Analyzing Movements over Time in Employment Status and Welfare Participation while Controlling for Seam Bias using SIPP[1]

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Economists and policymakers have long been interested in the determinants of employment and welfare dynamics among less-educated women. Estimating duration models on longitudinal data allows for time changing factors that may differentially impact entries and exits from various labor market states to be identified. However, in using such longitudinal data, researchers must confront particular data-quality issues. In this paper we develop a parametric approach to address seam bias, a common source of reporting errors in longitudinal surveys, in a duration model setting. "Seam bias" refers to the tendency for a much larger fraction of transitions to be reported as occurring at the end of the reference period than would be expected to occur by chance. We apply this approach to the analysis of transitions between employment and non-employment, and transitions between participation and non-participation in welfare, among less-educated single mothers using the Survey of Income and Program Participation (SIPP). We discuss identification of the model, and show that the model is identified without restricting the duration dependence. We find substantial evidence of misreporting. We compare results from our approach to those obtained following the standard approach in applied work of using only the observations from the last month of the reference period, and find some differences in the two sets of estimates. .

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I. Introduction

Transitions into and out of employment and welfare programs are of crucial importance to policymakers, as they determine unemployment rates, welfare caseloads, and the overall well-being of low-income individuals. In this paper we use monthly discrete time duration models to analyze the labor market and welfare participation dynamics of single mothers, a group that has been the focus of much recent policy. We estimate monthly transition rates into and out of employment and welfare receipt using the Survey of Income and Program Participation (SIPP). SIPP is a major longitudinal survey collected by the U.S. Bureau of the Census, and is particularly well-suited to estimate such models because of its detailed information on program participation and employment. Respondents in SIPP are interviewed every four months about the preceding four months - the reference period. However, response in SIPP suffers from telescoping – an apparent shifting of events from the earlier months of the reference period to the last month of the reference period, so that a much greater number of transitions are reported to have occurred in the last month, leading to "seam bias" in the estimation of transition models.

Labor market dynamics for less-educated women have been examined in numerous studies. These studies typically have focused on transitions out of welfare among single mothers, though a few have examined other groups or have examined employment dynamics in addition to welfare. Given the appropriateness of SIPP for analyzing program participation dynamics, most studies have estimated some form of discrete time hazard models using this data.[2] Consequently, these studies have been forced to confront the seam bias problem. The approaches used in the literature can be grouped into three types. One approach is to use the monthly data and to include a dummy variable for the fourth month of the reference period in addition to an indicator variable for each reference period (known as a "wave" in SIPP). Blank and Ruggles (1996) use this approach in their study of entry into and exit from welfare and Food Stamps using the 1986 and 1987 SIPP panels. Fitzgerald (2004) uses a similar approach in his study of welfare exits using the 1986, 1988, 1990, and 1992 panels. While this approach has the benefit of simplicity, it does not allow one to estimate a hazard function that can be used in simulating the model or calculating the effect of changing a variable on expected duration.[3]

[2] Other authors, including Gittleman (2001) and Hofferth, Stanhope, and Harris (2002), use data from the Panel Study of Income Dynamics (PSID) to estimate dynamic models of welfare exit or entry.

[3] One possible 'rule of thumb' adjustment one could make would be to drop the fourth month coefficient in the

A second approach is to collapse the monthly data into data by wave, setting participation and employment indicator variables to be 1 if they were 1 in a subperiod of a wave. Acs, Philips, and Nelson (2003) use the 1990 and 1996 panels to focus on welfare entry, setting welfare participation to be 1 for a wave if participation was reported in at least two months. Ribar (2005) uses the 1992 and 1993 panels of SIPP to examine welfare entries and exits, setting participation to be 1 for a wave if participation was reported for at least one month in the wave. This of course redefines the concept of participation and will result in the loss of short spells. To see this, note that an individual could have a two-month spell of nonparticipation in a reference period and this spell would not be counted in the analysis.

The most common solution to the seam bias problem in the SIPP data is to use only the last month observation from each wave, dropping the three other months. For example, Grogger (2004) uses this approach in the 1986 - 1996 panels to model time on and off-welfare for low-educated women, both unmarried and married. Aaronson and Pingle (2006) also use this approach in their study of employment dynamics among single mothers in the 1990-2001 panels.

We show that this practice leads to two potential problems. First, information on the timing of transitions that occur in months other than the last months is lost, potentially introducing severe distortions to the true employment and welfare participation patterns. A 4-month interval can be quite long when considering transitions into and out of employment, especially for low income women, who usually have difficulty holding a job. As we will discuss in Section III, short spells, particularly those ending in months other than the last months, may be lost completely, while the lengths of other spells may be miscalculated. Second, using only the last month observations results in an efficiency loss, since three-quarters of the data is discarded.

To save the valuable information contained in monthly data and to solve the seam bias problem, in this study we propose a parametric approach to seam bias in a duration model setting. Our methodology is most similar to that used by Pischke (1995) in a non-duration context. Using the SIPP data, Pischke estimates a structural income process jointly with a model of misreporting behavior. The SIPP variable of interest in Pischke's study is monthly income. His results show that adjusting the reporting error leads to rather different implications than using the raw data. We develop a monthly discrete time duration model with three extra parameters

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hazard function and increase the constant in the hazard by the fourth month coefficient divided by four. We discuss this in more detail below.

representing the propensity to underreport transitions in each of the three other months. We show that the model is identified without restricting the form of the duration dependence for spells starting after the beginning of the sample, but that this is not true for spells in progress at the start of the sample. Thus we assume that (nonemployment) employment spells in progress at the start of the sample and (nonemployment) employment spells beginning after the start of the sample share the same misreporting parameters. We also carry out duration analysis using only the last month data, and compare the results from the alternative approach with those from our seam bias correction model. As we show below (and as is intuitively obvious) the hazard functions are not directly comparable, although one can compare the statistical significance of the coefficients across the two approaches. We can make the estimation approaches comparable by examining the effect of individual variables on the expected duration of each type of spell, or by comparing the effect of the variable on the short-run, medium-run and steady-state fraction of time spent in employment (off welfare). Our preliminary results suggest that using only the last month observations leads to overestimates of the expected spell lengths.

The paper proceeds as follows. In Section II we discuss the SIPP data and the extent of the seam bias problem. In Section III we discuss the problems that occur when one uses only the last month observations. In Section IV we discuss our approach. We first outline our assumptions, which we believe to be reasonable. We then outline our approach to estimating parametric duration models in the presence of seam bias, and discuss identification of these models. We also estimate a model with no unobserved heterogeneity, no explanatory variables and a very flexible step function for duration dependence in each hazard function to provide empirical hazard functions from SIPP while correcting for seam bias. We present our empirical results in Section V. We compare results from our approach to those estimated following the standard approach in applied work of using only the last month observation and find differences between the two sets of estimates. We conclude the paper in Section VI.

II. The Data and Seam Bias in SIPP

Our primary data source is the 1986-1993 panels of SIPP.[4] SIPP was designed to provide detailed information on incomes and income sources, as well as labor force and program participation of individuals and households in the U.S. Our sample is restricted to single mothers who have a high school education or less. Since we investigate their employment status, we only consider women between the ages of 16 and 55.[5] We smooth out one-month welfare and off-welfare spells if they are in the middle of the sample period because it is very unlikely to receive public assistance for just one month. However, we keep any one-month spells observed at the beginning or end of the sample period since they may be either the beginning or end of a spell.[6] For the employment and non-employment spells we use the original data with all one-month spells intact because employment status is very unstable among low-educated women and it is common for them to have very short employment and non-employment spells.[7] Since we will use state level variables such as maximum welfare benefits, minimum wages, unemployment rates and whether the state obtained a welfare waiver and introduced positive incentives to leave welfare (carrots) or negative incentives to leave (sticks), we exclude women from the smaller states which are not separately identified in SIPP.

SIPP uses a rotation group design, with each rotation group consisting of about a quarter of the entire panel, randomly selected. For each calendar month, members of one rotation group are interviewed about the previous four months (the reference period), and over the course of any four month period, all rotation groups are interviewed. We call the four months within each reference period *month 1, month 2, month 3* and *month 4.* We will also refer to *month 4* as *the last month*. Empirical evidence in the literature shows that this structure has led to a disproportionate number of employment and welfare transitions being reported as occurring between one wave and the next, i.e. a transition being reported in the last month of the reference

[4] In later drafts we will report results adding the 1996 SIPP and hopefully the 2001 SIPP.
[5] Respondents are chosen based on their education and age at the beginning of the panel. If a single mother got married in the middle of the survey, we keep the observations before the marriage and treat the spell in progress at the time of marriage as right censored.
[6] See Blank and Ruggles (1996) footnote 21 for a discussion of this issue.
[7] Hamersma (2006) investigates unique a Wisconsin administrative data set containing information from all the Work Opportunity Tax Credit (WOTC) and Welfare-to-Work Tax Credit (WtW) applications. The majority of WOTC-certified workers in Wisconsin are either welfare recipients or food stamp recipients. She finds that over one-third of certified workers have less than 120 hours of employment (job duration), while another 29 percent of workers have employment less than 400 hours. Only a little over one-third of workers have employment more than 400 hours. These administrative data show that a significant share of employment spells are less than one month among disadvantaged individuals.

period. This phenomenon is known as *seam bias,* and seam bias is observed for most variables in SIPP (e.g. Young, 1989; Marquis and Moore, 1990; and Ryscavage, 1988). The rotation design guarantees about 25% of transitions should occur in month 1, month 2, month 3 and month 4. Calendar months are equally distributed among the months of the reference period (with the exception of months at the beginning and end of the panel, which appear in the reference period of only some of the rotation groups). Summary statistics show that for our sample more than 45.86 % of job transitions (from non-employment to employment and vice-versa) and 48.39% of welfare transitions are reported to occur in month 4, the last month. Both numbers are far greater than the 25% one would expect.

Following Heckman and Singer (1984a) and the standard duration literature, we distinguish between left-censored spells which are in progress at the start of the sample and fresh spells which begin after the start of the sample for both i) time spent in and out of employment and ii) time spent in and out of welfare.[8] Tables 1.1 and 1.2 provide summary statistics for our sample of single mothers by spell type. Some may be tempted to omit the left-censored spells and simply work with fresh spells. The problem with this approach is two-fold. First, left-censored spells tend to be longer than fresh spells, even when duration is measured from the start of the sample. Second, for the population of unmarried mothers with relatively low education, left censored spells make up a large fraction of the spells in progress even as the sample progresses. For example, individuals in left-censored welfare spells make up 64% of all of those on welfare even 36 months after the sample begins.

Table 1.1 presents summary statistics for employment and non-employment spells. Left-censored non-employment spells also are more likely to be right censored than fresh non-employment spells, and thus those in left censored non-employment spells are more likely to have a long non-employment spell. They are usually more disadvantaged than single mothers in fresh non-employment spells. We can see from the upper panel that single mothers in left-censored spells are less likely to have a high school diploma, less likely to have a previous marriage, more likely to be disabled or have a disabled child, and tend to have more children than those in fresh non-employment spells. The two groups are similar in age and in the proportion of minorities.

[8] Left-censored spells are sometimes called interrupted spells.

In general left-censored employment spells also are more likely to be right censored than fresh employment spells, indicating that those in left censored employment spells are more likely to hold a job for long period. The lower panel of Table 1.1 shows that single mothers in left-censored employment spells are older, less likely to be minority members, more likely to have a high school diploma, more likely to have a previous marriage, less likely to be disabled or have a disabled child , and tend to have fewer children than those in fresh employment spells.

Table 1.2 contains summary statistics for welfare and off-welfare spells. The upper panel shows that welfare recipients differ substantially between left-censored and fresh spells. Those in left-censored welfare spells are more likely to have a very long spell than those in fresh welfare spells, and are generally more disadvantaged than the average recipient in a fresh spell. Compared to those in fresh spells, single mothers in left-censored welfare spells are slightly more likely to be minority members, less likely to have a high school diploma, slightly older, less likely to have a previous marriage, and have more children.

The lower panel shows the characteristics of single mothers in left-censored and fresh off-welfare spells. Again the differences between these two groups are substantial. Grogger (2004) found that in panels prior to the 1996 panel, 95% of SIPP respondents in left-censored off-welfare spells had never received welfare, although we would expect this rate to be lower in our sample of single mothers with relatively low education. Still, on average, single mothers in left-censored off-welfare spells are older, far less likely to be minority members, more likely to have a high school diploma, more likely to have a previous marriage, and have fewer children than those in fresh off-welfare spells. Also it is not surprising that the left-censored off-welfare spells have much high rate of right censoring.

III. Problems with Using Only the Last Month Observations

For employment and non-employment spells, we let $U, U', E,$ and E' denote a fresh non-employment spell, a left-censored non-employment spell, a fresh employment spell and a left-censored employment spell respectively. We construct the following four examples to illustrate the potential problems resulting from using only the last month data. The first example illustrates the loss of a short spell falling between two interviews, as well as the subsequent spell. Assume that a respondent has four spells, as in Figure 1.1. In Figure 1.1, the numbers above the line indicate the survey months and the numbers below the line are reference period months. The first spell is a left-censored non-employment spell ending in a month 1, the second is a fresh employment spell reported to end in a month 3, the third is a fresh non-employment spell ending in another month 3, and the last spell is a right-censored fresh employment spell. Using only the last month data, we would treat this respondent's work history as consisting of a left-censored non-employment spell lasting 32 months and a right-censored employment spell lasting 4 months. We would lose both a 2-month fresh employment spell and a 24-month fresh non-employment spell. In addition, we would miscalculate the spell length of both the left-censored and right-censored spells.

The next example, illustrated in Figure 1.2, shows that using only the last month data may lead to spell lengths being miscalculated, but does not necessarily lead to omission of spells. In Figure 1.2 we keep everything else the same as in Figure 1.1 and only shift the ending point of the second spell, which is also the starting point of the third spell. Now the second fresh employment spell lasts for 5 months with a month 4 in the middle of the spell. For such a case, using only last month data will not lead to the omission of the second and third spells, but only to the miscalculation of the length of all four spells.

We construct another example to show how we can miscalculate the length of a left-censored spell using only the last month data. Assume that a respondent has three spells as in Figure 1.3. The first spell is a left-censored non-employment spell ending in month 3 of the first reference period; the second is a fresh employment spell ending in month 3 of the second reference period; the third is a right-censored fresh non-employment spell. Using only the last month data will record her work history as one non-employment spell, both left and right-censored.

Finally, we construct the last example to show how we can actually misclassify a left-censored spell using only the last month data. Assume that a respondent has three spells as in Figure 1.4. The first spell is the same as the above example, a left-censored non-employment spell ending in month 3 of the first reference period; the second is a completed fresh employment spell; and the third is a fresh non-employment spell censored at the end of the sample. Using only last month data will record her work history as a left-censored employment spell and a fresh non-employment spell. From the last two examples, obviously we will lose all left-censored spells less than or equal to 3 months by switching to the last month data. In addition, the side effects include both miscalculating spell length and even misclassifying spell type for left-censored spells.

To recap, the above four examples show that by using only the last month data, we could lose some spells, misclassify the spell type, and miscalculate the length of spells that remain. Further, the problem is more severe with short spells that are less than 4 months duration and that do not cover a month 4. From these examples it appears to be ambiguous whether using only the last month data will overestimate or underestimate the average duration. It is clear that in general using only the last month observations will lead to an overestimate of the length of left-censored spells[9]. However, for fresh spells using only the last month data may underestimate or overestimate the length of an observed fresh spell. The intuition is that both the start and finish of a fresh spell could be mistaken due to seam bias.

Of course, the above four examples compare the last month data to the true duration data, while in practice we do not know the true distribution of spells. Thus the relevant comparison is the last month data versus the monthly data contaminated by seam bias, as researchers only use the last month because of the seam bias. Here we would make three points. First, how individuals are likely to report short spells, especially spells falling between two interviews, in the presence of seam bias is not obvious. We can only get an accurate answer from administrative data. If short spells are omitted due to seam bias, switching to using only the last month data certainly will not help us capture these spells. Second, telescoping behavior—shifting events from the more distant past towards the time at which the recollection was made (at the time of the interview)—by its nature tends to overstate the true duration. Our approach below is an attempt to overcome some of this overestimation of spell length due to seam bias. Third, the implications of Figures 1.1 to 1.4 also hold for comparisons of estimates based on the monthly data contaminated by seam bias (the SIPP data) and estimates based on only the last month observations from the contaminated data.

[9] Hypothetically, if using the last month data leads to disproportionate misclassification, for example, from a short left-censored non-employment spells into a long left-censored employment spell as shown in Figure 1.4, there is possibility to underestimate the spell length of left-censored non-employment spells and overestimate the length of left censored employment spells. On the other hand, a left censored employment spell with duration less than or equal to three months could lead to an underestimate of left censored non-employment spells and an underestimate of left censored employment spells. However, our data do not indicate disproportionate misclassification of either left-censored employment or left-censored non-employment spells.

To shed more light on the issue of how the contaminated monthly data and only the last month data compare, we examine the number of completed spells and the empirical survivor functions for each data type. Comparing the number of *completed* spells, we find that we lose about 25% of fresh welfare spells, 22% of fresh off-welfare spells, 9% of left-censored welfare spells and 20% of left-censored off-welfare spells by shifting from monthly data to the last month data. Similarly, we lose about 47% of fresh employment spells, 48% of fresh non-employment spells, 20% of left-censored employment spells, and 18% of left-censored non-employment spells by shifting from monthly data to the last month data. The above numbers represent very dramatic changes in welfare and employment dynamics between the two types of data.

Investigating the empirical survivor functions for various spells corresponding to employment dynamics, we find that for our sample using only the last month data overestimates the length of all types of spells compared to using monthly data.[10] Figures 2.1 and 2.2 show that using only the last month data will increase the estimated survivor function for left-censored employment and non-employment spells by a considerable amount. Figures 2.3 and 2.4 show that this phenomenon is even more pronounced for fresh employment and non-employment spells. This latter result is expected since fresh spells are more likely to be short spells.[11]

These calculations indicate that shifting from the contaminated (by seam bias) data to only the last month data leads to omitting spells and overestimating the spell length. Since the monthly data contaminated by seam bias may already suffer from a loss of spells and overestimation of spell length, using only the last month data constructed from this contaminated data clearly exacerbates both problems. We would expect our approach outlined below to predict shorter durations than an analysis based on the last month data, and we examine this issue below.

IV. Correcting for Seam Bias: A Parametric Approach

To save the valuable information contained in monthly data and to solve the seam bias problem, we develop a monthly discrete time duration model with three extra parameters to capture the misreporting of transitions that is caused by seam bias. Under reasonable

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[10] The spells are constructed by pretending we only observe the last month data. When there is a status change from previous interview to current interview, we code the current last month as the end of a spell.
[11] We obtain a similar result comparing welfare spells. We will report on the survivor function for welfare spells in the next draft of the paper.

assumptions we can identify parameters describing the response errors due to seam bias. We first set up our notation before discussing our assumptions Let M^{obs} represent the month during a reference period when, according to an individual's reporting, a spell ended (either a transition took place or the individual reached the end of the sample period) and M^{true} represent the true transition month during a reference period. Both M^{obs} and M^{true} assume five possible values: $1, 2, 3, 4$ or 0. $M^{obs} = 1, 2, 3,$ or 4 means that a transition was reported to occur in month 1, month 2, month 3 and month 4, respectively, and $M^{obs} = 0$ indicates that no transition took place at the end of the survey, a right-censored spell. For M^{true} the values denote the timing of true, rather than reported, incidence. Thus $M^{obs} = 4$ indicates that a transition is reported to have occurred in the last month, and because of seam bias, this will occur even when $M^{true} \neq 4$.

4.1 Behavioral Assumptions

Before we set up the econometric model, we first make some assumptions about the nature of the reporting errors. As noted above, a consensus exists among researchers that respondents tend to move an earlier month's transition into the last month of the reference period. Taking into consideration the telescoping behavior and the survey design, we make the following assumptions: 1) in each interview, the respondents report all transitions that occurred during that reference period. In other words, there is no delayed report from the last reference period; 2) if a respondent reports that a transition happened in months 1, 2 or 3, we assume it is a truthful report; 3) if a respondent reports a transition happened in month 4, we assume with some pre-specified (but unknown) probabilities that the reported transition actually happened in month 1, month 2, or month 3 of that reference period; 4) if a transition *truly* happened in month 4, the respondent reports it as occurring in that month; 5) the true transition rate for a given duration does not depend on which month the transition occurs in a reference period.[12]

Given the first four behavioral assumptions, we have the following conditional probabilities:

[12] Our assumptions rule out the possibility that individuals forget about very short spells that fall between two interviews. As discussed before, without administrative data we have no way of verifying the truth of this assumption.

$$pr\left(M^{obs}=i \| M^{true}\neq i\right)=0 \text{ if } i=1,2,3 \tag{4.1}$$

$$pr\left(M^{obs}=4 \| M^{true}=i\right)=\alpha_i \text{ if } i=1,2,3 \tag{4.2}$$

$$pr\left(M^{obs}=4 \| M^{true}=4\right)=1 \tag{4.3}$$

$$pr\left(M^{obs}=0 \| M^{true}=0\right)=1 \tag{4.4}$$

4.2 Correcting for Seam Bias in a Single Spell Model

To illustrate the method in the simplest way, we first explore the problem involving a single spell. We define the hazard function as

$$\lambda(t\,|\,\theta)=\frac{1}{1+\exp\{-h(t)-X(\tau+t)\beta-\theta\}}$$

where t denotes current duration, $h(t)$ denotes duration dependence, τ denotes the calendar time of the start of the spell, $X(\tau+t)$ denotes a (possibly) time changing explanatory variable, and θ denotes unobserved heterogeneity. (Our analysis is equally applicable to any other choice for the discrete time hazard function.) For example, if a spell lasts K months, the likelihood function is:

$$L(K)=\int_\theta \lambda(K\,|\,\theta)\prod_{t=1}^{K-1}(1-\lambda(t\,|\,\theta))\Phi(\theta)d\theta\,, \tag{4.5}$$

where $\Phi(\cdot)$ is the distribution function for θ, which is distributed independently across individuals. Based on our behavioral assumptions, it is straightforward to derive the likelihood function given the observed month of the transition, M^{obs}, and the observed length of the spell, dur^{obs}, both of which potentially have been contaminated by seam bias. The contribution to the likelihood function for a completed spell of observed length K that ends in month 1 is given by:

$$pr\left(M^{obs}=1, dur^{obs}=K\right)$$
$$= pr\left(M^{obs}=1, M^{true}=1, dur^{obs}=K\right) + pr\left(M^{obs}=1, M^{true}\neq 1, dur^{obs}=K\right).$$

The second term is zero by assumption 4.1, thus,

$$pr\left(M^{obs}=1, dur^{obs}=K\right) = pr\left(M^{obs}=1, M^{true}=1, dur^{true}=K\right)$$
$$= pr\left(dur^{true}=K \middle| M^{obs}=1, M^{true}=1\right)\cdot pr\left(M^{obs}=1 \middle| M^{true}=1\right)\cdot pr\left(M^{true}=1\right),$$

By assumption 4.5 $pr\left(dur^{true}=K \middle| M^{obs}=1, M^{true}=1\right) = pr\left(dur^{true}=K\right)$ Thus, we have

$$pr\left(M^{obs}=1, dur^{obs}=K\right) = pr\left(dur^{true}=K\right)\cdot pr\left(M^{obs}=1 \middle| M^{true}=1\right)\cdot pr\left(M^{true}=1\right)$$
$$= \frac{1}{4}(1-\alpha_1)\cdot L(K) \qquad (4.6)$$

The last step follows because of assumption 4.2 and

$pr\left(M^{true}=1\right) = pr\left(M^{true}=2\right) = pr\left(M^{true}=3\right) = pr\left(M^{true}=4\right) = \frac{1}{4}$ due to the survey design.

Similarly if a transition is reported to end in month 2 or month 3 and to have lasted for K months, we have:

$$pr\left(M^{obs}=2, dur^{obs}=K\right) = \frac{1}{4}(1-\alpha_2)\cdot L(K) \qquad (4.7)$$

$$pr\left(M^{obs}=3, dur^{obs}=K\right) = \frac{1}{4}(1-\alpha_3)\cdot L(K) \qquad (4.8)$$

Finally, the contribution to the likelihood function for a completed spell of observed length K that ends in month 4 is given by:

$$
\begin{aligned}
& pr\left(M^{obs}=4, dur^{obs}=K\right) \\
& = pr\left(M^{obs}=4, M^{true}=1, dur^{true}=K-3\right)+ \\
& pr\left(M^{obs}=4, M^{true}=2, dur^{true}=K-2\right)+ \\
& pr\left(M^{obs}=4, M^{true}=3, dur^{true}=K-1\right)+ \\
& pr\left(M^{obs}=4, M^{true}=4, dur^{true}=K\right) \\
& = pr\left(dur^{true}=K-3 \middle| M^{obs}=4, M^{true}=1\right)\cdot pr\left(M^{obs}=4 \middle| M^{true}=1\right)\cdot pr\left(M^{true}=1\right)+ \\
& pr\left(dur^{true}=K-2 \middle| M^{obs}=4, M^{true}=2\right)\cdot pr\left(M^{obs}=4 \middle| M^{true}=2\right)\cdot pr\left(M^{true}=2\right)+ \\
& pr\left(dur^{true}=K-1 \middle| M^{obs}=4, M^{true}=3\right)\cdot pr\left(M^{obs}=4 \middle| M^{true}=3\right)\cdot pr\left(M^{true}=3\right)+ \\
& pr\left(dur^{true}=K \middle| M^{obs}=4, M^{true}=4\right)\cdot pr\left(M^{obs}=4 \middle| M^{true}=4\right)\cdot pr\left(M^{true}=4\right) \\
& =\frac{1}{4}\alpha_1 L\left(K-3\right)+\frac{1}{4}\alpha_2 L\left(K-2\right)+\frac{1}{4}\alpha_3 L\left(K-1\right)+\frac{1}{4}L\left(K\right).
\end{aligned} \tag{4.9}
$$

4.3 Correcting for Seam Bias in a Multiple Spell Model

In a multiple spell discrete time duration model, correcting for seam bias complicates the likelihood function dramatically since adjusting a response error in one spell involves shifting not only the end of the current spell but also the start of the subsequent spell. This is a serious problem as, for example, respondents in our sample have up to 7 spells and respondents can have several spells ending in month 4 in their history.

We estimate a discrete time duration model with multiple spells, duration dependence and unobserved heterogeneity. Due to the presence of unobserved heterogeneity and the lack of information on the start date, it is extremely complicated to derive the density function for time remaining in a left-censored spell (i.e. a spell in progress at the start of the sample) using the same set of parameters as for fresh spells. As noted above, we adopt the pragmatic suggestion of Heckman and Singer (1984a) and specify a separate hazard function and heterogeneity term for left-censored spells. We allow the unobserved heterogeneity terms to be correlated across different types of spells.

We again use employment and non-employment spells to facilitate our discussion. We let the employment spells, both left-censored and fresh, share one set of seam bias parameters, α_1^E, α_2^E, and α_3^E, as defined in (4.2); while we specify another set of parameters, α_1^U, α_2^U, and α_3^U, representing the seam bias associated with non-employment spells. (As we show below, we cannot let the seam bias parameters differ between left censored and fresh spells of the same type.) We specify the unobserved heterogeneity corresponding to the four types of spells through the vector $\theta = (\theta_U, \theta_{U'}, \theta_E, \theta_{E'})$, and assume θ is distributed independently across individuals and is fixed across spells for a given individual. Following Heckman and Singer (1984b) we let θ follow a discrete distribution with points of support $\theta_1, \theta_2, ..., \theta_J$, (where, e.g. $\theta_1 = (\theta_{U1}, \theta_{U'1}, \theta_{E1}, \theta_{E'1})$) and associated probabilities $p_1, p_2, ..., p_J$ respectively, where $p_J = 1 - \sum_{j=1}^{J-1} p_j$.

The following discussion is based on a relatively simple example, which covers all essential problems for multiple spells with seam bias. Assume a respondent reports three spells as in Figure 3, and her reporting history is $\left\{M_{U'}{}^{obs} = 1, M_E{}^{obs} = 4, M_U{}^{obs} = 0\right\}$, which indicates the first spell is a left-censored non-employment spell ending in month 1, the second is a fresh employment spell reported to end in month 4, and the third is a fresh non-employment spell which is censored at the end of the sample. (Again the numbers above the line are the survey months and the numbers below the line are reference period months.) Note that the second reported spell ended in a last month. According to our assumptions, the reported history could be true, and there are also three additional possible histories A, B, and C due to seam bias, namely the second spell could actually end in month 1, 2, or 3 of that particular reference period. Obviously the starting point of the third spell should be set according to the end point of the second spell in each possible work history.

In the Appendix we show that the respondent's contribution to the likelihood is:

$$L = c\int_\theta \left[\left(1-\alpha_1^U\right) \prod_{r=1}^{4} \left(1-\lambda_{U'}\left(r \mid \theta_{U'}\right)\right) \cdot \lambda_{U'}\left(5 \middle| \theta_{U'}\right) \right] \cdot$$

$$\left\{ \begin{array}{l} \left[\alpha_1^E \prod_{r=1}^{3} \left(1-\lambda_E(r \mid \theta_E)\right) \cdot \lambda_E(4 \mid \theta_E) \prod_{r=1}^{27} \left(1-\lambda_U(r \mid \theta_U)\right) \right] \\ + \left[\alpha_2^E \prod_{r=1}^{4} \left(1-\lambda_E(r \mid \theta_E)\right) \cdot \lambda_E(5 \mid \theta_E) \prod_{r=1}^{26} \left(1-\lambda_U(r \mid \theta_U)\right) \right] \\ + \left[\alpha_3^E \prod_{r=1}^{5} \left(1-\lambda_E(r \mid \theta_E)\right) \cdot \lambda_E(6 \mid \theta_E) \prod_{r=1}^{25} \left(1-\lambda_U(r \mid \theta_U)\right) \right] \\ + \left[\prod_{r=1}^{6} \left(1-\lambda_E(r \mid \theta_E)\right) \cdot \lambda_E(7 \mid \theta_E) \prod_{r=1}^{24} \left(1-\lambda_U(r \mid \theta_U)\right) \right] \end{array} \right\} d\Phi(\theta_{U'}, \theta_E, \theta_U), \qquad (4.10)$$

where c is a constant that does not affect the maximization. The first term in the integration is the contribution from the first spell, which lasted for five months and ended in month 1. The four terms in the subsequent parentheses represent the contribution from the second and third spells considering the four possible histories illustrated in Figure 3.

4.4 Identification of Duration Dependence and Seam Bias Parameters

At first glance, it may appear that we have to restrict the form of the duration dependence to identify our model. However, this is not the case, at least for fresh spells. Without loss of generality, consider a model with no explanatory variables and no duration dependence. Let $m_j(k)$ denote the empirical hazard function for spells ending at duration k in reference month j, $j=1,2,3,4$. To see where empirical identification comes from, consider the following expectations of the empirical hazard functions for $t \geq 4$:

$$\begin{aligned}
&E[m_1(t)] = (1-\alpha_1)\lambda(t), \\
&E[m_2(t)] \approx (1-\alpha_2)\lambda(t), \\
&E[m_3(t)] \approx (1-\alpha_3)\lambda(t), \\
&E[m_4(t)] \approx \alpha_1\lambda(t-3) + \alpha_2\lambda(t-2) + \alpha_3\lambda(t-1) + \lambda(t), \\
&E[m_1(t-3)] = (1-\alpha_1)\lambda(t-3), \\
&E[m_2(t-2)] \approx (1-\alpha_2)\lambda(t-2), \\
&E[m_3(t-1)] \approx (1-\alpha_3)\lambda(t-1).
\end{aligned} \tag{4.11}$$

Treating the '$\approx$' as equivalent to the '$=$' and replacing the $E[m_j(k)]$ with their empirical counterparts $m_j(k)$ gives us seven equations with which to solve for the seven unknowns $\alpha_1, \lambda(t-3), \alpha_2, \lambda(t-2), \alpha_3, \lambda(t-1)$, and $\lambda(t)$. Again treating the '$\approx$' as equivalent to the '$=$' we can write the last three equations in (4.11) as

$$\begin{aligned}
&\lambda(t-3) = E[m_1(t-3)]/(1-\alpha_1), \\
&\lambda(t-2) = E[m_2(t-2)]/(1-\alpha_2), \\
&\lambda(t-1) = E[m_3(t-1)]/(1-\alpha_3).
\end{aligned}$$

Substituting these expressions into the equation for the fourth equation (for $E[m_4(t)]$), and replacing the $E[m_j(k)]$ with their empirical counterparts $m_j(k)$, yields a system of four equations in four unknowns:

$$\begin{aligned}
&E[m_1(t)] = (1-\alpha_1)\lambda(t),\\
&E[m_2(t)] = (1-\alpha_2)\lambda(t),\\
&E[m_3(t)] = (1-\alpha_3)\lambda(t),\\
&E[m_4(t)] = \{\alpha_1 E[m_1(t-3)]/(1-\alpha_1)\} + \{\alpha_2 E[m_2(t-2)]/(1-\alpha_2)\} + \alpha_3\{E[m_3(t-1)]/(1-\alpha_3)\} + \lambda(t).
\end{aligned} \tag{4.12}$$

Note that the model has a lot of overidentification, since for $t \geq 4$ we have *4(T-3)* equations in *T-3+3=T* unknowns. We will consider the hazards for *t<4* in the next draft.

However, the situation is different for the left censored spells. Since we start duration in these spells at the start of the sample, we will only observe a spell of length 1, 5, 9, 13… ending in month 1, a spell of length 2, 6, 10, 14 ... ending in month 2, a spell of length 3, 7, 11, 15… ending in month 3, or a spell of length 4, 8, 12, 16… ending in month 4. Now consider the case where t is a multiple of 4. The available moment conditions are

$$\begin{aligned}
&E[m_4(t)] \approx \alpha_1\lambda(t-3) + \alpha_2\lambda(t-2) + \alpha_3\lambda(t-1) + \lambda(t),\\
&E[m_1(t-3)] \approx (1-\alpha_1)\lambda(t-3),\\
&E[m_2(t-2)] \approx (1-\alpha_2)\lambda(t-2),\\
&E[m_3(t-1)] = (1-\alpha_3)\lambda(t-1).
\end{aligned}$$

Now we have seven unknowns in four equations, so obviously the model is under-identified. But if we take the estimated α terms from the fresh spell of a given type (e.g. employment) and use them in the left censored spells of the same type (.e.g left censored unemployment) the parameters are identified. Note that this identification problem would disappear if we had and used the actual start date of the left-censored spells.

This analysis raises the issue of how to estimate empirical hazards in the presence of seam bias. To do this we estimate a multi-spell duration model with: i) no unobserved heterogeneity; ii) no explanatory variables and iii) a very flexible step function for duration dependence in each hazard function. Subject to the restriction that we constrain fresh and left-censored spells of the same type to share the same α's, we had no difficulty estimating this model or inverting the second derivative matrix for it.[13]

4.5 Alternative Misclassification Schemes

[13] Note that the functional form for the hazard function here is totally irrelevant – we would get the same estimated hazard functions if we simply estimated the $\lambda(t)$ directly.

Of course, there is the possibility that the transitions are misclassified in a way that differs from that assumed above. A seminar participant suggested the following alternative: some of month 1 is pushed into month 2, some of month 2 is pushed into month 3, and some of month 3 is pushed into month four, but none of month 4 is pushed into the next reference period (because it is the last month in the reference period). If 50% of the transitions in months 1, 2 and 3 are pushed to the next month, then we would see 12.5 % of the transitions in month 1, 25% in month 2, 25% in month 3, and 37.5% in month 4. Alternatively, suppose 75% of the transitions get pushed out of each month. Then we would see 6.25 % of the transitions in month 1, 25% in month 2, 25% in month 3, and 42.5% in month 4. The upshot is that months 2 and 3 should have 25% of the transitions each, month 1 should have a much smaller proportion of the transitions than months 2 and 3, and month 4 should have a much bigger proportion of the transitions than months 2 and 3.

Another possibility is that the data are generated by transitions in the interview month are pushed back into month 4 of the previous reference period. If this is the only source of misclassification, then the pattern should be similar to the scheme above – 25% of the observed transitions are reported in months 2 and 3, a smaller proportion are reported in month 1 and a larger proportion in month 4.

To shed some light on these alternative explanations, we look at the fraction of transitions reported in each reference month for employment/non-employment and welfare/non-welfare separately. However, it is also useful to ask what the misclassification scheme we have used predicts for the distribution of transitions across interview months. With the α terms unrestricted (and all non-zero) then the fraction of transitions reported in months 1, 2, and 3 should all be less than 25% , and the fraction of transitions in month 4 should be greater than 25%. However, if $\alpha_1 = \alpha_2 = \alpha_3$ then the transition rates in months 1, 2 and 3 should be equal and less than 25%.

When we considered observed transitions, we found that months 1, 2, 3, and 4 had 16.57, 19.08, 18.49, and 45.86 of the employment/non-employment transitions respectively; while months 1, 2, 3, and 4 had 15.49, 20.42, 15.70, and 48.39 of the welfare/non-welfare transitions respectively. The only model consistent with this data is the model we use; indeed it seems at this level that the strong hypothesis $\alpha_1 = \alpha_2 = \alpha_3$ is consistent with the data.

V. Empirical Results

Figures 4.1 to 4.4 show the empirical hazard functions estimated from the monthly data and the procedure described above for employment dynamics, where we parameterize the hazard function only in terms of a step function in duration.[14] One can see the jumps in the hazard functions from the raw data at 4, 8, 12 … months of duration. This is not surprising for left-censored spells given that duration is measured from the start of the sampling period, so durations of 4, 8, 12 … months just coincidentally end in survey month 4, 8, 12 …, which are all the last months of the respective reference period. For the fresh spells, which could start at any time during the survey period, it seems to be puzzling to see the spikes in the hazard function from the raw data at 4, 8, 12 … months of duration as well. We believe it reflects two aspects of one problem. First, an over-reporting of transitions in the last month leads to an over-reporting of spells starting in month 1 of the following reference period. Second, those spells starting in month 1 of a reference period are more likely to be reported to end in a month 4 due to seam bias. The combined effect of the two aspects leads to disproportional spells of lengths 4, 8, 12 … in the fresh spell data. The "adjusted" hazard is estimated from a multiple spell duration model that consists of seam bias parameters and unrestricted duration dependence, but no explanatory variables. We would argue that estimating such a model is a natural way of providing empirical hazard functions that correct for seam bias. Not surprisingly, the empirical hazard functions estimated in this way are much smoother than those estimated directly from the contaminated data.[15]

Tables 2.1 and 2.2 present results for the employment and non-employment spells. We focus on these coefficients in this draft as the employment dynamics of women with low levels of schooling has received much less attention in the literature. We let the data choose the best fitting polynomials for duration dependence according to the Schwartz criterion for the models estimated by our procedure and for the last month data only. We put estimates from the seam bias correction models and the last month data models side by side. Table 2.1 contains models with unobserved heterogeneity and Table 2.2 shows models with no unobserved heterogeneity.

[14] We will show these figures for welfare dynamics in a future draft of the paper.

[15] In general we had no trouble estimating this, and the standard errors were small relative to the values of the step function except in a few cases where we had very little data. In the next draft we will show the empirical hazard function based on using only the last month data.

The choice of explanatory variables is generally standard, with perhaps the minimum wage being the only innovation.[16] We find substantial evidence of misreporting as all of the α terms are statistically and economically significant. Using our seam bias correction estimates and focusing first on the parameters for a left censored non-employment spell, we first see that higher welfare benefits, a higher unemployment rate, growing older, having never been married, having more children under 6 years of age, and having a disability lowers the probability a woman leaves this type of spell. (Interestingly, those who did not answer the disability question also have longer spells.) On the other hand, schooling is the only variable that significantly increases the probability of leaving such a spell. There are no differences in the statistical significance of the coefficients between our seam bias estimates and estimates based on the last month data. In terms of a left-censored employment spell, we see that being African American or Hispanic, being never married, having a disability, or not answering the disability question is associated with shorter left-censored employment spells, while being older or having more schooling is associated with longer left-censored employment spells. The kids less than six coefficient and the disability variable missing coefficient are not significant in the last month estimates, while the age of the youngest child is significant in the last month data but not in the seam bias estimates.

For the fresh non-employment spells, being offered a 'carrot' to leave welfare significantly reduces the length of a non-employment spell, as does having more schooling. Being eligible for higher welfare benefits, being African American, having more kids less than six years, and having a disability decreases the hazard rate for a fresh non-employment spell. (Those who did not answer the disability question also have longer fresh non-employment spells.) Note that the maximum welfare benefits, unemployment rate and the welfare waiver carrot are not significant in the last month estimates. Finally, considering fresh employment spells, more education and growing older decrease the exit rate, while having a disability increases the exit rate. In the last month estimates being African American and being never married also significantly increases the exit rate from a fresh employment spell.

Tables 3.1 and 3.2 report results from the duration analysis for welfare and off-welfare spells. Table 3.1 contains models with unobserved heterogeneity and Table 3.2 shows models

[16] Yelowitz (1995) argued that one should also include the Medicaid income limits when looking at welfare participation or labor force participation. We do not include that variable here since Ham and Shore-Sheppard (2005) found that his result arose from a mis-imputation of the income limits and imposing a restriction not consistent with theory or the data.

with no unobserved heterogeneity. Again we find substantial evidence of misreporting as all of the α terms are statistically and economically significant; indeed the misreporting coefficients for welfare and off-welfare spells are substantially larger than those for employment and non-employment. A higher unemployment rate, being African American or Hispanic, being never married, and having more kids less than eighteen significantly raises the exit rate from a left censored off-welfare spell, while more education, being older and the youngest child being older significantly reduces the exit rate from a left censored off-welfare spell. The only difference in the seam bias and last month estimates in terms of statistical significance is the fact that the Hispanic dummy is not significant in the last month data. In terms of the left censored on-welfare spells, the seam bias estimates suggest that higher welfare benefits, a higher unemployment rate, a higher minimum wage, growing older, being never married, having more kids less than eighteen and having more kids less than six all significantly reduce the exit rate, while having more schooling and an older youngest child increases this exit rate. Comparing the seam bias estimates to the last months estimates, the minimum wage and age of the youngest child are not significant in the last month data, while being African American is significantly negative in the last month estimates but not in the seam bias estimates. We have much less data on the fresh on and off welfare spells and thus much fewer significant coefficients. Growing older significantly reduces the length of an off welfare spell for both the seam bias and last month estimates, and having more kids less than six significantly reduces the length of a fresh off-welfare spell only in the seam bias estimates. In terms of the fresh welfare spells, higher welfare benefits significantly lower the exit rate and higher education significantly raises the exit rate in both the seam bias and last month estimates.

The coefficients of the hazard model are not directly comparable between the seam bias correction models and the model using the last month data. The hazard rate $\lambda(t)$ from the seam bias correction models measures the probability that a spell ends in the t_{th} month given that it has lasted for $t-1$ months while the hazard rate $\lambda(t), t = 4,8,12,...$ from the model using the last month data measures (approximately) the probability that a spell ends in one of the $(t-3)th, (t-2)th$, or $(t-1)th$ month (where t is a multiple of 4 months), given that the spell has lasted up to and including the $\left(t-4\right)th$ month.[17] To compare the two sets of estimates, one practical solution is to compare the expected duration predicted by both models for each type of spell, and here we again focus on the employment/non-employment spells. In the next draft we will also compare the fraction of time spent in employment (on welfare) in the short-run, medium-run and steady state. Moreover, we will compare the predicted effect of changing the explanatory variables on these summary statistics. We will do this for both employment/non-employment spells and welfare/non-welfare spells.

Conditional on the unobserved heterogeneity, the probability that a spell of type $j,\ j=U',U,E',E$ (in the case of employment dynamics), lasts longer than $t-1$ months is given by the survivor function

$$S_j\left(t-1|\theta_j\right)=\prod_{\tau=1}^{t-1}\left[1-\lambda_j\left(\tau|\theta_j\right)\right].$$

The density of a spell of type *j* that lasts t months is given by

$$f_j\left(t|\theta_j\right)=\lambda_j\left(t|\theta_j\right)S_j\left(t-1|\theta_j\right).$$

The expected duration for a spell of type *j* is given by

$$ED_j=\int_{\Theta}t\cdot f_j\left(t|\theta_j\right)dG_j\left(\theta_j\right)$$

where $G_j\left(\cdot\right)$ is the distribution function for the unobserved heterogeneity term θ_j. Since there is no guarantee the expected duration will be finite, we instead calculate a truncated mean for each type of spell as follows[18]

$$ED_j=\int_{\Theta}\left(\left(\sum_{t-1}^{T^*}t\cdot f_j\left(t|\theta_j\right)\right)+S\left(T^*|\theta_j\right)\cdot T^*\right)dG_j\left(\theta_j\right)$$

We choose $T^*=60$.[19] We calculate the expected durations for each individual and take the sample average. To avoid the out-of-sample durations having disproportionate impact on estimated expected duration, we also calculate a modified version by i) freezing the hazard

[17] In the next draft we will give a more precise expression.
[18] See Eberwein, Ham and LaLonde (2002) for detailed discussion of sensitivity of expected duration to different specifications and length of spells in data.
[19] The longest panel in our current data lasts 40 months.

function for durations longer than 15 months at 15 months for fresh spells and ii) freezing the hazard function for durations longer than 25 months at 25 months for left-censored spells. The choice of 15 and 25 months at which to freeze the hazard is based on observed durations in our data. However we do not find this modification affect expected duration significantly, thus we do not report those results.

Table 4 reports expected employment and non-employment durations calculated based on the hazard model in Table 2.1. We find the estimated expected durations from the last month model are considerably longer than those from the seam bias correction model for all four types of spells. Expected durations of left censored spells from the last month method are likely to be overestimated because the model omits many short spells and lengthens others, and this prediction is borne out in our results. As we discussed above, expected durations of fresh spells may be over- or underestimated using the last month method. We find that the first effect dominates, perhaps not surprisingly.

VI. Summary and Conclusions

Estimating duration models on longitudinal data allows one to estimate the effect of (possibly) time changing factors that may differentially impact entries and exits from various labor market states. However, in using such longitudinal data, researchers must confront particular data-quality issues. In this paper we develop a parametric approach to address seam bias, a common source of reporting errors in longitudinal surveys, in a duration model setting. We investigate the identification of seam bias and duration dependence parameters, and conclude that they are separately identified without constraining the functional form of duration dependence. We apply this approach to the analysis of transitions between employment and non-employment, and transitions between participation and non-participation in welfare, among less-educated single mothers using the Survey of Income and Program Participation. We find that seam bias is an important problem for employment and welfare dynamics, and seems considerably larger in reporting transitions in and out of welfare. We compare results from our approach to those estimated following the standard approach in applied work of using only the last month observations and find some differences in the estimates.

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Figure 1.1

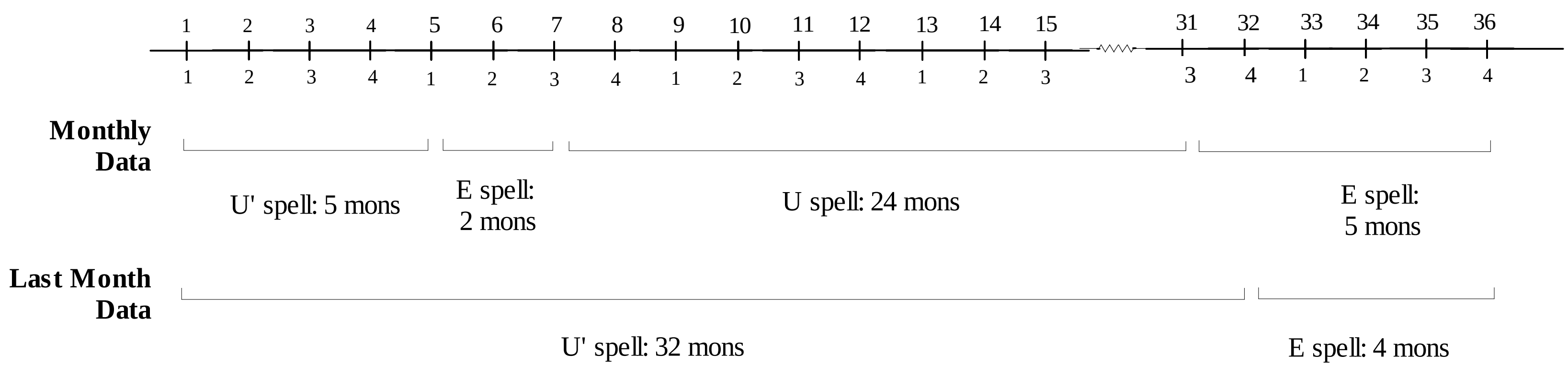


Figure 1.2

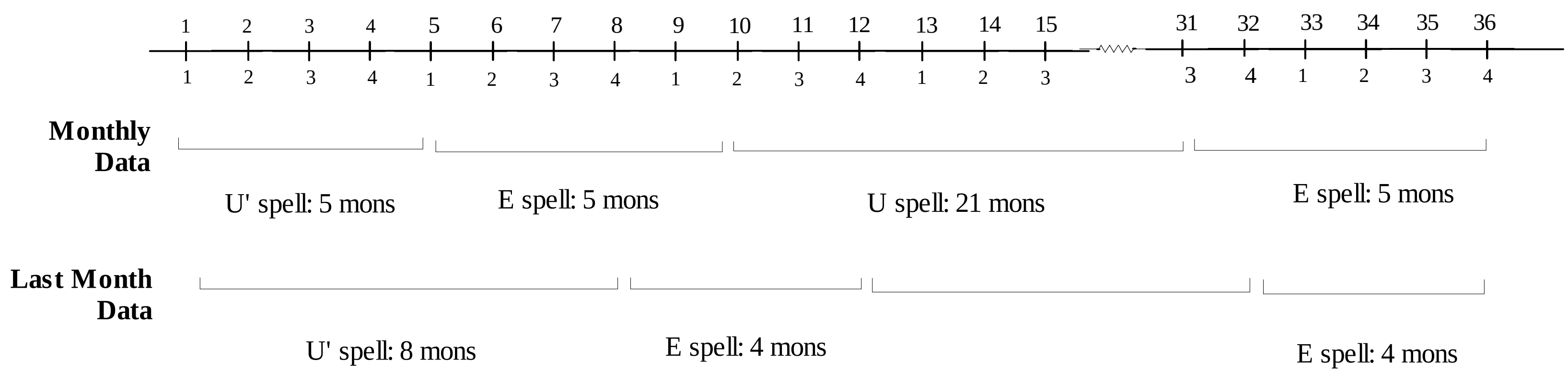
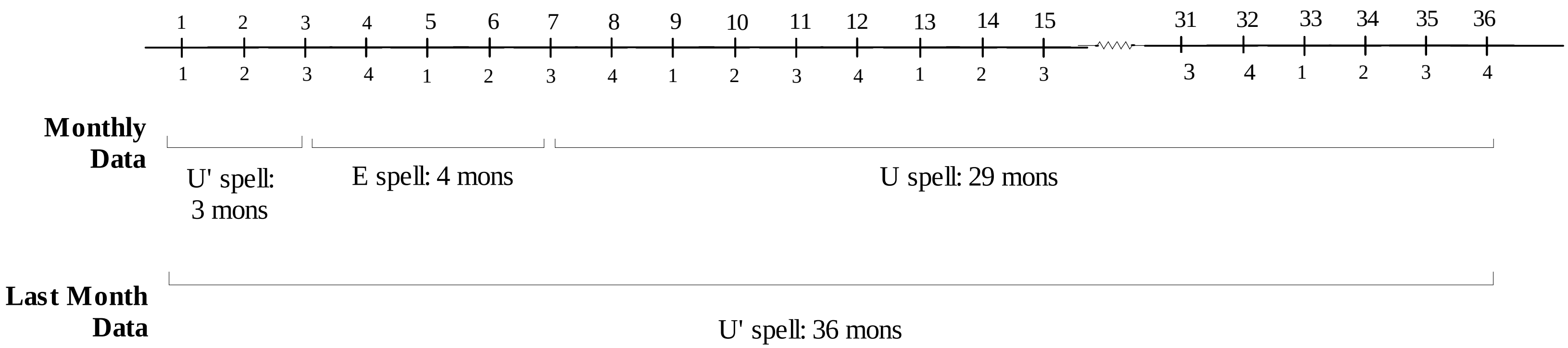


Figure 1.3



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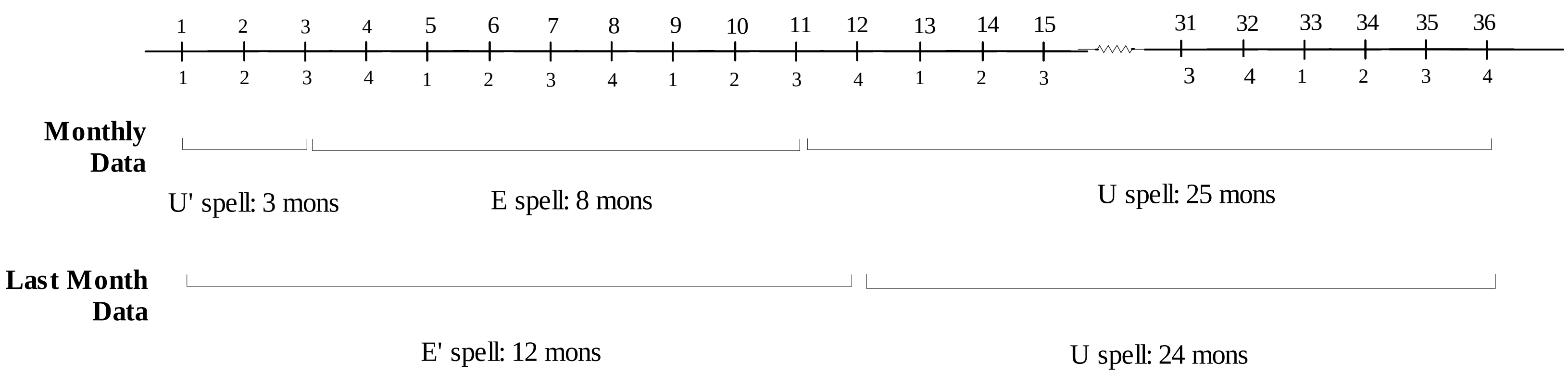
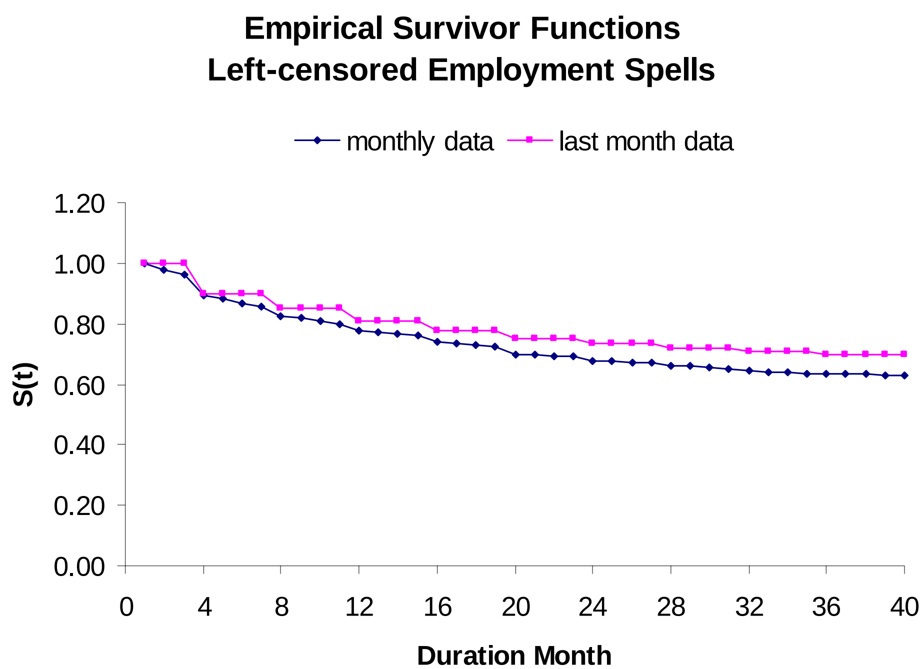


Figure 2.1


Empirical Survivor Functions
Left-censored Employment Spells
monthly data
last month data
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1.00
0.80
0.60
0.40
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S(t)
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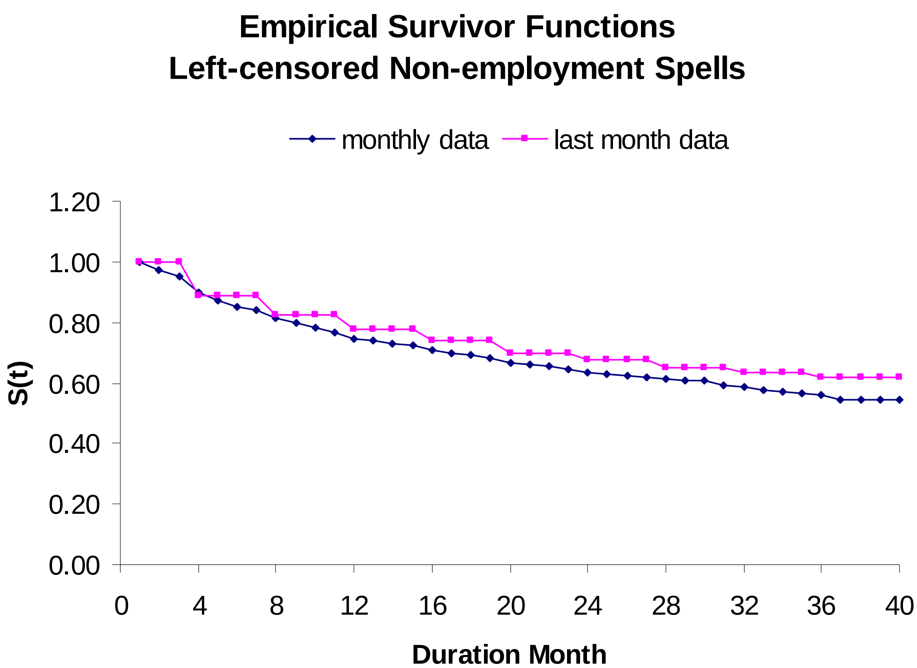
36
40
Duration Month

Figure 2.2


Empirical Survivor Functions
Left-censored Non-employment Spells
monthly data
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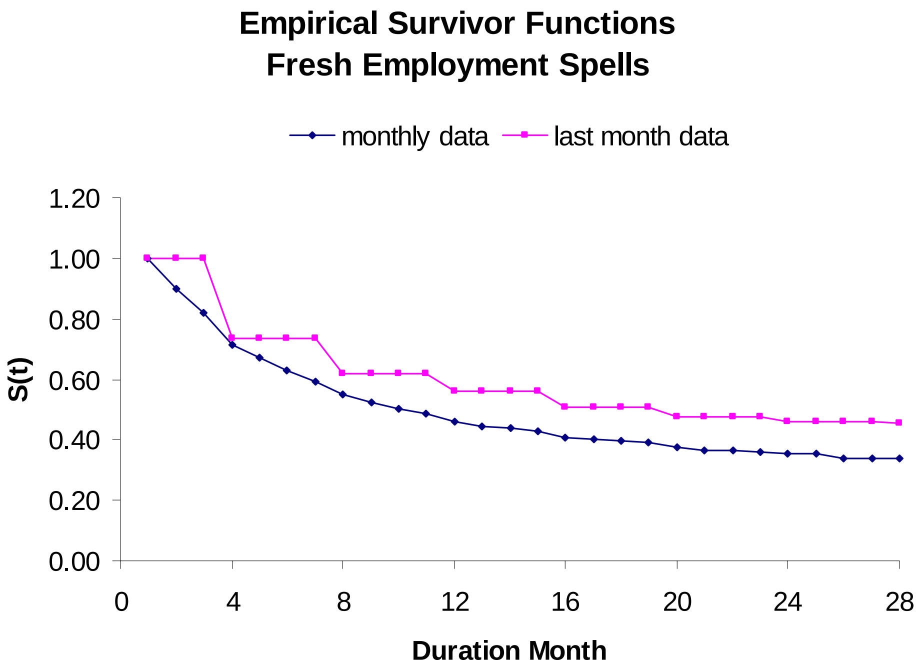
Duration Month

Figure 2.3


Empirical Survivor Functions
Fresh Employment Spells
monthly data
last month data
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24
28
Duration Month

Figure 2.4

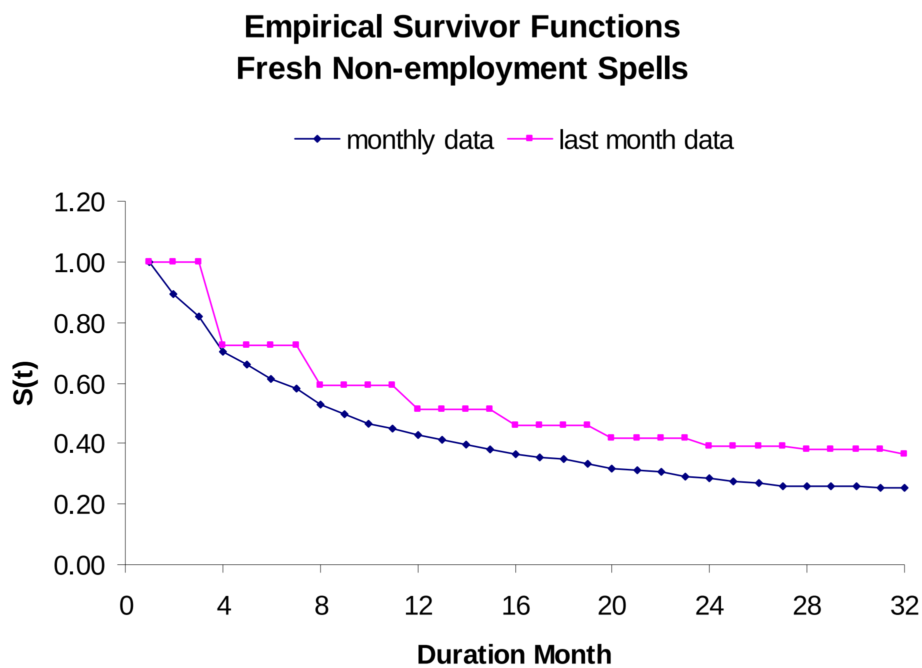
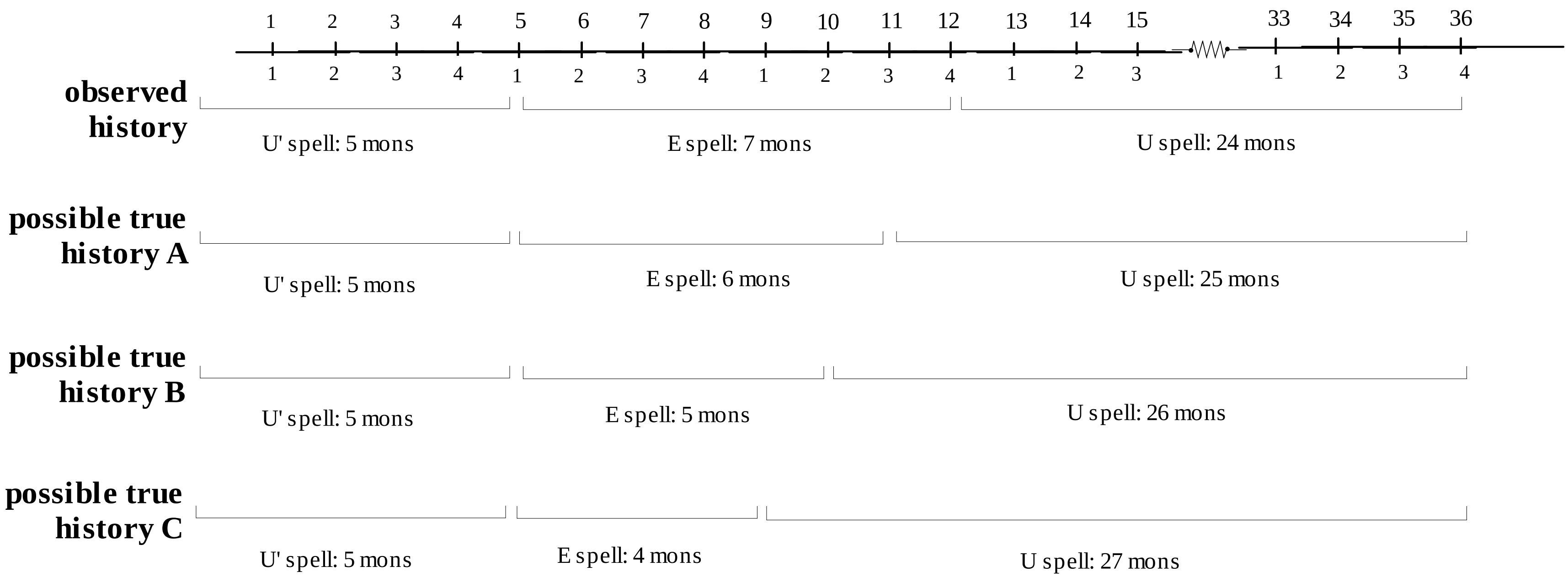

Empirical Survivor Functions
Fresh Non-employment Spells
monthly data
last month data
1.20
1.00
0.80
0.60
0.40
0.20
0.00
S(t)
0
4
8
12
16
20
24
28
32
Duration Month

Figure 3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1 2 3 4 1 2 3 4 1 2 3 4 1 2 3 1 2 3 4
observed history
U' spell: 5 mons
E spell: 7 mons
U spell: 24 mons
possible true history A
U' spell: 5 mons
E spell: 6 mons
U spell: 25 mons
possible true history B
U' spell: 5 mons
E spell: 5 mons
U spell: 26 mons
possible true history C
U' spell: 5 mons
E spell: 4 mons
U spell: 27 mons

Figure 4.1

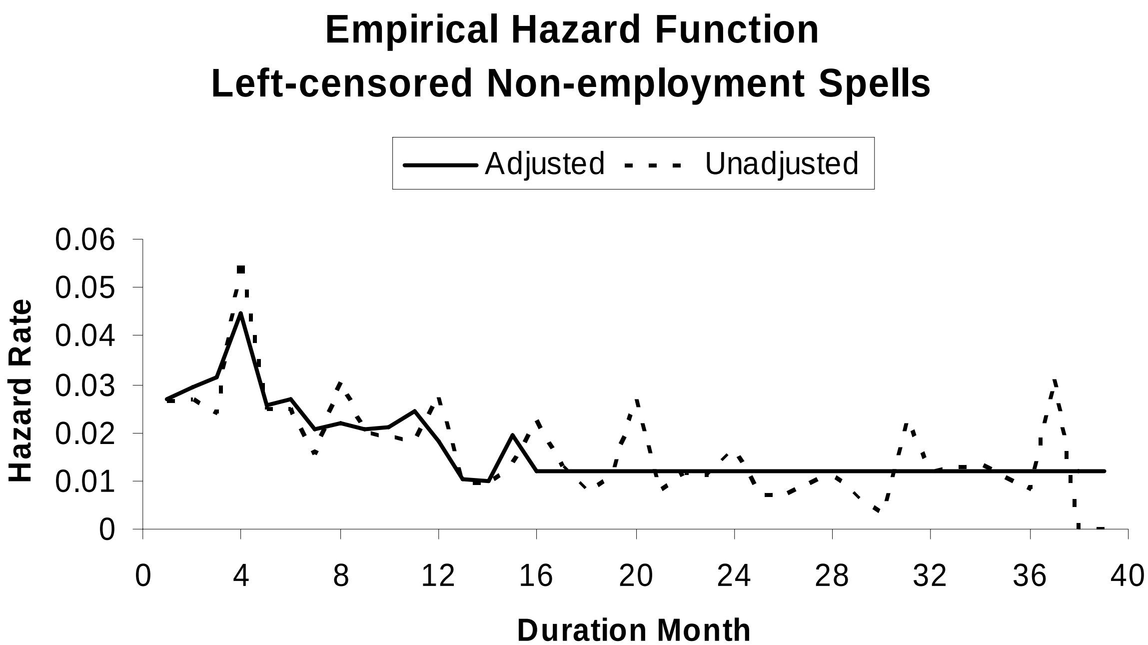
Empirical Hazard Function
Left-censored Non-employment Spells
Adjusted
Unadjusted
Hazard Rate
0.06
0.05
0.04
0.03
0.02
0.01
0
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 4.2

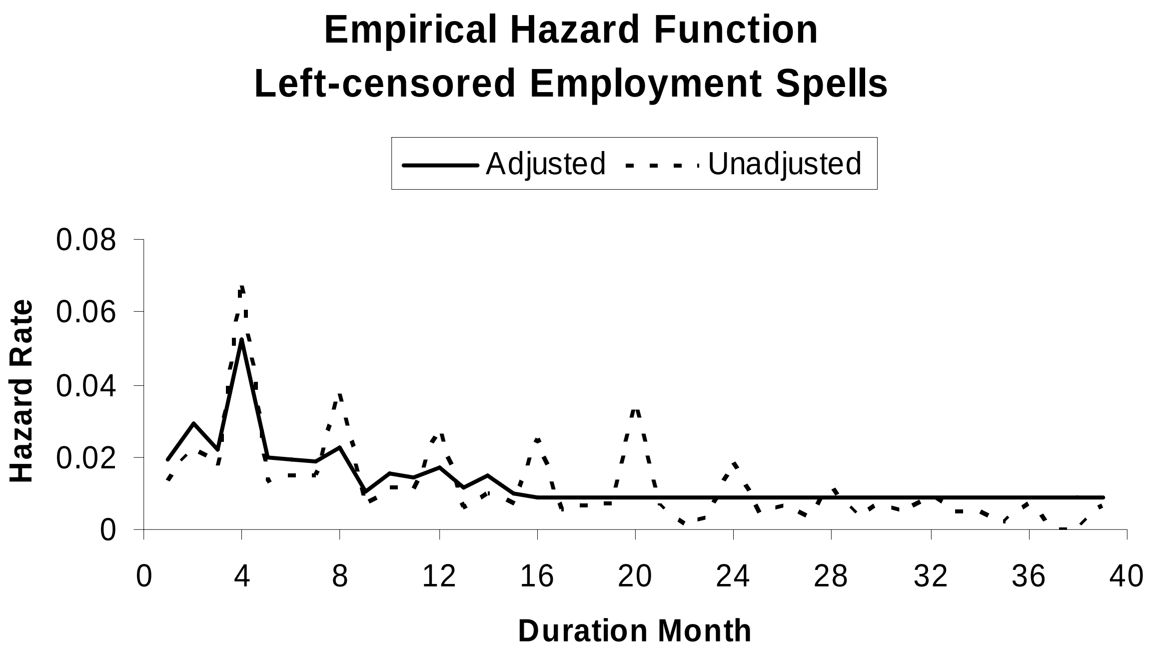
Empirical Hazard Function
Left-censored Employment Spells
Adjusted
Unadjusted
Hazard Rate
0.08
0.06
0.04
0.02
0
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 4.3

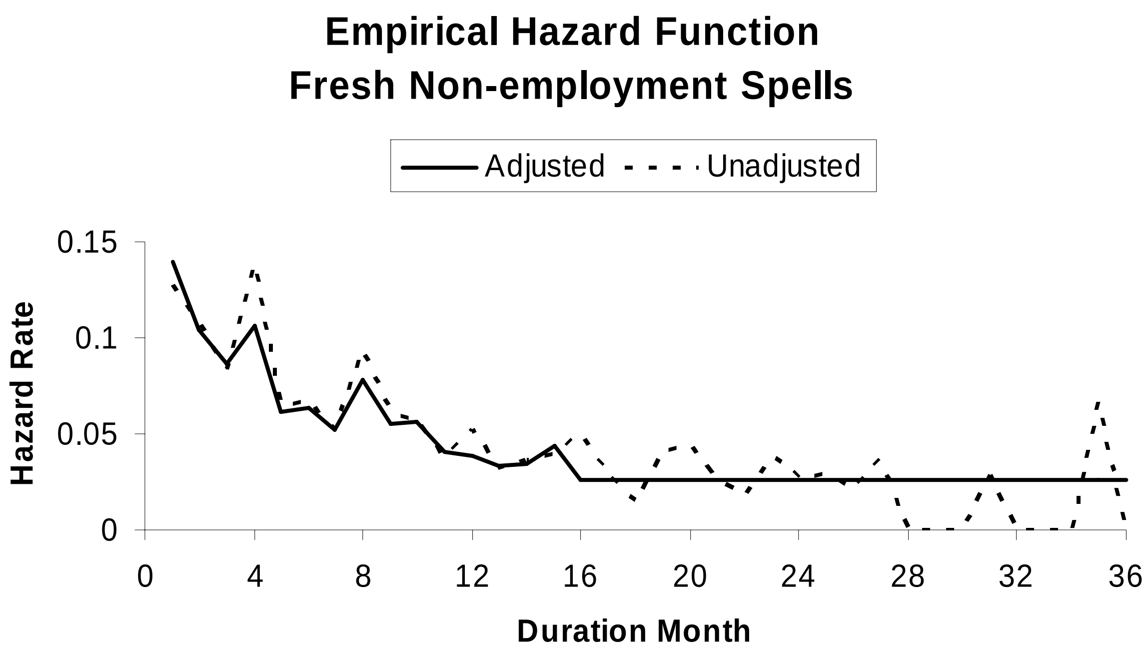
Empirical Hazard Function
Fresh Non-employment Spells
Adjusted
Unadjusted
0.15
0.1
0.05
0
Hazard Rate
0
4
8
12
16
20
24
28
32
36
Duration Month

Figure 4.4

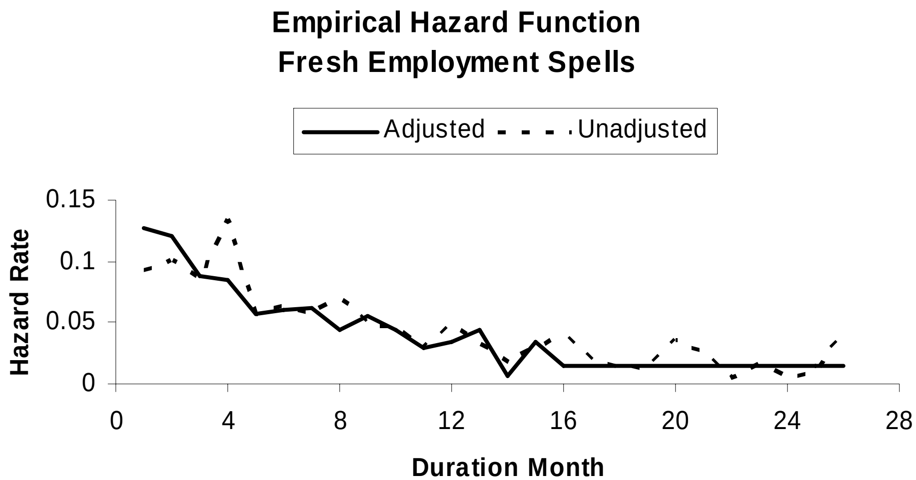
Empirical Hazard Function
Fresh Employment Spells
Adjusted
Unadjusted
Hazard Rate
0.15
0.1
0.05
0
0
4
8
12
16
20
24
28
Duration Month

Table 1.1 Characteristics of Employment and Non-employment Spells
Single Mothers with High School or Lower Education

	Left-censored spells		Fresh spells	
A. Non-employment spell	Mean	Std Dev	Mean	Std Dev
Right censored (%)	64.5%		42.6%	
African American	0.36	0.48	0.38	0.48
Hispanic	0.24	0.43	0.17	0.37
High school diploma	0.44	0.50	0.60	0.49
Age	31.38	8.89	31.22	8.48
Never married	0.53	0.50	0.48	0.50
# of kids < 18	2.12	1.27	1.82	0.99
Age of youngest child	5.57	5.06	6.25	4.98
# of kids < 6	0.91	0.95	0.72	0.81
Disability (adult or child)	0.31	0.46	0.23	0.42
number of spells[1]	3,528		2,578	
number of individuals	3,528		1,889	
number of observations: year*individual	63,384		18,811	

	Left-censored spells		Fresh spells	
B. Employment spell	Mean	Std Dev	Mean	Std Dev
Right censored (%)	69.9%		47.8%	
African American	0.25	0.43	0.32	0.47
Hispanic	0.13	0.34	0.19	0.39
High school diploma	0.79	0.41	0.66	0.47
Age	35.60	8.02	32.29	8.51
Never married	0.25	0.43	0.42	0.49
# of kids < 18	1.51	0.85	1.77	1.03
Age of youngest child	9.17	5.21	7.03	5.18
# of kids < 6	0.35	0.58	0.60	0.73
Disability (adult or child)	0.12	0.33	0.17	0.37
number of spells[1]	3,826		2,732	
number of individuals	3,826		2,000	
number of observations: year*individual	71,613		21,376	
total number of individuals[2]	7,354			
total number of observations	175,184			

Note:
1. including both completed spells and right-censored spells.
2. This is not the sum of number of individuals from the 4 types of spells because some individuals have multiple spells belonging to different types .

Table 1.2 Characteristics of Welfare and Off Welfare Spells
Single Mothers with High School or Lower Education

	Left-censored spells		Fresh spells	
A. Welfare Spells	Mean	Std Dev	Mean	Std Dev
Right censored (%)	70.0%		60.8%	
BLACK	0.40	0.49	0.40	0.49
HISPANIC	0.23	0.42	0.20	0.40
High school diploma	0.45	0.50	0.53	0.50
Age	30.81	8.14	29.91	8.47
Never married	0.57	0.50	0.54	0.50
# of kids < 18	2.26	1.26	1.97	1.08
Age of youngest child	5.17	4.58	5.06	4.61
# of kids < 6	0.97	0.96	0.90	0.87
number of spells[1]	2,450		1,076	
number of individuals	2,450		975	
number of observations: year*individual	50,468		11,701	

	Left-censored spells		Fresh spells	
B. Off Welfare Spells	Mean	Std Dev	Mean	Std Dev
Right censored (%)	85.2%		69.7%	
BLACK	0.25	0.43	0.38	0.49
HISPANIC	0.15	0.36	0.20	0.40
High school diploma	0.73	0.44	0.58	0.49
Age	34.96	8.58	31.49	8.40
Never married	0.28	0.45	0.49	0.50
# of kids < 18	1.54	0.90	1.87	1.11
Age of youngest child	8.70	5.41	6.48	5.08
# of kids < 6	0.41	0.65	0.69	0.78
number of spells[1]	4,904		1,159	
number of individuals	4,904		1,050	
number of observations: year*individual	101,530		11,485	

total number of individuals[2]	7,354
total number of observations	175,184

Note:
1. including both completed spells and right-censored spells.
2. This is not the sum of number of individuals from the 4 types of spells because some individuals have multiple spells belonging to different types .

Table 2.1 Duration Models of Employment and Nonemployment Spells (With Unobserved Heterogeneity)
Single Mothers with High School or Lower Education

	Left-censored non-employment spells		Left-censored employment spells		Fresh non-employment spells		Fresh employment spells	
	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data
Maximum Welfare Benefit	-12.557**	-11.918**	0.070	1.561	-6.441**	-3.875	4.086*	11.675**
	(2.654)	(2.392)	(2.621)	(2.556)	(2.158)	(3.666)	(2.165)	(3.924)
Unemployment Rate	-0.080**	-0.081**	0.054**	0.046**	-0.049**	-0.033	0.024	0.055
	(0.026)	(0.024)	(0.025)	(0.025)	(0.020)	(0.036)	(0.020)	(0.039)
Minimum Wage	0.138	0.088	0.100	0.051	0.117	-0.232	-0.041	0.418
	(0.194)	(0.194)	(0.200)	(0.199)	(0.177)	(0.296)	(0.191)	(0.321)
Welfare Waiver Stick	-0.241	0.072	-0.051	-0.091	-0.248	-0.215	-0.145	-0.508
	(0.279)	(0.225)	(0.290)	(0.278)	(0.194)	(0.309)	(0.199)	(0.348)
Welfare Waiver Carrot	-0.009	0.106	-0.168	-0.255	0.285**	0.243	-0.067	-0.276
	(0.203)	(0.176)	(0.197)	(0.199)	(0.133)	(0.223)	(0.137)	(0.235)
African American	-0.161	-0.129	0.423**	0.388**	-0.253**	-0.089	0.018	0.247*
	(0.100)	(0.086)	(0.090)	(0.086)	(0.075)	(0.128)	(0.075)	(0.144)
Hispanic	-0.159	-0.101	0.273**	0.133**	0.021	0.194	-0.079	-0.093
	(0.110)	(0.095)	(0.106)	(0.105)	(0.083)	(0.149)	(0.090)	(0.164)
Highest Grade Completed	0.487**	0.509**	-0.679**	-0.670**	0.172**	0.284**	-0.264**	-0.365**
	(0.085)	(0.072)	(0.079)	(0.077)	(0.060)	(0.111)	(0.064)	(0.119)
Age	-0.050**	-0.026**	-0.033**	-0.026**	-0.002	-0.001	-0.028**	-0.039**
	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.006)	(0.011)
Never Married	-0.460**	-0.276**	0.270**	0.279**	-0.097	-0.130	0.088	0.239*
	(0.100)	(0.085)	(0.090)	(0.088)	(0.073)	(0.130)	(0.075)	(0.146)
# of Kids < 18	-0.021	0.038	0.047	0.017	0.028	-0.004	0.008	0.061
	(0.043)	(0.038)	(0.048)	(0.045)	(0.036)	(0.061)	(0.035)	(0.064)
Age of Youngest Child	0.013	-0.012	-0.015	-0.024*	0.004	-0.001	-0.002	0.002
	(0.014)	(0.013)	(0.013)	(0.012)	(0.011)	(0.019)	(0.011)	(0.020)
# of Kids < 6	-0.336**	-0.363**	0.189**	0.103	-0.138**	-0.232**	-0.018	-0.060
	(0.068)	(0.068)	(0.080)	(0.074)	(0.065)	(0.111)	(0.060)	(0.110)
Disability	-0.800**	-0.615**	0.905**	0.844**	-0.366**	-0.590**	0.368**	0.426**
	(0.114)	(0.092)	(0.103)	(0.094)	(0.078)	(0.148)	(0.079)	(0.158)
Disability Variable Missing	-0.243**	-0.428**	0.411**	0.009	-0.431**	-0.715**	0.051	-0.418
	(0.130)	(0.127)	(0.121)	(0.140)	(0.134)	(0.278)	(0.134)	(0.270)

log(duration)	-0.093	-0.705**	-0.267**	-0.707**	-0.329**	-0.688**	-0.097	-0.706**
	(0.072)	(0.064)	(0.046)	(0.065)	(0.100)	(0.116)	(0.115)	(0.135)
log(duration)^2					-0.079**		-0.193**	
					(0.035)		(0.042)	
Unobserved Heterogeneity								
Theta1	0.295	1.386*	-7.464	-0.369	-1.854**	-0.004	-0.922	-0.319
	(0.757)	(0.846)	(6.715)	(0.800)	(0.631)	(1.117)	(0.692)	(1.224)
Theta2	-1.929**	0.685	-2.604**	-0.878	-1.500**	1.454	-1.347*	-2.172*
	(0.751)	(0.718)	(0.723)	(0.742)	(0.628)	(1.104)	(0.693)	(1.247)
Seam Bias Correction Model					**Last Month Data Model**			
Heterogeneity Probability	0.306**				0.265**			
	(0.053)				(0.125)			
Seam Bias Correction Parameters								
H1 (non-employment)	0.140**	**H1 (employment)**	0.515**					
	(0.032)		(0.027)					
H2 (non-employment)	0.135**	**H2 (employment)**	0.269**					
	(0.033)		(0.031)					
H3 (non-employment)	0.255**	**H3 (employment)**	0.256**					
	(0.030)		(0.031)					

Notes:
Year dummies are included in each regression. Coefficients are omitted.
Standard errors are in parentheses.
Maximum benefit variable has been divided by 10,000.
* significant at 10% level.
** significant at 5% level.

Table 2.2 Duration Models of Employment and Nonemployment Spells (No Unobserved Heterogeneity)
Single Mothers with High School or Lower Education

	Left-censored non-employment spells		**Left-censored employment spells**		**Fresh non-employment spells**		**Fresh employment spells**	
	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data
Intercept	-1.502**	0.934	-3.175**	-0.724	-1.523**	0.975	-1.082	-0.844
	(0.620)	(0.698)	(0.658)	(0.712)	(0.623)	(0.974)	(0.685)	(0.996)
Maximum Welfare Benefit	-9.760**	-11.750**	-0.115	1.575	-6.331**	-2.986	4.674**	9.078**
	(2.032)	(2.316)	(2.234)	(2.532)	(2.158)	(3.181)	(2.124)	(3.023)
Unemployment Rate	-0.074**	-0.081**	0.042*	0.046*	-0.050**	-0.028	0.031	0.047
	(0.020)	(0.023)	(0.022)	(0.025)	(0.020)	(0.032)	(0.020)	(0.032)
Minimum Wage	0.137	0.085	0.086	0.052	0.089	-0.205	-0.068	0.349
	(0.167)	(0.191)	(0.185)	(0.198)	(0.175)	(0.267)	(0.190)	(0.272)
Welfare Waiver Stick	-0.208	0.060	-0.122	-0.098	-0.244	-0.160	-0.146	-0.435
	(0.229)	(0.223)	(0.254)	(0.278)	(0.194)	(0.274)	(0.197)	(0.296)
Welfare Waiver Carrot	0.095	0.107	-0.115	-0.249	0.287**	0.177	-0.075	-0.227
	(0.161)	(0.172)	(0.173)	(0.198)	(0.133)	(0.192)	(0.135)	(0.186)
African American	-0.107	-0.127	0.340**	0.385**	-0.252**	-0.074	0.029	0.170
	(0.073)	(0.084)	(0.075)	(0.084)	(0.075)	(0.110)	(0.074)	(0.112)
Hispanic	-0.141*	-0.100	0.190**	0.129	0.007	0.094	-0.059	-0.036
	(0.081)	(0.093)	(0.088)	(0.104)	(0.083)	(0.127)	(0.089)	(0.127)
Highest Grade Completed	0.406**	0.501**	-0.590**	-0.666**	0.204**	0.258**	-0.302**	-0.313**
	(0.060)	(0.069)	(0.064)	(0.075)	(0.060)	(0.093)	(0.062)	(0.092)
Age	-0.039**	-0.025**	-0.029**	-0.026**	0.000	-0.002	-0.029**	-0.033**
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.008)	(0.006)	(0.008)
Never Married	-0.350**	-0.273**	0.226**	0.277**	-0.124*	-0.116	0.104	0.215*
	(0.072)	(0.082)	(0.075)	(0.086)	(0.073)	(0.113)	(0.074)	(0.112)
# of Kids < 18	0.001	0.040	0.033	0.015	0.011	-0.020	0.013	0.065
	(0.034)	(0.037)	(0.039)	(0.044)	(0.036)	(0.054)	(0.035)	(0.050)
Age of Youngest Child	0.006	-0.012	-0.016	-0.024**	0.005	0.003	-0.005	0.001
	(0.011)	(0.013)	(0.010)	(0.012)	(0.011)	(0.016)	(0.011)	(0.017)
# of Kids < 6	-0.284**	-0.357**	0.125**	0.099	-0.139**	-0.183*	-0.015	-0.050
	(0.055)	(0.066)	(0.061)	(0.074)	(0.064)	(0.096)	(0.060)	(0.089)
Disability	-0.574**	-0.602**	0.750**	0.832**	-0.386**	-0.490**	0.399**	0.341**
	(0.079)	(0.088)	(0.080)	(0.091)	(0.077)	(0.122)	(0.076)	(0.120)
Disability Variable Missing	-0.208**	-0.423**	0.334**	0.012	-0.401**	-0.642**	0.096	-0.315
	(0.101)	(0.123)	(0.105)	(0.138)	(0.134)	(0.249)	(0.132)	(0.221)

log(duration)	0.380**	-0.727**	0.312**	-0.717**	-0.306**	-0.831**	-0.104	-0.953**
	(0.126)	(0.058)	(0.154)	(0.061)	(0.099)	(0.078)	(0.115)	(0.084)
log(duration)^2	-0.221**		-0.210**		-0.087**		-0.187**	
	(0.036)		(0.043)		(0.035)		(0.041)	
Seam Bias Correction Model - Seam Bias Correction Parameters								
H1 (non-employment)	0.111**	**H1 (employment)**	0.489**					
	(0.034)		(0.029)					
H2 (non-employment)	0.140**	**H2 (employment)**	0.274**					
	(0.032)		(0.031)					
H3 (non-employment)	0.265**	**H3 (employment)**	0.270**					
	(0.030)		(0.031)					

Notes:
Year dummies are included in each regression. Coefficients are omitted.
Standard errors are in parentheses.
Maximum benefit variable has been divided by 10,000.
* significant at 10% level.
** significant at 5% level.

Table 3.1 Duration Models of Welfare and Off Welfare Spells (With Unobserved Heterogeneity)
Single Mothers with High School or Lower Education

	Left-censored off welfare spells		Left-censored welfare spells		Fresh off welfare spells		Fresh welfare spells	
	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data
Maximum Welfare Benefit	-3.775	-0.621	-11.828**	-16.364**	6.995	8.537*	-15.683**	-13.856**
	(3.699)	(4.169)	(2.825)	(3.179)	(4.304)	(5.139)	(4.345)	(5.255)
Unemployment Rate	0.099**	0.079**	-0.090**	-0.063**	0.012	-0.024	-0.065*	-0.053
	(0.033)	(0.040)	(0.030)	(0.029)	(0.044)	(0.052)	(0.038)	(0.051)
Minimum Wage	0.069	0.010	-0.509*	0.028	0.250	0.035	0.568	0.117
	(0.270)	(0.311)	(0.274)	(0.265)	(0.395)	(0.519)	(0.370)	(0.463)
Welfare Waiver Stick	-0.255	-0.126	-0.128	-0.282	-0.305	-0.541	-0.265	-0.309
	(0.376)	(0.412)	(0.326)	(0.319)	(0.374)	(0.450)	(0.330)	(0.396)
Welfare Waiver Carrot	0.300	0.317	-0.143	-0.144	0.087	0.075	0.266	0.196
	(0.271)	(0.294)	(0.209)	(0.220)	(0.255)	(0.311)	(0.255)	(0.283)
African American	0.680**	0.892**	-0.112	-0.191*	0.083	0.181	-0.006	0.121
	(0.127)	(0.159)	(0.104)	(0.111)	(0.146)	(0.174)	(0.131)	(0.166)
Hispanic	0.344**	0.254	-0.100	-0.023	-0.075	-0.140	0.159	0.318
	(0.144)	(0.173)	(0.122)	(0.127)	(0.171)	(0.208)	(0.161)	(0.208)
Highest Grade Completed	-0.924**	-0.264**	0.275**	0.087**	-0.109	-0.017	0.303**	0.072*
	(0.110)	(0.037)	(0.088)	(0.026)	(0.123)	(0.041)	(0.114)	(0.038)
Age	-0.042**	-0.037**	-0.029**	-0.019**	-0.027**	-0.033**	-0.008	-0.002
	(0.009)	(0.010)	(0.008)	(0.009)	(0.012)	(0.015)	(0.010)	(0.012)
Never Married	0.261**	0.420**	-0.372**	-0.276**	0.084	0.005	-0.127	-0.063
	(0.130)	(0.156)	(0.105)	(0.110)	(0.153)	(0.185)	(0.140)	(0.180)
# of Kids < 18	0.265**	0.227**	-0.121**	-0.030	0.038	0.046	0.041	0.049
	(0.063)	(0.074)	(0.049)	(0.050)	(0.075)	(0.087)	(0.075)	(0.091)
Age of Youngest Child	-0.070**	-0.084**	0.028*	0.016	-0.037	-0.040	0.016	-0.005
	(0.017)	(0.020)	(0.016)	(0.017)	(0.025)	(0.030)	(0.020)	(0.024)
# of Kids < 6	0.087	-0.025	-0.149**	-0.171**	-0.202*	-0.202	-0.146	-0.166
	(0.095)	(0.119)	(0.075)	(0.080)	(0.120)	(0.141)	(0.110)	(0.134)

log(duration)	0.056	-0.363**	0.620**	0.695	4.544**	-1.142**	5.047**	-0.799**
	(0.197)	(0.121)	(0.220)	(0.534)	(1.786)	(0.139)	(1.633)	(0.123)
log(duration)^2	-0.098*		-0.200**	-0.237**	-3.103**		-2.800**	
	(0.057)		(0.059)	(0.121)	(1.095)		(0.925)	
log(duration)^3					0.523**		0.412**	
					(0.202)		(0.161)	
Unobserved Heterogeneity								
Theta1	-5.352**	-1.359	-0.108	-1.971*	-4.654**	0.627	-6.556**	-1.158
	(1.050)	(1.271)	(1.027)	(1.113)	(1.714)	(1.921)	(1.545)	(1.720)
Theta2	-2.489**	1.681	-3.805	-5.405	-5.059**	0.297	-6.162**	-0.692
	(1.013)	(1.273)	(5.777)	(9.377)	(1.724)	(1.934)	(1.557)	(1.754)
Seam Bias Correction Model			**Last Month Data Model**					
Heterogeneity Probability	0.721**		0.748**					
	(0.051)		(0.063)					
Seam Bias Correction Parameters								
alpha_1 (off welfare)	0.431**	**alpha_1 (welfare)**	0.506**					
	(0.051)		(0.043)					
alpha_2 (off welfare)	0.348**	**alpha_2 (welfare)**	0.385**					
	(0.047)		(0.046)					
alpha_3 (off welfare)	0.503**	**alpha_3 (welfare)**	0.569**					
	(0.042)		(0.037)					

Notes:
Year dummies are included in each regression. Coefficients are omitted.
Standard errors are in parentheses.
Maximum benefit variable has been divided by 10,000.
* significant at 10% level.
** significant at 5% level.

Table 3.2 Duration Models of Welfare and Off Welfare Spells (No Unobserved Heterogeneity)
Single Mothers with High School or Lower Education

	Left-censored off welfare spells		Left-censored welfare spells		Fresh off welfare spells		Fresh welfare spells	
	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data	Seam Bias Correction	Last Month Data
Intercept	-3.809**	-0.129	-0.740	-2.279**	-4.594**	0.672	-6.532**	-1.188
	(0.873)	(1.033)	(0.946)	(1.043)	(1.732)	(1.948)	(1.543)	(1.713)
Maximum Welfare Benefit	-2.350	-0.469	-10.658**	-15.256**	8.128*	9.072*	-15.909**	-13.875**
	(2.721)	(3.071)	(2.544)	(2.746)	(4.312)	(5.140)	(4.306)	(5.166)
Unemployment Rate	0.077**	0.064**	-0.086**	-0.064**	0.009	-0.026	-0.059	-0.047
	(0.026)	(0.031)	(0.027)	(0.027)	(0.044)	(0.052)	(0.038)	(0.050)
Minimum Wage	0.092	0.027	-0.477*	0.016	0.208	0.018	0.597	0.148
	(0.240)	(0.273)	(0.262)	(0.255)	(0.399)	(0.528)	(0.369)	(0.462)
Welfare Waiver Stick	-0.371	-0.195	-0.114	-0.264	-0.297	-0.518	-0.267	-0.300
	(0.326)	(0.353)	(0.301)	(0.299)	(0.374)	(0.451)	(0.331)	(0.398)
Welfare Waiver Carrot	0.189	0.176	-0.130	-0.112	0.101	0.080	0.234	0.180
	(0.207)	(0.219)	(0.185)	(0.199)	(0.255)	(0.310)	(0.254)	(0.285)
African American	0.541**	0.699**	-0.108	-0.170*	0.089	0.174	-0.021	0.108
	(0.093)	(0.108)	(0.091)	(0.099)	(0.146)	(0.174)	(0.130)	(0.164)
Hispanic	0.278**	0.230*	-0.085	-0.013	-0.100	-0.154	0.161	0.323
	(0.107)	(0.130)	(0.108)	(0.115)	(0.172)	(0.209)	(0.160)	(0.204)
Highest Grade Completed	-0.671**	-0.183**	0.244**	0.080**	-0.130	-0.020	0.337**	0.079**
	(0.077)	(0.022)	(0.077)	(0.023)	(0.123)	(0.040)	(0.111)	(0.036)
Age	-0.030**	-0.029**	-0.025**	-0.018**	-0.029**	-0.035**	-0.005	0.002
	(0.007)	(0.007)	(0.007)	(0.008)	(0.012)	(0.015)	(0.010)	(0.012)
Never Married	0.183*	0.274**	-0.326**	-0.253**	0.097	0.005	-0.145	-0.092
	(0.096)	(0.111)	(0.091)	(0.097)	(0.155)	(0.186)	(0.139)	(0.178)
# of Kids < 18	0.164**	0.147**	-0.112**	-0.031	0.041	0.047	0.027	0.026
	(0.045)	(0.052)	(0.044)	(0.046)	(0.076)	(0.088)	(0.074)	(0.089)
Age of Youngest Child	-0.060**	-0.070**	0.024*	0.013	-0.034	-0.037	0.012	-0.009
	(0.014)	(0.016)	(0.014)	(0.015)	(0.025)	(0.030)	(0.020)	(0.024)
# of Kids < 6	0.040	-0.043	-0.137**	-0.161**	-0.194	-0.195	-0.146	-0.171
	(0.069)	(0.086)	(0.070)	(0.075)	(0.119)	(0.140)	(0.110)	(0.133)

log(duration)	-0.064	-0.636**	0.635**	0.753	4.570**	-1.132**	5.116**	-0.765**
	(0.185)	(0.077)	(0.215)	(0.520)	(1.788)	(0.138)	(1.629)	(0.123)
log(duration)^2	-0.131**		-0.224**	-0.264**	-3.132**		-2.852**	
	(0.054)		(0.056)	(0.115)	(1.096)		(0.921)	
log(duration)^3					0.531**		0.425**	
					(0.202)		(0.160)	

Seam Bias Correction Model - Seam Bias Correction Parameters

alpha_1 (off welfare)	0.432**	**alpha_1 (welfare)**	0.506**
	(0.050)		(0.043)
alpha_2 (off welfare)	0.346**	**alpha_2 (welfare)**	0.384**
	(0.047)		(0.046)
alpha_3 (off welfare)	0.503**	**alpha_3 (welfare)**	0.569**
	(0.042)		(0.037)

Notes:
Year dummies are included in each regression. Coefficients are omitted.
Standard errors are in parentheses.
Maximum benefit variable has been divided by 10,000.
* significant at 10% level.
** significant at 5% level.

Table 4 Expected Durations of Employment and Nonemployment Spells (With Unobserved Heterogeneity)[1]

spell type	length of expected duration	
	Seam Bias Correction	Last Month Data
interrupted non-employment spell	34.58877	38.83405
interrupted employment spell	36.85882	41.15719
fresh non-employment spell	20.20564	23.3422
fresh employment spell	24.88404	32.56326

1. Expected durations are calculated based on the two models reported in Table 2.1.

Appendix

Derivation of the Multi-Spell Contribution to the Likelihood Function

We must find the contribution of the employment history in Figure 3 in the text. We have

$$pr(\{M_{U'}{}^{obs}=1, dur_{U'}^{obs}=5, M_E{}^{obs}=4, dur_E^{obs}=7, M_U{}^{obs}=0, dur_U^{obs}=24\}$$
$$= pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{obs}=4, M_E{}^{true}=4, dur_E^{true}=7, M_U{}^{obs}=0, M_U{}^{true}=0, dur_U^{true}=24\}+$$
$$pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{obs}=4, M_E{}^{true}=3, dur_E^{true}=6, M_U{}^{obs}=0, M_U{}^{true}=0, dur_U^{true}=25\}+$$
$$pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{obs}=4, M_E{}^{true}=2, dur_E^{true}=5, M_U{}^{obs}=0, M_U{}^{true}=0, dur_U^{true}=26\}+$$
$$pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{obs}=4, M_E{}^{true}=1, dur_E^{true}=4, M_U{}^{obs}=0, M_U{}^{true}=0, dur_U^{true}=27\}$$

$$=\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=4\}\bullet pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{true}=4, dur_E^{true}=7, M_U{}^{true}=0, dur_U^{true}=24\}\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=3\}\bullet pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{true}=3, dur_E^{true}=6, M_U{}^{true}=0, dur_U^{true}=25\}\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=2\}\bullet pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{true}=2, dur_E^{true}=5, M_U{}^{true}=0, dur_U^{true}=26\}\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=1\}\bullet pr\{M_{U'}{}^{true}=1, dur_{U'}^{true}=5, M_E{}^{true}=1, dur_E^{true}=4, M_U{}^{true}=0, dur_U^{true}=27\}\right]$$

$$=\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=4\}\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=7, M_U{}^{true}=0, dur_U^{true}=24]\bullet pr(M_{U'}{}^{true}=1, M_E{}^{true}=4)\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=3\}\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=6, M_U{}^{true}=0, dur_U^{true}=25]\bullet pr(M_{U'}{}^{true}=1, M_E{}^{true}=3)\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=2\}\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=5, M_U{}^{true}=0, dur_U^{true}=26]\bullet pr(M_{U'}{}^{true}=1, M_E{}^{true}=2)\right]+$$
$$\left[pr\{M_E{}^{obs}=4 \middle| M_E{}^{true}=1\}\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=4, M_U{}^{true}=0, dur_U^{true}=27]\bullet pr(M_{U'}{}^{true}=1, M_E{}^{true}=1)\right]$$

$$=\left[pr[dur_{U'}^{true}=5, dur_E^{true}=7, M_U{}^{true}=0, dur_U^{true}=24]\bullet(1/16)\right]+$$
$$\left[\alpha_3\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=6, M_U{}^{true}=0, dur_U^{true}=25]\bullet(1/16)\right]+$$
$$\left[\alpha_2\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=5, M_U{}^{true}=0, dur_U^{true}=26]\bullet(1/16)\right]+$$
$$\left[\alpha_1\bullet pr[dur_{U'}^{true}=5, dur_E^{true}=4, M_U{}^{true}=0, dur_U^{true}=27]\bullet(1/16)\right]$$

Thus we have

$$L = (1/16)\int_{\theta}\left[\left(1-\alpha_1^U\right)\prod_{r=1}^{4}\left(1-\lambda_{U'}\left(r \mid \theta_{U'}\right)\right)\cdot\lambda_{U'}\left(5\middle|\theta_{U'}\right)\right]\cdot$$

$$\left\{\begin{array}{l}\left[\alpha_1^E\prod_{r=1}^{3}\left(1-\lambda_E(r \mid \theta_E)\right)\cdot\lambda_E(4 \mid \theta_E)\prod_{r=1}^{27}\left(1-\lambda_U(r \mid \theta_U)\right)\right] \\ +\left[\alpha_2^E\prod_{r=1}^{4}\left(1-\lambda_E(r \mid \theta_E)\right)\cdot\lambda_E(5 \mid \theta_E)\prod_{r=1}^{26}\left(1-\lambda_U(r \mid \theta_U)\right)\right] \\ +\left[\alpha_3^E\prod_{r=1}^{5}\left(1-\lambda_E(r \mid \theta_E)\right)\cdot\lambda_E(6 \mid \theta_E)\prod_{r=1}^{25}\left(1-\lambda_U(r \mid \theta_U)\right)\right] \\ +\left[\prod_{r=1}^{6}\left(1-\lambda_E(r \mid \theta_E)\right)\cdot\lambda_E(7 \mid \theta_E)\prod_{r=1}^{24}\left(1-\lambda_U(r \mid \theta_U)\right)\right]\end{array}\right\} d\Phi(\theta_{U'},\theta_E,\theta_U).$$