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**MEASUREMENTS OF JOB EXITS: WHAT  
DIFFERENCE DOES AMBIGUITY MAKE?**

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# MEASUREMENT OF JOB EXITS: WHAT DIFFERENCE DOES AMBIGUITY MAKE?

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## 1. Introduction

At the theoretical level, variation in job exits with job and worker characteristics represents a potentially important source of variation in labor market earnings and other measures of economic status among major groups in the United States. At the empirical level, the picture is less clear.

The limits of our knowledge about job exits reflect the limits of job exit data that have been available. Longitudinal data sets for large, representative samples for the United States have not been available. Consequently, job exit studies have focused on the experience of some groups in the population (low income workers, young workers, and male household heads, in particular). Relatively little is known about others. Potentially as important, ambiguity in the job exit data used in previous research has complicated the interpretation of empirical results. In data from the National Longitudinal Surveys (NLS), for example, job identification labels are available for interview week jobs, but information about labor market activity and jobs held between interviews is limited. Consequently, studies based on NLS data have generally focused on job exits between interview dates and ignored additional separations.<sup>2</sup> Data from the Panel Study on Income Dynamics (PSID) have also been studied extensively. In contrast to the NLS, job identification labels are not available in the PSID. In turn, job exit studies that have used PSID data have had no choice but to focus on interview week jobs, and then use reported starting dates, tenures, and job characteristics to identify job exits.<sup>3</sup>

Given this situation, this paper turns to job exit data from an alternative source--the 1986 Panel of the Survey of Income and Program Participation (SIPP). The length of the 1986 SIPP Panel survey period is relatively short; the full sample is followed for just 24 months. On the upside, however, the 1986 Panel is relatively large, and it includes all groups in the U.S.

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<sup>2</sup>Two recent exceptions to this interview week job approach should be noted. Light and Ureta (1992) work with data for the earlier NLS cohorts, use dates for jobs held between interviews when dates are available, and interpolate when dates are not available. Farber (1992) works with data for the NLS Youth and uses employment dates for all jobs reportedly held between interviews. As he carefully notes, however, basic job characteristics (including earnings) are not available for all jobs. For a review of the empirical literature on job exits, see Chapter 8 in Devine and Kiefer (1991).

<sup>3</sup>See Brown and Light (1992) for discussion of problems in the PSID job exit data.

population.<sup>4</sup> SIPP interviews are also four months apart, as opposed to annual or bi-annual. Due in part to this frequent interview design, SIPP data on labor market activity are more detailed than data from alternative sources. As important, the Core questionnaire used in all SIPP interviews contains multiple questions about labor market activity at weekly, monthly, and 4-month levels, and identification numbers assigned to individual employment arrangements allow direct measurement of job changes between 4-month reference periods.

This paper uses these detailed SIPP data for a sample of full-time wage-and-salary workers to carry out first round analysis of the importance of ambiguity in job exit data. Specifically, the paper examines the consequences of using broad versus narrow definitions of job exits, where the breadth of an exit definition is based on the amount of information used to determine whether an exit has occurred.

Overall, the findings suggest that ambiguity matters. First, workers in similar situations seem to describe their situations differently. In particular, some workers describe temporary separations as unpaid absence due to layoff, while others describe temporary separations as periods of joblessness. How one handles these differences in description has nonnegligible effects on estimates of the frequency of job exits. If responses to questions about "job" versus "no job" status are used to determine the frequency of permanent exits, some temporary separations get counted. Conversely, if one wants to count temporary separations as job exits (e.g., because they may ultimately turn into permanent exits), some exits are missed if responses to questions about the incidence and reasons for unpaid absence are ignored.

It might be argued that variation in respondents' descriptions of similar situations should concern us only if descriptions vary systematically with job and worker characteristics. If this were not true, differences in exits resulting from differences in description could be treated as classical measurement error. On this count, some of the results presented below are encouraging. For both males and females, exits rates are found to be significantly lower for high wage jobs and jobs that offer health care coverage, regardless of the amount of information used to measure exits. The same holds for exit rates for married workers, workers in families with relatively low incomes (excluding their own earnings), and workers with more potential work experience. On the downside, however, results for union membership, industry, occupation, and demographic characteristics such as race and education appear sensitive to the amount of information used. That is, the results may or may not imply that job exit rates vary significantly with these job and worker characteristics, depending on the types of information used.

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<sup>4</sup>The designated SIPP sample is actually representative for the U.S. population, but attrition is nonnegligible. Weights are available for the samples remaining at each interview and also for initial sample members who remain in the sample for the full duration of the survey. There are problems with the interpretation of the longitudinal weights, however, and therefore they are not used in the work presented here.

In addition to the use of different types of information, the timing of measurement may also affect inferences about variation in job exit rates with job and worker characteristics. During a given year, a worker may leave and then return to a given job one or more times. In turn, some exits during the year will not be counted if one looks only at jobs held during two weeks that are one year apart, as in the case of the annual interview week approach to measurement. If the likelihood of temporary exits varies with job and worker characteristics, it follows that the choice between annual interview week and more continuous measurement approaches will affect inference about variation in job exit rates with job and worker characteristics.

Here, again, the results presented below go both ways. The results for wages, health care coverage, potential experience, marital status, and family income do not appear sensitive to the choice between annual and more continuous measurement methods, but the results for remaining job and worker characteristics generally do appear sensitive to this choice.

The remainder of the paper is organized as follows. Section 2 presents a description of the data set analyzed below, the criteria used to select the sample, and a summary of the sample's characteristics. Section 3 first presents job exit definitions based on varying amounts of available data, and then presents sample job exit proportions, tenures, and nonparametric exit rate and Survivor estimates calculated using the alternative job exit definitions. Section 4 presents results from estimation of a basic job duration model using alternative job exit definitions. Section 5 concludes.

## **2. Data and Sample Summary**

The basic data set used here consists of weekly labor market histories for the first sixteen months of the 1986 Panel reference period constructed from data in the 1986 SIPP Microdata Files.<sup>5</sup> Specifically, for each week during the reference period, there are observations on (i) labor market status, and (ii) main and (if relevant) side employer identification numbers for the job or jobs that the worker potentially held during the week.

### **2.1 Labor Market Status**

Observations on weekly labor market status are based on data generated by a series of questions about labor market activity (without direct reference to specific employment arrangements) in the Labor Force and Reciprocity (LFR) section of the Core 1986 Panel interviews. Most LFR questions refer to activity at the weekly level, but some are aggregated to monthly or 4-month reference period levels. The responses to all of these questions are used here to classify respondents as being in one of the following labor market status categories during each week:

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<sup>5</sup>See Devine (1991) for more detail on the structure and labor market content of SIPP.

- (i) had an arrangement for regular work, worked (or got paid for) some hours, and gave a reason other than layoff if they reported working part-time during the surrounding month;
- (ii) had an arrangement for regular work, but reported unpaid absence for the entire week and gave a reason other than layoff for unpaid absence during the surrounding 4-month reference period;
- (iii) had an arrangement for regular work, worked (or got paid for) some hours, and reported layoff as the main reason if they reported working part-time during the surrounding month;
- (iv) had an arrangement for regular work, but reported absence without pay for the entire week and gave layoff as the major reason for unpaid absence during the surrounding 4-month reference period;
- (v) had no arrangement for regular work, but spent time looking or on layoff; or
- (vi) had no arrangement for regular work and reported no time looking or on layoff.

## **2.2 Employer Identification Numbers**

The assignment of employer identification numbers to particular weeks is based primarily on first and last employment dates that are collected for up to two employers for each reference period in the Employment and Earnings section (EE) of the Core 1986 SIPP interviews. Thereafter, usual hours worked for each employer are used to rank multiple arrangements for a given week as "main" and "side." Exceptions to this procedure are the following. First, in cases where last and first employment dates of sequentially held jobs fall within the same week, the job being left is generally assigned as the main job for the transition week and the new job is assigned as the main job for the next week. The purpose of this assignment rule is to avoid overstatement of multiple-job holding, and the reverse rule is used only when this rule would eliminate the new job entirely from a respondent's work history (which is almost never). Second, if a worker reported having three or more employers during a reference period, supposedly had an arrangement during a given week (i.e., according to data collected in the LFR), and had no employer identification number for that week based on reported employment dates, a dummy employer identification number is assigned.

Note that the adjective "potential" is used here when referring to employer arrangements. This is for good reason. Gaps in arrangements between first and last employment dates within 4-month reference periods are not identified in the EE. So, for example, if the EE data indicate two employer arrangements for a respondent for a given week between the first and last employment dates for a reference period, this does not mean that the respondent worked for both employers during that week. The worker may not have been employed by either employer during that week. The same holds when there is a single employer.

### 2.3 Sample Selection and Characteristic Summary

Several criteria were used to select the sample used here. First, the sample is restricted to workers aged 18 to 60 in the last week of the first reference period who were not self-employed, not enrolled in school, not unpaid family business workers, and not members of the armed forces at any time during the first four reference periods. These restrictions are standard in job exit studies, since the behavior and methods of compensation for workers in the excluded groups generally differ from those of non-student wage-and-salary civilians.

Second, the sample is restricted to workers who reported full-time usual hours (defined as 35 or more hours per week) for the main job held in the last week of the first reference period, reportedly worked some hours during that week, and spent no time on layoff during that week. In what follows, the focus is on main jobs held during the last week of the first reference period and exits from these jobs during the following year. This second set of restrictions translates into satisfaction of the tightest definition of job attachment during the last week of the first reference period.

Third, the sample is restricted to workers with observations on all variables used in the analysis. Specifically, the sample is restricted to workers who had interviews conducted with themselves or proxies for the first four reference periods,<sup>6</sup> did not move into another SIPP sample household during the first four reference periods, did not change rotation groups, had consistent employer identification numbers and interviewer verification flags for previous and new employers discussed in the first four reference periods,<sup>7</sup> provided sufficient information to determine a starting date for the main full-time job held in the last week of the first reference period,<sup>8</sup> reported usual hours and gross monthly earnings for the first reference period for the

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<sup>6</sup>Approximately thirty percent of the interviews for the final sample were proxy interviews (i.e., conducted with another household member).

<sup>7</sup>If an employer identification number matches an employer identification number from the previous reference period, but the interviewer check flag indicates that the job was not discussed in the previous interview, the respondent is dropped. A respondent is also dropped if an employer is flagged as previously discussed, but there is no matching identification number for the previous reference period. Given the purpose of the present analysis, these restrictions are unavoidable. Overall, they result in a sample reduction of about five percent. Note that the characteristics of the dropped cases differ from the average member of the remaining sample. About two-thirds had proxy respondents in each interview, and they are relatively young, less likely to be married, and disproportionately black and Spanish.

<sup>8</sup>A pre-reference-period tenure can be calculated for the arrangement that a respondent regarded as their "main" job during the first 4-month reference period when the respondent was asked about their employment history at their second interview. Since the label "main" used in the Employment History supplement may be based on chronological order or hours (versus the definition based strictly on usual hours that is used here), some main jobs in progress at the start of the survey reference period are not initialized for workers with two or more jobs during the first 4-month reference period. In addition, some data are missing because of nonresponse. The basic requirement used here is a reported starting year (i.e., a starting year that is not imputed) and, after imposing the other sample restrictions, this restriction reduced the sample size just

initial main job (or, if applicable, the gross hourly wage at the end of the first reference period), and had complete information available for additional job and worker characteristics analyzed below.<sup>9</sup>

Table 1 presents a summary of the basic characteristics of the final sample and Table 2 presents a summary of the characteristics of main jobs held during the last week of the first reference period.

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slightly. Months are also available for about 85 percent of all jobs that reportedly started before the survey reference period and these data are used when calculating pre-reference period tenures. For the 15 percent, July is assigned as the starting month.

<sup>9</sup>Data on demographic characteristics, family income, region of residence, work-limiting disabilities, earnings, and other employer arrangement characteristics are taken directly from the Microdata or Longitudinal Research Files for the 1986 Panel or constructed from the raw data in these files. These data availability restrictions have a negligible effect on the final sample size after imposing the other sample selection criteria.

TABLE 1. SAMPLE DEMOGRAPHIC CHARACTERISTICS\*

	MALES Mean (Std. Dev.)	FEMALES Mean (Std. Dev.)		MALES Mean (Std. Dev.)	FEMALES Mean (Std. Dev.)
Age	37.988 (10.818)	37.572 (11.126)	Marital Status:		
			Married Spouse Present	0.737	0.602
			Married Spouse Absent	0.004	0.006
Black	0.070	0.101	Widowed	0.004	0.033
Other Nonwhite	0.025	0.030	Divorced	0.067	0.142
Spanish Origin	0.062	0.062	Separated	0.017	0.027
Immigrant	0.071	0.073	Never Married	0.171	0.189
Education:					
Years Completed (up to 18)	12.992 (2.856)	13.036 (2.547)	Number of Children in Household:		
Highest Level:			under age 1	0.058 (0.236)	0.033 (0.182)
Ph.D.	0.015	0.006	ages 1 to 5	0.269 (0.566)	0.165 (0.431)
Professional	0.012	0.006	ages 6 to 14	0.445 (0.794)	0.373 (0.726)
Master's	0.048	0.052	ages 15 to 17	0.151 (0.410)	0.172 (0.445)
Bachelor's	0.150	0.145	Baby born to self or spouse during year	0.041	0.041
Some College	0.171	0.154			
Associate's	0.044	0.046	Family 12-Month Income excluding Individual's Earnings	12,555 (13,869)	20,796 (19,411)
H.S. Diploma or Equivalent	0.394	0.450			
Vo-Tech	0.015	0.030	Employer-Provided Health Insurance	0.855	0.722
No Diploma	0.152	0.111			
Potential Experience (1986-year last attended school)	17.651 (11.493)	17.369 (12.080)	Region of the U.S.:		
			Northeast	0.213	0.200
			Midwest	0.284	0.272
Veteran	0.348	0.012	South	0.327	0.359
			West	0.176	0.169
Work-Limiting Disability:	0.047	0.042			
Side Job	0.030	0.031	Total Number of Observations	2,987	2,135

\*All characteristics are measured in the last month of the first reference period except for: Family 12-Month Income, which is summed over the second, third, and fourth reference periods; the Baby variable, which indicates a birth in the second through fourth reference periods; and Side Job, which indicates a second job in the last week of the first reference period. Note that numbers of children in the household are numbers of children belonging to either the respondent or their spouse as of the last month in the first reference period, based on the parent/guardian identification number provided for each child. Number of Children Ever Born is based on responses to a Fertility History supplement to the second interview, with births during the second reference period subtracted.



TABLE 2. INITIAL MAIN JOB CHARACTERISTICS\*

	MALES		FEMALES	
	Mean (Std. Dev.)	Mean (Std. Dev.)	Mean (Std. Dev.)	Mean (Std. Dev.)
Usual Hours	44.371 (8.304)	40.935 (5.143)	Industry: Agriculture, Forestry & Fishing	0.025
Average Hourly Earnings	11.332 (6.286)	7.774 (4.090)	Wholesale Trade	0.059
Union Member	0.279	0.154	Retail Trade	0.107
Non-member Union with Contract Coverage	0.030	0.025	Nondurable Manufacturing	0.111
Occupation: Managerial & Professional Specialty	0.263	0.272	Mining & Durable Manufacturing	0.231
Technical, Sales & Administra- tive Support	0.192	0.476	Construction	0.079
Precision Production, Craft & Repair	0.213	0.023	Business & Repair Services	0.044
Personal Services	0.002	0.014	Professional Services	0.102
Other Services	0.071	0.099	Finance, Insurance & Real Estate	0.039
Operators, Fabricators & Laborers	0.229	0.112	Personal Services	0.010
Farming, Forestry, & Fishing	0.030	0.004	Entertainment Services	0.009
			Transportation, Communica- tions, & Public Utilities	0.118
			Public Administration	0.065

\*All variables are measured in the first reference period and pertain to the main job held in the last week. For salaried workers, average hourly earnings equals total earnings for the reference period in the main job divided by usual hours times the number of weeks paid on the job. For hourly workers, reported wages are used unless average hourly earnings are between 100 and 200 percent of the reported hourly wage. Since many workers in the sample work in excess of 40 hours, this adjustment is made to allow for overtime pay premiums.

### 3. Measuring Job Exits

The basic approach taken here is to follow workers for one year after the last week in the first reference period and record exits from the main jobs held in the initial week. Given this approach, the definition of a "job exit" obviously depends on how one defines "remaining in the job." In turn, the description of weekly histories provided in Section 2 should convey the range of choices available for these definitions when using SIPP.

#### 3.1 Job and Exit Definitions

Table 3 provides the nested set of main job definitions and corresponding job exit definitions used here. Note that the nesting is based primarily on the amount of information used, with the EE employment dates serving as the basic source. That is, the job definitions J1 to J5 and the corresponding exit definitions X1 to X5 incorporate varying amounts of the LFR labor market activity data for the periods between the first and last dates of employment reported in the EE in each interview, while the remaining definitions ignore all of the LFR data.

Definitions X1 and X2 are based on all available LFR information about the timing and reasons for unpaid absence, short workweeks (i.e., part-time work in jobs that are normally full-time), weeks without arrangements for regular work, and time spent looking or on layoff during weeks without arrangements. X1 is the broadest exit definition. Under X1, a short workweek due to layoff, a week-long unpaid absence for any reason, and a week-long period without an arrangement for regular work (with or without time spent looking or on layoff) would all be regarded as exits--even though the EE dates indicate potential attachment. Under X2, a week-long unpaid absence for a reason other than layoff would not be counted as an exit, but the other separations listed for X1 would be counted. Under X3, a week-long unpaid absence due to layoff and a week without an arrangement for regular work would both be counted as exits, but a short workweek due to layoff would not be counted as an exit unless the respondent had potential attachments to two jobs. This distinction is made because all hours could be worked on the second job.

Definitions X4 and X5 ignore the data on unpaid absence and short workweeks, but use the weekly information about arrangements for regular work and time spent looking or layoff. Under X4, an exit would be recorded when a respondent has a week without a regular arrangement for pay, regardless of looking or layoff status. Under X5, an exit would be recorded when a respondent reported that they had a week without a regular arrangement for work only if they also reported that no time was spent looking or on layoff. The idea here is to treat the mention of layoff as possibly indicating a temporary layoff and thus continued attachment to the job.

TABLE 3. JOB AND IMPLIED JOB EXIT DEFINITIONS

JOB DEFINITIONS		EXIT DEFINITIONS	
J1	EE=attachment as main job, and LFR=paid for some hours and no mention of layoff	X1	J1=having the job, so LFR=unpaid absence for full week is an exit (or events under X2-X7)
J2	J1 or EE=attachment as main job, and LFR=unpaid absence for a full week for reason other than layoff	X2	J2=having the job, so LFR=reduction in hours due to layoff is an exit (or events under X3-X7)
J3	J1, J2 or EE=attachment as main job, and LFR=reduced hours due to layoff	X3	J3=having the job, so LFR=unpaid absence for a full week due to layoff and reduced hours due to layoff given the existence of a second job are exits (or events under X4-X7)
J4	J1 to J3 or EE=attachment as main job, and LFR=unpaid absence for full week due to layoff, OR LFR=reduced hours due to layoff and second job is held (so main job hours may be 0)	X4	J4=having the job, so LFR=no job is an exit (or events under X5- X7)
J5	J1 to J4 or EE=attachment as main job, and LFR=no job and looking or on layoff	X5	J5=having the job, so LFR=no job with no mention of layoff is an exit (or events under X6-X7)
J6	J1 to J5 or EE=attachment as main job, and LFR=no job and not looking or on layoff	X6	J6=having the job, so EE=attachment as side job (and LFR=anything) is an exit (or events under X7)
J7	J1 to J6 or EE=attachment, but as a side job	X7	J7=having the job, so EE=no attachment (and LFR=anything) is an exit
Year-J7	EE=attachment one year later	Year-X7	Year-J7=having the job, so EE=no attachment as of one year later is an exit

Exit definition X6 ignores all of the LFR data, but uses data on usual weekly hours reported for each employer for each reference period. Under X6, an arrangement with the initial main employer may continue, but an exit will be recorded if usual hours worked for this employer fall below usual hours for an alternative employer, i.e., if the initial main job ceases to be the respondent's main job. This hours distinction is not made under definition X7. Under X7, which only uses the EE employment dates, a person might have any LFR labor market status and actually work more hours for another employer, but an exit would not be recorded unless the EE employment dates indicated an end to the arrangement.

Note that all X2 exits are counted under the definition X1, that all X3 exits are counted under the definition X2, etc., but the reverse is not true. In turn, a respondent may have two distinct separations under one of the broader exit definitions, but one or both of these exits may not get counted under a more narrow definition. For example, short workweeks and week-long unpaid absences due to layoff would not be counted as exits under definitions X5-X7.

Also note that the definition X7 should not be interpreted as the definition of a "permanent" exit. As shown below, it is possible for an X7 exit to be followed by a return. Similarly, a separation that initially satisfies only one of the broader exit definitions may be temporary, but there is no guarantee. For example, a separation recorded initially as an unpaid absence may subsequently satisfy the more narrow definitions based on LFR job status or EE employment dates. This may happen because a respondent's situation changes over time. Alternatively, a respondent's interpretation or description of their circumstances might change between interviews, or a proxy respondent might provide a description of the same circumstances that differs from the individual's own description in an earlier interview. In addition, perceptions and descriptions of the same set of circumstances may vary across workers or interviewers. Consequently, the same type of exit may get recorded through responses to different questions by different workers. Finally, the timing of events affects the reporting of X6 and X7 exits. These are based on first and last dates of employment during each reference period. Consequently, only separations at the start or end of a reference period are picked up. In sum, the lines drawn between exit definitions are not necessarily based on true differences in circumstances. They are based on how much available information gets used.

The definitions X1 to X7 are based on continuous measurement of job status over the course of the year. In addition, Table 3 includes Year-X7, which is an exit definition based on observations on employer attachments during the last weeks of the first and fourth reference periods, i.e., two weeks that are one year apart, as in an annual interview week framework. No exit is recorded under Year-X7 if an attachment is indicated one year later. In particular, exits under definition X7 are not counted unless the respondent reported no attachment during the last week of the year. As in the case of an X7 exit, however, there is no guarantee that a Year-X7 exit from an employer is permanent. A lack of attachment in the last week of the year may simply be a temporary separation.

### 3.2 Exit Frequencies

Table 4 presents a summary of the frequency of first job exits under the alternative exit definitions. Focusing on the first columns, which report the numbers of exits and sample proportions for males and females, one observes some major differences between definitions. In particular, the number of exits changes substantially when one moves from X1 to X2, X2 to X3, X3 to X4, and X5 to X6. Note, however, that the changes between X4 and X5 are relatively small--when workers report having no regular arrangement for work during weeks between reported employment dates, they generally do not indicate that they are looking or on layoff. The small changes between X6 and X7 also indicate that main jobs rarely become side jobs. Large changes also appear between X7 and Year-X7; when temporary separations during the year are counted, the number of exits increases by more than 10 percent for both males and females.

The additional columns in Table 4 summarize returns to the initial main jobs during the year that the workers are followed, with and without employment elsewhere. As noted above, an X7 exit may be temporary. In this particular sample, about one out of seven workers returns to their main jobs following an X7 exit--and more than one-third of these returning workers report employment elsewhere before returning.

**TABLE 4.A JOB EXIT FREQUENCY UNDER ALTERNATIVE EXIT DEFINITIONS: MALES**

<b>Exit Definition</b>	<b>Number with an Exit and Number as Proportion of Sample</b>	<b>Proportion of First Exits that Return to Job</b>	<b>Proportion of First Exits followed by Employment Elsewhere</b>	<b>Proportion of First Exits followed by Employment Elsewhere and Return</b>
X1 LFR=Unpaid absence without pay for full week for any reason (X2+ included)	713 0.239	0.459	0.429	0.034
X2 LFR=Reduction in hours due to layoff (X3+ included)	596 0.200	0.337	0.532	0.042
X3 LFR=Unpaid absence for full week due to layoff OR LFR=Reduced hours due to layoff and second job is held (X4+ included)	527 0.176	0.277	0.607	0.049
X4 LFR=No job (X5+ included)	485 0.162	0.212	0.666	0.054
X5 LFR=No job with no mention of layoff (X6+ included)	478 0.160	0.203	0.676	0.054
X6 EE=Switch to side job (X7+ included)	451 0.151	0.142	0.727	0.058
X7 EE=No attachment	444 0.149	0.140	0.721	0.054
Year-X7 EE=No attachment one year later	403 0.135	?	0.710	?

**TABLE 4.B JOB EXIT FREQUENCY UNDER ALTERNATIVE EXIT DEFINITIONS: FEMALES**

Exit Definition	Number with an Exit and Number as Proportion of Sample	Proportion of First Exits that Return to Job	Proportion of First Exits followed by Employment Elsewhere	Proportion of First Exits followed by Employment Elsewhere and Return
X1 LFR=Unpaid absence without pay for full week for any reason (X2+ included)	669 0.313	0.451	0.380	0.027
X2 LFR=Reduction in hours due to layoff (X3+ included)	529 0.248	0.282	0.505	0.036
X3 LFR=Unpaid absence for full week due to layoff OR LFR=Reduced hours due to layoff and second job is held (X4+ included)	486 0.228	0.224	0.558	0.039
X4 LFR=No job (X5+ included)	471 0.221	0.191	0.577	0.040
X5 LFR=No job with no mention of layoff (X6+ included)	470 0.220	0.187	0.579	0.040
X6 EE=Switch to side job (X7+ included)	452 0.212	0.135	0.619	0.044
X7 EE=No attachment	448 0.210	0.129	0.614	0.038
Year-X7 EE=No attachment one year later	402 0.188	?	0.585	?

### 3.3 Tenure, Survival, and Exit Rates

Table 5 provides a summary of completed tenures under the alternative exit definitions. Note that the approach used to calculate tenure under the definitions X1 to X7 is symmetric around the last week of the first reference period. Under X1, for example, tenure accumulated by the last week in the first reference period is calculated by following a worker's history backward from that week and adding weeks as long as job definition J1 is satisfied; if J1 is satisfied in all weeks, then tenure at the start of the first reference period implied by the Employment History starting date are added. Tenure accumulated under job definition J1 between the last week of the first reference period and the first X1 exit is then added to this number to get the tenure reported here for X1. The same procedure is followed for X2 to X7.

The tenure measures used in the last two rows, Year-X7 and H-Year-X7, are both based on definition Year-X7 for an exit. In both cases, tenure is measured up until the first X7 exit from the main job held in the last week of the first reference period unless the respondent holds the same job in the last week of the fourth reference period. If this is the case, X7 exits during the year are ignored. The distinction between the last two rows is the measurement of tenure accumulated by the last week of the first reference period. Year-X7 tenure is based on the starting date reported for the main employer within the first 4-month reference period if it falls after the first day of the reference period, regardless of the date reported in the Employment History, and the Employment History date is used otherwise. This is the same as the approach used under definition X7. In contrast to this, H-Year-X7 uses the Employment History starting date and ignores starting dates within the first reference period. That is, H-Year-X7 ignores all gaps in the arrangement during the 16 months of the SIPP survey period for jobs that have Employment History starting dates before the start of the survey reference period.

With the exception of changes that appear when all unpaid absences and short workweeks are treated as separations, changes in the centers and tails of the completed tenure distributions are not very large for either males or females. The more interesting aspect of the changes that appear in Table 5 are the changes in the direction of change as less information is used to determine whether an exit has occurred and, thus, to measure tenure. The means follow a W pattern.

Figures 1 and 2 present nonparametric estimates of exit rates under the exit definitions X2 and H-Year-X7 over the first 60 months of tenure, and Figures 3 and 4 present the corresponding Survivor function estimates.<sup>10</sup> In each of these figures, the message is the same. The general pattern of change with tenure is similar using the alternative exit definitions, but there are nonnegligible differences in magnitude.

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<sup>10</sup>These estimates are calculated using the life table method with one month intervals.

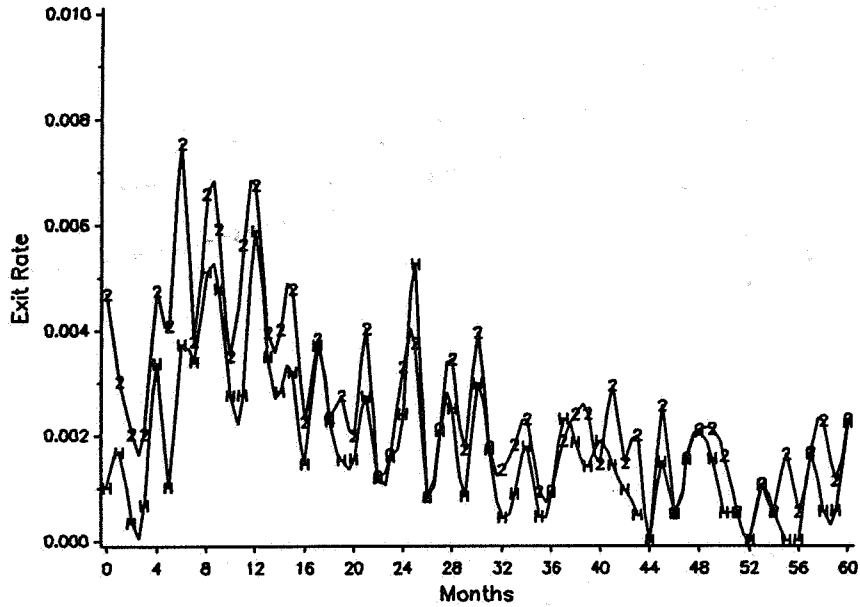


TABLE 5. MONTHS TENURE AT FIRST EXIT -- BY EXIT DEFINITION

Exit Definition	MALES				FEMALES			
	Mean (Std. Dev.)	25	Percentiles 50	75	Mean (Std. Dev.)	25	Percentiles 50	75
X1 LFR=any unpaid absence for full week (X2+ included)	68.802 (90.007)	11	31	95	54.306 (72.451)	10	26	70
X2 LFR=reduction in hours due to layoff (X3+ included)	65.477 (87.196)	11	30	86	51.611 (68.995)	11	24	65
X3 LFR=unpaid absence for full week due to layoff OR LFR=reduced hours due to layoff and second job is held (X4+ included)	62.156 (88.009)	11	27	72	49.802 (84.753)	11	24	60
X4 LFR=no job (X5+ included)	64.313 (90.012)	12	29	75	50.628 (68.051)	12	24	61
X5 LFR=no job with no mention of layoff (X6+ included)	63.864 (90.230)	12	28	74	50.547 (68.097)	12	24	61
X6 EE=switch to side job (X7 included)	61.747 (89.573)	12	27	70	49.153 (67.476)	12	24	59
X7 EE=no attachment (LFR=anything)	62.027 (90.123)	12	27	70	49.417 (67.706)	12	24	60
Year-X7 EE=no attachment as of one year later (X7 value if not returned)	63.387 (93.093)	12	27	70	47.286 (66.830)	11	24	55
H-Year-X7 (Like Year-X7, but uses history starting date)	64.481 (92.781)	12	27	71	47.915 (66.583)	13	24	56

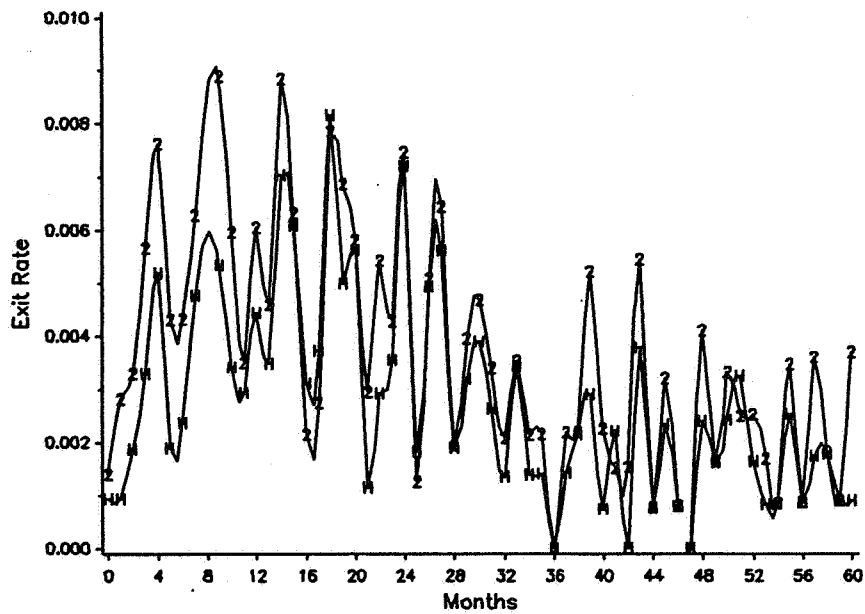
**FIGURE 1. EXIT RATE ESTIMATES: MALES**

2: Definition X2 H: Definition H-YEAR-X7

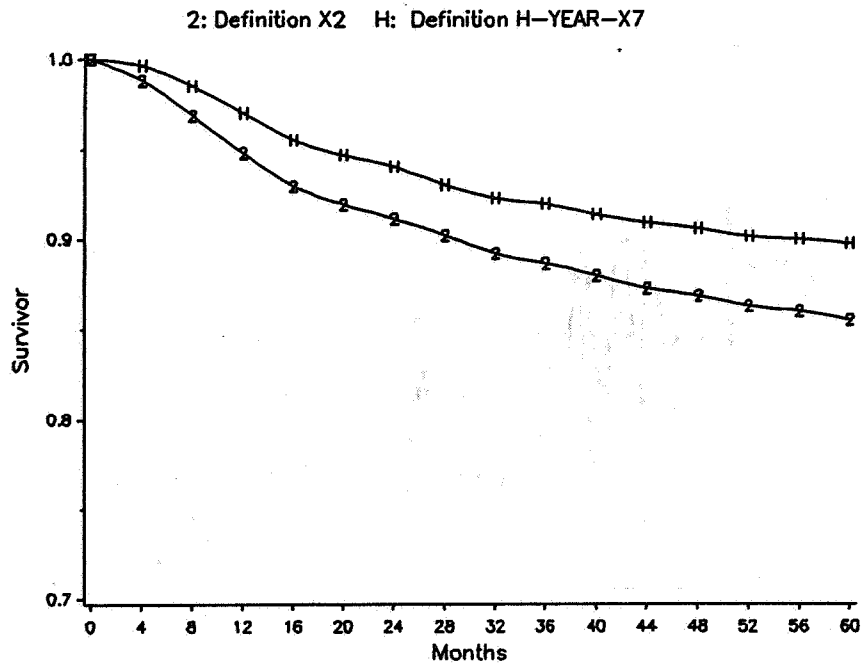


**FIGURE 2. EXIT RATE ESTIMATES: FEMALES**

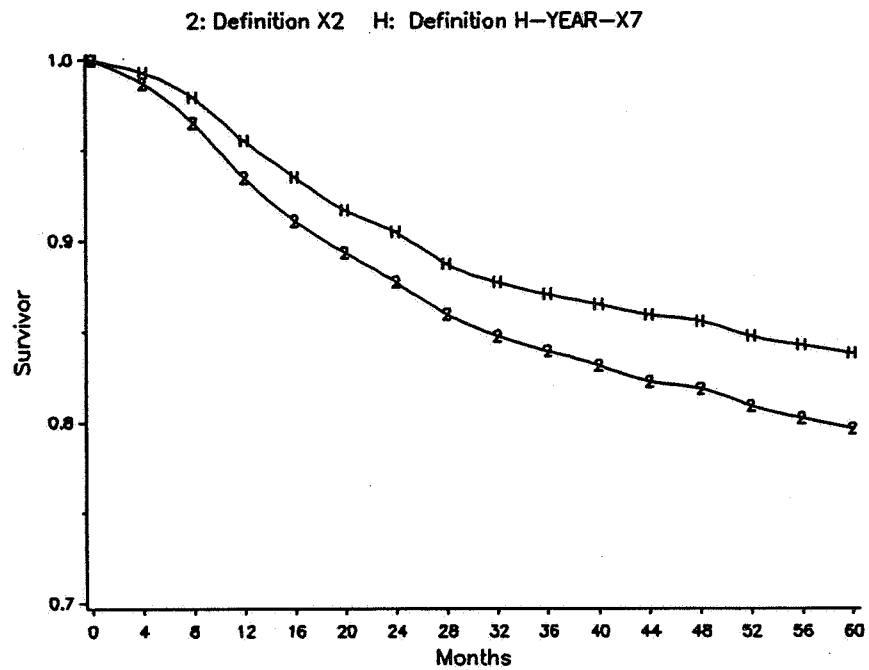
2: Definition X2 H: Definition H-YEAR-X7



**FIGURE 3. SURVIVOR ESTIMATES: MALES**



**FIGURE 4. SURVIVOR ESTIMATES: FEMALES**



#### 4. Analysis of Job Durations using Alternative Job Exit Definitions

The differences in exit frequencies and exit rates that are reported above are striking. However, one might reasonably argue that they need not concern us unless the use of differing amounts of information affects inferences about variation in job exits with job and worker characteristics.

To gain some initial evidence on this issue, basic job duration models were fit separately for males and females using several of the alternative exits measures described above.<sup>11</sup> The specification for the duration distribution in all cases is the loglogistic, with hazard function

$$\lambda(t) = \gamma\alpha t^{\alpha-1}/(1+\gamma t^\alpha),$$

where  $\sigma=1/\alpha$  is the scale parameter for the distribution and  $\gamma=\exp(-x'\beta/\sigma)$ . An attractive feature of the loglogistic specification is that it allows nonmonotonic change in the hazard with duration. Specifically, the hazard first increases with duration and then decreases for  $0 < \sigma \leq 1$ , and decreases with duration for  $\sigma > 1$ . Note also that the model is loglinear, so that the coefficients  $\beta$  indicate the effects of the regressors on the log of duration and  $-\beta$  indicates their (unscaled) effects on the hazard.

The regressors specified as potentially affecting job tenure are those suggested by basic on-the-job search and matching models, and their measurement is standard with a few exceptions. First, education is measured in terms of highest degree or diploma. Second, potential experience is measured as (1986 - the last year of school attendance), under the assumption that people use their most recently acquired education and also due to the within-sample observation that different people move through similar programs at differing rates.<sup>12</sup> The third exception is a dummy variable for private health care coverage through employment.<sup>13</sup> In the U.S., health care benefits have come to represent a large proportion of total compensation when received, and these benefits are not received by all workers. Finally, region of the U.S. is used as an indicator of economic conditions because geographic identifiers (including state of residence) are

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<sup>11</sup>A likelihood ratio test allows rejection of the same models for males and females at all standard levels of significance.

<sup>12</sup>The standard Mincer measure for potential experience, age-education-6, differs from the school-exit date experience measure for a majority of respondents, but the difference is generally small.

<sup>13</sup>Note that this variable is not employer-specific. It is based on responses to three questions. Respondents are asked whether they had private health care coverage during the reference period, whether coverage was through their own jobs, and what months they had the coverage. In some cases, the coverage may be through a previous employer, a union (which is independent of a worker's employer), or through a job which is treated as a side job here.

aggregated in the public-use SIPP Microdata Files for respondents living in sparsely populated states.

Tables 6 and 7 report results for males and females separately, with the alternative methods used to measure tenure indicated at the tops of the columns. The number of exits shown at the bottom of each column indicates the number of exits under the definition used in the column and, in all cases, the sample sizes are 2,987 for males and 2,135 for females.

TABLE 6. LOGLOGISTIC JOB TENURE RESULTS FOR MALES<sup>1</sup>

Variable	X2	X3	X4	X7	Year-X7	H-Year-X7
Intercept	1.337* (.378)	1.658* (.406)	1.805* (.400)	1.673* (.416)	2.138* (.437)	2.181* (.423)
Race:						
Black	0.429*** (.220)	0.705* (.259)	0.756* (.265)	0.909* (.293)	0.749** (.296)	0.715** (.286)
Other Nonwhite	0.224 (.367)	0.097 (.387)	0.022 (.372)	-0.131 (.381)	-0.057 (.408)	-0.075 (.395)
Spanish Origin	0.176 (.232)	0.049 (.248)	0.061 (.245)	0.044 (.258)	0.165 (.278)	0.191 (.271)
Married with Spouse Present	0.291** (.141)	0.312** (.151)	0.421* (.149)	0.415* (.157)	0.410** (.165)	0.396** (.160)
Number of Children:						
Ages Less than 6	-0.071 (.089)	-0.109 (.094)	-0.173*** (.093)	-0.187*** (.098)	-0.132 (.105)	-0.126 (.102)
Ages 6 to 14	-0.116 (.072)	-0.107 (.078)	-0.075 (.079)	-0.096 (.084)	-0.091 (.088)	-0.087 (.085)
Baby during the Year	-0.116 (.257)	-0.066 (.276)	-0.130 (.269)	-0.195 (.277)	-0.168 (.296)	-0.139 (.286)
Family Annual Income (Excluding Person's Earnings) ÷ \$1,000	-0.007*** (.004)	-0.008** (.004)	-0.009** (.004)	-0.011* (.004)	-0.011* (.004)	-0.011* (.004)
Education:						
Graduate Degree	-0.444 (.276)	-0.379 (.294)	-0.473*** (.285)	-0.539*** (.299)	-0.591*** (.311)	-0.583*** (.301)
Bachelor's Degree	-0.126 (.205)	-0.123 (.217)	-0.203 (.211)	-0.254 (.221)	-0.231 (.229)	-0.221 (.222)
Associate's Degree	0.532 (.332)	0.654*** (.361)	0.588 (.358)	0.693*** (.395)	0.912** (.442)	0.884** (.428)
Vo-Tech Certificate	0.622 (.494)	0.482 (.506)	0.301 (.486)	0.142 (.493)	0.314 (.534)	0.306 (.517)
Some College	0.501* (.167)	0.565* (.182)	0.568* (.183)	0.546* (.191)	0.616* (.201)	0.601* (.195)
High School Dropout	-0.136 (.163)	-0.075 (.177)	-0.056 (.176)	-0.027 (.185)	-0.082 (.193)	-0.084 (.187)
Experience	0.103* (.018)	0.111* (.020)	0.109* (.019)	0.111* (.020)	0.107* (.021)	0.103* (.020)
Experience <sup>2</sup> /10	-0.012* (.004)	-0.013* (.004)	-0.014* (.004)	-0.015* (.005)	-0.014* (.005)	-0.013* (.005)
Immigrant	-0.164 (.228)	-0.123 (.246)	-0.137 (.241)	0.051 (.261)	0.055 (.278)	0.060 (.270)
Veteran	0.017 (.129)	-0.009 (.139)	-0.089 (.138)	-0.079 (.146)	-0.170 (.152)	-0.171 (.147)
Work Disability	-0.145 (.240)	-0.199 (.255)	0.230 (.248)	-0.132 (.0269)	-0.154 (.280)	-0.158 (.271)
Region:						
Northeast	0.655* (.176)	0.510* (.189)	0.491* (.187)	0.494** (.195)	0.424** (.206)	0.413** (.200)
Midwest	0.542* (.163)	0.404** (.174)	0.402** (.173)	0.481* (.181)	0.395** (.191)	0.383** (.185)
South	0.434* (.157)	0.327*** (.169)	0.337** (.167)	0.404** (.175)	0.315*** (.185)	0.293 (.179)

TABLE 6. MALES (CONTINUED)

Variable	X2	X3	X4	X7	Year-X7	H-Year-X7
<b>Industry:</b>						
Agriculture, Farming, and Fishing	0.077 (.313)	-0.087 (.337)	-0.278 (.327)	-0.196 (.342)	-0.448 (.358)	-0.342 (.346)
Wholesale Trade	0.354 (.274)	0.252 (.294)	0.261 (.295)	0.360 (.313)	0.153 (.321)	0.129 (.311)
Retail Trade	-0.030 (.225)	-0.154 (.244)	-0.205 (.241)	-0.232 (.215)	-0.356 (.264)	-0.369 (.258)
Business Services	-0.139 (.283)	-0.263 (.302)	-0.397 (.294)	-0.274 (.310)	-0.285 (.330)	-0.294 (.320)
Professional Services	0.565** (.266)	0.514*** (.289)	0.416 (.282)	0.556*** (.300)	0.700** (.322)	0.675** (.312)
Personal Services	-0.728 (.509)	-0.495 (.563)	-0.590 (.545)	-0.686 (.559)	-0.685 (.596)	-0.699 (.579)
Finance, Insurance, and Real Estate	0.059 (.336)	-0.036 (.358)	-0.136 (.347)	-0.120 (.363)	-0.192 (.378)	-0.214 (.366)
Entertainment Services	-0.033 (.506)	0.191 (.579)	0.091 (.557)	0.334 (.614)	0.421 (.693)	0.478 (.668)
Construction	-0.854* (.222)	-0.897* (.244)	-0.865* (.244)	-0.755* (.258)	-0.938* (.268)	-0.924* (.267)
Durable Manu- facturing & Mining	0.041 (.183)	-0.053 (.202)	-0.024 (.203)	-0.036 (.213)	-0.084 (.227)	-0.078 (.220)
Transportation, Communications & Public Utilities	0.626* (.237)	0.549** (.260)	0.412 (.257)	0.392 (.269)	0.358 (.286)	0.341 (.277)
Public Administration	2.226* (.485)	2.033* (.502)	1.846* (.483)	2.213* (.581)	2.453* (.666)	2.381* (.645)
<b>Occupation:</b>						
Managerial and Professional Specialty	0.690* (.205)	0.609* (.219)	0.480** (.216)	0.450** (.226)	0.244 (.236)	0.236 (.229)
Technical Support and Administrative Services	0.395** (.182)	0.277 (.193)	0.159 (.191)	0.177 (.201)	0.063 (.212)	0.070 (.205)
Other Services	0.325 (.264)	0.327 (.286)	0.167 (.277)	0.246 (.294)	-0.033 (.304)	-0.045 (.294)
Personal Services	0.068 (1.109)	0.957 (1.379)	0.820 (1.322)	0.812 (1.351)	0.424 (1.368)	0.405 (1.329)
Craft	0.201 (.151)	0.188 (.163)	0.131 (.164)	0.098 (.172)	-0.057 (.181)	-0.064 (.176)
Union Member	0.127 (.136)	0.152 (.148)	0.255*** (.149)	0.307*** (.159)	0.338** (.169)	0.331** (.163)
Employer Health Plan	0.938* (.148)	1.002* (.157)	0.932* (.154)	0.995* (.160)	0.890* (.168)	0.850* (.162)
Log Hourly Earnings	0.756* (.137)	0.738* (.147)	0.784* (.144)	0.856* (.151)	0.865* (.158)	0.856* (.153)
Scale Parameter ( $\sigma$ )	1.062 (.037)	1.089 (.041)	1.041 (.041)	1.055 (.043)	1.063 (.046)	1.028 (.044)
Loglikelihood	-2003.45	-1848.25	-1710.07	-1598.47	-1489.84	-1475.65
Number of Exits	596	527	485	444	403	403
Number of Observations	2,987	2,987	2,987	2,987	2,987	2,987

<sup>1</sup>Dependent variable is log tenure, with the value of tenure based on the "job exit" definition indicated at the top of the column. Standard errors are in parentheses.

\*Significant at 1 percent level.

\*\*Significant at 5 percent level.

\*\*\*Significant at 10 percent level.

**TABLE 7. LOGLOGISTIC JOB TENURE RESULTS FOR FEMALES<sup>1</sup>**

Variable	X2	X3	X4	X7	Year-X7	H-Year-X7
Intercept	0.744** (.351)	1.114* (.362)	1.483* (.364)	1.611* (.374)	1.523* (.400)	1.564* (.396)
Race:						
Black	0.488* (.178)	0.416** (.182)	0.391** (.180)	0.390** (.185)	0.383*** (.199)	.388** (.197)
Other Nonwhite	0.164 (.327)	0.295 (.336)	0.184 (.331)	0.235 (.341)	0.247 (.366)	0.232 (.361)
Spanish Origin	0.344 (.220)	0.371 (.230)	0.278 (.226)	0.235 (.230)	0.486*** (.264)	0.484*** (.262)
Married with Spouse Present	0.301** (.120)	0.347* (.124)	0.372* (.124)	0.427* (.127)	0.482* (.137)	0.482* (.135)
Number of Children:						
Ages Less than 6	-0.192*** (.101)	-0.224** (.103)	-0.216** (.102)	-0.251** (.104)	-0.256** (.112)	-0.253** (.111)
Ages 6 to 14	-0.126*** (.075)	-0.130*** (.077)	-0.144*** (.077)	-0.163** (.079)	-0.179** (.084)	-0.173** (.083)
Baby during the Year	-0.369*** (.206)	-0.465** (.207)	-0.438** (.207)	-0.443*** (.213)	-0.416*** (.230)	-0.422*** (.227)
Family Annual Income (Excluding Person's Earnings) ÷ \$1,000	-0.006*** (.003)	-0.007** (.003)	-0.007** (.003)	-0.007** (.003)	-0.007** (.003)	-0.007** (.003)
Education:						
Graduate Degree	0.444 (.311)	0.500 (.321)	0.490 (.318)	0.452 (.328)	0.532 (.378)	0.522 (.373)
Bachelor's Degree	-0.160 (.178)	-0.139 (.183)	-0.158 (.181)	-0.210 (.185)	-0.193 (.199)	-0.189 (.196)
Associate's Degree	-0.072 (.244)	-0.077 (.250)	-0.044 (.249)	0.030 (.262)	0.196 (.297)	0.187 (.294)
Vo-Tech Certificate	0.313 (.328)	0.362 (.343)	0.358 (.339)	0.298 (.343)	0.289 (.369)	0.276 (.364)
Some College	0.188 (.152)	0.138 (.155)	0.110 (.155)	0.087 (.159)	0.933 (.168)	0.084 (.166)
High School Dropout	-0.449* (.172)	-0.310*** (.183)	-0.373** (.183)	-0.397** (.187)	-0.341*** (.200)	-0.351*** (.198)
Experience	0.114* (.017)	0.112* (.017)	0.112* (.017)	0.112* (.018)	0.106* (.019)	0.106* (.019)
Experience <sup>2</sup> /10	-0.014* (.004)	-0.013* (.004)	-0.013* (.004)	-0.013* (.004)	-0.012* (.005)	-0.012* (.005)
Immigrant	0.035 (.226)	-0.121 (.231)	-0.120 (.229)	-0.199 (.233)	-0.325 (.250)	-0.318 (.248)
Veteran	-0.765*** (.384)	-0.828** (.386)	-0.859** (.380)	-0.890** (.381)	-1.020* (.392)	-1.030* (.388)
Work Disability	-0.361 (.223)	-0.479** (.230)	-0.487** (.230)	-0.510** (.236)	-0.412*** (.259)	-0.437*** (.256)
Region:						
Northeast	0.271 (.167)	0.205 (.171)	0.170 (.172)	0.134 (.177)	0.158 (.191)	0.139 (.189)
Midwest	0.302*** (.156)	0.259 (.163)	0.214 (.164)	0.160 (.169)	0.206 (.182)	0.203 (.181)
South	0.446* (0.153)	0.431* (.160)	0.348** (.160)	0.302*** (.165)	0.326*** (.177)	0.309*** (.175)



TABLE 7. FEMALES (CONTINUED)

Variable	X2	X3	X4	X7	Year-X7	H-Year-X7
<b>Industry:</b>						
Agriculture, Farming, and Fishing	-0.419 (.546)	-0.468 (.538)	-0.249 (.539)	-0.323 (.543)	-0.109 (.594)	-0.136 (.587)
Wholesale Trade	-0.686** (.296)	-0.715** (.305)	-0.797* (.304)	-0.914* (.310)	-0.666** (.331)	-0.640*** (.328)
Retail Trade	0.148 (.229)	0.192 (.237)	0.125 (.239)	0.052 (.246)	0.158 (.260)	0.144 (.257)
Business Services	-0.588** (.284)	-0.517*** (.295)	-0.584** (.293)	-0.677** (.299)	-0.543*** (.315)	-0.496 (.312)
Professional Services	0.239 (.215)	0.228 (.222)	0.146 (.224)	0.106 (.231)	0.311 (.246)	0.300 (.244)
Personal Services	-0.192 (.316)	-0.068 (.330)	-0.125 (.329)	-0.150 (.338)	0.067 (.362)	0.068 (.359)
Finance, Insurance and Real Estate	0.171 (.247)	0.110 (.253)	0.040 (.253)	-0.042 (.260)	0.054 (.273)	0.045 (.270)
Entertainment Services	-0.742 (.583)	-0.813 (.588)	-0.582 (.644)	-0.267 (.753)	-0.197 (.777)	-0.198 (.768)
Construction	-0.256 (.427)	-0.344 (.441)	-0.422 (.436)	-0.544 (.440)	-0.355 (.470)	-0.371 (.464)
Durable Manufacturing & Mining	-0.129 (.213)	-0.105 (.225)	-0.157 (.229)	-0.226 (.236)	-0.132 (.254)	-0.130 (.251)
Transportation, Communications, & Public Utilities	0.075 (.311)	0.176 (.324)	0.163 (.326)	0.020 (.332)	-0.007 (.346)	-0.016 (.342)
Public Administration	0.690** (.348)	0.614*** (.354)	0.534 (.352)	0.577 (.375)	0.652 (.400)	0.641 (.395)
<b>Occupation:</b>						
Managerial and Professional Specialty	0.754* (.226)	0.531** (.234)	0.328 (.235)	0.342 (.240)	0.218 (.261)	0.210 (.258)
Technical Support and Administrative Services	0.495* (.191)	0.325 (.200)	0.146 (.202)	0.174 (.207)	-0.010 (.223)	-0.012 (.221)
Other Services	0.675* (.238)	0.442*** (.248)	0.275 (.248)	0.333 (.255)	0.195 (.274)	0.183 (.272)
Personal Services	0.369 (.440)	0.139 (.458)	-0.069 (.454)	-0.139 (.460)	-0.370 (.490)	-0.382 (.485)
Craft	0.707** (.352)	0.490 (.370)	0.686*** (.413)	0.818*** (.440)	0.570 (.458)	0.554 (.453)
Union Member	0.193 (.157)	0.230 (.165)	0.315*** (.168)	0.407** (.177)	0.740* (.214)	0.746* (.212)
Employer Health Plan	0.927* (.117)	0.912* (.121)	0.946* (.121)	0.949* (.124)	0.976* (.132)	0.946* (.131)
Log Hourly Earnings	0.930* (.149)	0.921* (.154)	0.865* (.153)	0.877* (.157)	0.974* (.169)	0.969* (.167)
Scale Parameter ( $\sigma$ )	.910 (.033)	0.917 (.035)	0.903 (.035)	0.909 (.036)	0.936 (.040)	0.925 (.039)
Loglikelihood	-1593.04	-1505.63	-1464.55	-1411.61	-1310.31	-1304.72
Number of Exits	529	486	471	448	402	402
Number of Observations	2,135	2,135	2,135	2,135	2,135	2,135

<sup>1</sup>Dependent variable is log tenure, with the value of tenure based on the "job exit" definition indicated at the top of the column. Standard errors are in parentheses.

\*Significant at 1 percent level.

\*\*Significant at 5 percent level.

\*\*\*Significant at 10 percent level.

## 4.1 Job Characteristics

In many ways, the results for job characteristics can be regarded as encouraging. The estimated coefficients for log hourly earnings differ slightly in magnitude as one changes the definition of a job exit and thus the measurement of tenure, but all coefficients are positive and significant at the 1 percent level, as predicted by on-the-job search and matching models. Similar results appear for the coefficients for employer-provided health care coverage. Interpreting the theoretical concept of a wage in broader terms, these health care results can also be regarded as consistent with on-the-job search and matching models. The results for potential experience and its square also appear robust. Together they imply an increase and then a decrease in the job exit rate with accumulated experience. Again, these findings are consistent with search and matching models.

On the downside, however, results for other job characteristics do indicate sensitivity to measurement of job exits. Most striking are the results for union membership. Under the most narrow definitions of a job exit, Year-X7 and H-Year-X7, which ignore temporary separations for workers who have returned at the end of the year, the union effect on log tenure is large and positive for both males and females, significant at the 1 percent level for females, and significant at the 5 percent level for males. Given only these results, one might conclude that exit rates from union jobs are significantly lower. However, as one uses more information and thus includes exits that are more likely to be temporary, the union coefficients drop sharply in size and statistical significance for both males and females. When unpaid absence due to layoff or short work weeks due to layoff are counted as exits, the union effects on job exit rates are not significantly different from zero at any standard level.

The results for occupation, where Laborers are the excluded group, also appear sensitive. Under the broadest exit definitions, X2 and X3, the estimated coefficients for Managerial and Professional Specialty occupations are large, positive, and significant for both males and females. However, when shortened workweeks and unpaid absence due to layoff are not counted as exits, the male and female coefficients for Managerial and Professional Specialty occupations drop substantially in size, and the female coefficient is no longer significant at the 10 percent level. The male coefficient remains significant at the 5 percent level when exits that are more likely to temporary are counted under X4 and X7. However, when data on jobs held one year apart are used, as with Year-X7 and H-Year-X7, the male coefficient for Managerial and Professional Specialty occupations is also small and not significantly different from zero at the 10 percent level. Other occupation results also indicate sensitivity to the handling of shortened workweeks due to layoff and, in the case of females, the treatment of weeks without arrangements between employment dates also matters.

Turning to the results for industry, where the excluded industry is Nondurable Manufacturing, the female results for Public Administration indicate sensitivity to the handling of short workweeks and absence due to layoff. Under definitions X2 and X3, the coefficients

for Public Administration are large, positive, and significant at the 5 percent level; when short workweeks and unpaid absences are not counted as exits, there is no evidence of a significant difference. Note, however, that this sensitivity does not appear for males; the coefficient for Public Administration is positive and significant at the 1 percent level under all exit definitions. The results for Wholesale Trade and Business Services also appear sensitive for females, but the pattern of change is not as clean. The results for X7, Year-X7, and H-Year-X7 indicate that much depends on the measurement of tenure prior to the last week of the first reference period (i.e., the origin week in the current analysis). When all separations between the first and fourth reference periods are ignored for respondents with the same employers in last week of fourth reference period, as done under the definition H-Year-X7, the estimates for Wholesale Trade and Business Services are not significantly different from zero. However, when temporary gaps in attachment are counted as separations, exit rates for females in these industries appear significantly higher. This sensitivity observed for females in Wholesale Trade and Business Services does not appear for males, but the male results for Professional Services appear sensitive. First, when short workweeks are counted as exits under definition X2, the estimated coefficient for Professional Services is positive and significant at the 5 percent level. When short workweeks are not counted as exits, but other separations during the year are counted as exits (i.e., X3 to X7), the male coefficient for Professional Services is smaller and either marginally significant or insignificant at the 10 percent level. However, under the definitions based on jobs held one year apart, exit rates from jobs in Professional Services again appear significantly lower for males. The male results for Transportation, Communication, and Public Utilities appear also sensitive to the treatment of short workweeks and unpaid absence due to layoff; exit rates appear to be significantly lower in these industries under definitions X2 and X3, but not otherwise.

#### **4.2 Personal and Household Characteristics**

Like the results for job characteristics, results for several personal and household characteristics are qualitatively the same across all job exit definitions. For example, there is some change in the magnitude of the estimated effect of being married with spouse present for both males and females, but all estimates are positive and significant at the 1 or 5 percent level. Thus, the results suggest that married males and married females are less likely than their unmarried counterparts to experience separations from their main jobs. Similarly, there is some change in the magnitude of the coefficient for Family Annual Income (excluding the individual's earnings), but all coefficient estimates are negative and significant at the 5 or 10 percent level. As expected, the results generally imply that males and females in higher income families have higher exit rates from their main jobs. The results for immigrant status are also about the same--all coefficients are insignificant at standard levels.

Again, however, the results for many other personal and household characteristics show sensitivity to exit measurement. When X2 is used, it appears that black females have lower exit rates from their jobs than white females, all else constant. However, the estimated coefficient

drops in size and statistical significance when short workweeks due to layoff are excluded, and the coefficient is just marginally significant at the 10 percent level when the one-year measure Year-X7 is used. The race results for males also show sensitivity, but the pattern of change is basically the opposite of the pattern that appears for females. Black males appear significantly less likely to leave their main jobs when short workweeks due to layoff are not counted as exits, but the estimated difference drops dramatically in magnitude and becomes marginally significant at the 10 percent level when short workweeks are counted as exits.

As for variation in exit rates with educational attainment, there is some evidence that high school dropout females have higher exit rates than females with a high school diploma or the equivalent. However, this female dropout difference is large and significant at the 5 percent level only when short workweeks are counted as exits, as under X2, or absences due to layoff are excluded if additional potentially temporary separations are counted as exits, as under X4 and X7. The estimated difference is smaller and significant only at the 10 percent level when the one year measures Year-X7 and H-Year-X7 are used, or when unpaid absences due to layoff are counted as exits under X3. Some different but also somewhat curious patterns appear in the education results for males. Under both one-year measures, males with some college education or an associate's degree appear to have significantly lower exit rates than high school graduates, but none of the other education variables appears significant at the 5 percent level. When temporary gaps are counted as exits, however, the coefficients for those with some college but no degree are smaller but remain significant at the 1 percent level, while the coefficients for an associate's degree become marginally significant at the 10 percent level or insignificant. One set of results that might come as a surprise are the male results for a graduate degree. The coefficient estimates are all negative and significant at the 10 percent level when short workweeks and absence without pay due to layoff are not counted as exits, suggesting that males with graduate degrees are somewhat more likely to have more permanent exits.

The results for the children variables also indicate some sensitivity to measurement. As might be expected, the results indicate significantly higher job exit rates for females with births during the year. Note, however, that the level of significance and magnitude of the coefficient estimates are greatest under definitions X3 and X4, i.e., when short workweeks are not counted as exits, but unpaid absence due to layoff and weeks without regular arrangements are counted as exits. The female results for the number of own children under age 6 also appear sensitive to the treatment of short workweeks due to layoff, but not to additional changes in the exit definition. Turning to the results for males in Table 6, we see that the coefficients for the children variables vary in magnitude, but they are generally insignificant at all standard levels. The exception is the coefficient for the number of children under 6, which is negative and significant at the 10 percent level under definitions X4 and X7. An interpretation of this result is not obvious.

The coefficients for veteran and disability status are all insignificant at standard levels for males, but not for females. The female coefficient for veteran status is negative and significant

at the 5 percent level under definitions X2, X3, X4, and X7, and it is relatively large in absolute value and significant at the 1 percent level under the one-year definitions Year-X7 and H-Year-X7. The disability results for females move in the reverse direction--the coefficients are all negative, but drop in absolute value and statistical significance when temporary exits during the year are ignored.

The results for region of residence for males show some variation in terms of magnitude and statistical significance, but they generally indicate that exit rates are lower for males living in regions outside the West. In contrast, the results for females generally indicate that there is little difference in exit rates between females in the West and females in either the Northeast or the Midwest, while the results for the South appear sensitive to the exit definition used. When gaps between first and last employment dates within reference periods are counted as exits, it appears that female exit rates in the South are significantly lower. When these temporary separations are ignored, the difference is significant only at the 10 percent level.

#### 4.3 Estimates of $\sigma$

Finally, there are the results for movement in the exit rate with tenure, as indicated by the estimates of the scale parameter  $\sigma$ . Under all exit definitions except X3, the results for males are generally indeterminate; under X3, the results are consistent with a monotonic decline in the job exit rate with tenure. Turning to the results for females, we find fairly strong evidence of an increasing and then decreasing job exit rate when using definitions X2, X3, X4, and X7. When the one-year exit definitions are used, however, the evidence of a nonmonotonic exit rate for females is weaker; a decreasing exit rate cannot be rejected at standard levels of significance.<sup>14</sup>

#### 5. Conclusion

This paper has presented first round evidence on the consequences of ambiguity in job exit data for inference about variation in job exit rates with job and worker characteristics. In particular, the paper has examined the consequences of using different types of information, differences in the timing of measurement, and different treatments of pre-reference period tenure.

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<sup>14</sup>Estimation under Gamma and Weibull specifications for the duration distribution, and also using the Cox partial likelihood proportional hazards estimation method produces essentially the same results for the explanatory variables for males as those reported here. The male exit rate is also found to be decreasing with job duration under all specifications, although the Gamma results allows rejection of the Weibull. For females, the Gamma results for females also allow rejection of a Weibull distribution, though not a lognormal; this is consistent with the evidence of nonlinearity found using the loglogistic model. However, some of the regressor results for females appear sensitive to the specification of the duration distribution. In the Cox model, the coefficients for a graduate degree are all significant at the five percent level, and the coefficients for the baby variable are also relatively large in absolute value and significant at the 1 percent level.

In some ways, the findings are comforting. Results for several key variables--including earnings, health care coverage, and potential work experience--do not appear sensitive to the amount of information used to measure exits.

On the downside, however, results for union membership, occupation, industry, several demographic characteristics, and duration dependence do seem sensitive. These findings suggest that additional research which more carefully models the effects of measurement error introduced by different sources of ambiguity would be useful. They also suggest that interpretation of results from job exits studies should proceed with caution.

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