0.022)
	[-0.039]	[-0.047]	[-0.046]	[-0.001]	[-0.032]	[-0.032]	[-0.029]	[-0.036]
Some college	-.221***				-.196***			
	(0.033)				(0.013)			
	[-0.084]				[-0.077]			
College +	-.285***				-.245***			
	(0.039)				(0.014)			
	[-0.108]				[-0.096]			
Chi2(2)	5.12	1.50	5.48	7.94	12.16	17.75	7.80	1.88
Prob>chi2	0.077	0.473	0.065	0.019	0.002	0.000	0.020	0.391
Number of observations	8293	4361	2350	1582	54705	25185	16312	13208
Age at which ∂Pr/∂yr=0	39	n.a.	25	54	45	42	51	n.a.

Notes: (1) Sample includes all exits.
(2) Standard errors are in parentheses.
(3) Derivative at the mean in brackets.
(4) Significant at the: *10%, **5%, ***1% level.
(5) Year measured as year plus month divided by 12.

Table 9a
Cox Proportional Hazard
Estimates of Job Re-Entry
SIPP Males

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	0.974***	.958***	1.022	0.972	.990***	.975***	0.989	0.987
	(0.010)	(0.011)	(0.025)	(0.033)	(0.004)	(0.005)	(0.009)	(0.011)
Year*age	1.000	1.001*	0.999	1.001	1.000	1.000**	1.000	1.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age	1.031	1.020	1.105	0.868	1.002	1.024*	.938**	.870***
	(0.028)	(0.031)	(0.082)	(0.075)	(0.010)	(0.013)	(0.024)	(0.024)
Age squared	0.999***	.999***	0.999***	1.000*	1.000***	.999***	1.000	1.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	3.947***	3.711***	3.928***	6.487***	2.962***	2.728***	3.041***	3.960***
	(0.136)	(0.153)	(0.295)	(0.707)	(0.041)	(0.048)	(0.089)	(0.145)
Some college	1.972***				2.289***			
	(0.056)				(0.026)			
College +	2.144***				2.228***			
	(0.069)				(0.026)			
Number of non-employment spells	17890	12992	2936	1962	103722	71331	17004	15387
Number of exits	8147	5819	1403	925	49886	33969	8796	7121
Chi2(2)	44.94	45.34	1.06	1.12	60.42	111.58	3.36	4.09
Prob > chi2	0.000	0.000	0.590	0.571	0.000	0.000	0.186	0.129
Age at which dh/dyr=0	172	77	na	na	1703	87	na	na

Notes (1) Exponentiated coefficients are shown.
(2) Standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, **5%, ***1% level.
(4) Year measured as year plus month divided by 12.

Table 9b
Cox Proportional Hazard
Estimates of Job Re-Entry
SIPP Females

	Nonwhites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
Year	.964***	.957***	0.981	0.971	.983***	.982***	.968***	.977*
	(0.010)	(0.012)	(0.023)	(0.032)	(0.004)	(0.005)	(0.009)	(0.012)
Year*age	1.000	1.001**	1.000	1.000	1.000**	1.000**	1.001***	1.001*
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age	1.009	1.016	0.981	0.954	0.989	1.033**	0.898***	.891***
	(0.028)	(0.033)	(0.066)	(0.084)	(0.011)	(0.014)	(0.024)	(0.029)
Age squared	.999***	.999***	1.000	1.001**	1.000***	.999***	1.000***	1.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	3.961***	3.703***	4.134***	5.275***	3.057***	2.857***	3.049***	3.857***
	(0.142)	(0.172)	(0.289)	(0.516)	(0.045)	(0.057)	(0.090)	(0.131)
Some college	1.924***				1.949***			
	(0.051)				(0.022)			
College +	2.134***				2.334***			
	(0.069)				(0.029)			
Number of non-employment spells	20400	14325	3874	2201	104999	71314	19545	14140
Number of exits	8459	5616	1830	1013	46522	30031	9420	7071
Chi2(2)	52.46	25.40	2.61	15.22	36.32	20.24	14.91	3.96
Prob > chi2	0.000	0.000	0.272	0.001	0.000	0.000	0.001	0.138
Age at which dh/dyr=0	92	59	na	-124	63	55	30	na

Notes: (1) Exponentiated coefficients are shown.
(2) Standard errors in parentheses.
(3) Exponentiated coefficients are significantly different from 1 at the: *10%, **5%, ***1% level.
(4) Year measured as year plus month divided by 12.

Table 10a
Net Wage Changes Accompanying Job Switches
SIPP Males

	Non-whites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
All Job Changes								
Year	0.059	-0.003	.173**	0.207	0.011	0.013	0.005	0.004
	(0.046)	(0.050)	(0.071)	(0.188)	(0.015)	(0.022)	(0.022)	(0.036)
Year*age	-0.002	0.000	-.006***	-0.005	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.002)	(0.005)	(0.000)	(0.001)	(0.001)	(0.001)
Age	0.125	-0.067	.613***	0.399	-0.011	0.015	-0.043	-0.048
	(0.122)	(0.128)	(0.209)	(0.483)	(0.039)	(0.057)	(0.061)	(0.096)
Age squared	0.000	0.001	0.000	0.001	0.000***	0.000	.001**	0.000
	(0.000)	(0.000)	(0.001)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	-0.029	-0.061	-0.092	0.096	-0.001	-0.033	0.034	0.022
	(0.105)	(0.117)	(0.161)	(0.357)	(0.033)	(0.049)	(0.049)	(0.074)
Some college	-0.139				0.015			
	(0.116)				(0.038)			
College +	-0.158				0.011			
	(0.121)				(0.038)			
Number of observations	6506	3638	1623	1245	45889	22850	12322	10717
F-test	0.82	0.14	4.33	0.67	0.47	0.19	0.37	0.35
Probability value	0.440	0.872	0.013	0.510	0.628	0.827	0.691	0.706
Job to Job Changes								
Year	0.018	-0.013	.082**	-0.008	0.001	0.006	0.003	-0.014
	(0.017)	(0.023)	(0.034)	(0.037)	(0.006)	(0.008)	(0.010)	(0.015)
Year*age	-0.001	0.000	-.002**	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.058	-0.028	0.222**	0.019	-0.014	0.007	-0.006	-0.073*
	(0.046)	(0.061)	(0.102)	(0.097)	(0.017)	(0.022)	(0.029)	(0.042)
Age squared	0.000	0.000	0.000	0.000	0.000**	0.000	0.000	.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	-0.058	-0.040	-0.078	-0.060	-0.003	-0.010	-0.004	0.009
	(0.038)	(0.054)	(0.078)	(0.069)	(0.013)	(0.019)	(0.022)	(0.029)
Some college	-0.061				0.012			
	(0.040)				(0.015)			
College +	-0.003				-0.009			
	(0.041)				(0.015)			
Number of observations	3303	1705	884	714	27589	12919	7774	6896
F-test	0.80	0.20	3.20	1.92	0.88	1.01	0.18	0.51
Probability value	0.449	0.816	0.041	0.147	0.414	0.363	0.837	0.601

Notes (1) Standard errors in parentheses.
(2) Coefficients significant at the: *10%, **5%, ***1% level.
(3) Year measured as year plus month divided by 12.

Table 10b
Net Wage Changes Accompanying Job Switches
SIPP Females

	Non-whites				Whites			
	All	HS or less	Some college	College +	All	HS or less	Some college	College +
All Job Changes								
Year	-0.013	-0.042	0.033	-0.031	0.004	0.010	-0.008	-0.008
	(0.036)	(0.054)	(0.060)	(0.094)	(0.019)	(0.030)	(0.033)	(0.035)
Year*age	0.001	0.001	-0.001	0.002	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.050	-0.087	0.093	-0.205	-0.012	0.013	-0.004	-0.108
	(0.095)	(0.132)	(0.169)	(0.256)	(0.049)	(0.075)	(0.090)	(0.096)
Age squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	0.063	0.102	0.047	-0.011	0.036	0.025	-0.017	0.117*
	(0.076)	(0.111)	(0.124)	(0.175)	(0.039)	(0.063)	(0.070)	(0.067)
Some college	0.071				-0.059			
	(0.080)				(0.045)			
College +	-0.133				-0.018			
	(0.089)				(0.047)			
Number of observations	6348	3238	1884	1226	38926	17394	11660	9872
F-test	0.28	0.32	0.26	1.23	0.31	0.06	0.31	3.08
Probability value	0.756	0.724	0.772	0.293	0.730	0.940	0.734	0.046
Job to Job Changes								
Year	-0.006	0.000	-0.009	-0.023	0.000	-.022**	.022**	0.001
	(0.017)	(0.023)	(0.027)	(0.058)	(0.007)	(0.010)	(0.010)	(0.017)
Year*age	0.000	0.000	0.000	0.000	0.000	0.001**	-.001**	0.000
	(0.000)	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)	(0.000)	(0.001)
Age	-0.003	-0.003	0.000	-0.031	-0.004	-0.051**	.063**	-0.017
	(0.046)	(0.058)	(0.077)	(0.164)	(0.018)	(0.024)	(0.030)	(0.048)
Age squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Seam	-0.021	0.041	-0.037	-0.127	0.003	0.023	-0.005	-0.014
	(0.034)	(0.047)	(0.053)	(0.093)	(0.014)	(0.020)	(0.022)	(0.030)
Some college	-0.030				-0.032**			
	(0.036)				(0.016)			
College +	-.092**				-0.016			
	(0.039)				(0.016)			
Number of observations	3123	1490	950	683	22202	9063	7054	6085
F-test	0.97	0.18	0.68	0.24	0.40	2.68	3.11	0.00
Probability value	0.381	0.832	0.505	0.786	0.669	0.068	0.045	0.997

Notes: (1) Standard errors in parentheses.
(2) Coefficients significant at the: *10%, **5%, ***1% level.
(3) Year measured as year plus month divided by 12.