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

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ABSTRACT

Panel surveys generally suffer from "seam bias." This refers to the fact that transitions or changes in status within reference periods are underreported while too many transitions or changes are reported at the seams between interviews. Seam bias is observed for most variables in the Survey of Income and Program Participation (SIPP). We focus on the employment dynamics of disadvantaged single mothers using duration models while addressing the issues that occur when reported employment status is contaminated by seam bias. Duration models are likely to be severely affected by seam bias, since seam bias will affect the timing of transitions.

We begin by developing parametric misreporting models for use with single-state, single-spell duration analysis. We discuss identification and show that a necessary condition for identification (without restricting the form of the duration dependence) is satisfied for fresh spells but not for spells in progress at the beginning of the sample, i.e. left-censored spells. We then extend the analysis to multi-state, multi-spell models, and address this problem by assuming that misreporting parameters are the same for fresh and left-censored employment (non-employment) spells. We then compare our results to simpler solutions suggested by previous researchers of: i) using only data on the last month of reference periods, and ii) adding a dummy variable for the last month of reference periods. We find that our seam bias estimates are somewhat different from those obtained with a last month dummy, and very different from the results using the last month data. The existence of seam bias is well-documented, but the causes of it have not been clearly identified and there are many explanations. We consider several extensions of our basic misreporting model to shed light on the underlying causes of seam effects.

1. Introduction

Panel surveys generally suffer from "seam bias." This refers to the fact that transitions or changes in status within reference periods are underreported while too many transitions or changes are reported as occurring between interviews. To the best of our knowledge, this effect was noted first by Czajka (1983) for benefit receipt in the U.S. Income Survey Development Program. Since then seam effects have been documented for various longitudinal surveys, e.g. the Current Population Survey, the Panel Study of Income Dynamics, the Canadian Survey of Labour and Income Dynamics, the European Community Household Panel Survey, the British Household Panel Survey and the Survey of Income and Program Participation (SIPP), and banking data,[1] Lemaitre (1992) concludes that all current longitudinal surveys appear to be affected by seam problems, regardless of differences in the length of recall periods or other design features. Seam bias is an important problem for SIPP, which is intended to provide accurate reporting of labor force and program participation dynamics at the monthly level. The existence of seam bias seems like it would be most serious for estimating duration models, since it affects the timing of transitions. The Census Bureau, which collects the SIPP, has long recognized this problem and has attempted to reduce it in the SIPP, most recently by incorporating "dependent interviewing" procedures in the 2004 panel of the survey. Notwithstanding the adoption of such procedures, which explicitly link the wording of current interview questions to information provided in the previous interview, seam bias continues to be a substantial problem in the SIPP (Moore 2008).

Pischke (1995) proposes a method for dealing with seam bias in SIPP when using data on continuous variables such as income. Here we address the issues that occur with duration models when seam bias is present; the extension to other limited dependent variable models is straightforward. Specifically, we allow for seam bias when estimating monthly discrete time duration models to analyze the employment dynamics of single mothers with a high school education or less, a group that has been the focus of much recent policy. We estimate monthly transition rates into and out of employment for the period 1986-1995, prior to the replacement of Aid to Families with Dependent Children (AFDC) with Temporary Assistance to Needy Families (TANF). 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. The SIPP is particularly well-suited to estimate such models because of its detailed monthly information on employment and program participation.

[1] See Moore (2008) for a summary of seam bias research. See Chakravarty and Sarkar (1999) for information about seam bias in banking data.

With the exception of studies focusing on job training outcomes (see, for example, Ham and LaLonde (1996) and Eberwein, Ham and LaLonde (1997) who analyze employment dynamics using data from the National Supported Work and Job Training Partnership Act training experiments respectively) there have been relatively few studies of employment dynamics for less-educated women. (An exception is Aaronson and Pingle (2006), who study employment dynamics among single mothers in the 1990-2001 SIPP panels.) On the other hand, welfare dynamics for less-educated women have been examined in numerous studies, e.g. Blank and Ruggles (1996), Acs, Philips, and Nelson (2003) and Ribar (2005). 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. A good understanding of the employment dynamics is essential for policy; for example policy makers are likely to be very interested in the determinants of employment duration, since short employment spells prevent individuals from acquiring on-the-job human capital. Further, as we show below our results have important implications for the debate over whether transitions into nonemployment are pro-cyclical (Elsby, Michaels, and Solon 2009 and Shimer 2005a, 2005b).

Most of the welfare duration studies have estimated some form of discrete time hazard models using the SIPP or other panel surveys.[2] Consequently, these studies have been forced to confront the seam bias problem. The approaches used in the literature to address this problem can be grouped into three types. One approach is to use the monthly data and to include a dummy variable for the last month of the reference period in addition to an indicator variable for each reference period (known as a "wave" in the 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. A second approach is to collapse the monthly data into data by wave, setting participation and employment indicator variables to be 1 if respondents were employed or on welfare 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 of the four months in a wave and zero otherwise. Ribar (2005) uses the 1992 and 1993 panels of SIPP to examine welfare entry and exit, setting participation equal to one for a wave if participation was reported for at least one month in the wave and zero otherwise. This 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 but this spell would not be counted in

[2] For example 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.

the analysis. This approach will be especially problematic if used in modeling employment dynamics, since employment spells are often quite short for the disadvantaged population.

Another very 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, Ham and Shore-Sheppard 2005b, and Aaronson and Pingle 2006 use this approach.) Two reasons are given for using the last month data. One is that most transitions take place between waves, i.e. in the last month. Second, it is argued that many of the transitions in months other than the last month are likely to be due to imputation.[3] However, as we show in Section 3, the first reason ignores the fact that one loses almost one half of fresh spells (i.e. those starting after the beginning of the sample) by using only the last month data. Further we show below that information on the timing of transitions that occur in months other than the last month is lost, potentially introducing severe distortions to the true employment and welfare participation patterns. Secondly, for the time period we consider (1986-1993 SIPP panels), we find that about 80% of the transitions in the imputed data are reported to have occurred in the last (seam) month, while 50% of the transitions in the non-imputed data are reported to have taken place in the last month.

We propose a parametric approach to correct for seam bias in a duration model setting and use maximum likelihood to estimate the model. Our approach is most similar in spirit to that used by Pischke (1995) for continuous variables. We first develop a monthly discrete time duration model with parameters representing the propensity to under-report transitions in each of the first three other months of the reference period by allocating them to the last month of the previous wave. (This form of misreporting will occur if respondents simply replace their actual employment status in the previous three months of a wave with their month 4 employment status.) We show that this model is identified without restricting the form of the duration dependence for *fresh spells* which begin after the start of the sample, but that this is not true for *left-censored spells* which are in progress at the start of the sample. Thus we assume that left-censored employment (non-employment) spells and fresh employment (non-employment) spells share the same misreporting parameters when we estimate a multiple-state, multiple-spell model of employment dynamics. These latter models explicitly allow for the fact that if seam bias causes the end month of a spell to be mismeasured, it also causes the start month of the next spell to be mismeasured. Next we also consider several richer misreporting models to shed light on the underlying causes of seam effects, since the existing literature has not come to a consensus about the causes of seam bias. First, we consider the case

[3] For example, Grogger (2004, p.674) states "... some of the within-wave transitions that exist are due to the SIPP imputation procedures rather than changes in behavior (Westat 2001). Since most transitions are reported to occur between waves, I only make use of data from month 4 of each wave."

where the misreporting probability varies with demographic characteristics. Next, we allow for the possibility that a fraction of people never misreport, and allow this fraction to depend on demographics. Finally we allow individuals to misreport in two directions, first as described above and secondly by shifting transitions in months 1, 2, and 3 of a wave to month 4 of the current (as opposed to previous) wave. The first two models indicate whites have significantly lower probability of misreporting. This confirms a pattern found by Kalton and Miller (1991) for Social Security income reports in the SIPP 1984 panel. The third model overwhelmingly supports our base model where respondents sometimes report the employment status of the last month of a reference period (the month closest to the interview month) as applying to all months in the reference period. Interestingly, this is consistent with the findings of Goudreau, Oberheu and Vaughan (1984), who link AFDC income reporting in the Income Survey Development Program data to administrative records.

For comparison we also carry out the estimation of the duration models using two of the alternative models discussed previously: i) using only the last month data and ii) putting a dummy for the last month in the hazard and then adjusting the constant using the coefficient of this dummy variable.[4] We find that our seam bias estimates are somewhat different from those obtained with a last month dummy, and very different from the results using the last month data. We find a number of interesting empirical results concerning the employment dynamics of disadvantaged women. For example, we estimate the effect of several policy changes and economic variables on the short term, intermediate term and long term fraction of time a woman spends in employment. These include states implementing positive incentives to leave welfare ("carrots"); changes in the overall unemployment rate or welfare benefits in states; states introducing punitive incentives to leave welfare ("sticks"), and changes in the minimum wage. Interestingly the first three changes have statistically significant effects in the expected direction while the last two changes do not. This finding is interesting in its own right because our models have taken into consideration the minimum wage effects on transitions out of employment and transitions into employment simultaneously while current studies on this issue using micro data mainly focus on the minimum wage effects on transitions out of employment. Finally, we find that both employment and non-employment hazards are cyclical when we control for observed and unobserved heterogeneity. Our results indicate that at least for this population, the results of Elsby, Michaels, and Solon (2009) that both hazards are cyclical (as opposed to the results in Shimer 2005a, 2005b indicating that only the employment

[4] Previous researchers who used the month four dummy variable method did not adjust the constant, which will lead to an overestimate of expected durations.

hazard is cyclical) are not simply due to changes in the composition of those in employment and non-employment over the business cycle.

The paper proceeds as follows. In Section 2 we discuss the SIPP data and the extent of the seam bias problem. In Section 3 we discuss the problems that occur when one uses only the last month observations. In Section 4 we present our approach. We first outline the assumptions underlying our parametric approach, which we believe to be reasonable. We then outline our estimation approach for estimating parametric single spell duration models in the presence of seam bias, and discuss identification of these models. Finally we consider the estimation of our misreporting models with multi-spell, multi-state duration models. We present our empirical results in Section 5, and conclude the paper in Section 6.

2. Seam Bias in the SIPP Data

Our primary data consist of the 1986-1993 panels of the SIPP. The SIPP was designed to provide detailed information on incomes and income sources, as well as on labor force and program participation, of U.S. individuals and households. Our sample is restricted to single mothers who have twelve years of schooling or fewer. Since we investigate employment status, we only consider women between the ages of 16 and 55.[5] Although researchers investigating welfare durations often smooth out one-month spells, we use the original data with all one-month spells intact because employment status is often very unstable among low-educated women and it is common for them to have very short employment and non-employment spells.[6] Since we use state level variables such as maximum welfare benefits, minimum wage rates, 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 the SIPP.

The 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

[5] Respondents are chosen based on their education and age at the beginning of the panel. If a single mother marries 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] Hamersma (2006) investigates a unique Wisconsin administrative data set containing information from all 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 fewer than 120 annual hours of employment (job duration), while another 29 percent of workers have fewer than 400 annual hours. Only a little over one-third of workers have annual employment of more than 400 hours. These administrative data show that a significant share of employment spells are less than one month among disadvantaged individuals. In practice in our data it makes surprisingly little difference whether we smooth out the one-month spells.

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. Calendar months are thus equally distributed among the months of the reference period. 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*. The rotation design guarantees about 25% of transitions should occur in month 1, month 2, month 3 and month 4 respectively. 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. This seam effect, which researchers have attributed to both too much change across waves and too little change within waves, is observed for most variables in SIPP (see, e.g. Young 1989; Marquis and Moore 1990; Ryscavage 1988, Moore 2008).[7]

As noted earlier, some have worried that many of the off-seam transitions are simply the result of an imputation procedure by the Census Bureau, making their use suspect. In the SIPP, monthly data are imputed when a sample member either refuses to be interviewed or was unavailable for that interview (and a proxy interview could not be obtained), or when someone who entered a sample household after the start of the panel leaves the household during the reference period.[8] The Census Bureau indicates whether a monthly observation is imputed using a variable INTVW, which equals 1 or 2 if a self or proxy interview was obtained (and hence the data were not imputed) and 3 or 4 if the respondent refused to be interviewed or left, respectively (and hence the data were imputed). Using this variable, we compare the frequency of transitions at the seam in the imputed and non-imputed data (see Appendix Table A1). We find that approximately half the transitions take place in month 4 in the non-imputed data, but eighty percent take place in month 4 in the imputed data. That is, imputation accentuates seam bias rather than ameliorating it, negating part of the argument for omitting data on the first three months of a wave.

In our examination of employment dynamics using the SIPP, we follow Heckman and Singer (1984a) and the standard duration literature and 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.[9] The left-censored spells constitute the great majority of all spells in our data. For example, even in month 30 of the various

[7] An experimental study of seam effects (Rips, Conrad, and Fricker 2003) suggests that seam effects may arise from respondents forgetting the timing of events together with "constant wave responding" in which respondents simply give the same answer for all four months of a wave.

[8] All adults in sampled households at the start of the panel are considered original sample members and are followed to any new address. Someone entering a sample household after the start of the panel is interviewed while a member of the household but not followed if he or she leaves. In that case, the balance of the months of a reference period after the departure will be imputed.

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

SIPP samples, i.e. two and half 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 an accurate picture of the employment dynamics of a typical woman in our sample, who is in a left-censored spell. Second, there is likely to be an important issue of selection in which women are observed in a fresh spell. Thus we analyze left-censored and fresh spells jointly to correct for this selection.[10]

Table 1 provides summary statistics for our sample of employment and non-employment spells. Panel A shows that single mothers in left-censored non-employment spells are usually more disadvantaged than those in fresh non-employment spells. Specifically, those in left-censored non-employment spells are more likely to be minorities, less likely to have twelve years of schooling (as opposed to less schooling), less likely to have been previously married, more likely to be disabled or have missing disability information than those in fresh non-employment spells. Also the single mothers in left-censored non-employment spells tend to have more children, and their youngest children tend to be younger, compared to those in fresh non-employment spells. The two groups are similar only in terms of age.

Panel B shows that those in left-censored employment spells tend to be less disadvantaged than those in fresh employment spells. Specifically, they are older, less likely to be minority group members, more likely to have twelve years of schooling, more likely to have had a previous marriage, less likely to be disabled or have missing disability information, and tend to have both fewer children and older children than those in fresh employment spells.

III. Problems in Measuring Spells Using Only the Last Month Observations

As noted above, a common approach to the problem of seam bias is to use only the observation from the last month of a reference period. Here we construct three examples representative of our data to illustrate the problems that may arise when adopting this approach. In these examples, which are shown in *Figures 1.1 – 1.3*, 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. In each figure, the numbers above the line indicate the survey months and the numbers below the line are reference period months. The first example illustrated in *Figure 1.1* assumes that a respondent has four spells. 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

[10] While this selection bias may be important in principle, in practice Eberwein, Ham and LaLonde (1997) found it was not important in analyzing employment dynamics for similar women using data from the National Job Training Partnership Act Study, and in fact we find little evidence of this selection bias here, thus reinforcing the earlier result of Eberwein et al.

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 would not lead to the omission of the second and third spells, but only to the miscalculation of the length of all four spells.

Finally, we construct our last example to show how we can actually misclassify the type 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 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 we would record her work history as a left-censored employment spell and a fresh non-employment spell. From the last example it is clear that we will lose all left-censored spells less than or equal to three months in length by switching to the last month data. In addition, using the last month data will lead to both miscalculating spell length and even misclassifying spell type for left-censored spells.

To recap, the above three examples show that by using only the last month data, we could lose some spells, miscalculate the length of spells, and misclassify the spell type. 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. 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 mismeasured due to seam bias.

Of course, the above three 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 two 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, the implications of *Figures 1.1 to 1.3* 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 the last month only data compare, we examine the number of completed spells and the empirical survivor functions for each data type. First, comparing the number of *completed* spells (which provide the empirical identification for the parameters of the hazard functions), we find dramatically fewer spells when only the last month data are used. Shifting from monthly data to the last month data results in the loss of 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. Second, when considering all spells, shifting from monthly data to the last month data still results in the loss of 34 to 35 percent of fresh spells (see Appendix Table A2, which presents distributions of spell length and total number of spells of monthly data versus last month data). However, it is difficult to ascertain the effect of using the last month data on spell lengths from Table A2 because of the right censored spells. Instead, we investigate the empirical survivor functions.[11] *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.

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. Moreover, using only the last month data will lead to a loss of efficiency since data from three-fourths of the months are being discarded, and nearly 55 percent of transitions occur in these months.

IV. Correcting for Seam Bias: A Parametric Approach

We consider a model of misreporting behavior that allows us to save the valuable information contained in the monthly SIPP data and to address the seam bias problem. Specifically, we first develop a monthly discrete time duration model with three extra parameters that capture the

[11] The spells are constructed by pretending we only observe the last month of all waves. When there is a status change from the previous interview to the current interview, we code the last month of the current wave as the end of a spell.

misreporting of transitions.[12] Under reasonable assumptions, we show below that we can identify these parameters in our model. In fact the model is significantly over identified, and this overidentification allows us to consider several extensions of the model.

4.1 Notation

We first set up our notation before discussing our assumptions. Let $M^{obs} = (m,l)$ represent a spell reported to end in month m of reference period (wave) l.[13] Note that $M^{obs} = (m,l)$ could be misreported because of seam bias. Further, let $M^{true} = (m,l)$ represent the fact that a spell truly ended in interview month m of reference period l. The variable m in M^{obs} and M^{true} assumes five possible values: 1,2,3,4, or 0, where $m = 1,2,3$ or 4 indicates a transition in month 1, month 2, month 3 or month 4, respectively, while $m = 0$ indicates a right-censored spell ending with the survey. For our sample l takes on the values from 1 to L, where L is the number of waves which depends on the specific SIPP panel being used.[14] For example, $M^{obs} = (4,4)$ indicates that a transition was reported to occur in month 4 of reference period 4, while $M^{true} = (3,5)$ denotes that the transition actually occurred in month 3 of reference period 5.

4.2 Behavioral Assumptions

As discussed in the introduction, seam bias is observed for many variables in the SIPP. The employment status variable we use to construct our measure of transitions is no exception. This constancy within waves of employment status in the SIPP is plausibly a feature of the interview structure. The respondent is first asked whether she had a job or business at any time during the four-month period; if the answer is yes, the respondent is then asked whether she had a job or business during all weeks of the period. Only individuals who report some time employed and some time not employed are asked further questions to determine the timing of their periods of employment and non-employment. Suppose an individual continues a spell of nonemployment into a given wave and does not have a job for months 1 and 2 of the new wave, but she gets a job in month 3 that continues into month 4 of the wave. Given the interview structure, this individual may report that she has a job for the whole wave based on the fact she has a job in month 4, which is the month closest to (right before) the interview month. For this particular example, a non-employment spell

[12] See Romeo (2001) for a very different approach to dealing with seam bias in a duration model.
[13] The end of a spell can occur either because a transition took place or because the individual reached the end of the sample period.
[14] There are 7 waves in the 1986 and 1987 panels, 6 in the 1988 panel, 8 in the 1990 and 1991 panels, 10 in the 1992 panel, and 9 in the 1993 panel.

ending in month 2 of the current reference period will be reported to end in month 4 of the previous reference period. Goudreau, Oberheu and Vaughan (1984) document this as the most common type of misreporting behavior for AFDC benefits in the Income Survey Development Program.[15]

Based on the interview structure, documented empirical evidence of seam bias in SIPP variables, and previous research based on survey and administrative data, we make the following main assumption: respondents sometimes move a *transition* in months 1, 2 or 3 of a given reference period to month 4 of the *previous* reference period. This shifting of transitions will occur if respondents sometimes report the employment *status* of month 4 as applying to all months in the reference period. In terms of labor market state, this reflects the well known phenomenon of telescoping (respondents recall that previous states are the same as the state in the current period (Sudman and Bradburn 1974, p. 69)), while in terms of transitions, it reflects the well known phenomenon of "reverse telescoping" of transitions (respondents recall transitions as having occurred more distantly than in fact was the case (Lemaitre 1992)). This is also similar to the third type of household behavior considered by Pischke (1995, p. 824), where he assumes that respondents report their permanent income (but not transitory income) in month 4 as applying to all months in the wave.

We make the following five assumptions for each interview:

A1) the respondents report all transitions that occurred during reference period l as occurring either in the true month or month 4 of reference period $l-1$;

A2) if a respondent reports that a transition happened in months 1, 2 or 3 of reference period, we assume it is a truthful report;

A3) a respondent reports a transition that actually occurred in months 1, 2 or 3 of reference period l as taking place in month 4 of reference period $l-1$ with some pre-specified (but unknown) probabilities γ_1, γ_2 and γ_3;

A4) if a transition *truly* happened in month 4 of a reference period, the respondent reports it as occurring in that month;

A5) the true transition rate for a given duration does not depend on the *interview* month during which the transition occurs.[16]

[15] Their study is conducted by comparing respondents' reports obtained from interviews with administrative record information.

[16] This assumption is the consequence of the survey design. For ease of interviewing, the entire sample is randomly split into four rotation groups, and one rotation group (1/4 of the sample) is interviewed each calendar month. Each rotation group in a SIPP panel is interviewed once every four months about employment and program participation during the previous four months.

Given these behavioral assumptions,[17] we have the following conditional probabilities:

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

$$P\left(M^{obs}=(4,l-1)\middle\| M^{true}=(m,l)\right)=\gamma_{\mathrm{i}} \quad \text{for } m=1,2,3 \tag{4.2}$$

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

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

4.3 **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 of employment. We define the hazard function for individual *i* as

$$\lambda_i(t\,|\,\theta)=\frac{1}{1+\exp\left\{-h(t)-X_i(\tau+t)\beta-\theta_i\right\}} \tag{4.5}$$

where t denotes current duration, $h(t)$ denotes duration dependence, τ denotes the calendar time of the start of the spell, and $X_i(\tau+t)$ denotes a (possibly) time changing explanatory variable. Further, θ_i denotes unobserved heterogeneity, and following Heckman and Singer (1984b), we assume that it is i.i.d. across *i* and is drawn from a discrete distribution function with points of support $\theta_1,...,\theta_{J-1},\theta_J$ and associated probabilities $p_1,...,p_{J-1}$ and $p_J=1-\sum_{k=1}^{J-1}p_k$ [18]. For example, if a spell lasts K months, the contribution to the likelihood function for individual *i* is

$$L_i\left(K\right)=\sum_{j=1}^{J}p_j\lambda_i(K\,|\,\theta_j)\prod_{t=1}^{K-1}(1-\lambda_i(t\,|\,\theta_j))\ . \tag{4.6}$$

For notational simplicity, we drop the individual subscript i in what follows.

Based on our behavioral assumptions, it is straightforward to derive the likelihood function given that we observe $M^{obs}=\left(m,l\right)$, and length of the spell, dur^{obs}, both of which potentially have been contaminated by seam bias.[19] The contribution to the likelihood function for a completed spell of observed length K that ends in month 1 of reference period l is given by:

[17] 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.

[18] Our analysis is equally applicable to any other choice for the discrete time hazard function.

[19] Here we assume seam bias affects only the end date, and not start date, of a spell. We relax this assumption when we consider multiple spell data.

$P\left(M^{obs}=\left(1,l\right),dur^{obs}=K\right)$

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

Since the second term is zero by assumption A2 we have

$$P\left(M^{obs}=\left(1,l\right),dur^{obs}=K\right)=P\left(M^{obs}=\left(1,l\right),M^{true}=\left(1,l\right),dur^{true}=K\right)$$
$$=P\left(M^{obs}=\left(1,l\right)|M^{true}=\left(1,l\right),dur^{true}=K\right)\cdot P\left(M^{true}=\left(1,l\right)\middle|dur^{true}=K\right)\cdot P\left(dur^{true}=K\right)$$
$$=\left(1-\gamma_1\right)L(K), \qquad , \qquad (4.7)$$

since by assumption A3 $P\left(M^{obs}=\left(1,l\right)|M^{true}=\left(1,l\right),dur^{true}=K\right)=\left(1-\gamma_1\right)$; morevover $P\left(M^{true}=\left(1,l\right)\middle|dur^{true}=K\right)=1$ and $P\left(dur^{true}=K\right)=L(K).$

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

$$P\left(M^{obs}=\left(2,l\right),dur^{obs}=K\right)=\left(1-\gamma_2\right)\cdot L\left(K\right) \qquad (4.8)$$

$$P\left(M^{obs}=\left(3,l\right),dur^{obs}=K\right)=(1-\gamma_3)\cdot L\left(K\right) \qquad (4.9)$$

Finally, the contribution to the likelihood function for a completed spell of observed length K that is observed to end in month 4 of reference period l is given by

$$P\left(M^{obs}=\left(4,l\right),dur^{obs}=K\right)$$
$$=P\left(M^{obs}=\left(4,l\right),M^{true}=\left(1,l+1\right),dur^{true}=K+1\right)+$$
$$P\left(M^{obs}=\left(4,l\right),M^{true}=\left(2,l+1\right),dur^{true}=K+2\right)+$$
$$P\left(M^{obs}=\left(4,l\right),M^{true}=\left(3,l+1\right),dur^{true}=K+3\right)+$$
$$P\left(M^{obs}=\left(4,l\right),M^{true}=\left(4,l\right),dur^{true}=K\right)$$
$$=P\left(M^{obs}=\left(4,l\right)|M^{true}=\left(1,l+1\right)dur^{true}=K+1\right)\cdot P\left(M^{true}=\left(1,l+1\right)\middle|dur^{true}=K+1\right)\cdot P\left(dur^{true}=K+1\right)$$
$$+\left(M^{obs}=\left(4,l\right)|M^{true}=\left(2,l+1\right)dur^{true}=K+2\right)\cdot P\left(M^{true}=\left(2,l+1\right)\middle|dur^{true}=K+2\right)\cdot P\left(dur^{true}=K+2\right)$$
$$+\left(M^{obs}=\left(4,l\right)|M^{true}=\left(3,l+1\right)dur^{true}=K+3\right)\cdot P\left(M^{true}=\left(3,l+1\right)\middle|dur^{true}=K+3\right)\cdot P\left(dur^{true}=K+3\right)$$
$$+\left(M^{obs}=\left(4,l\right)|M^{true}=\left(4,l\right),dur^{true}=K\right)\cdot P\left(M^{true}=\left(4,l\right)\middle|dur^{true}=K\right)\cdot P\left(dur^{true}=K\right).$$

Note that $P\left(M^{obs}=\left(4,l\right)|M^{true}=\left(4,l\right),dur^{true}=K\right)=1$ by assumption A4, and

$P\left(M^{obs}=\left(4,l\right),M^{true}=\left(j,l+1\right),dur^{true}=K+1\right)=\gamma_j,\ j=1,2,3$ by assumption A3. Thus

the contribution of a spell that ends in month 4 of wave l is

$$P\left(M^{obs}=\left(4,l\right),dur^{obs}=K\right)=\gamma_1 L\left(K+1\right)+\gamma_2 L\left(K+2\right)+\gamma_3 L\left(K+3\right)+L\left(K\right). \qquad (4.10)$$

A natural question to ask is whether the model is identified without restricting the form of the duration dependence. In the next section we show that the model is significantly over-identified for fresh spells, but that this is not true for left-censored spells. To solve this identification problem in the multiple spell model below we assume that left-censored employment (non-employment) spells and fresh employment (non-employment) spells share the same misreporting parameters.

4.4 Identification of Duration Dependence and Seam Bias Parameters in Single Spell Data

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. For simplicity we consider a model for employment spells with duration dependence but no explanatory variables and no unobserved heterogeneity. (The argument for non-employment spells is identical.). We will consider estimating the parameters of this simplified model using the Analog principle (Manski 1994) where we compare sample moments and their probability limits, which we will refer to as population moments.

Let $m_j(k)$ denote the empirical hazard function for spells ending at duration *k* in reference month j, $j=1,2,3,4$. These are our sample moments and denote their population counterparts by $p_j(k)$. To obtain these population moments, first assume that for the population there are N_t, N_{t-1}, N_{t-2}, and N_{t-3} individuals having current duration equal to t, $t-1$, $t-2$, and $t-3$ in month 4 (over all reference periods). (One may think of N_t, N_{t-1}, N_{t-2}, and N_{t-3} as being large but finite for now, as they will drop out of the population moments below.)

In terms of the terminology of the duration literature, for $t\geq 4$, N_t, N_{t-1}, N_{t-2}, and N_{t-3} represent the total number of individuals at risk at durations t, $t-1$, $t-2$, and $t-3$ respectively in month 4. As discussed in Section 4.2, we assume that seam bias only occurs when transitions in month 1, 2, and 3 of the following reference period are heaped into current month 4, so N_t, N_{t-1}, N_{t-2}, and N_{t-3} are not contaminated by seam bias. For month 1, in large samples the number of individuals who *actually* enter month 1 at duration t is $N_{t-1}\cdot\left[1-\lambda\left(t-1\right)\right]$. This is the number of individuals at risk at duration t in month 1, i.e. the difference between the number entering previous month 4 at duration $t-1$ and the number who leave in previous month 4 at duration $t-1$.

However, note that the number *observed* to be at risk consists of those actually at risk minus the sum of:

1. The number of individuals who *actually* left in month 1 at duration t but were *observed* to exit in the previous month 4 due to seam bias, $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right] \cdot \lambda\left(t\right) \cdot \gamma_1$;
2. The number of individuals who *actually* left in month 2 at duration $t+1$ but were *observed* to exit in the previous month 4 due to seam bias $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right] \cdot \left[1-\lambda\left(t\right)\right] \cdot \lambda\left(t+1\right) \cdot \gamma_2$;
3. The number of individuals who *actually* left in month 3 at duration $t+2$ but were *observed* to exit in the previous month 4 due to seam bias $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right] \cdot \left[1-\lambda\left(t\right)\right] \cdot \left[1-\lambda\left(t+1\right)\right] \lambda\left(t+2\right) \gamma_3$.

Thus the total number observed at risk in month 1 at duration t is $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right]$ minus the sum of the above three terms.

Of those *actually* at risk in month 1, in large samples a fraction $\lambda\left(t\right)$ *actually* leave, but only a fraction $\lambda\left(t\right) \cdot \left(1-\gamma_1\right)$ were *reported* to leave in month 1, and the rest $\lambda\left(t\right) \cdot \gamma_1$ were reported to leave in previous month 4 due to seam bias. Thus the number *observed* leaving equals $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right] \cdot \lambda\left(t\right) \cdot \left(1-\gamma_1\right)$. Given this, it is straightforward to formulate the first population moment condition (after deleting the common factor $N_{t-1} \cdot \left[1-\lambda\left(t-1\right)\right]$ from both the numerator and denominator) as:

$$p_1(t) = \frac{\lambda\left(t\right) \cdot \left(1-\gamma_1\right)}{1-\lambda\left(t\right) \cdot \gamma_1 - \left[1-\lambda\left(t\right)\right] \cdot \lambda\left(t+1\right) \gamma_2 - \left[1-\lambda\left(t\right)\right] \cdot \left[1-\lambda\left(t+1\right)\right] \lambda\left(t+2\right) \gamma_3} \quad (4.11)$$

For month 2, the expected number of individuals who *actually* enter month 2 at duration t is $N_{t-2} \cdot \left[1-\lambda\left(t-2\right)\right]\left[1-\lambda\left(t-1\right)\right]$. Due to seam bias, the number *observed* to be at risk in month 2 consists of those actually at risk minus the sum of

1. those who actually left in month 2 at duration t but were observed to exit in previous month 4, $N_{t-2} \cdot \left[1-\lambda\left(t-2\right)\right]\left[1-\lambda\left(t-1\right)\right] \cdot \lambda\left(t\right) \cdot \gamma_2$ and

2. those who actually left in month 3 at duration $t+1$ but were observed to exit in previous month 4, $N_{t-2} \cdot \left[1-\lambda\left(t-2\right)\right]\left[1-\lambda\left(t-1\right)\right] \cdot \left[1-\lambda\left(t\right)\right] \lambda\left(t+1\right) \cdot \gamma_3$.

Thus the total number observed at risk in month 2 at duration t is

$N_{t-2} \cdot \left[1-\lambda(t-2)\right]\left[1-\lambda(t-1)\right]$ minus the sum of the above two terms.

Because of seam bias, the number *observed* to leave at duration t equals

$N_{t-2} \cdot \left[1-\lambda(t-2)\right]\left[1-\lambda(t-1)\right] \cdot \lambda(t) \cdot \left[1-\gamma_2\right]$. Thus our second population moment (after deleting the common factor, $N_{t-2} \cdot \left[1-\lambda(t-2)\right]\left[1-\lambda(t-1)\right]$, from both the numerator and denominator) is

$$p_2(t) = \frac{\lambda(t) \cdot \left[1-\gamma_2\right]}{1-\lambda(t) \cdot \gamma_2 - \left[1-\lambda(t)\right]\lambda(t+1) \cdot \gamma_3}. \tag{4.12}$$

For month 3, the number of individuals who *actually* entered month 3 at duration t is $N_{t-3} \cdot \left[1-\lambda(t-3)\right]\left[1-\lambda(t-2)\right]\left[1-\lambda(t-1)\right]$. Due to seam bias, the number *observed* to be at risk in month 3 consists of those actually at risk minus those who actually left in month 3 at duration t but were observed to exit in previous month 4,

$N_{t-3} \cdot \left[1-\lambda(t-3)\right]\left[1-\lambda(t-2)\right] \cdot \left[1-\lambda(t-1)\right]\lambda(t) \cdot \gamma_3$. The number *observed* leaving equals

$N_{t-3} \cdot \left[1-\lambda(t-3)\right]\left[1-\lambda(t-2)\right] \cdot \left[1-\lambda(t-1)\right]\lambda(t) \cdot \left[1-\gamma_3\right]$. Thus our third population moment (after deleting the common factor $N_{t-3} \cdot \left[1-\lambda(t-3)\right]\left[1-\lambda(t-2)\right] \cdot \left[1-\lambda(t-1)\right]$ from both the numerator and denominator) is

$$p_3(t) = \frac{\lambda(t) \cdot \left[1-\gamma_3\right]}{1-\lambda(t) \cdot \gamma_3}. \tag{4.13}$$

For month 4, under our assumptions the number of individuals who *actually (and were observed to)* enter month 4 at duration t is N_t. The number of individuals who *actually* leave in month 4 at duration t equals $N_t \cdot \lambda(t)$. However due to seam bias, we also observe leaving in month 4

1. $N_t \cdot \left[1-\lambda(t)\right] \cdot \lambda(t+1) \cdot \gamma_1$ individuals who actually left in month 1 of the next reference period but reported leaving in month 4,
2. $N_t \cdot \left[1-\lambda(t)\right]\left[1-\lambda(t+1)\right] \cdot \lambda(t+2) \cdot \gamma_2$ individuals who actually left in month 2 of the next reference period but reported leaving in month 4 and
3. $N_t \cdot \left[1-\lambda(t)\right]\left[1-\lambda(t+1)\right] \cdot \left[1-\lambda(t+2)\right] \cdot \lambda(t+3) \cdot \gamma_3$ individuals who actually left in month 3 of the next reference period but reported leaving in month 4.

The number of individuals *observed* leaving month 4 equals the sum of $N_t \cdot \lambda(t)$ and the three terms above. Thus after deleting N_t from both the numerator and denominator we have

$$\begin{aligned} p_4(t) = \lambda(t) + \left[1-\lambda(t)\right]\cdot\lambda(t+1)\cdot\gamma_1 + \left[1-\lambda(t)\right]\left[1-\lambda(t+1)\right]\cdot\lambda(t+2)\cdot\gamma_2 \\ + \left[1-\lambda(t)\right]\left[1-\lambda(t+1)\right]\cdot\left[1-\lambda(t+2)\right]\cdot\lambda(t+3)\cdot\gamma_3. \end{aligned} \tag{4.14}$$

If we equate population and sample moments, we only have four equations in seven unknowns, $\lambda(t),\lambda(t+1),\lambda(t+2),\lambda(t+3),\gamma_1,\gamma_2$ and γ_3. However, we can have seven equations in seven unknowns by considering, for example, the additional population moments

$$p_3(t+1) = \frac{\lambda(t+1)\cdot[1-\gamma_3]}{1-\lambda(t+1)\cdot\gamma_3},$$

$$p_3(t+2) = \frac{\lambda(t+2)\cdot[1-\gamma_3]}{1-\lambda(t+2)\cdot\gamma_3} \quad \text{and}$$

$$p_3(t+3) = \frac{\lambda(t+3)\cdot[1-\gamma_3]}{1-\lambda(t+3)\cdot\gamma_3}.$$

In fact, the model is actually over-identified, since we have many other moment conditions; for example

$$p_1(t+1) = \frac{\lambda(t+1)\cdot(1-\gamma_1)}{1-\lambda(t+1)\cdot\gamma_1 - \left[1-\lambda(t+1)\right]\cdot\lambda(t+2)\gamma_2 - \left[1-\lambda(t+1)\right]\cdot\left[1-\lambda(t+2)\right]\lambda(t+3)\gamma_3}$$

and

$$p_2(t+1) = \frac{\lambda(t+1)\cdot[1-\gamma_2]}{1-\lambda(t+1)\cdot\gamma_2 - \left[1-\lambda(t+1)\right]\lambda(t+2)\cdot\gamma_3},$$

and we now have nine equations in seven unknowns.[20] Since we can add many more moments without introducing new parameters, the model is significantly overidentified. Of course having the number of equations be greater than or equal to the number of unknowns is a necessary, but not sufficient, condition for identification. To pursue this issue further, for the exactly identified models we used several reasonable sets of values for the empirical moments and let Maple solve for the parameters. For each set of empirical moments, we found only one set of real solutions for the parameters when we restricted them to the unit interval.

[20] Here we are abstracting from an endpoint issue. In the last month of the sample, which will be month 4, we would not expect any misreported transitions, since there are no future spells to misreport from. Thus we assume no misreporting in the last month of the sample, and this will aid in identification.

However, without any auxiliary information, the parameters for the left-censored spells are not identified. Since we measure the duration of these spells from the start of the sample, we will only observe a spell of length 1, 5, 9, 13… ending in month 1 of the reference period, 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. Adding a superscript *lc* to denote left-censored spells, for $t \geq 4$ the available population moment conditions are as follows. For t = 5,9,13,… we have

$$p_1^{lc}(t) = \frac{\lambda^{lc}(t)\cdot\left(1-\gamma_1^{lc}\right)}{1-\lambda^{lc}(t)\cdot\gamma_1^{lc} - \left[1-\lambda^{lc}(t)\right]\cdot\lambda^{lc}(t+1)\gamma_2^{lc} - \left[1-\lambda^{lc}(t)\right]\cdot\left[1-\lambda^{lc}(t+1)\right]\lambda^{lc}(t+2)\gamma_3^{lc}}.$$

For t = 6,10,14,… we have

$$p_2^{lc}(t) = \frac{\lambda^{lc}(t)\cdot\left[1-\gamma_2^{lc}\right]}{1-\lambda^{lc}(t)\cdot\gamma_2^{lc} - \left[1-\lambda^{lc}(t)\right]\lambda^{lc}(t+1)\cdot\gamma_3^{lc}}.$$

For t = 7,11,15,… we have

$$p_3^{lc}(t) = \frac{\lambda^{lc}(t)\cdot\left[1-\gamma_3^{lc}\right]}{1-\lambda^{lc}(t)\cdot\gamma_3^{lc}}.$$

For t = 8,12,16 we have

$$\begin{aligned} p_4^{lc}(t) = {} & \lambda^{lc}(t) + \left[1-\lambda^{lc}(t)\right]\cdot\lambda^{lc}(t+1)\cdot\gamma_1^{lc} + \left[1-\lambda^{lc}(t)\right]\left[1-\lambda^{lc}(t+1)\right]\cdot\lambda^{lc}(t+2)\cdot\gamma_2^{lc} \\ & + \left[1-\lambda^{lc}(t)\right]\left[1-\lambda^{lc}(t+1)\right]\cdot\left[1-\lambda^{lc}(t+2)\right]\cdot\lambda^{lc}(t+3)\cdot\gamma_3^{lc}. \end{aligned}$$

For $t = 4,5,6,7$ we only have four equations in seven unknowns. Unfortunately, if we add four more population conditions for $t =$ 8,9,10,11, we also will add four more unknowns $\lambda^{lc}(8), \lambda^{lc}(9), \lambda^{lc}(10), \lambda^{lc}(11)$. Thus we now have 8 moments for 11 unknowns and the identification problem remains. However, if we take the estimated seam bias parameters from the fresh employment spells and plug them into the left-censored employment spells, i.e. $\gamma_k = \gamma_k^{lc}$, for k= 1,2,3, the number of equations equals the number of unknowns, and the model becomes exactly identified. We thus impose this constraint and an analogous constraint for non-employment spells. Note that this identification problem would disappear if we had information on (and used) the actual start date of the left-censored spells.

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

$$\gamma_k^E = \frac{1}{1+\exp\left(-\alpha_{0k}^E - \alpha_{1k}^E NW\right)} \quad k = 1, 2, 3, \tag{4.15}$$

where NW is a dummy variable equal to 1 if an individual is non-White and zero otherwise. We use an analogous specification for non-employment spells. Since one could estimate this model by simply estimating the base model separately for Whites and non-Whites with constant misreporting probabilities and then use a minimum distance procedure to estimate the parameters in (4.15), this extended model is also identified. Below we find that the misreporting probabilities indeed vary significantly by race. Interestingly the misreporting probabilities do not vary by level of education, independent of whether we let the misreporting probability depend on race.

4.5 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 a respondent can have several spells ending in month 4 in her 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. Thus we continue to use separate hazard functions for the left-censored spells (Heckman and Singer 1984a). We allow the unobserved heterogeneity terms to be correlated across different types of spells.

As noted above, for identification we let the employment spells, both left-censored and fresh, share one set of seam bias parameters, γ_1^E, γ_2^E, and γ_3^E as defined in equation (4.2), while we specify another set of parameters, γ_1^U, γ_2^U and γ_3^U representing the seam bias associated with non-employment spells.[21] Let U' and U represent left-censored and fresh non-employment spells

[21] If stigma is an issue, individuals may report longer employment spells than actually occurred and shorter non-employment spells. This would increase the γ^E terms and decrease the γ^U terms. This concern motivates our setting separate misreporting parameters for employment and non-employment spells.

respectively, and let E' and E represent left-censored and fresh employment spells respectively.[22] We follow standard practice and specify the unobserved heterogeneity corresponding to these four types of spells through a vector $\theta = (\theta_U, \theta_{U'}, \theta_E, \theta_{E'})$, and assume that θ is distributed independently across individuals and is fixed across spells for a given individual. Following McCall (1996) 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 the relatively simple example in Figure 3, which covers all essential problems for multiple spells with seam bias. To distinguish among the four types of spells, we add a subscript to the transition indicator defined in Section 4.3. The respondent reports three spells with a reporting history given by

$\left\{ M_{U'}^{obs} = (1,2), dur_{U'}^{obs} = 5, M_E^{obs} = (4,3), dur_E^{obs} = 7, M_U^{obs} = (0,9), dur_U^{obs} = 24 \right\}$, which indicates the first spell is a left-censored non-employment spell ending in month 1 of reference period 2, the second is a fresh employment spell reported to end in month 4 of reference period 3, and the third is a fresh non-employment spell which is censored at the end of the sample. (Again in Figure 3 the numbers above the line are the survey months and the numbers below the line are reference period months.) Since the first spell ends in month 1 of the reference period, we assume it is reported correctly. However, the second spell is reported to have ended in month 4 of the third reference period. Given our assumptions the reported history could be true, but there are also three additional possible histories A, B, and C. Specifically, the second spell could actually have ended in month 1 of the following reference period, which implies that we would need to reduce the duration of the subsequent (censored) spell by one month. Alternatively, it also could have ended in months 2 or 3 of the following reference period, in which case we would need to shorten the length of the subsequent spell by two or three months respectively.

As we show in an Appendix at the end of the paper, the contribution to the likelihood function for the reported history in Figure 3 is given by [23]

[22] As we show in the previous section, we cannot let the seam bias parameters differ between left-censored and fresh spells of the same type.

[23] Grogger (2004) suggests clustering the spells for the same individual on top of allowing for person specific unobserved heterogeneity to obtain robust standard errors. Unfortunately, the parameter estimates will not be consistent if there is spell specific heterogeneity that is not taken into account in estimation.

$$L=\sum_{j=1}^{J}p_j\left(\begin{array}{l}\left[\left(1-\gamma_1^U\right)\prod_{r=1}^{4}\left(1-\lambda_{U'}\left(r\,|\,\theta_{U'j}\right)\right)\cdot\lambda_{U'}\left(5|\theta_{U'j}\right)\right]\cdot\\ \left\{\begin{array}{l}\left[\gamma_1^E\prod_{r=1}^{7}\left(1-\lambda_E(r\,|\,\theta_{Ej})\right)\cdot\lambda_E(8\,|\,\theta_{Ej})\prod_{r=1}^{23}\left(1-\lambda_U(r\,|\,\theta_{Uj})\right)\right]\\ +\left[\gamma_2^E\prod_{r=1}^{8}\left(1-\lambda_E(r\,|\,\theta_{Ej})\right)\cdot\lambda_E(9\,|\,\theta_{Ej})\prod_{r=1}^{22}\left(1-\lambda_U(r\,|\,\theta_{Uj})\right)\right]\\ +\left[\gamma_3^E\prod_{r=1}^{9}\left(1-\lambda_E(r\,|\,\theta_{Ej})\right)\cdot\lambda_E(10\,|\,\theta_{Ej})\prod_{r=1}^{21}\left(1-\lambda_U(r\,|\,\theta_{Uj})\right)\right]\\ +\left[\prod_{r=1}^{6}\left(1-\lambda_E(r\,|\,\theta_{Ej})\right)\cdot\lambda_E(7\,|\,\theta_{Ej})\prod_{r=1}^{24}\left(1-\lambda_U(r\,|\,\theta_{Uj})\right)\right]\end{array}\right\}\end{array}\right) \tag{4.16}$$

4.6 Model Extensions

In our first extended model, we consider the possibility that there are two types of people: type A who truthfully report their employment histories, and type B who misreport in the way described in Section 4.2 above. Type A people comprise a fraction P_A of the population, but of course we cannot identify what type an individual is in our sample. Denote the likelihood function for individual i by $L_i^A(\cdot)$ if she is a type A person, and the likelihood by $L_i^B(\cdot)$ if she is a type B person. In this case the overall contribution to the likelihood function for this person is

$$\hat{L}_i(\cdot)=P_A L_i^A(\cdot)+(1-P_A)L_i^B(\cdot). \tag{4.17}$$

Maximizing the log likelihood based on (4.17) involves estimating only one additional parameter, P_A.[24]

Our second extension considers the possibility that certain individuals move some transitions forward to month 4 of the *current* (as opposed to the *previous*) reference period, (i..e standard telescoping of transition) while others continue to use the model of reverse telescoping described in section 4.2. This new type of reporting would occur if individuals thought that transitions which happened in month 1, 2 or 3 in the reference period just happened in month 4 of the *same* reference period, which is the month right before their interview month. Specifically, we assume that individuals move the transition of a non-employment spell forward to month 4 from

[24] One would have to be especially careful if one wanted to test the null hypothesis $P_A=1$, since the misreporting parameters γ_1, γ_2 , and γ_3 are not identified under this null hypothesis (Davies 1985). Fortunately the estimate of P_A is quite far from 1.

months 1, 2 and 3 of the same reference period with probabilities φ_1^U, φ_2^U and φ_3^U respectively. Individuals behave in an analogous way in an employment spell and each individual commits the same type of misreporting in each type of spell. Denote the contribution to the likelihood function for these type of individuals by $L_i^C(\cdot)$,[25] and assume that a fraction of individuals P_C follow this misreporting scheme and a fraction $1-P_C$ follow the misreporting scheme described in Section 4.2. (In this extension no one always reports correctly.) Then we can investigate this second misreporting scenario by considering the (mixture) contribution to the likelihood function

$$\tilde{L}_i(\cdot) = P_C L_i^C(\cdot) + (1-P_C) L_i^B(\cdot). \tag{4.18}$$

Maximizing a likelihood based on (4.18) involves estimating the fraction parameter P_C plus six additional misreporting probabilities (the $\varphi's$) compared to just maximizing a contribution to the likelihood based on $L_i^B(\bullet)$ as in Section 4.3.

4.7 Using Aggregated Data to consider Additional Misclassification Schemes

Of course, there is the possibility that the transitions are misclassified in a way that differs from the scenarios considered above. Here we consider several other possibilities which we argue can be rejected on the basis on aggregates of our micro data. The first possibility we consider is: some of the month 1 transitions are pushed into month 2, some of the month 2 transitions are pushed into month 3, and some of the month 3 transitions are pushed into month 4, but none of the month 4 transitions are 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 month 1 should have a much smaller proportion of the transitions than months 2 and 3, and month 4 should have a much larger proportion of the transitions than months 2 and 3. However, in our data months 1, 2, 3, and 4 have 16.57%, 19.08%, 18.49%, and 45.86% of the employment/non-employment transitions respectively, which is inconsistent with this alternative model. (Note that the model is Section 4.3 is consistent with this pattern).

Secondly, we consider the possibility that some of the interview month (month 1 of the reference period $p+1$) transitions are pushed back into month 4 of reference period p. If this is

[25] This derivation of this likelihood function is very similar to the derivation in Section 4.3 so we omit it to save space.

the only source of misclassification, then the pattern should be similar to the scheme above: about 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. This model would also be rejected by the summary statistics presented in the previous paragraph.

A third possible explanation is 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 under reported. 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, and certainly this explanation is consistent with the aggregate data reported above. However, there is another way to examine this explanation. We know from administrative data that short spells are much more frequent in employment duration than in welfare duration for the mothers we study. Thus if we compare the transitions in months 1,2, 3 and 4 for employment duration data and welfare duration data we would expect to see more transitions in month 4 for the employment data if this explanation is correct. However, we find that 52.7% of all transitions out of employment were reported to have occurred in month 4, while 62.7% of all transitions out of welfare were reported to have occurred in month 4, casting doubt on this explanation.

5. Empirical Results

5.1 Hazard Estimates from Four Seam Bias Correction Models

Table 2A presents estimates of the hazard function parameters for left-censored spells using four models. The first and second models are the seam bias correction model from section 4.6 when the misreporting probabilities are constant across individuals (constant misreporting probability model hereafter) and variable across individuals (variable misreporting probability model hereafter). The third model consists of adding a month 4 (last month of any reference period) dummy to the model and adding one-quarter of this estimate to each point of support (last month dummy model hereafter). The last model uses month 4 data only (last month data model hereafter). Estimates from the last month dummy and last month data models allow us to compare our approach with those that are currently used. All models are estimated with unobserved heterogeneity. We let the data choose the number of points of support for the unobserved heterogeneity (as specified in Section 4.5) and the best fitting polynomials for duration dependence according to the Schwartz criterion for each model, as suggested by Ham, Svejnar and Terrell (1998) and Baker and Melino (2001). Note that we believe the parameters of the hazard coefficients are of substantial interest since the employment dynamics of women with low levels of schooling have received much less attention in the literature than the welfare dynamics. Table 2B contains the estimates for the fresh non-employment and employment spells,[26] while Table 2C reports on the misreporting probabilities for the first two models.

Our choice of explanatory variables is relatively standard and includes a mix of policy and demographic variables, except that we also use the minimum wage as an explanatory variable.[27] The hazard is parameterized such that a negative coefficient implies that the hazard decreases if the explanatory variable increases. Considering first our seam bias correction estimates with respect to left-censored non-employment spells, we see that higher welfare benefits, a higher unemployment rate, being African American or Hispanic, growing older, having never been married, having more children under six years of age, and having a disability all significantly lower the probability (in a partial correlation sense) that a woman leaves a left-censored non-employment spell. The minimum wage and the implementation of welfare waiver policies (sticks and carrots) at the state level have no significant effect on left-censored non-employment duration. On the other hand, having twelve years of schooling (as opposed to less schooling) 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 higher welfare benefits, having twelve years of schooling, and being older are associated with significantly longer left-censored employment spells. The sign for the welfare benefits variable is puzzling, but we will see in Table 4A that the effects of increasing this benefits variable by 10% on the expected duration of left-censored employment spells are quite small in practice for both constant and variable misreporting probability models. Being Hispanic, never having been married, having more children under age six, having a disability, or having missing disability status are associated with significantly shorter left-censored employment spells. Again the minimum wage and the two welfare waiver variables have no significant effect.

Moving to Table 2B, the fact that we have substantially fewer fresh employment and non-employment spells (see Table 1 for number of spells), leads to fewer variables being statistically significant. For the fresh non-employment spells, facing a higher unemployment rate, being African American, having more children under age eighteen, having more children under age six, having a disability or having missing disability status decreases the hazard rate for leaving a fresh non-employment spell. Being offered a 'carrot' to leave welfare significantly reduces the length of a fresh

[26]Note that unlike the standard case, the log-likelihood function does not become additively separable in the different types of spells if we ignore unobserved heterogeneity, or allow for unobserved heterogeneity that is independent across spell type, because we still must allow for seam bias.

[27] 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 (2005a) found that his result arose from a mis-imputation of the income limits and imposing a restriction not consistent with theory or the data.

non-employment spell, as does having twelve years of schooling. Finally, considering fresh employment spells, increasing the minimum wage, having twelve years of schooling and growing older significantly decrease the exit rate from these spells, while an increase in welfare benefits, higher unemployment rate, and having a disability or missing disability status increases the exit rate from a fresh employment spell.

In the constant and variable misreporting probability models, the unemployment rate coefficients suggest that both left-censored and fresh non-employment durations are counter-cyclical. Further, as in Elsby, Michaels, and Solon (2009), fresh employment duration is cyclical, but as in Shimer (2005a, 2005b), left-censored employment duration is not affected by the business cycle.

Finally, we discuss the estimated misreporting probabilities from both of our models as presented in Table 2C. For the constant misreporting probability model, the misreporting probabilities for employment spells are re-parameterized as $\gamma_k^E = \dfrac{1}{1+\exp\left(-\alpha_k^E\right)}$ $k = 1,2,3$ to constrain the probabilities to be between 0 and 1. For the variable misreporting probability model, the misreporting probabilities for employment spells are parameterized as in equation (4.15). We use analogous specifications for non-employment spells. In the variable misreporting model, we find that only race significantly affects misreporting behavior. Panel A of Table 2C reports parameter estimates of α's and Panel B reports calculated misreporting probabilities according to the estimates in Panel A. Standard errors in Panel B are calculated using the delta method. All of the misreporting probabilities are statistically and economically significant. In general, the misreporting probabilities are larger for the employment spells than for the non-employment spells. Another interesting pattern is that the probabilities are descending from month 1 through month 3. The upshot is that among all transitions occurring in months 1, 2 and 3, the longer the time distance between the transition and the interview the more likely the respondent heaps that transition into the previous month 4. (Recall that interviews were conducted in month 1 of the following reference period, thus month 1 transitions occurred furthest from the interview time). According to the constant misreporting probability model, 44% to 57% of month 1, 2, and 3 transitions out of non-employment spells have been shifted to month 4; while 58% to 73% of month 1, 2, and 3 transitions out of employment spells have been shifted to month 4. Being African American or Hispanic significantly increases the probabilities of misreporting by about 7 to 8 percentage points for non-employment spells and by about 7 to 10 percentage points for employment spells.

Interestingly, while we find that Whites have significantly lower misreporting probabilities, the coefficients and significance levels are remarkably similar for the estimates of the parameters of the hazard functions for the two misreporting models. For the sole purpose of correcting for seam bias, our results suggest a constant misreporting probability model will be sufficient. However, if a researcher is also interested in investigating misreporting behavior, a richer model allowing misreporting probability to vary with individual characteristics will be needed.

There are differences among the estimates based on our seam bias correction models and the estimates from the other two approaches in the literature, the last month dummy model and the last month data model, in terms of point estimates and standard errors. (Note that the magnitudes of the coefficients of the last month data model are not directly comparable with our seam bias correction models and the last month dummy model.) For the seam bias correction models and the last month dummy model, the time unit for the discrete hazard is *month* while for the last month data model, the time unit for the discrete hazard is *four months* (refer to footnote 11 for details of the spell construction for the last month data model). However, we can compare standard errors and statistical significance across the four models, and below we can compare the expected duration calculations for the four models. For left-censored spells, the magnitudes of the standard errors are comparable among the four models with those from the last month dummy model being slightly smaller in general. For fresh spells, the standard errors from the last month data model in general are 1/3 to 1/2 larger than those from the other three models using monthly data. The larger standard errors could be explained by loss of transitions when implementing the last month data model. As discussed in Section 3, in terms of completed spells (spells ending in a transition), shifting from monthly data to the last month data results in the loss of about 20% of left-censored spells, but about 50% of fresh spells.

We next look at differences in the significant coefficients produced by each model, since often empirical researchers focus on these estimates. (The magnitudes of the coefficients are better compared using the estimated effects on expected duration of changing an explanatory variable in Table 3 below.) Considering the left censored non-employment spells, it is interesting to note that our seam bias approach finds significant effects of being African American, while this is not true for the simpler models. Further, all models except the last month data model find a significant effect of being Hispanic. On the other hand, the simpler models find a significant effect of having disability status missing, but this is not true for our seam bias models. Considering the estimates for the left-censored employment spells, the seam bias models find a significant effect for welfare benefits but the simpler models do not; the opposite is true for the unemployment rate and being African American. Finally the seam bias model and the last month dummy model find significant effects of the number of children less than 6 years and of the disability variable missing, but this is not true for the last month data model, and the opposite is true for the age of the youngest child. Considering the fresh non-employment spells, our seam bias models and the last month dummy model find a significant role for the unemployment rate, the welfare waiver carrot variable and being an African American. On the other hand, welfare benefits are only significant using the last month dummy model, and the number of children less than 18 years is only significant for our seam bias models. Finally, considering the fresh employment spells, we note that only our seam bias models find a significant role for the unemployment rate and the minimum wage, and only the last month dummy variable model finds a significant effect of having never been married. In summary, our seam bias estimates are somewhat different from the last month dummy variable model estimates, and very different from the last month data estimates.

5.2 The Effect of Changing an Explanatory Variable on Expected Durations

To put the four models under the same footing for comparison, 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 $j,\ j = U', U, E', E$ (as defined in Section 4.5), lasts longer than $t-1$ months is given by the survivor function

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

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

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

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

$$ED_j = \int_\Theta\left[\sum_{t=1}^{\infty} t\cdot f_j\left(t\middle|\theta_j\right)\right]d\Phi_j\left(\theta_j\right)$$

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:

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

We choose $T^* = 60$.[28] We calculate the expected durations for each individual and take the sample average. To test whether the out-of-sample durations are having a disproportionate impact on estimated expected duration, we also 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

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[28] The longest panel in our data lasts 40 months.

hazard function for durations longer than 25 months at 25 months for left-censored spells. We find freezing the hazard function does not make a noticeable difference in estimated expected durations. We also estimate the effect on expected duration of changing an explanatory variable for the four models. We get these effects by setting an explanatory variable at two different levels and calculating the corresponding expected durations. The difference between the two expected durations represents the effect of changing that particular explanatory variable. Standard errors for expected durations and effects on expected duration of changing explanatory variables are calculated using the delta method relying on numerical partial derivatives.

Our estimated expected durations and the effects on expected duration of changing explanatory variables are presented in Tables 3A and 3B. Note that when we calculate the expected duration for the last month data model, we have taken into consideration that this model provides a 4-month interval hazard (as opposed to the other models' monthly hazards). The first row of Table 3A reports the expected durations (without freezing the duration function for longer spells). The expected durations for left-censored spells are comparable across the four models, with those from the last month dummy model being slightly shorter. The expected durations for fresh spells are quite similar between the constant and variable misreporting probability models with both being about 11 to 12 months. However the estimates from the last month data model are much longer, with the expected fresh non-employment duration being about 23 months and the expected fresh employment duration being about 33 months. The estimates from the last month dummy model are in the middle, with the expected fresh non-employment duration being about 16 months and the expected fresh employment duration being about 28 months. The longer expected durations for fresh spells estimated from the last month data model can at least be partly explained by the loss of short fresh spells as discussed in Section 3.

The rest of Table 3A presents the effects of changing welfare policy or macroeconomic condition on expected durations. Overall, the differences in these effects across the four models are minor compared to the differences in expected duration. We first discuss the effects on left-censored spells. Increasing state maximum welfare benefits by 10% lengthens left-censored non-employment spells by about half a month in all models, although this effect is not statistically significant in the last month data model. There are no detectable effects of implementing welfare stick or carrot waivers, nor is there a statistically distinguishable effect of increasing the minimum wage. If the state unemployment rate increases by 25%, the expected duration of left-censored non-employment spells increases by about 1.4 to 1.8 months. None of the above variables has a precisely estimated effect in any model for left-censored employment spells.

Next we discuss the estimated effects for fresh non-employment spells. Increasing the state maximum welfare benefit by 10% has a very small positive effect, but it is statistically significant only for the last month dummy model. Implementing a carrot waiver policy reduces the expected duration by 3 to 4 months, but again this effect is not significant for the last month data model. Implementing a stick waiver policy has no statistically distinguishable effect on the expected duration (the estimated effects have the wrong sign but are not statistically significant). There is no effect of increasing the minimum wage. As with the left-censored non-employment spells, if the state unemployment rate increases by 25%, the expected duration increases, in this case by about 0.8 to 1.2 months. This effect is not significant for the last month data model.

Last we discuss the effects on fresh employment spells. Only the constant and variable misreporting probability models estimate significant effects of increasing the state maximum welfare benefits by 10%. While these effects are oddly signed, they are practically small, only shortening the expected duration by about 0.2 month. None of the four models precisely estimate any effects of the two welfare policies, carrot and stick waivers. Again only the constant and variable misreporting probability models estimate significant effects of increasing the minimum wage by 10%, with the change increasing the expected duration by about 1.5 months. Finally, the constant and variable misreporting probability models predict that increasing the unemployment rate by 25% reduces the expected duration by about 0.7 month. This effect is statistically insignificant in the other two models.

Table 3B presents the effects of individual characteristics on expected durations. Compared with the effects presented in Table 3A, in general these effects exhibit larger discrepancies between models in terms of both magnitude and statistical significance. Again we first discuss the effects on left-censored non-employment spells. Being older (age 35 versus age 25) makes the expected duration significantly longer. This effect is about 7.5 months according to the two misreporting probability models, about 6.3 months according to the last month dummy model, and about 3.6 months according to the last month data model. Having 12 years of education versus having less education shortens the expected duration by 5 to 7 months with the last month data model predicting this effect to be the largest. Only the two misreporting probability models estimate significant effects of being an African American (versus being White), lengthening the expected duration by about 2.5 months. The two misreporting probability models and the last month dummy model estimate being Hispanic (versus White) makes the expected duration about 2 to 3 months longer. All of these effects are only significant at the 10% level. The last month data model does not show a statistically significant effect of being Hispanic. All four models estimate that having one child under six years old relative to having none increases the expected duration by 4 to 5 months.

Next we discuss the effects on left-censored employment spells. All models predict growing older (again 35 versus 25) makes the expected duration 3 to 5 months longer with the estimate from the last month data model being the smallest. Having 12 years of education versus less education increases the expected duration by 7 to 8 months. Surprisingly, the two misreporting probability models estimate the effect of being African American to be small and insignificant while the other two models estimate this effect to shorten expected employment duration by a statistically significant 5 months. The two misreporting probability models and the last month dummy model estimate the effect of being Hispanic to shorten the expected duration by about 2.6 months, while the last month data model does not detect a significant effect of this variable. Except for the last month data model, all of the other three models predict that having one child less than 6 years old reduces the expected duration by about 2 months.

Turning to the fresh non-employment spells, none of the models predict a significant effect of age. The two misreporting probability models estimate the effect of having more education to be about 2 months while the last month data model predicts this effect to be about 4 months. The estimated education effect from the last month dummy model is between these at about 3 months. Among the four models, the last month dummy model predicts the largest effect of being African American, with the expected duration being about 4.6 months longer. The two misreporting probability models predict this effect to be about 1.8 months, while the effect in the last month data model is a statistically insignificant 1.2 months. None of the models show a significant effect of being Hispanic. The four models predict quite different effects of having one child less than six years old. According to the two misreporting probability models, this effect is about 1.2 months, while it is about 2 months according to the last month dummy model and about 3 months according to the last month data model.

Last we discuss the effects on fresh employment spells in Table 3B. The two misreporting probability models estimate a small effect of age 35 versus age 25, with the expected duration being about 1 month longer. However, the other two models predict this effect to be much larger, more than 5 months. Again the effect of having 12 years of education relative to having less is predicted to be much larger by the last month dummy and last month data models, about 6 and 5 months respectively, while the misreporting probability models show an effect of about 3 months. Only the last month data model predicts a significant effect of being African American, with the expected duration being about 3 months shorter. None of the models predict significant effects of being Hispanic or having one child less than 6 years old.

5.3 Results from the Extended Model with a Fraction of Individuals Accurately Reporting Their Employment Histories

To further explore the misreporting behavior, we estimate an extended model as specified in Section 4.6 by equation (4.17). This even richer misreporting model allows some individuals to report accurately all the time. Compared to our variable misreporting probability model, this model involves only one additional parameter P_A, the fraction of individuals reporting accurately. In addition, we also estimate a version of this richer model allowing P_A and the six seam bias parameters to vary with individual characteristics. Again we find only race significantly affects these probabilities. Table 4A presents coefficients from the constant and variable probability versions of this richer model. The coefficients and standard errors are remarkably similar between the two models and indeed they are quite close to the results from the constant and variable probability models discussed in Section 5.1 above. Table 4B shows the estimates of the probabilities governing misreporting behavior. The top panel of this table (Panel A) contains the seam bias parameter estimates for both the model with constant misreporting probabilities and the model allowing the probabilities to vary with race. The bottom panel (Panel B) shows the misreporting probabilities calculated using the corresponding estimates in Panel A. Standard errors in Panel B are calculated using the delta method. The first three rows are misreporting probabilities corresponding to months 1, 2, and 3 for non-employment spells (on the left) and employment spells (on the right). These probabilities share some similarities with those in Table 2C. For example, the misreporting probabilities are larger for the employment spells than for the non-employment spells and the misreporting probabilities are descending from month 1 through month 3. Again minorities are somewhat more likely to misreport. Note that each misreporting probability in Table 4B is larger than the corresponding estimate in Table 2C; presumably this reflects the fact that the richer models allow a fraction of individuals not to misreport at all. The last row of Table 4B reports the fraction of individuals reporting accurately. The constant probability model estimates that fraction to be 15.5%, and the variable probability model estimates that fraction to be about 3 percentage points higher for Whites.

Comparing the two richer models in this section with the constant and variable misreporting probability models presented in Section 5.1 (called simple misreporting probability models here to distinguish them from the two richer models presented in this section), we find that the coefficients in the hazards are remarkably similar across these 4 models, as are the calculated expected durations and the effect of changing an explanatory variable on expected duration.[29]

[29] The expected durations and the effect of changing an explanatory variable on expected duration from the two richer models are available in an online appendix.

We apply likelihood ratio tests pairwise on a series of nested models. Considering the simple constant misreporting probability model versus the simple variable misreporting probability model (two more parameters for the alternative model), the likelihood ratio test rejects the constant misreporting probability model at the 1% level. Comparing the richer constant misreporting probability model versus the richer variable misreporting probability model (two more parameters for the alternative model), again the likelihood ratio test rejects the constant misreporting probability model at the 1% level. The likelihood ratio test also rejects the simple constant misreporting probability model against the richer constant misreporting model (one more parameter for the alternative model) at the 1% level. Finally, the likelihood ratio test rejects the simple variable misreporting probability model versus the richer variable misreporting probability model (three more parameters for the alternative model), again at the 1% level. To sum up, we find that a simple constant misreporting probability model will serve the purpose of correcting for seam bias. However, if a researcher is also interested in the misreporting behavior per se, increasing model complexity provides added valuable information about the misreporting behavior.

5.4 Long Term Employment Fraction – Simulation Results

Another useful measure for policy purposes besides expected durations and the effect of changing an explanatory variable on expected duration is the fraction of a disadvantaged population that is employed. Based on simulations, we predict employment fractions over 3-year, 6-year and 10-year horizons as well as how these fractions change with macro and public policy variables. We use our simple constant misreporting probability model for the simulation. We simulate an employment/unemployment history for each individual over a particular horizon and then calculate the sample fraction of employment based on simulated individual histories. Note that the simulated fractions depend on the parameter estimates for all four types of spells (but not on the misreporting probabilities). Because the simulations are discontinuous functions of the parameter estimates we cannot use the delta method to obtain standard errors; instead to obtain standard errors we follow Ham and Woutersen (2009) and sample from the asymptotic distribution of the parameters.

We outline our simulation procedure in the following steps:

1. For each individual simulate her 10-year monthly employment history by unobserved heterogeneity type. For example, if the model indicates there are two points of support for unobserved heterogeneity ($J = 2$ in the specification described in Section 4.5), we simulate two employment histories for each individual conditional on belonging to each of the two types.

1.1. The starting point of an employment history is determined by the data. If a person was in a left-censored non-employment spell at the beginning of the sample, her simulated employment history will start with a left-censored non-employment spell.

1.2. From the starting month, an individual monthly hazard rate is estimated based on unobserved heterogeneity type, observed heterogeneity (individual means over the sample period are used), spell type (left-censored employment spell or left-censored non-employment spell) and spell length.[30]

1.3. A uniform random number is drawn to compare with the calculated hazard and determine whether the individual exits into the next spell. Steps 1.2 and 1.3 are repeated for 120 months for each individual and unobserved heterogeneity type. If individual i ($i = 1, N$) of unobserved type j ($j = 1, J$) is employed in month t ($t = 1,120$) then $E_{ij}^t = 1$, otherwise $E_{ij}^t = 0$.

1.4. For individual i, average simulated histories across unobserved heterogeneity type according to estimated probability yield weighted employment history

$$E_i^t = \sum_{j=1}^{J} p_j E_{ij}^t$$

where $p_j \quad j = 1, J$ are probabilities associated with points of support as defined in Section 4.5.

2. Average the simulated histories over the sample and over the first 36, 72, or all 120 months to get estimated three-year, six-year, and ten-year employment fractions

$$ER_{3yr} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{36} E_i^t}{36 \cdot N}$$

$$ER_{6yr} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{72} E_i^t}{72 \cdot N}$$

$$ER_{10yr} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{120} E_i^t}{120 \cdot N}$$

3. Simulate standard errors for estimates of fractions.

[30] Again we face the issue of what to do with the duration dependence once we get out of sample. We choose to 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.

3.1. Assume that all estimators are normally distributed and rely on this assumption to generate alternative values of the parameters (1000 of them).

3.2. Repeat steps 1 to 2 for each set of parameter values to get new estimates of ER_{3yr}, ER_{6yr} and ER_{10yr}.

3.3. Construct standard errors for all three fractions using the 1000 estimates of ER_{3yr}, ER_{6yr} and ER_{10yr}.

4. To predict the effect of changing one of the macro or policy variables on the above three fractions, we follow the previous steps by setting the variable of interest at different levels and then taking the difference in estimated fractions. For example to get the effect of implementing positive incentives to leave welfare (carrot waiver policy), we first set the carrot waiver dummy to 1 (carrot waivers are implemented for all states) to calculate ER^1_{3yr}, ER^1_{6yr} and ER^1_{10yr}; then set the carrot waiver dummy to 0 (carrot waivers are not implemented for any states) to calculate ER^0_{3yr}, ER^0_{6yr} and ER^0_{10yr}. $ER^1_{3yr} - ER^0_{3yr}$, $ER^1_{6yr} - ER^0_{6yr}$ and $ER^1_{10yr} - ER^0_{10yr}$ are the estimated effects of implementing a carrot waiver policy.

5. Standard errors for the effects of changing macro or public policy variables are estimated by following step 3 at each level of those variables.

Table 5 presents our simulation results. The first row contains the estimated employment fractions for 3-year, 6-year and 10-year periods respectively. The predicted employment fractions are around 44% for our sample (single mothers with 12 years of education or less). It is not surprising that the labor force attachment of this disadvantaged population is very low. The rest of the table presents how those employment fractions would change with welfare policies and general macroeconomic conditions. Increasing the state maximum monthly welfare benefits by 10% reduces the predicted employment fractions by 0.2 to 0.3 percentage points. This effect is statistically significant but practically small. Implementing a carrot waiver policy increases predicted employment fractions by 3.6 to 4.2 percentage points, although these effects are only significant at the 10% level. Implementing a stick waiver policy has no effect on employment fractions. Note that for our sample period (1986-1995), only a small number of states implemented welfare waivers. It is thus not surprising that we could not discern an effect for stick waiver policies and that the carrot waiver policy effect is estimated less precisely. We find that increasing the minimum wage by 10% has essentially no effect on the employment fraction.[31] Finally if the overall state unemployment rate

[31] The minimum wage changes infrequently in the US, so there may not be enough sample variation in our data to allow us to detect an effect.

increases by 25%, the employment fraction of this disadvantaged population would fall by 1.4 to 1.8 percentage points.

V. 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 SIPP for single mothers with a high school degree or less. Such employment dynamics have been relatively understudied in the literature, given the policy focus on single mothers, and the emphasis that policy makers put on increasing employment durations (as means of increasing on the job human capital) for disadvantaged women..

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 that our seam bias estimates are not sensitive to allowing the misreporting probability to depend on race. We do find that our seam bias estimates are somewhat different from those obtained with a last month dummy, and very different from the results using the last month data. Our seam bias results are also robust to allowing for the possibility that a certain fraction of individuals do not misreport, or that some individuals misreport in a way different from that in our base model.

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Appendix: Derivation of the Multi-Spell Contribution to the Likelihood Function

We derive the contribution of the employment history in Figure 3. We first do this for one draw of the heterogeneity vector θ and then sum over the heterogeneity distribution.

$$P\left\{M_{U'}^{obs}=(1,2),dur_{U'}^{obs}=5,M_{E}^{obs}=(4,3),dur_{E}^{obs}=7,M_{U}^{obs}=(0,9),dur_{U}^{obs}=24\,|\,\theta\right\}$$

$$=P\left\{\begin{matrix}M_{U'}^{obs}=(1,2),M_{U'}^{true}=(1,2),dur_{U'}^{true}=5,M_{E}^{obs}=(4,3),M_{E}^{true}=(4,3),dur_{E}^{true}=7,\\ M_{U}^{obs}=(0,9),M_{U}^{obs}=(0,9),dur_{U}^{true}=24\end{matrix}\middle|\theta\right\}$$

$$+P\left\{\begin{matrix}M_{U'}^{obs}=(1,2),M_{U'}^{true}=(1,2),dur_{U'}^{true}=5,M_{E}^{obs}=(4,3),M_{E}^{true}=(3,4),dur_{E}^{true}=10,\\ M_{U}^{obs}=(0,9),M_{U}^{obs}=(0,9),dur_{U}^{true}=21\end{matrix}\middle|\theta\right\}$$

$$+P\left\{\begin{matrix}M_{U'}^{obs}=(1,2),M_{U'}^{true}=(1,2),dur_{U'}^{true}=5,M_{E}^{obs}=(4,3),M_{E}^{true}=(2,4),dur_{E}^{true}=9,\\ M_{U}^{obs}=(0,9),M_{U}^{obs}=(0,9),dur_{U}^{true}=22\end{matrix}\middle|\theta\right\}$$

$$+P\left\{\begin{matrix}M_{U'}^{obs}=(1,2),M_{U'}^{true}=(1,2),dur_{U'}^{true}=5,M_{E}^{obs}=(4,3),M_{E}^{true}=(1,4),dur_{E}^{true}=8,\\ M_{U}^{obs}=(0,9),M_{U}^{obs}=(0,9),dur_{U}^{true}=23\end{matrix}\middle|\theta\right\}$$

Using the results from Section 4.2

$$=\left[\left(1-\gamma_1^U\right)\prod_{r=1}^{4}\left(1-\lambda_{U'}\left(r\mid\theta_{U'}\right)\right)\cdot\lambda_{U'}\left(5\middle|\theta_{U'}\right)\right]\bullet$$

$$\left\{\begin{array}{l}+\left[\prod_{r=1}^{6}\left(1-\lambda_E(r\mid\theta_E)\right)\cdot\lambda_E(7\mid\theta_E)\prod_{r=1}^{24}\left(1-\lambda_U(r\mid\theta_U)\right)\right]\\+\left[\gamma_1^E\prod_{r=1}^{7}\left(1-\lambda_E(r\mid\theta_E)\right)\cdot\lambda_E(8\mid\theta_E)\prod_{r=1}^{23}\left(1-\lambda_U(r\mid\theta_U)\right)\right]\\+\left[\gamma_2^E\prod_{r=1}^{8}\left(1-\lambda_E(r\mid\theta_E)\right)\cdot\lambda_E(9\mid\theta_E)\prod_{r=1}^{22}\left(1-\lambda_U(r\mid\theta_U)\right)\right]\\+\left[\gamma_3^E\prod_{r=1}^{9}\left(1-\lambda_E(r\mid\theta_E)\right)\cdot\lambda_E(10\mid\theta_E)\prod_{r=1}^{21}\left(1-\lambda_U(r\mid\theta_U)\right)\right]\end{array}\right\}$$

Sum over the heterogeneity distribution we have

$$L=\sum_{j=1}^{J}p_j\left(\begin{array}{l}\left[\left(1-\gamma_1^U\right)\prod_{r=1}^{4}\left(1-\lambda_{U'}\left(r\mid\theta_{U'j}\right)\right)\cdot\lambda_{U'}\left(5\middle|\theta_{U'j}\right)\right]\cdot\\ \left\{\begin{array}{l}\left[\gamma_1^E\prod_{r=1}^{7}\left(1-\lambda_E(r\mid\theta_{Ej})\right)\cdot\lambda_E(8\mid\theta_{Ej})\prod_{r=1}^{23}\left(1-\lambda_U(r\mid\theta_{Uj})\right)\right]\\+\left[\gamma_2^E\prod_{r=1}^{8}\left(1-\lambda_E(r\mid\theta_{Ej})\right)\cdot\lambda_E(9\mid\theta_{Ej})\prod_{r=1}^{22}\left(1-\lambda_U(r\mid\theta_{Uj})\right)\right]\\+\left[\gamma_3^E\prod_{r=1}^{9}\left(1-\lambda_E(r\mid\theta_{Ej})\right)\cdot\lambda_E(10\mid\theta_{Ej})\prod_{r=1}^{21}\left(1-\lambda_U(r\mid\theta_{Uj})\right)\right]\\+\left[\prod_{r=1}^{6}\left(1-\lambda_E(r\mid\theta_{Ej})\right)\cdot\lambda_E(7\mid\theta_{Ej})\prod_{r=1}^{24}\left(1-\lambda_U(r\mid\theta_{Uj})\right)\right]\end{array}\right\}\end{array}\right)$$

Table 1 Characteristics of Employment and Non-employment Spells
Single Mothers with Twelve Years of Scholing or Lower Education

	Left-censored spells		Fresh spells	
Panel A: Non-employment spells	**Mean**	**Std Dev**	**Mean**	**Std Dev**
Right censored (%)	64.5%		42.6%	
African American	0.34	0.48	0.33	0.47
Hispanic	0.23	0.42	0.17	0.38
12 years of schooling	0.44	0.50	0.61	0.49
Age	30.08	9.01	31.24	8.65
Never married	0.50	0.50	0.45	0.50
# of children < 18	1.98	1.19	1.72	0.97
Age of youngest child	5.00	5.09	6.56	5.10
# of children < 6	0.93	0.91	0.65	0.75
Disability (adult or child)	0.24	0.42	0.19	0.39
Disability variable missing	0.17	0.37	0.08	0.27
number of spells	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	
Panel B: Employment spells	**Mean**	**Std Dev**	**Mean**	**Std Dev**
Right censored (%)	69.9%		47.8%	
African American	0.26	0.44	0.32	0.47
Hispanic	0.14	0.35	0.18	0.39
12 years of schooling	0.74	0.44	0.61	0.49
Age	33.99	8.49	30.86	8.58
Never married	0.27	0.45	0.45	0.50
# of children < 18	1.55	0.86	1.78	1.02
Age of youngest child	8.14	5.44	6.28	5.12
# of children < 6	0.46	0.66	0.68	0.76
Disability (adult or child)	0.12	0.33	0.18	0.38
Disability variable missing	0.13	0.34	0.07	0.26
number of spells	3,826		2,732	
number of individuals	3,826		2,000	
number of observations: year*individual	71,613		21,376	
total number of individuals	7,354			
total number of observations	175,184			

Notes:
1. sample means are taken at the first month of spells.
2. summary statistics across spells are not independent in the sense that some individuals show in both left-censored and fresh spells.
3. number of spells reported in this table include both completed spells and right-censored spells.
4. Total number of individuals of the sample is not the sum of number of individuals in the 4 types of spells because some individuals have multiple spells of different types, e.g a left-censored non-employeemnt spell followed by a fresh employment spell.

Table 2A. Duration Models of Employment and Non-employment Spells single Mothers with Twelve Years of Scholing or Lower Education - ***Left-censored Spells***

	Left-censored non-employment spells				**Left-censored employment spells**			
	Constant Probabilities	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**	**Constant Probabilities**	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**
Maximum Welfare Benefit	-9.985**	-9.998**	-10.130**	-11.918**	-7.757**	-7.754**	-0.756	1.561
	(2.386)	(2.386)	(2.122)	(2.392)	(2.957)	(2.957)	(2.262)	(2.556)
Unemployment Rate	-0.0613**	-0.061**	-0.0742**	-0.081**	-0.0004	-0.0003	0.038*	0.046*
	(0.023)	(0.023)	(0.021)	(0.024)	(0.028)	(0.028)	(0.021)	(0.025)
Minimum Wage	0.133	0.133	0.140	0.088	0.280	0.281	0.127	0.051
	(0.189)	(0.189)	(0.166)	(0.194)	(0.238)	(0.238)	(0.175)	(0.199)
Welfare Waiver Stick	-0.171	-0.171	-0.217	0.072	-0.397	-0.398	-0.119	-0.091
	(0.259)	(0.259)	(0.235)	(0.225)	(0.344)	(0.344)	(0.246)	(0.278)
Welfare Waiver Carrot	0.043	0.044	0.085	0.106	-0.014	-0.014	-0.104	-0.255
	(0.190)	(0.190)	(0.166)	(0.176)	(0.218)	(0.218)	(0.170)	(0.199)
African American	-0.171**	-0.175**	-0.117	-0.129	0.088	0.085	0.346**	0.388**
	(0.085)	(0.087)	(0.077)	(0.086)	(0.100)	(0.103)	(0.076)	(0.086)
Hispanic	-0.184**	-0.188*	-0.140*	-0.101	0.209*	0.207*	0.199**	0.133**
	(0.096)	(0.098)	(0.085)	(0.095)	(0.111)	(0.114)	(0.090)	(0.105)
12 years of schooling	0.365**	0.365**	0.420**	0.509**	-0.549**	-0.549**	-0.600**	-0.670**
	(0.070)	(0.070)	(0.064)	(0.072)	(0.084)	(0.084)	(0.066)	(0.077)
Age	-0.049**	-0.049**	-0.040**	-0.026**	-0.039**	-0.039**	-0.029**	-0.026**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)
Never Married	-0.441**	-0.441**	-0.367**	-0.276**	0.203**	0.204**	0.229**	0.279**
	(0.084)	(0.084)	(0.076)	(0.085)	(0.097)	(0.097)	(0.076)	(0.088)
# of children < 18	0.010	0.011	-0.005	0.038	0.079	0.079	0.035	0.017
	(0.039)	(0.039)	(0.035)	(0.038)	(0.052)	(0.052)	(0.040)	(0.045)
Age of Youngest Child	0.003	0.003	0.008	-0.012	-0.016	-0.016	-0.016	-0.024*
	(0.013)	(0.013)	(0.011)	(0.013)	(0.014)	(0.014)	(0.011)	(0.012)
# of children < 6	-0.284**	-0.284**	-0.292**	-0.363**	0.160**	0.160**	0.137**	0.103
	(0.063)	(0.063)	(0.057)	(0.068)	(0.079)	(0.079)	(0.062)	(0.074)
Disability	-0.466**	-0.466**	-0.596**	-0.615**	0.815**	0.817**	0.769**	0.844**
	(0.092)	(0.092)	(0.085)	(0.092)	(0.103)	(0.103)	(0.083)	(0.094)
Disability Variable Missing	-0.106	-0.106	-0.209**	-0.428**	0.363**	0.363**	0.332**	0.009
	(0.118)	(0.118)	(0.106)	(0.127)	(0.139)	(0.139)	(0.107)	(0.140)

Table 2A (continued) Duration Models of Employment and Non-employment Spells single Mothers with Twelve Years of Scholing or Lower Education - ***Left-censored Spells***

	Left-censored non-employment spells				Left-censored employment spells			
	Constant Probabilities	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**	**Constant Probabilities**	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**
log(duration)	-0.363**	-0.363**	0.288**	-0.705**	-0.314**	-0.314**	0.432**	-0.707**
	(0.041)	(0.041)	(0.124)	(0.064)	(0.049)	(0.049)	(0.150)	(0.065)
Square of log(duration)			-0.188**				-0.238**	
			(0.035)				(0.040)	
Last-Month Dummy	-	-	0.700**	-	-	-	1.359**	-
			(0.064)				(0.065)	
Unobserved Heterogeneity								
Theta1	-1.195*	-1.193*	-1.121*	1.386*	-2.971**	-2.970**	-3.579**	-0.369
	(0.708)	(0.708)	(0.673)	(0.846)	(0.854)	(0.854)	(0.660)	(0.800)
Theta2	-1.131	-1.132	-1.993**	0.685	-3.691**	-3.692**	-4.048**	-0.878
	(0.707)	(0.707)	(0.632)	(0.718)	(0.842)	(0.843)	(0.657)	(0.742)
Heterogeneity Probability								
	0.396**	0.396**	0.365**	0.265**				
	(0.022)	(0.022)	(0.106)	(0.125)				

Notes:
1. We allow unobserved heterogeneity to be correlated across different type of spells (see Section 4.5). For each model, the heterogeneity probability is the same for each of the 4 types of spells.
2. Year dummies are included in each regression. Coefficients are omitted.
3. Standard errors are in parentheses.
4. Maximum welfare benefit variable has been divided by 10,000.
* significant at 10% level.
** significant at 5% level.

Table 2B. Duration Models of Employment and Non-employment Spells single Mothers with Twelve Years of Scholing or Lower Education - ***Fresh Spells***

	Fresh non-employment spells				**Fresh employment spells**			
	Constant Probabilities	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**	**Constant Probabilities**	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**
Maximum Welfare Benefit	-3.414	-3.419	-7.348**	-3.875	8.481**	8.474**	5.542**	11.675**
	(2.252)	(2.252)	(2.462)	(3.666)	(2.338)	(2.339)	(2.393)	(3.924)
Unemployment Rate	-0.054**	-0.054**	-0.054**	-0.033	0.060**	0.060**	0.023	0.055
	(0.021)	(0.021)	(0.023)	(0.036)	(0.021)	(0.021)	(0.022)	(0.039)
Minimum Wage	-0.150	-0.149	0.072	-0.232	-0.504**	-0.505**	-0.094	0.418
	(0.181)	(0.182)	(0.189)	(0.296)	(0.198)	(0.198)	(0.200)	(0.321)
Welfare Waiver Stick	-0.321	-0.322	-0.309	-0.215	0.176	0.176	-0.149	-0.508
	(0.199)	(0.199)	(0.208)	(0.309)	(0.181)	(0.182)	(0.204)	(0.348)
Welfare Waiver Carrot	0.380**	0.380**	0.370**	0.243	-0.120	-0.120	-0.060	-0.276
	(0.127)	(0.128)	(0.150)	(0.223)	(0.139)	(0.139)	(0.148)	(0.235)
African American	-0.200**	-0.196**	-0.332**	-0.089	0.061	0.061	0.065	0.247*
	(0.076)	(0.077)	(0.087)	(0.128)	(0.079)	(0.080)	(0.083)	(0.144)
Hispanic	-0.049	-0.046	0.023	0.194	-0.012	-0.012	-0.062	-0.093
	(0.086)	(0.086)	(0.097)	(0.149)	(0.097)	(0.098)	(0.099)	(0.164)
12 years of schooling	0.211**	0.211**	0.226**	0.284**	-0.420**	-0.420**	-0.358**	-0.365**
	(0.063)	(0.063)	(0.070)	(0.111)	(0.069)	(0.069)	(0.071)	(0.119)
Age	0.001	0.001	0.002	-0.001	-0.015**	-0.015**	-0.032**	-0.039**
	(0.006)	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)	(0.007)	(0.011)
Never Married	-0.075	-0.074	-0.131	-0.130	0.079	0.079	0.080	0.239*
	(0.075)	(0.075)	(0.084)	(0.130)	(0.080)	(0.080)	(0.083)	(0.146)
# of children < 18	-0.063*	-0.063*	0.021	-0.004	-0.018	-0.018	-0.005	0.061
	(0.038)	(0.038)	(0.041)	(0.061)	(0.037)	(0.037)	(0.039)	(0.064)
Age of Youngest Child	0.004	0.003	0.005	-0.001	-0.006	-0.006	-0.002	0.002
	(0.011)	(0.011)	(0.013)	(0.019)	(0.012)	(0.012)	(0.012)	(0.020)
# of children < 6	-0.128**	-0.128*	-0.157**	-0.232**	-0.023	-0.023	0.003	-0.060
	(0.064)	(0.064)	(0.071)	(0.111)	(0.062)	(0.062)	(0.065)	(0.110)
Disability	-0.595**	-0.596**	-0.486**	-0.590**	0.586**	0.587**	0.480**	0.426**
	(0.083)	(0.083)	(0.092)	(0.148)	(0.090)	(0.090)	(0.090)	(0.158)
Disability Variable Missing	-0.283*	-0.283*	-0.443**	-0.715**	0.702**	0.702**	0.152	-0.418
	(0.157)	(0.157)	(0.149)	(0.278)	(0.155)	(0.155)	(0.143)	(0.270)

Table 2B (continued) Duration Models of Employment and Non-employment Spells single Mothers with Twelve Years of Scholing or Lower Education - ***Fresh Spells***

	Fresh non-employment spells				**Fresh employment spells**			
	Constant Probabilities	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**	**Constant Probabilities**	**Variable Probabilities**	**Last Month Dummy**	**Last Month Data**
log(duration)	0.103	0.101	-0.041	-0.688**	0.089	0.087	0.362**	-0.706**
	(0.099)	(0.099)	(0.100)	(0.116)	(0.101)	(0.101)	(0.109)	(0.135)
Square of log(duration)	-0.019	-0.019	-0.116**		0.065*	0.065*	-0.278**	
	(0.031)	(0.031)	(0.037)		(0.035)	(0.035)	(0.040)	
Last-Month Dummy	-	-	0.950**	-	-	-	1.360**	-
			(0.060)				(0.061)	
Unobserved Heterogeneity								
Theta1	0.541	0.533	-2.448**	-0.004	-2.118**	-2.113**	-1.169	-0.319
	(0.662)	(0.662)	(0.694)	(1.117)	(0.711)	(0.713)	(0.743)	(1.224)
Theta2	-1.634**	-1.640**	-1.251*	1.454	0.264	0.270	-2.254	-2.172*
	(0.663)	(0.663)	(0.694)	(1.104)	(0.711)	(0.713)	(0.740)	(1.247)
Heterogeneity Probability								
	0.396**	0.396**	0.365**	0.265**				
	(0.022)	(0.022)	(0.106)	(0.125)				

See Table 2A footnotes.

Table 2C. Misreporting Probabilities Due to Seam Bias
Constant Probabilities vs. Probabilities Varying by Race

Panel A: Parameter Estimates

	Non-employment Spells		Employment Spells	
	Constant Probabilities	Probabilities Varying with Race	Constant Probabilities	Probabilities Varying with Race
Month 1 Intercept	0.275*	0.796**	0.989**	0.0986
	(0.142)	(0.168)	(0.149)	(0.17)
Month 2 Intercept	-0.174	0.211	0.410**	-0.350*
	(0.174)	(0.181)	(0.158)	(0.198)
Month 3 Intercept	-0.236	0.1349	0.345**	-0.375*
	(0.177)	(0.184)	(0.161)	(0.194)
Minority Dummy		0.384**		0.299*
		(0.159)		(0.156)

Panel B: Misreporting Probabilities

	Non-employment Spells			Employment Spells		
	Constant Probabilities	Probabilities Varying with Race		Constant Probabilities	Probabilities Varying with Race	
Month 1	0.568**	White	0.525**	0.729**	White	0.689**
	(0.035)		(0.042)	(0.029)		(0.036)
		Minorities	0.598**		Minorities	0.765**
			(0.039)			(0.030)
Month 2	0.456**	White	0.413**	0.601**	White	0.553**
	(0.043)		(0.048)	(0.038)		(0.045)
		Minorities	0.487**		Minorities	0.645**
			(0.040)			(0.035)
Month 3	0.441**	White	0.407**	0.585**	White	0.534**
	(0.044)		(0.047)	(0.039)		(0.046)
		Minorities	0.481**		Minorities	0.627**
			(0.037)			(0.035)

Notes:
1. For the constant probability model, parameters in Panel A are based on reparameterization specified in Section V. For the model with seam bias probabilities varying with the minority dummy variable, parameters in Panel A are specified in equation (4.15).
2. Standard errors in Panel B are calculated using the delta method.

Table 3A. Expected Durations and the Effects of Changes in Macro and Public Policy Variables
Employment and Non-employment 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)	39.305**	39.299**	35.478**	38.834**	42.248**	42.252**	38.999**	41.157**
	(0.731)	(0.725)	(0.574)	(0.600)	(0.608)	(0.634)	(0.533)	(0.601)
Changes with respect to:								
Maximum welfare benefits increasing by 10%	0.515**	0.516**	0.552**	0.585	0.352	0.352	0.040	-0.074
	(0.143)	(0.169)	(0.185)	(0.495)	(0.359)	(0.259)	(0.112)	(0.228)
Carrot waiver (implemented - Not implemented)	-0.639	-0.654	-1.313	-1.498	0.168	0.165	1.436	3.055
	(2.892)	(2.827)	(2.608)	(2.497)	(2.655)	(2.537)	(2.253)	(2.225)
Stick waiver (implemented - Not implemented)	2.427	2.427	3.228	-1.005	4.483	4.485	1.639	1.125
	(3.458)	(3.503)	(3.416)	(3.195)	(3.514)	(3.537)	(3.400)	(3.374)
Minimum wage increasing by 10%	-0.768	-0.773	-0.841	-0.485	-1.381	-1.383	-0.707	-0.250
	(1.423)	(1.079)	(1.142)	(1.345)	(1.043)	(1.201)	(0.972)	(0.873)
Unemployment rate increasing by 25%	1.446**	1.445**	1.834**	1.806**	0.007	0.006	-0.898	-0.980
	(0.580)	(0.713)	(0.643)	(0.599)	(0.548)	(0.587)	(0.525)	(0.628)
	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)	11.821**	11.827**	16.458**	23.342**	11.929**	11.920**	27.711**	32.563**
	(0.516)	(0.514)	(1.312)	(2.254)	(0.495)	(0.497)	(1.394)	(2.197)
Changes with respect to:								
Maximum welfare benefits increasing by 10%	0.118	0.119	0.375**	0.196	-0.220**	-0.220**	-0.341*	-0.565
	(0.119)	(0.149)	(0.136)	(0.407)	(0.070)	(0.062)	(0.206)	(0.446)
Carrot waiver (implemented - Not implemented)	-3.099**	-3.103**	-4.531**	-3.139	0.881	0.885	0.988	3.471
	(1.009)	(0.933)	(1.703)	(2.790)	(1.074)	(1.014)	(2.471)	(2.826)
Stick waiver (implemented - Not implemented)	3.138	3.152	4.388	2.950	-1.222	-1.220	2.453	6.266
	(2.100)	(2.075)	(3.212)	(4.323)	(1.217)	(1.228)	(3.355)	(4.187)
Minimum wage increasing by 10%	0.545	0.540	-0.375	1.238	1.484**	1.486**	0.608	-2.133
	(0.613)	(0.715)	(1.027)	(1.420)	(0.538)	(0.570)	(1.180)	(1.499)
Unemployment rate increasing by 25%	0.835**	0.837**	1.226**	0.732	-0.693**	-0.695**	-0.633	-1.193
	(0.343)	(0.354)	(0.550)	(0.892)	(0.277)	(0.249)	(0.593)	(0.826)

Note: Standard errors in parentheses are calculated using the delta method.

Table 3B. The Effects of Changes in Demographic Variables - Employment and Non-employment 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**
Changes with respect to:								
Age (age=35) - (age=25)	7.471**	7.475**	6.253**	3.619**	5.095**	5.099**	4.328**	3.455**
	(1.069)	(1.098)	(0.875)	(0.902)	(0.996)	(1.012)	(0.892)	(1.005)
12 years of schooling (s = 12) - (s < 12)	-5.293**	-5.295**	-6.398**	-6.999**	7.013**	7.013**	8.813**	8.822**
	(1.051)	(1.126)	(0.977)	(0.992)	(1.142)	(1.111)	(1.026)	(1.106)
Race (Black - White)	2.524*	2.584**	1.802	1.791	-1.074	-1.036	-4.938**	-4.991**
	(1.299)	(1.268)	(1.206)	(1.299)	(1.182)	(1.218)	(1.117)	(1.142)
Race (Hispanic - White)	2.708*	2.759*	2.139*	1.402	-2.616*	-2.583*	-2.773**	-1.630
	(1.408)	(1.449)	(1.292)	(1.357)	(1.534)	(1.479)	(1.281)	(1.528)
Number of children less than 6 years old (one - zero)	4.225**	4.225**	4.512**	5.120**	-1.965*	-1.967**	-1.940**	-1.299
	(0.906)	(0.929)	(0.895)	(0.954)	(1.029)	(0.953)	(0.919)	(0.967)

	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**
Age (age=35) - (age=25)	-0.070	-0.077	-0.220	0.106	1.027**	1.029**	5.479**	5.195**
	(0.532)	(0.514)	(0.847)	(1.261)	(0.430)	(0.420)	(1.052)	(1.439)
12 years of schooling (s = 12) - (s < 12)	-1.940**	-1.940**	-3.057**	-3.861**	2.970**	2.969**	5.984**	4.737**
	(0.595)	(0.593)	(0.961)	(1.497)	(0.504)	(0.503)	(1.196)	(1.671)
Race (Black - White)	1.842**	1.808**	4.581**	1.214	-0.440	-0.438	-1.079	-3.199**
	(0.718)	(0.730)	(1.252)	(1.769)	(0.590)	(0.591)	(1.381)	(1.787)
Race (Hispanic - White)	0.435	0.407	-0.291	-2.521	0.090	0.086	1.027	1.175
	(0.771)	(0.773)	(1.224)	(1.904)	(0.725)	(0.708)	(1.670)	(2.116)
Number of children less than 6 years old (one - zero)	1.151**	1.155**	2.096**	3.101**	0.165	0.164	-0.043	0.763
	(0.581)	(0.575)	(0.963)	(1.493)	(0.452)	(0.452)	(1.102)	(1.424)

Table 4A. Duration Models of Employment and Non-employment Spells Allowing a Fraction of Individuals Reporting without Seam Bias constant Seam Bias Probabilities vs. Seam Bias Probabilities 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	-9.987**	-10.002**	-7.763**	-7.763**	-3.318	-3.324	8.383**	8.375**
	(2.387)	(2.388)	(2.957)	(2.958)	(2.257)	(2.260)	(2.337)	(2.339)
Unemployment Rate	-0.061**	-0.061**	-0.0003	-0.0003	-0.053**	-0.053**	0.060**	0.060**
	(0.023)	(0.023)	(0.028)	(0.028)	(0.021)	(0.021)	(0.021)	(0.021)
Minimum Wage	0.134	0.135	0.282	0.282	-0.151	-0.150	-0.502**	-0.503**
	(0.189)	(0.189)	(0.238)	(0.238)	(0.182)	(0.182)	(0.198)	(0.199)
Welfare Waiver Stick	-0.173	-0.173	-0.398	-0.399	-0.321	-0.322	0.176	0.177
	(0.259)	(0.259)	(0.344)	(0.344)	(0.199)	(0.199)	(0.182)	(0.182)
Welfare Waiver Carrot	0.044	0.045	-0.014	-0.013	0.376**	0.376**	-0.118	-0.119
	(0.190)	(0.190)	(0.218)	(0.219)	(0.128)	(0.128)	(0.139)	(0.139)
African American	-0.172**	-0.177**	0.087	0.085	-0.197**	-0.195**	0.061	0.062
	(0.085)	(0.087)	(0.100)	(0.103)	(0.076)	(0.077)	(0.079)	(0.080)
Hispanic	-0.184*	-0.188**	0.209*	0.207	-0.048	-0.046	-0.013	-0.012
	(0.096)	(0.098)	(0.111)	(0.114)	(0.086)	(0.086)	(0.097)	(0.098)
12 years of schooling	0.365**	0.365**	-0.547**	-0.548**	0.209**	0.209**	-0.418**	-0.418**
	(0.070)	(0.070)	(0.084)	(0.084)	(0.063)	(0.063)	(0.069)	(0.069)
Age	-0.049**	-0.049**	-0.039**	-0.039	0.001	0.001	-0.015**	-0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Never Married	-0.440**	-0.441**	0.205**	0.205**	-0.076	-0.075	0.080	0.080
	(0.084)	(0.084)	(0.097)	(0.097)	(0.075)	(0.075)	(0.080)	(0.080)
# of children < 18	0.010	0.011	0.079	0.079	-0.064*	-0.064*	-0.018	-0.018
	(0.039)	(0.039)	(0.052)	(0.052)	(0.038)	(0.038)	(0.037)	(0.037)
Age of Youngest Child	0.003	0.003	-0.016	-0.016	0.003	0.003	-0.006	-0.006
	(0.013)	(0.013)	(0.014)	(0.014)	(0.011)	(0.011)	(0.012)	(0.012)
# of children < 6	-0.284**	-0.284**	0.159**	0.159**	-0.131**	-0.131**	-0.022	-0.022
	(0.063)	(0.063)	(0.079)	(0.079)	(0.064)	(0.064)	(0.062)	(0.062)
Disability	-0.465**	-0.465**	0.814**	0.816**	-0.594**	-0.595**	0.585**	0.586**
	(0.092)	(0.092)	(0.103)	(0.103)	(0.083)	(0.083)	(0.090)	(0.090)
Disability Variable Missing	-0.106	-0.106	0.363**	0.363**	-0.282*	-0.282**	0.703**	0.703**
	(0.118)	(0.118)	(0.139)	(0.139)	(0.157)	(0.157)	(0.155)	(0.155)

Table 4A. Duration Models of Employment and Non-employment Spells Allowing a Fraction of Individuals Reporting without Seam Bias Constant Seam Bias Probabilities vs. Seam Bias Probabilities 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)	-0.363**	-0.363**	-0.314**	-0.314**	0.105	0.104	0.091	0.089
	(0.041)	(0.041)	(0.049)	(0.049)	(0.099)	(0.099)	(0.101)	(0.101)
Square of log(duration)					-0.020	-0.019	0.064*	0.064
					(0.031)	(0.031)	(0.035)	(0.035)
Unobserved Heterogeneity								
Theta1	-1.205*	-1.205*	-2.978**	-2.978**	0.539	0.534	-2.122**	-2.115**
	(0.708)	(0.708)	(0.854)	(0.854)	(0.662)	(0.663)	(0.712)	(0.714)
Theta2	-1.135	-1.136	-3.699**	-3.699**	-1.638**	-1.642**	0.258	0.265
	(0.707)	(0.707)	(0.843)	(0.843)	(0.663)	(0.663)	(0.712)	(0.714)
Heterogeneity Probability	0.397**	0.397**						
	(0.022)	(0.022)						

Table 4B. Misreporting Probabilities Due to Seam Bias
Constant Probabilities vs. Probabilities Varying by Race

Panel A: Parameter Estimates

	Non-employment Spells		Employment Spells	
	Constant Probabilities	Probabilities Varying with Race	Constant Probabilities	Probabilities Varying with Race
Seam Bias Parameters:				
Month 1 Intercept	0.814**	1.614**	1.822**	0.605**
	(0.217)	(0.389)	(0.314)	(0.264)
Month 2 Intercept	0.257	0.837**	1.024**	0.041
	(0.220)	(0.321)	(0.259)	(0.270)
Month 3 Intercept	0.083	0.592*	0.802**	-0.089
	(0.216)	(0.314)	(0.222)	(0.254)
Minority Dummy		0.3559		0.3499
		(0.348)		(0.253)
Fraction of individuals reporting accurately				
Intercept	-1.694**	-1.596**		
	(0.227)	(0.365)		
Minority Dummy		-0.224		
		(0.457)		

Panel B: Misreporting Probabilities

	Non-employment Spells			Employment Spells		
	Constant Probabilities	Probabilities Varying with Race		Constant Probabilities	Probabilities Varying with Race	
Month 1	0.693	White	0.647	0.861	White	0.834
	(0.046)		(0.060)	(0.038)		(0.054)
		Minorities	0.722		Minorities	0.878
			(0.048)			(0.036)
Month 2	0.564	White	0.510	0.736	White	0.698
	(0.054)		(0.067)	(0.050)		(0.068)
		Minorities	0.597		Minorities	0.767
			(0.058)			(0.055)
Month 3	0.521	White	0.478	0.690	White	0.644
	(0.054)		(0.063)	(0.047)		(0.072)
		Minorities	0.565		Minorities	0.721
			(0.062)			(0.051)
Fraction of Accurate Reporting	0.155	White	0.169			
	(0.030)		(0.051)			
		Minorities	0.139			
			(0.035)			

Notes:
1. For the constant probability model, parameters in Panel A are based on reparameterization specified in Section 5. For the model with seam bias probabilities varying with the minority dummy variable, parameters in Panel A are specified in equation (4.15).
2. Standard errors in Panel B are calculated using the delta method.

Table 5. Employment Fraction over 3-year, 6-year and 10-year Periods Simulation Results Based on the Simple Constant Misreporting Probability Model

	3-year Period	6-year Period	10-year Period
Average Expected Employment Fraction	0.431** (0.009)	0.439** (0.009)	0.449** (0.010)
Changes with respect to:			
Maximum welfare benefits increasing by 10%	-0.002** (0.001)	-0.003** (0.001)	-0.003** (0.001)
Carrot waiver (implemented - Not implemented)	0.036* (0.020)	0.039* (0.022)	0.042* (0.023)
Stick waiver (implemented - Not implemented)	-0.002 (0.032)	-0.006 (0.034)	-0.011 (0.036)
Minimum wage increasing by 10%	0.002 (0.010)	0.002 (0.011)	0.003 (0.011)
Unemployment rate increasing by 25%	-0.014** (0.005)	-0.016** (0.005)	-0.018** (0.005)

Table A1. Transitions of Employment Spells Ending in Month 4 As a Fraction of All Transitions for Imputed and Non-imputed Data

year	Non-imputed INTVW=1: self interview		Non-imputed INTVW=2: proxy interview		Imputed INTVW=3: refusal		Imputed INTVW=4: left the sample	
	Percentage	Sample Size	Percentage	Sample Size	Percentage	Sample Size	Percentage	Sample Size
1986	0.48	2481	0.50	1123	0.84	111	0.70	83
1987	0.54	308	0.51	1105	0.77	174	0.86	95
1988	0.40	2519	0.49	1030	0.78	105	0.77	97
1990	0.49	4629	0.55	2166	0.85	331	0.83	332
1991	0.48	2663	0.52	1301	0.87	151	0.72	120
1992	0.48	4177	0.53	2271	0.88	346	0.76	238
1993	0.50	3837	0.56	1960	0.83	326	0.77	268
Mean	0.48		0.52		0.83		0.77	
	Mean of Non-imputed				**Mean of Imputed**			
	0.50				0.80			

Table A2. Distributions of Spell Length - All Employment and Unemployment Spells Including Both Completed and Right Censored Spells

Panel A: Monthly Data

	Employment Spells				Unemployment Spells			
	Left-censored		Fresh		Left-censored		Fresh	
Spell Length	Percent	Cumulative Percent	Percent	Cumulative Percent	Percent	Cumulative Percent	Percent	Cumulative Percent
1	1.5	1.5	13.1	13.1	2.8	2.8	16.4	16.4
2	2.6	4.1	12.7	25.8	2.8	5.6	12.2	28.6
3	2.0	6.2	9.4	35.1	2.4	7.9	8.0	36.6
4	13.0	19.2	14.6	49.7	12.5	20.5	15.8	52.3
5	2.1	21.2	4.8	54.6	2.6	23.0	5.1	57.4
6	1.8	23.1	4.6	59.2	2.6	25.6	4.2	61.6
7	1.9	25.0	3.9	63.0	1.7	27.3	3.2	64.8
8	7.2	32.2	5.8	68.8	7.9	35.2	7.3	72.1
9	1.2	33.4	2.8	71.6	1.8	37.0	2.9	74.9
10	1.6	35.0	2.7	74.3	1.8	38.7	2.5	77.4
11	1.5	36.4	1.9	76.2	1.4	40.1	1.6	79.0
12	5.1	41.6	3.8	80.0	4.9	45.0	4.0	83.1
> 12	58.5	100.0	20.0	100.0	55.0	100.0	16.9	100.0
Number of Spells	3805		2726		3495		2574	

Panel B: Last Month Data

	Employment Spells				Unemployment Spells			
	Left-censored		Fresh		Left-censored		Fresh	
Spell Length	Percent	Cumulative Percent	Percent	Cumulative Percent	Percent	Cumulative Percent	Percent	Cumulative Percent
4	19.9	19.9	44.4	44.4	21.4	21.4	46.6	46.6
8	11.5	31.4	20.0	64.3	13.9	35.3	20.2	66.8
12	8.8	40.2	12.0	76.4	8.1	43.4	12.7	79.5
16	7.2	47.4	8.7	85.1	6.2	49.6	6.8	86.3
20	5.4	52.8	6.9	92.0	6.6	56.2	5.2	91.5
24	9.6	62.4	3.9	95.8	8.7	64.9	3.9	95.5
28	10.1	72.4	2.6	98.4	9.4	74.2	2.8	98.3
32	15.5	87.9	0.9	99.3	14.7	88.9	1.1	99.4
36	7.6	95.5	0.7	100.0	7.3	96.2	0.6	100.0
40	4.5	100.0			3.8	100.0		
Number of Spells	3809		1789		3448		1676	
Spells Lost	-0.11%		34.37%		1.34%		34.89%	

Figure 1.1

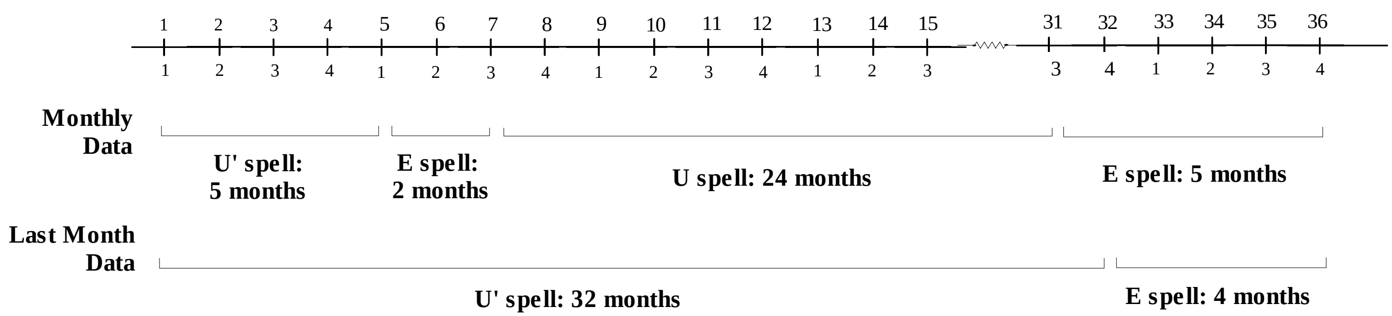


Figure 1.2

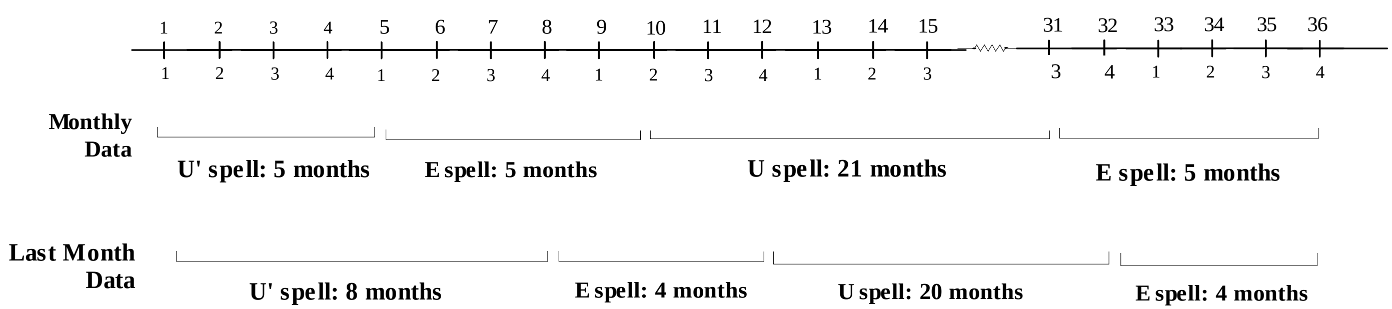


Figure 1.3

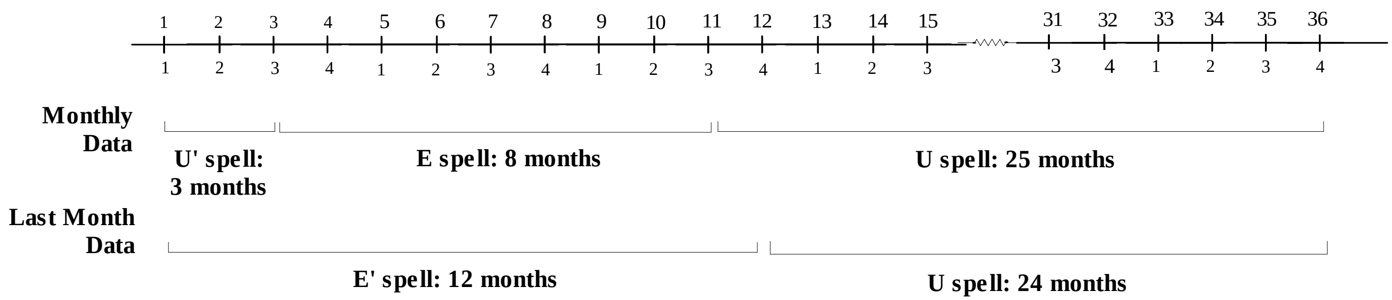

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 31 32 33 34 35 36
1 2 3 4 1 2 3 4 1 2 3 4 1 2 3 3 4 1 2 3 4
Monthly Data
U' spell: 3 months
E spell: 8 months
U spell: 25 months
Last Month Data
E' spell: 12 months
U spell: 24 months

Figure 2.1

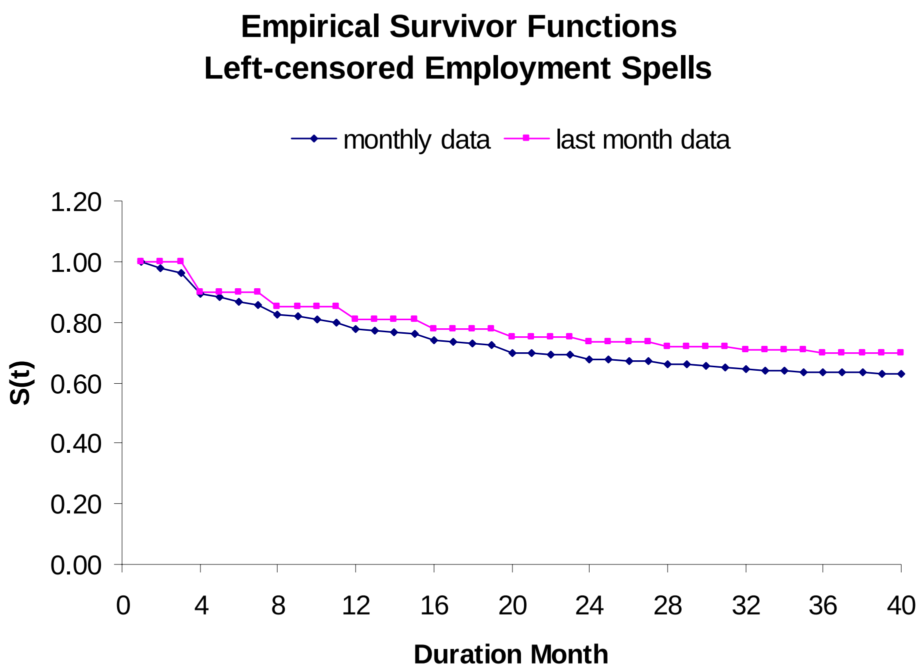

Empirical Survivor Functions
Left-censored Employment Spells
monthly data
last month data
1.20
1.00
0.80
0.60
0.40
0.20
0.00
S(t)
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 2.2

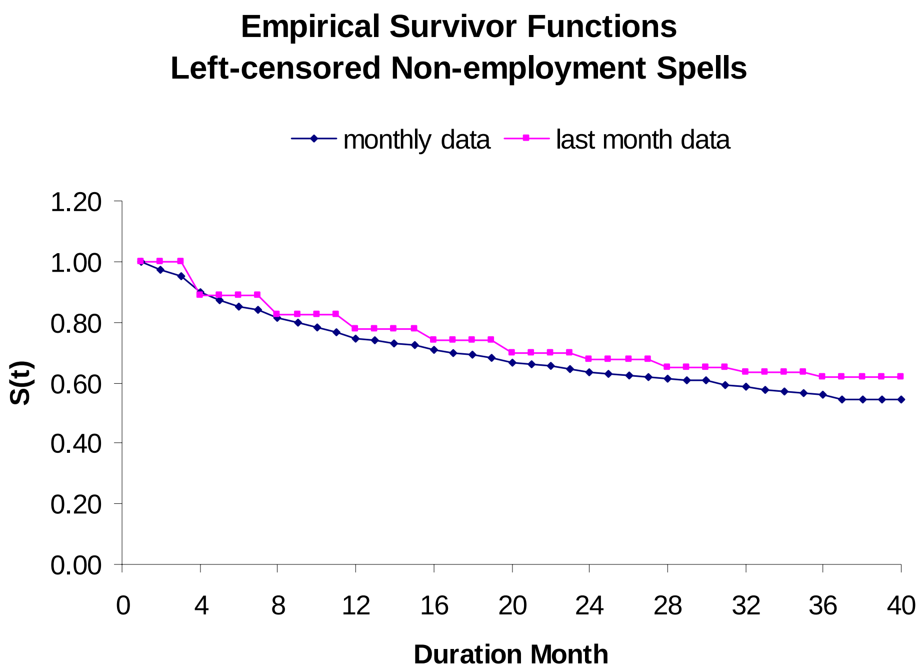

Empirical Survivor Functions
Left-censored Non-employment Spells
monthly data
last month data
1.20
1.00
0.80
0.60
0.40
0.20
0.00
S(t)
0
4
8
12
16
20
24
28
32
36
40
Duration Month

Figure 2.3

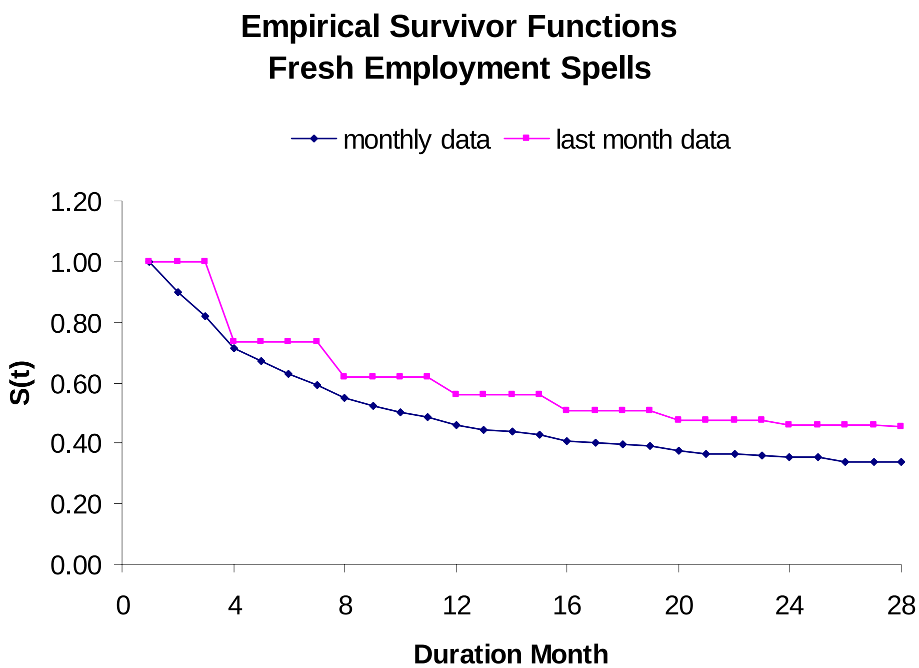
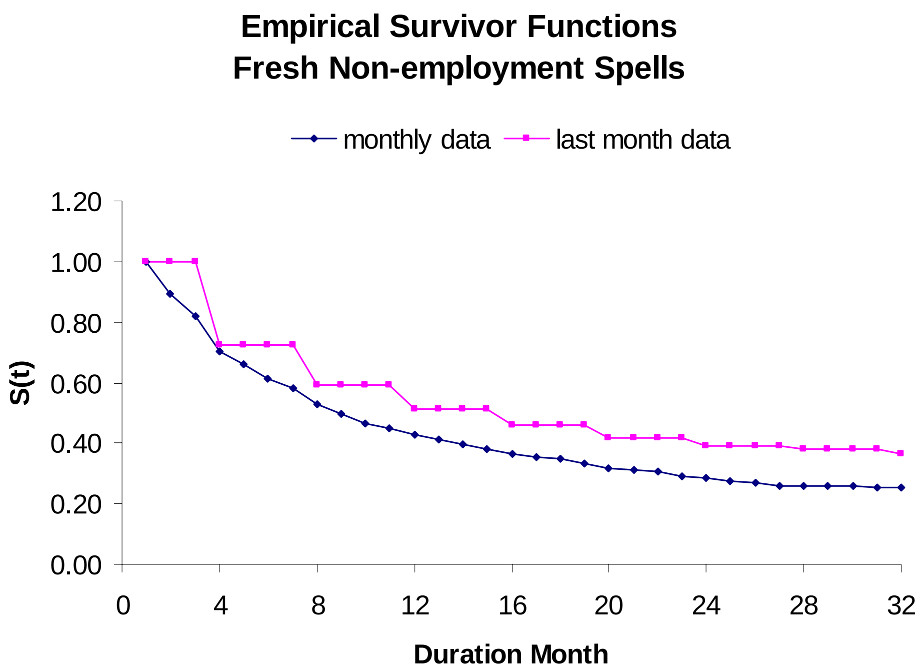
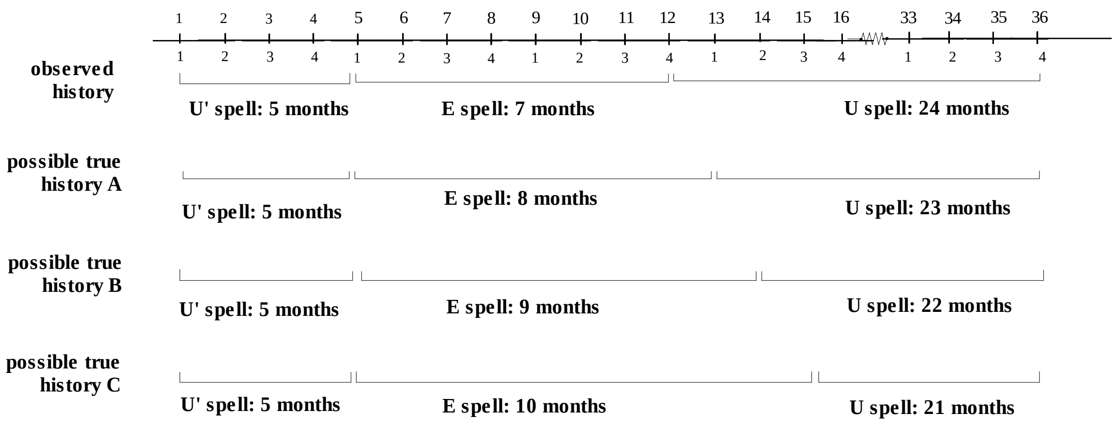

Empirical Survivor Functions
Fresh Employment Spells
monthly data
last month data
1.20
1.00
0.80
0.60
0.40
0.20
0.00
S(t)
0
4
8
12
16
20
24
28
Duration Month

Figure 2.4



Empirical Survivor Functions
Fresh Non-employment Spells
monthly data
last month data
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



1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 33 34 35 36
1 2 3 4 1 2 3 4 1 2 3 4 1 2 3 4 1 2 3 4
observed history
U' spell: 5 months
E spell: 7 months
U spell: 24 months
possible true history A
U' spell: 5 months
E spell: 8 months
U spell: 23 months
possible true history B
U' spell: 5 months
E spell: 9 months
U spell: 22 months
possible true history C
U' spell: 5 months
E spell: 10 months
U spell: 21 months