Correcting for Seam Bias when Estimating Discrete Variable Models, with an Application to Analyzing the Employment Dynamics of Disadvantaged Women in the SIPP[1]

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Seam Bias occurs when respondents are asked retrospectively about economic actions and conditions, and tend to heap changes to the end of the retrospective period. It occurs, for example, in the Survey of Income and Program Participation (SIPP), various data sets from the National Longitudinal Survey (NLS) and banking data. Pischke (1995) proposes a method for dealing with seam bias when using continuous data. Here we address the issues that occur with discrete variable models when seam bias is present. We focus specifically on estimating duration models as these are likely to be most severely affected by seam bias since the seam bias affects the timing of transitions. The extension to other discrete variable models is straight forward, as is the extension to fully structural models based explicitly on optimizing behavior. In particular we focus on the employment dynamics of single mothers with a high school degree or less using duration models. Researchers and policymakers have long been interested in this group of women, in particular because these are the women whose families are most likely to be in poverty. 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 seam bias, and we propose and implement a number of potential solutions for this problem. These include maximum likelihood estimation (MLE) of misreporting models both where the misreporting is constant across individuals and where it varies with demographic characteristics, as well as the simpler solutions of i) using only data on the month prior to the interview, and ii) using a dummy variable coded one for the seam months. We discuss identification of the MLE models, and show that they are identified without restricting the functional form of the duration dependence. We find that using only the seam month data affects the statistical significance of individual explanatory variables, but does not appear to affect the estimates of the effect of changing an explanatory variable on expected duration. Using the seam month dummy approach appears to produce estimates quite similar to those from the seam bias correction models in terms of significance and the effect of an explanatory variable on the expected duration. Finally, even though race significantly affects the misreporting probability, assuming this probability is constant across individuals does not appear to lead to biased estimates.

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I. Introduction

Seam Bias occurs when respondents are asked retrospectively about economic actions and conditions, and tend to heap changes to the end of the retrospective period. It occurs, for example, in the Survey of Income and Program Participation (SIPP), the Current Population Survey (CPS), various data sets from the National Longitudinal Survey (NLS) and banking data.[2] Pischke (1995) proposes a method for dealing with seam bias when using continuous data. Here we address the issues that occur with limited dependant variable models when seam bias is present. We focus specifically on estimating duration models as these are likely to be most severely affected by seam bias since the seam bias affects the timing of transitions, but the extension to other limited dependent variable models is straight forward.[3]

Transitions into and out of employment are of crucial importance to policymakers, as they determine unemployment rates, poverty rates and the overall well-being of low-income individuals. In this paper we use monthly discrete time duration models to analyze the employment 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 using SIPP for the period 1986-1995. (Welfare reform via TANF occurred in 1996, motivating our use of this sample period.) 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, as noted above, 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.

Welfare 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 also have examined transitions into welfare. Note that employment dynamics and welfare dynamics will differ, as single mothers can work and collect welfare simultaneously, and can certainly be out of employment and off-welfare simultaneously. Ham and LaLonde (1996) and Eberwein, Ham and LaLonde (1997) have analyzed employment dynamics using data from the National Supported Work and Job Training Partnership Act

[2] See Chakravarty and Sarkar (1999).
[3] See Romeo (2001) for a very different approach to dealing with seam bias in a duration model.

training experiments respectively. Aaronson and Pingle (2006) study employment dynamics among single mothers in the 1990-2001 SIPP panels.[4]

Given the appropriateness of SIPP for analyzing program participation dynamics, most of the welfare duration studies have estimated some form of discrete time hazard models using this data.[5] 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.[6]

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 sub period 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.

We show that this latter 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 four-

[4] As noted above, we prefer not to pool the pre-TANF and post-TANF data.
[5] 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.
[6] Below we suggest dropping the fourth month coefficient in the hazard function and increase the constant in the hazard by the fourth month coefficient divided by four when calculating expected durations or simulating the model.

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 parameters representing the propensity to underreport transitions in each of the three other months. In the first case we assume that misreporting behavior is constant across individuals, while in the second case we allow for misreporting to depend on demographic variables. We show that both models are 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 (non-employment) employment spells in progress at the start of the sample and (non-employment) employment spells beginning after the start of the sample share the same misreporting parameters. The extension to other discrete variable models is straight forward, as is the extension to fully structural models based explicitly on optimizing behavior.

We also carry out the estimation of the duration models using i) only the last month data and ii) putting in a dummy in the hazard for interview month 4 and then adjusting the constant using the coefficient of this dummy variable.[7] We find that using only the seam month data affects the statistical significance of individual explanatory variables, but does not appear to affect the estimates of the effect of changing an explanatory variable on expected duration. Using the seam month dummy approach appears to produce estimates quite similar to those from the seam bias correction models in terms of significance and the effect of an explanatory variable on the expected duration. Finally, even though race significantly affects the misreporting probability, assuming this probability is constant across individuals does not appear to lead to biased estimates.

[7] Previous work using a month four dummy variable did not adjust the constant, which will lead to an overestimate of expected durations.

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 present our empirical results in Section V. 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.[8] 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.[9] 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.[10] 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 period. This

[8] In later drafts we will report results adding the 1996 SIPP and hopefully the 2001 SIPP.

[9] 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.

[10] Hamersma (2006) investigates unique a Wisconsin administrative data set containing information from all the Work Opportunity Tax Credit (WOTC) and Welfare-to-Work (WtW) Tax Credit 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.

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) are reported to occur in month 4, the last month. This number is 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 time spent in employment and time spent out of employment.[11] The left-censored spells constitute the great majority of all spells as is shown Table 1. For example, even in month 24 of the various SIPP samples, i.e. two years after the samples began, left-censored spells constitute over sixty percent of both employment spells and non-employment spells. This raises two issues. First, using only fresh spells will not give a picture of the employment dynamics of a typical woman in our sample, who is in a left-censored spell. Secondly, there is likely to be an important selection issue on who makes it to a fresh spell. Thus we feel it is important to analyze left-censored and fresh spells jointly.[12]

Table 2 provides summary statistics for our sample 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. Those in left-censored non-employment spells 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 non-employment 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. 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 a longer period. The lower panel of Table 2 shows that single mothers in left-censored employment spells

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

[12] While this selection bias may be important in principal, in practice Eberwein, Ham and LaLonde (1997) found it was not important in analyzing employment dynamics for similar women using data on SIPP, and in fact we find little evidence of this selection bias here, thus reinforcing the earlier result of Eberwein et al.

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 missing disability information, and tend to have fewer children than those in fresh employment spells.

III. Problems in Duration Models 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 four months. We would lose both a two-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 five 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 three 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 four 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.[13] 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

[13] Hypothetically, if using the last month data leads to disproportionate misclassification, for example, from a short left-censored non-employment spell into a long left-censored employment spell as shown in Figure 1.4, it is possible 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.

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.

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 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 differences in the number of spells 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.[14] 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.[15]

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. Under reasonable assumptions, we can identify parameters describing the response errors due to seam bias. We first set up our notation before

[14] 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.
[15] 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.

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 the month during which the transition occurs in a reference period.[16]

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

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

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

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

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

[16] 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.

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

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

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:

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

where $\Phi(\theta)$ 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

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

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:

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

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

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 seven 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 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 $\{M_{U'}^{obs} = 1, M_E^{obs} = 4, M_U^{obs} = 0\}$, 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:

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

where c is a constant that does not affect the maximization. The first term in square brackets is the contribution from the first spell, which lasted for five months and ended in month 1. Given our assumptions, there is no uncertainty when this spell ended. 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 for employment spells. (The argument for non-employment spells is identical.) 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

$$\lambda(t-3) = E[m_1(t-3)]/(1-\alpha_1),$$
$$\lambda(t-2) = E[m_2(t-2)]/(1-\alpha_2),$$
$$\lambda(t-1) = E[m_3(t-1)]/(1-\alpha_3).$$

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 the model's substantial 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

$$\begin{aligned}
&E[m_4(t)] \approx \alpha_1\lambda(t-3) + \alpha_2\lambda(t-2) + \alpha_3\lambda(t-1) + \lambda(t),\\
&E[m_1(t-3)] \approx (1-\alpha_1)\lambda(t-3),\\
&E[m_2(t-2)] \approx (1-\alpha_2)\lambda(t-2),\\
&E[m_3(t-1)] = (1-\alpha_3)\lambda(t-1).
\end{aligned} \tag{4.13}$$

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 employment) 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.

We also consider models where the misreporting probabilities depend on individual characteristics. For example, suppose the probabilities differ among Whites and non-Whites (African Americans and Latinos). We assume that for individual *i* the misreporting probability in interview month j for employment spells is given by

$$\alpha_{ij}^{e} = (1 + \exp - (\gamma_{0j}^{e} + \gamma_{1}^{e} NW_{i}))^{-1} \ , \tag{4.14}$$

where NW_i is a dummy variable equal to 1 if the individual is non-White and zero otherwise.

Note that one could estimate this model by estimating the model with constant misreporting probabilities on a sample of Whites and then on a sample of non-Whites, and then use these probabilities in a minimum distance procedure to estimate the parameters in (4.14). Thus this extended model is also identified, and we found that the misreporting probabilities did indeed vary significantly by race. In our empirical work we also let the misreporting probabilities depend on whether a woman had a high school degree but not on her race, and the misreporting probabilities did not vary significantly by education. Finally, when we tried to let the misreporting probabilities in (4.14) depend on both education and race, we lost empirical identification and our model tended to 'blow up'.

4.5 Alternative Misclassification Schemes

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 4, 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 seminar participant raised the possibility that the data 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.

Finally, a seminar participant suggested that individuals may forget about very short spells starting in interview months 1, 2 and 3. In other words, the number of transitions in month 4 is accurately reported, but the number of transitions in months 1, 2 and 3 are underestimated.

To shed some light on the first two alternative explanations, we look at the fraction of transitions reported in each reference month for employment/non-employment. 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. The only model consistent with this data is the model we use; indeed it seems at this level that the even stronger hypothesis $\alpha_1 = \alpha_2 = \alpha_3$ is consistent with the data.

It is difficult to address the last suggestion that individuals forget about short spells starting in months 1, 2 and 3 without access to administrative data. However, we do know that in administrative data short spells are much more frequent in employment duration than in welfare duration for the mothers we study. Thus we would expect the differences between month 4 and the other months to be much more pronounced in employment data than in welfare data We will provide evidence on this issue at the seminar.

V. Empirical Results

Tables 3.1 and 3.2 present results for our misreporting model and the approach of using only the last month data for the case with and without unobserved heterogeneity respectively. Note that we believe these coefficients are of substantial interest since the employment dynamics of women with low levels of schooling has received much less attention in the literature than the welfare dynamics. For these models and those we report below, 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, as suggested by Ham, Svejnar and Terrell (1998) and Baker and Melino (2001). Columns (1) and (2) of Table 3.1 contain the estimates for the left-censored non-employment spells from our misreporting model and the last month data when we allow for unobserved heterogeneity, while columns (1) and (2) of Table 3.2 show our results when we ignore unobserved heterogeneity. Note that unlike the standard case, the log-likelihood function does become additively separable in the different types of spells because we still must allow for seam bias, and thus the computational gain from ignoring unobserved heterogeneity is not as large as in the standard case.

The choice of explanatory variables is standard and detailed in the Tables.[17] The hazard is parameterized such that a negative coefficient implies that the hazard decreases if we increase the explanatory variable. We find substantial evidence of misreporting as all of the α terms are statistically and economically significant. Considering first our seam bias correction estimates, we see that higher welfare benefits, a higher unemployment rate, growing older, having never been married, having more children under six years of age, having a disability, or reporting a missing value for whether she has a disability all significantly (at the 10% significance level or lower) lowers the probability (in a partial correlation sense) that a woman leaves a left-censored non-employment spell. On the other hand, having a high school diploma is the only variable that significantly increases the probability of leaving such a spell. In terms of a left-censored employment spell, we see that an increase in the unemployment rate, being African American or Hispanic, never having been married, having more children under age six, having a disability, or not answering the disability question is associated with significantly shorter left-censored employment spells, while being older or having a high school diploma is associated with significantly longer left-censored employment spells. We have substantially less fresh employment and non-employment spells so it is not surprising that fewer variables are statistically significant. For the fresh non-employment spells, being offered a 'carrot' to leave welfare significantly reduces the length of a fresh non-employment spell, as does having a high school diploma. An increase in maximum welfare benefits, an increase in the unemployment rate, being African American or Hispanic, having more children under the age of six, 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.) Finally, considering fresh employment spells, having a high school diploma and growing older significantly decrease the exit rate from employment, while an increase in benefits and having a disability increases the exit rate from a fresh employment spell. Interestingly, all statistically significant coefficients have the expected sign. Also this study is the first to inquire whether one can see a role for the minimum wage in terms of employment dynamics of disadvantaged women at the micro level, and we do not find such an effect.

There are differences between the estimates based on our procedure and those based on using only the last month data in terms of statistical significance. While there are no differences in the left-censored non-employment spell, in the left-censored employment spells the number of

[17] 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.

kids less than six and disability missing are statistically significant with the seam bias correction but not using the last month data, while the age of the youngest child is significant with the last month data but not with the seam bias correction. In the fresh non-employment spells, maximum weekly benefits, the unemployment rate, the welfare waiver carrot, and being African American are significant in the seam bias correction estimates but not in the estimates based on the last month data. Finally, being African American and never married significantly increases the probability of leaving a fresh employment spell when we use the last month data but not when we use the seam bias correction, and in even here the difference in p-values is not very large.

In Tables 5 and 6 we compare estimates from a seam bias correction where the misreporting probabilities are constant across individuals to one where the misreporting probabilities depend on whether one is non-White. Interestingly, while we find that Whites have significantly lower misreporting probabilities, the coefficients and significance levels are remarkably similar for the two models.

The magnitudes of the coefficients of the hazard model are not directly comparable between the seam bias correction models and the model using the last month data, although they are comparable between the seam bias correction models and the last month dummy variable model. 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 $(t-1)th$ months (where t is a multiple of 4 months), given that the spell has lasted up to and including the $(t-4)th$ month.[18] To compare the two sets of estimates, below we calculate expected durations and the effect of changing an explanatory variable on expected duration.

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

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

[18] In the next draft we will give a more precise expression.

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

$$ED_j = \int_{\Theta} t \cdot f_j\left(t \middle| \theta_j\right) d\Phi_j\left(\theta_j\right),$$

where $\Phi_j\left(\theta_j\right)$ 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:

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

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 follow Eberwein, Ham and LaLonde (2002) and freeze the hazard function for durations longer than 15 months at 15 months for fresh spells and freeze 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.[20]

In Table 7 we compare the average expected duration and the effect on expected duration of changing an explanatory variable for the four models. In this draft we only present point estimates; in future drafts we will also present standard errors. As expected, using only the last month data leads to longer expected durations than the estimates based on the seam bias correction. However, we should not overstate this result, since there are also substantial (percentage) differences in the expected durations based on the two seam bias corrections and the last month dummy. This latter result occurs because the duration dependence parameters and the parameters of the unobserved heterogeneity distribution are affected by how we estimate the model. Fortunately, the estimated effects on expected duration of changing an explanatory variable are quite similar across models, including the model based on the last month data. Our results are consistent with the Heckman-Singer (1984b) Monte Carlo experiments that suggest that allowing for unobserved heterogeneity can help one avoid bias in the coefficients on the explanatory variables, but that it is difficult to accurately recover the distribution of the unobserved heterogeneity.

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[19] The longest panel in our data lasts 40 months.
[20] Our results are insensitive to reasonable changes in these limits.

VI. Summary and Conclusions

Transitions into and out of employment are of crucial importance to policymakers, as they determine unemployment rates, poverty rates and the overall well-being of low-income individuals. In this paper we estimate monthly transition rates into and out of employment using the Survey of Income and Program Participation (SIPP). Such employment dynamics have been relatively understudied in the literature, given the policy focus on single mothers.

In this study we propose a parametric approach to seam bias in a duration model setting. We develop a monthly discrete time duration model with parameters representing the propensity to underreport transitions in the first three of the four months in an interview wave. We first assume that misreporting behavior is constant across individuals, and then consider a second case where we allow for misreporting to depend on demographic variables. We show that both models are identified without restricting the form of the duration dependence. We also carry out the estimation of the duration models using i) only the last month data and ii) putting in a dummy in the hazard for interview month 4 and then adjusting the constant using the coefficient of this dummy variable.

We find a number of interesting empirical results concerning the employment dynamics of disadvantaged women. We find that using only the seam month data affects the statistical significance of individual explanatory variables, but does not appear to affect the estimates of the effect of changing an explanatory variable on expected duration. Using the seam month dummy variable approach appears to produce estimates quite similar to those from the seam bias correction models in terms of significance and the effect of an explanatory variable on the expected duration. Further, even though race significantly affects the misreporting probability, assuming this probability is constant across individuals does not appear to lead to biased estimates. Finally we find that while selection bias is a significant potential problem when one uses only fresh spells, it does not seem to be a serious problem in practice. This reinforces a similar result in Eberwein, Ham and LaLonde (1997).

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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

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

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

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

$$
\begin{aligned}
&= \Big[\, pr[dur_{U'}^{true} = 5, dur_{E}^{true} = 7, M_U{}^{true} = 0, dur_{U}^{true} = 24] \bullet (1/16) \Big] + \\
&\Big[\alpha_3 \bullet pr[dur_{U'}^{true} = 5, dur_{E}^{true} = 6, M_U{}^{true} = 0, dur_{U}^{true} = 25] \bullet (1/16) \Big] + \\
&\Big[\alpha_2 \bullet pr[dur_{U'}^{true} = 5, dur_{E}^{true} = 5, M_U{}^{true} = 0, dur_{U}^{true} = 26] \bullet (1/16) \Big] + \\
&\Big[\alpha_1 \bullet pr[dur_{U'}^{true} = 5, dur_{E}^{true} = 4, M_U{}^{true} = 0, dur_{U}^{true} = 27] \bullet (1/16) \Big].
\end{aligned}
$$

Thus we have

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

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

Figure 1.1

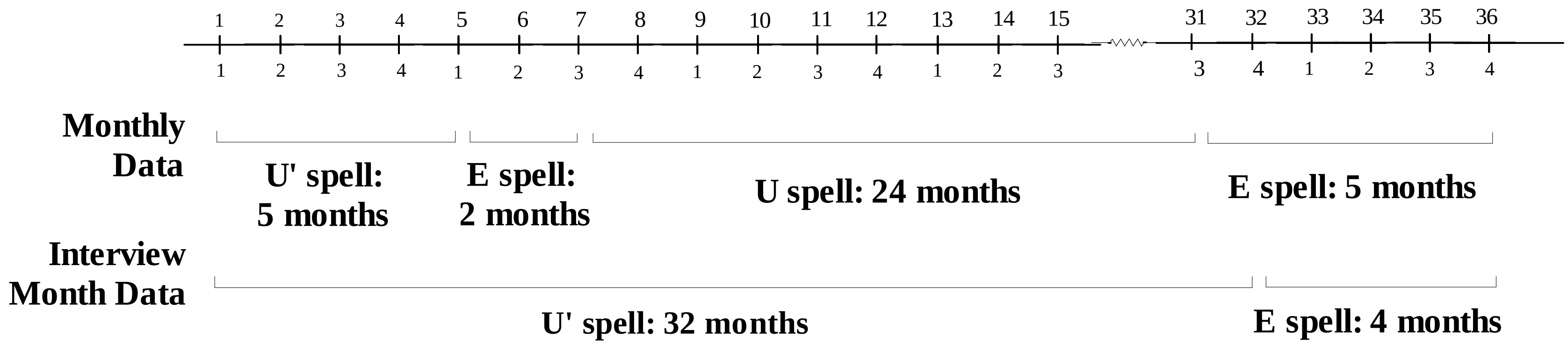

Figure 1.2

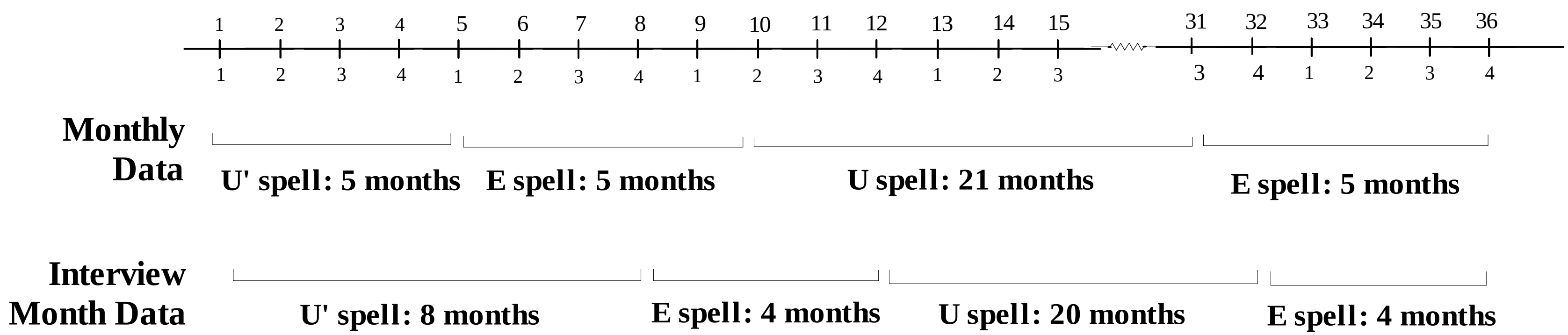

Figure 1.3

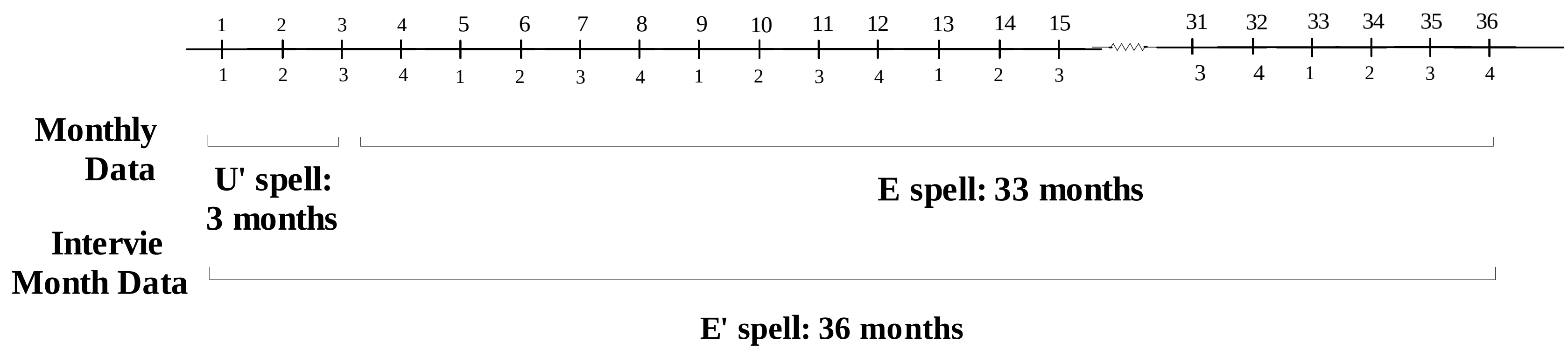

Figure 1.4

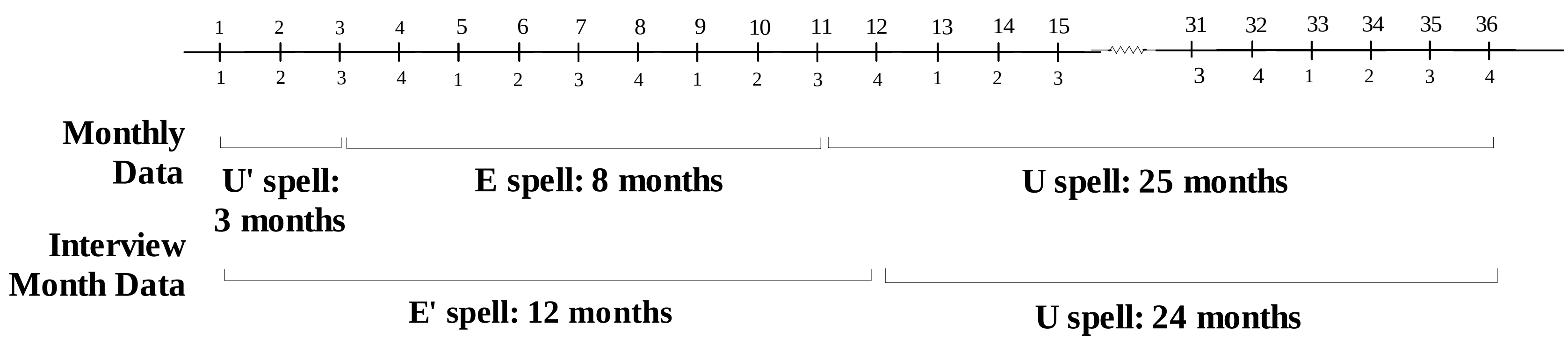

Figure 2.1

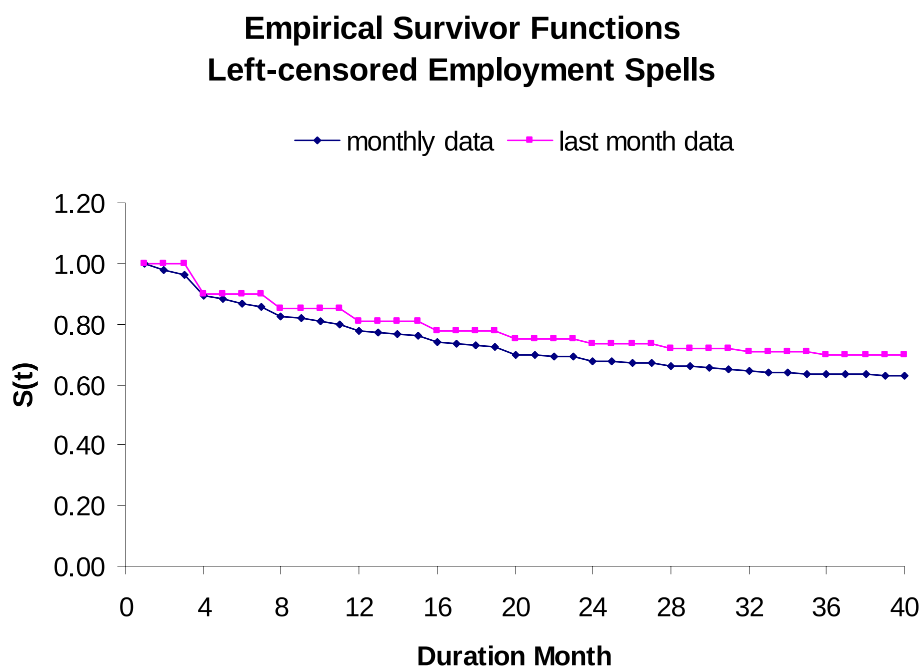
Empirical Survivor Functions
Left-censored Employment Spells
monthly data
last month data
S(t)
1.20
1.00
0.80
0.60
0.40
0.20
0.00
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 2.2

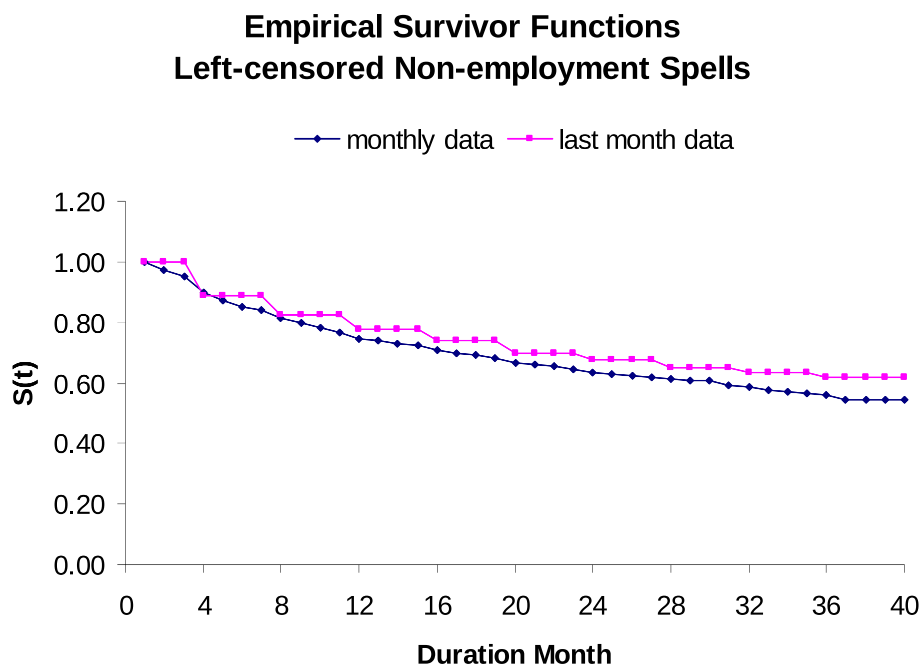
Empirical Survivor Functions
Left-censored Non-employment Spells
monthly data
last month data
1.20
1.00
0.80
0.60
0.40
0.20
0.00
S(t)
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 2.3

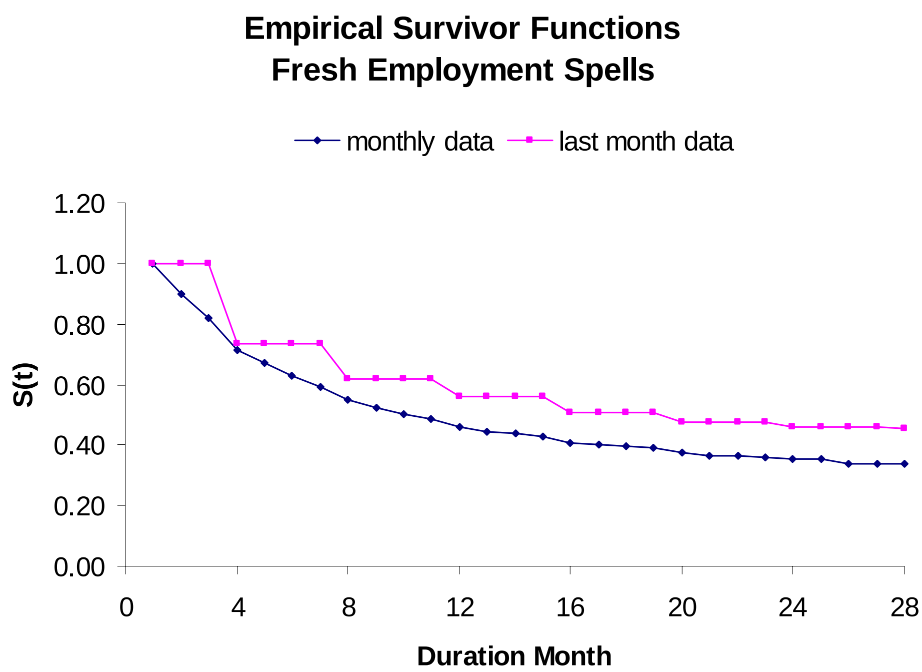
Empirical Survivor Functions
Fresh Employment Spells
monthly data
last month data
S(t)
1.20
1.00
0.80
0.60
0.40
0.20
0.00
0
4
8
12
16
20
24
28
Duration Month

Figure 2.4

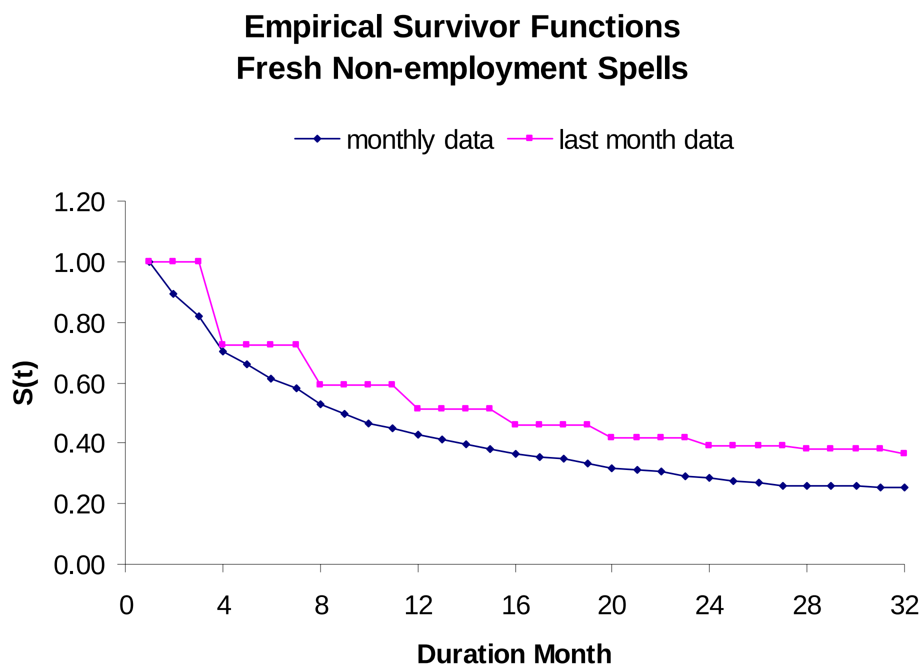
Empirical Survivor Functions
Fresh Non-employment Spells
monthly data
last month data
S(t)
1.20
1.00
0.80
0.60
0.40
0.20
0.00
0
4
8
12
16
20
24
28
32
Duration Month

Figure 3

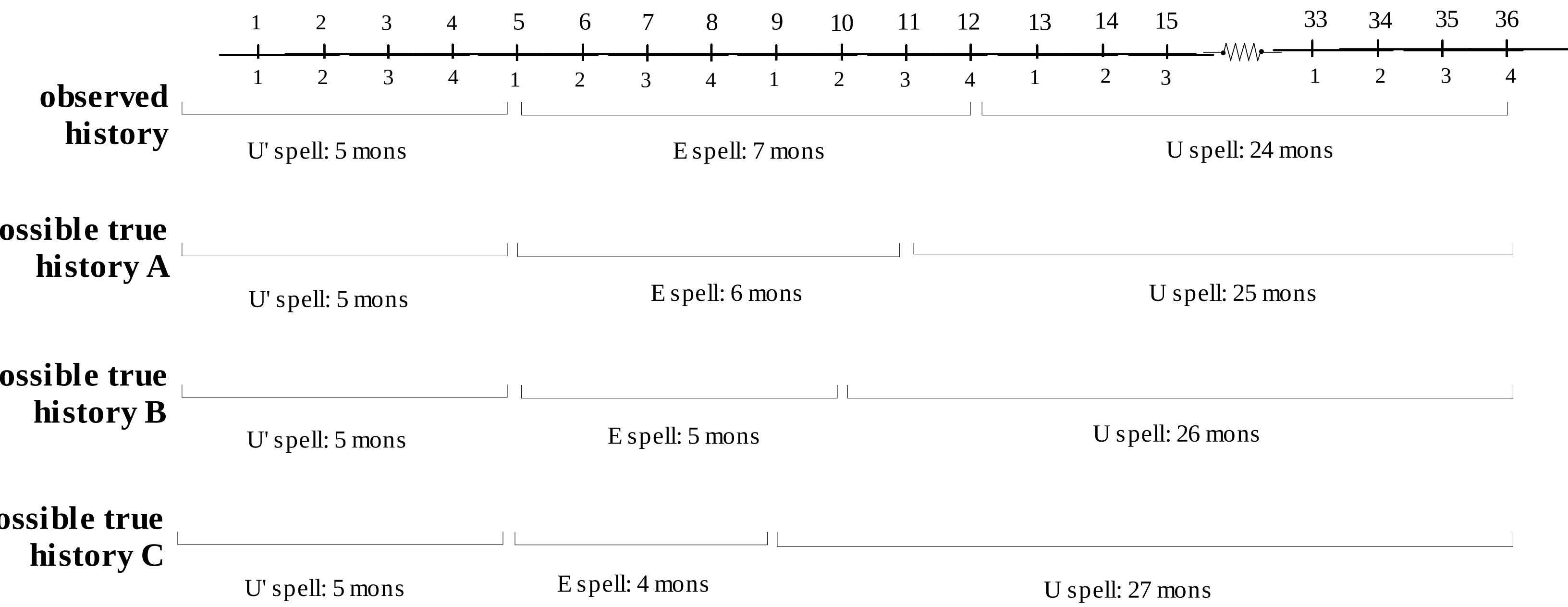
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 33 34 35 36
1 2 3 4 1 2 3 4 1 2 3 4 1 2 3 1 2 3 4
observed history
U' spell: 5 mons
E spell: 7 mons
U spell: 24 mons
ossible true history A
U' spell: 5 mons
E spell: 6 mons
U spell: 25 mons
ossible true history B
U' spell: 5 mons
E spell: 5 mons
U spell: 26 mons
ossible true history C
U' spell: 5 mons
E spell: 4 mons
U spell: 27 mons

Table 1 Fresh, Left Censored and All Spells by Survey Month

Survey Month	Fresh employment	Left-censored employment	Total	Left-censored/Total
2	92	3,759	3,851	0.9761
18	760	1,945	2,705	0.7190
24	862	1,667	2,529	0.6592
32	606	961	1,567	0.6133
36	318	416	734	0.5668

Survey Month	Fresh employment	Left-censored employment	Total	Left-censored/Total
2	50	3,409	3,459	0.9855
18	696	1,694	2,390	0.7088
24	641	1,440	2,081	0.6920
32	473	848	1,321	0.6419
36	89	127	216	0.5880

Table 2 Characteristics of Employment and Non-employment Spells
Single Mothers with High School or Lower Education

A. Non-employment spell	Left-censored spells Mean	Left-censored spells Std Dev	Fresh spells Mean	Fresh spells 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	

B. Employment spell	Left-censored spells Mean	Left-censored spells Std Dev	Fresh spells Mean	Fresh spells 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			

Notes:
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 3.1 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)
High School Diploma	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)

Table 3.1 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**
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)
Squared of log(duration)					-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)
Heterogeneity Probability	0.306**	0.265**						
	(0.053)	(0.125)						
Seam Bias Correction Parameters								
α1 (non-employment)	0.140**	**α1 (employment)**	0.515**					
	(0.032)		(0.027)					
α2 (non-employment)	0.135**	**α2 (employment)**	0.269**					
	(0.033)		(0.031)					
α3 (non-employment)	0.255**	**α3 (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 3.2 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)
High School Diploma	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)

Table 3.2 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**
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)
Squared of log(duration)	-0.221**		-0.210**		-0.087**		-0.187**	
	(0.036)		(0.043)		(0.035)		(0.041)	
Seam Bias Correction Model - Seam Bias Correction Parameters								
α1 (non-employment)	0.111**	**α1 (employment)**	0.489**					
	(0.034)		(0.029)					
α2 (non-employment)	0.140**	**α2 (employment)**	0.274**					
	(0.032)		(0.031)					
α3 (non-employment)	0.265**	**α3 (employment)**	0.270**					
	(0.030)		(0.031)					

Notes:
See notes to Table 3.1.

Table 4 Duration Models of Employment and Nonemployment Spells
Seam Bias Correction vs. A Simple Last-Month Dummy Approach

	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**	-10.130**	0.070	-0.756	-6.441**	-7.348**	4.086*	5.542**
	(2.654)	(2.122)	(2.621)	(2.262)	(2.158)	(2.462)	(2.165)	(2.393)
Unemployment Rate	-0.080**	-0.0742**	0.054**	0.038*	-0.049**	-0.054**	0.024	0.023
	(0.026)	(0.021)	(0.025)	(0.021)	(0.020)	(0.023)	(0.020)	(0.022)
Minimum Wage	0.138	0.140	0.100	0.127	0.117	0.072	-0.041	-0.094
	(0.194)	(0.166)	(0.200)	(0.175)	(0.177)	(0.189)	(0.191)	(0.200)
Welfare Waiver Stick	-0.241	-0.217	-0.051	-0.119	-0.248	-0.309	-0.145	-0.149
	(0.279)	(0.235)	(0.290)	(0.246)	(0.194)	(0.208)	(0.199)	(0.204)
Welfare Waiver Carrot	-0.009	0.085	-0.168	-0.104	0.285**	0.370**	-0.067	-0.060
	(0.203)	(0.166)	(0.197)	(0.170)	(0.133)	(0.150)	(0.137)	(0.148)
African American	-0.161	-0.117	0.423**	0.346**	-0.253**	-0.332**	0.018	0.065
	(0.100)	(0.077)	(0.090)	(0.076)	(0.075)	(0.087)	(0.075)	(0.083)
Hispanic	-0.159	-0.140*	0.273**	0.199**	0.021	0.023	-0.079	-0.062
	(0.110)	(0.085)	(0.106)	(0.090)	(0.083)	(0.097)	(0.090)	(0.099)
High School Diploma	0.487**	0.420**	-0.679**	-0.600**	0.172**	0.226**	-0.264**	-0.358**
	(0.085)	(0.064)	(0.079)	(0.066)	(0.060)	(0.070)	(0.064)	(0.071)
Age	-0.050**	-0.040**	-0.033**	-0.029**	-0.002	0.002	-0.028**	-0.032**
	(0.008)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
Never Married	-0.460**	-0.367**	0.270**	0.229**	-0.097	-0.131	0.088	0.080
	(0.100)	(0.076)	(0.090)	(0.076)	(0.073)	(0.084)	(0.075)	(0.083)
# of Kids < 18	-0.021	-0.005	0.047	0.035	0.028	0.021	0.008	-0.005
	(0.043)	(0.035)	(0.048)	(0.040)	(0.036)	(0.041)	(0.035)	(0.039)
Age of Youngest Child	0.013	0.008	-0.015	-0.016	0.004	0.005	-0.002	-0.002
	(0.014)	(0.011)	(0.013)	(0.011)	(0.011)	(0.013)	(0.011)	(0.012)
# of Kids < 6	-0.336**	-0.292**	0.189**	0.137**	-0.138**	-0.157**	-0.018	0.003
	(0.068)	(0.057)	(0.080)	(0.062)	(0.065)	(0.071)	(0.060)	(0.065)
Disability	-0.800**	-0.596**	0.905**	0.769**	-0.366**	-0.486**	0.368**	0.480**
	(0.114)	(0.085)	(0.103)	(0.083)	(0.078)	(0.092)	(0.079)	(0.090)
Disability Variable Missing	-0.243*	-0.209**	0.411**	0.332**	-0.431**	-0.443**	0.051	0.152
	(0.130)	(0.106)	(0.121)	(0.107)	(0.134)	(0.149)	(0.134)	(0.143)

Table 4 Duration Models of Employment and Nonemployment Spells
Seam Bias Correction vs. A Simple Last-Month Dummy Approach

	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**
log(duration)	-0.093	0.288**	-0.267**	0.432**	-0.329**	-0.041	-0.097	0.362**
	(0.072)	(0.124)	(0.046)	(0.150)	(0.100)	(0.100)	(0.115)	(0.109)
Squared of log(duration)		-0.188**		-0.238**	-0.079**	-0.116**	-0.193**	-0.278**
		(0.035)		(0.040)	(0.035)	(0.037)	(0.042)	(0.040)
Last-Month Dummy		0.700**		1.359**		0.950**		1.360**
		(0.064)		(0.065)		(0.060)		(0.061)
Unobserved Heterogeneity								
Theta1	0.295	-1.121*	-7.464	-3.579**	-1.854**	-2.448**	-0.922	-1.169
	(0.757)	(0.673)	(6.715)	(0.660)	(0.631)	(0.694)	(0.692)	(0.743)
Theta2	-1.929**	-1.993**	-2.604**	-4.048**	-1.500**	-1.251*	-1.347*	-2.254
	(0.751)	(0.632)	(0.723)	(0.657)	(0.628)	(0.694)	(0.693)	(0.740)
Heterogeneity Probability	0.306**	0.365**						
	(0.053)	(0.106)						
Seam Bias Correction Parameters								
α1 (non-employment)	0.140**	**α1 (employment)**	0.515**					
	(0.032)		(0.027)					
α2 (non-employment)	0.135**	**α2 (employment)**	0.269**					
	(0.033)		(0.031)					
α3 (non-employment)	0.255**	**α3 (employment)**	0.256**					
	(0.030)		(0.031)					

Notes:
See notes to Table 3.1.

Table 5 Duration Models of Employment and Nonemployment Spells
Constant Seam Bias Probablities vs. Seam Bias Probablities as a Function of Race

	Left-censored non-employment spells		Left-censored employment spells		Fresh non-employment spells		Fresh employment spells	
	Constant Probabilities	Variable Probabilities	Constant Probabilities	Variable Probabilities	Constant Probabilities	Variable Probabilities	Constant Probabilities	Variable Probabilities
Maximum Welfare Benefit	-12.557**	-12.128**	0.070	-0.414	-6.441**	-6.575**	4.086*	4.446**
	(2.654)	(2.521)	(2.621)	(2.422)	(2.158)	(2.272)	(2.165)	(2.280)
Unemployment Rate	-0.080**	-0.073**	0.054**	0.049**	-0.049**	-0.053**	0.024	0.029
	(0.026)	(0.024)	(0.025)	(0.023)	(0.020)	(0.021)	(0.020)	(0.021)
Minimum Wage	0.138	0.159	0.100	0.094	0.117	0.110	-0.041	-0.060
	(0.194)	(0.191)	(0.200)	(0.194)	(0.177)	(0.181)	(0.191)	(0.199)
Welfare Waiver Stick	-0.241	-0.242	-0.051	-0.101	-0.248	-0.258	-0.145	-0.130
	(0.279)	(0.283)	(0.290)	(0.269)	(0.194)	(0.200)	(0.199)	(0.205)
Welfare Waiver Carrot	-0.009	0.012	-0.168	-0.142	0.285**	0.315**	-0.067	-0.088
	(0.203)	(0.199)	(0.197)	(0.185)	(0.133)	(0.140)	(0.137)	(0.144)
African American	-0.161	-0.133	0.423**	0.382**	-0.253**	-0.263**	0.018	0.021
	(0.100)	(0.096)	(0.090)	(0.083)	(0.075)	(0.080)	(0.075)	(0.080)
Hispanic	-0.159	-0.120	0.273**	0.231**	0.021	0.019	-0.079	-0.078
	(0.110)	(0.105)	(0.106)	(0.097)	(0.083)	(0.088)	(0.090)	(0.095)
High School Diploma	0.487**	0.473**	-0.679**	-0.638**	0.172**	0.204**	-0.264**	-0.296**
	(0.085)	(0.080)	(0.079)	(0.075)	(0.060)	(0.064)	(0.064)	(0.067)
Age	-0.050**	-0.048**	-0.033**	-0.032**	-0.002	0.000	-0.028**	-0.030**
	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Never Married	-0.460**	-0.446**	0.270**	0.248**	-0.097	-0.121	0.088	0.093
	(0.100)	(0.095)	(0.090)	(0.083)	(0.073)	(0.077)	(0.075)	(0.079)
# of Kids < 18	-0.021	-0.029	0.047	0.046	0.028	0.019	0.008	0.010
	(0.043)	(0.041)	(0.048)	(0.043)	(0.036)	(0.038)	(0.035)	(0.037)
Age of Youngest Child	0.013	0.016	-0.015	-0.017	0.004	0.005	-0.002	-0.004
	(0.014)	(0.013)	(0.013)	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)
# of Kids < 6	-0.336**	-0.310**	0.189**	0.140**	-0.138**	-0.144**	-0.018	-0.016
	(0.068)	(0.065)	(0.080)	(0.069)	(0.065)	(0.067)	(0.060)	(0.063)
Disability	-0.800**	-0.754**	0.905**	0.830**	-0.366**	-0.412**	0.368**	0.420**
	(0.114)	(0.105)	(0.103)	(0.096)	(0.078)	(0.083)	(0.079)	(0.085)
Disability Variable Missing	-0.243*	-0.232*	0.411**	0.365**	-0.431**	-0.430**	0.051	0.070
	(0.130)	(0.123)	(0.121)	(0.114)	(0.134)	(0.140)	(0.134)	(0.138)

Table 5 Duration Models of Employment and Nonemployment Spells
Constant Seam Bias Probablities vs. Seam Bias Probablities as a Function of Race

	Left-censored non-employment spells		Left-censored employment spells		Fresh non-employment spells		Fresh employment spells	
	Constant Probabilities	**Variable Probabilities**	**Constant Probabilities**	**Variable Probabilities**	**Constant Probabilities**	**Variable Probabilities**	**Constant Probabilities**	**Variable Probabilities**
log(duration)	-0.093	-0.135**	-0.267**	-0.335**	-0.329**	-0.306**	-0.097	-0.091
	(0.072)	(0.058)	(0.046)	(0.050)	(0.100)	(0.103)	(0.115)	(0.116)
Squared of log(duration)					-0.079**	-0.075**	-0.193**	-0.183**
					(0.035)	(0.036)	(0.042)	(0.042)
Unobserved Heterogeneity								
Theta1	0.295	-0.192	-7.464	-2.309**	-1.854**	-1.800**	-0.922	-0.848
	(0.757)	(0.730)	(6.715)	(0.693)	(0.631)	(0.653)	(0.692)	(0.723)
Theta2	-1.929**	-2.439**	-2.604**	-3.529**	-1.500**	-1.038	-1.347*	-1.782**
	(0.751)	(0.764)	(0.723)	(0.739)	(0.628)	(0.675)	(0.693)	(0.759)
Heterogeneity Probability	0.306**	0.431**						
	(0.053)	(0.066)						

Notes:
See notes to Table 3.1.

Table 6 Misreporting Probabilities Due to Seam Bias
Constant Probablities vs. Probablities Varying by Race

Panel A: Parameter Estimates

	Non-employment Spells		**Employment Spells**	
Coefficients	Constant Probabilities	Probabilites Varying with Race	Constant Probabilities	Probabilites Varying with Race
Month 1 Intercept	-1.818**	-2.355**	0.059**	-0.114
	(0.265)	(0.304)	(0.107)	(0.126)
Month 2 Intercept	-1.861**	-2.379**	-1.000**	-1.183**
	(0.280)	(0.315)	(0.156)	(0.174)
Month 3 Intercept	-1.073**	-1.526**	-1.065**	-1.295**
	(0.159)	(0.206)	(0.165)	(0.186)
Minority Dummy[1]		0.842**		0.355**
		(0.196)		(0.127)

Panel B: Misreporting Probabilities

	Non-employment Spells			**Employment Spells**		
	Constant Probabilities	Probabilites Varying with Race		Constant Probabilities	Probabilites Varying with Race	
Month 1	0.140**	**White**	0.087**	0.515**	**White**	0.472**
	(0.032)		(0.024)	(0.027)		(0.031)
		Minorities	0.181**		**Minorities**	0.560**
			(0.039)			(0.030)
Month 2	0.135**	**White**	0.085**	0.269**	**White**	0.234**
	(0.033)		(0.024)	(0.031)		(0.031)
		Minorities	0.177**		**Minorities**	0.304**
			(0.040)			(0.035)
Month 3	0.255**	**White**	0.179**	0.256**	**White**	0.215**
	(0.030)		(0.030)	(0.031)		(0.031)
		Minorities	0.335**		**Minorities**	0.281**
			(0.037)			(0.035)

Notes:
1. Month 1, 2 and 3 misreporting probabilities are functions of minority status in variable probability model. Three employment spells misreporting probabilities share one coefficient for the minority status coefficient, and three non-employment spells misreporting probabilities share another coefficient for the minority status coefficient.

Table 7 Expected Durations and the Effects of Changes in Demographic Variables-Employment and Nonemployment Spells

	Left-censored non-employment spells				Left-censored employment spells			
	Constant Misreporting Probabilities	Variable Misreporting Probabilities	Last-Month Dummy Model	Last Month Data	Constant Misreporting Probabilities	Variable Misreporting Probabilities	Last-Month Dummy Model	Last Month Data
Average Expected Duration (in months)	34.59	34.98	29.29	38.83	36.86	35.96	33.95	41.16
Changes with respect to								
Age (plus 1 year)	0.57	0.53	0.69	0.35	0.35	0.42	0.47	0.33
High school degree (hs=1) - (hs=0)	-5.55	-5.21	-7.32	-7.00	7.42	8.91	10.17	8.82
Race (White - Black)	-1.84	-1.46	-2.04	-1.79	4.53	5.22	5.69	4.99
Number of kids less than 6 years old (one vs. zero)	3.89	3.42	5.08	5.12	-2.06	-1.90	-2.25	-1.30

	Fresh non-employment spells				Fresh employment spells			
	Constant Misreporting Probabilities	Variable Misreporting Probabilities	Last-Month Dummy Model	Last Month Data	Constant Misreporting Probabilities	Variable Misreporting Probabilities	Last-Month Dummy Model	Last Month Data
Average Expected Duration (in months)	20.21	15.98	10.11	23.34	24.88	28.96	22.81	32.56
Changes with respect to								
Age (plus 1 year)	0.03	0.00	-0.02	0.01	0.52	0.51	0.56	0.50
High school degree (hs=1) - (hs=0)	-3.08	-3.09	-2.64	-3.86	4.95	5.13	6.26	4.74
Race (White - Black)	-4.57	-4.04	-4.00	-1.21	0.33	0.36	1.13	3.20
Number of kids less than 6 years old (one vs. zero)	2.45	2.15	1.79	3.10	0.34	0.28	-0.04	0.76