The EITC, Tax Refunds, and Unemployment Spells

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Abstract

The Earned Income Tax Credit generates large average tax refunds for low-income parents, and these refunds are distributed in a narrow time frame. I rely on this plausibly exogenous source of variation in liquidity to investigate the effect of cash-on-hand on unemployment duration. Among EITC-eligible women, unemployment spells beginning just after tax refund receipt last longer than unemployment spells beginning at other times of year. There is no evidence that tax refund receipt is associated with longer unemployment duration for men, or that the longer durations for women are associated with higher-quality subsequent job matches.

The Earned Income Tax Credit (EITC) has grown to be an important part of the safety net for low-income families. In 2003, EITC-recipient households received payments averaging $150 per family per month, while the average TANF-recipient household received $140 in monthly welfare benefits and the average food stamp participant got benefits of $80 per month (Congressional Budget Office 2005). Although most transfer programs deliver benefits in periodic payments spread over time, EITC payments typically arrive in one annual lump sum. Beverly, Schneider, and Tufano (2006) point out that for many EITC recipients, their federal tax refund will be the single largest payment received during the year. As a result, EITC recipients tend to have temporarily high levels of liquid assets just after tax refunds are distributed. Building on recent evidence showing that higher levels of liquid assets are associated with longer unemployment spells, this paper uses refund-related variation to investigate the sensitivity of unemployment duration to cash-on-hand among low-income parents.

The distinctive pattern of EITC-related tax refunds received by low-income parents provides the foundation for my empirical strategy. Low income families with children receive tax refunds that are large relative to their annual income. Averaged over 1993 to 2007, filers with children and with income in the EITC range received refunds equal to 30% of their annual adjusted gross income. Payment of tax refunds is more temporally concentrated for low-income filers than for the population as a whole. In recent years more than half of EITC payments have

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been made in the month of February. This pattern of large lump sum payments delivered in a narrow window of time generates plausibly exogenous variation in cash-on-hand across different calendar months.

Using data from the Survey of Income and Program Participation (SIPP), I compare demographically similar EITC-eligible individuals who enter unemployment at different times. Individuals who become unemployed in February will typically receive tax refunds shortly after their entry into unemployment. Those who become unemployed in, say, July or August will generally not receive tax refunds during the first several months of unemployment. I estimate hazard models of re-employment, controlling for the month of entry into unemployment. I find evidence that unemployment spells beginning around the time of tax refund receipt are longer, but only among women. Among mothers with low levels of income and education, unemployment spells that begin in February have about a 26% lower hazard rate of re-employment than do spells beginning at other times. This corresponds to spells that are about four weeks longer on average. Despite this longer period of job search, I find no evidence that subsequent jobs are of higher quality. Unemployment spells beginning in February are not associated with greater pre- to post-unemployment wage gains or with other measures of job quality.

Relying on seasonal variation raises the concern that it is something else about February, rather than higher levels of cash-on-hand associated with tax refunds, generating longer unemployment durations. I address this concern in two ways. First, I use variation in the predicted size of EITC payments. Entering an unemployment spell in February has a more negative effect on the re-employment hazard rate for individuals eligible for larger EITC payments. Second, I consider three groups who are similar to my primary sample in certain ways but who receive smaller average tax refunds—parents with income somewhat above the EITC range, low-income individuals without children, and low-income parents observed in earlier years when the EITC was less generous. In these groups, beginning an unemployment spell in February is not associated with longer unemployment duration.

Understanding transitions out of unemployment for low-income parents with low levels of education is particularly important for three reasons. First, unemployment is prevalent among this group. When the unemployment rate was at a relatively high level of 9.3% in 2009, the unemployment rate was nearly twice as high (18.2%) for those with less than a high school diploma. Individuals with low levels of education have been disproportionately affected by recessions over the last three decades (Allegretto and Lynch 2010). Second, low levels of education and income prior to unemployment are associated with lower rates of unemployment insurance (UI) receipt during an unemployment spell. Only about 20% of the unemployment spells in my sample involve receipt of UI benefits. As there is evidence that UI helps to smooth consumption during an unemployment spell (Gruber 1997, Bloemen and Stancanelli 2005), the unemployment spells considered here are likely associated with large relative declines in consumption and potentially large welfare losses. Third, the unemployment of a parent can have negative effects on his or her children. Recent evidence on short-run consequences of fathers' job losses indicates a negative effect on babies' birth weights (Lindo forthcoming) and on children's secondary-school grade point averages (Rege et al. forthcoming). Maternal job loss is associated with more frequent problem behaviors in the classroom among low-income preschool children (Hill et al. 2011), and job losses of a household head are associated with an increased probability that a child repeats a grade (Stevens and Schaller 2011).

This paper proceeds as follows. Section 1 formalizes the relationship between cash-on-hand and job search behavior, and describes existing evidence on the nature of this relationship. Section 2 documents three facts about the tax refunds of EITC recipients that are critical for my empirical strategy: Tax refunds are large, they arrive in a well-defined and narrow time frame, and the money is spent down quickly. Section 3 outlines my empirical strategy, section 4 describes the SIPP data I use, and section 5 presents results and discussion. Section 6 concludes.

1 Literature Review

In this section I briefly describe a theoretical model that has been used to explain how cash-on-hand can affect job search behavior. The key prediction of the model is that an increase in wealth reduces job search effort. Adapting this prediction to the case of tax refunds, search effort is predicted to be lower just after a tax refund is received. In the remainder of this section I review existing empirical evidence on the relationship between cash-on-hand and unemployment, and on other behaviors affected by receipt of infrequent cash payments.

1.1 A Model of Cash-on-Hand and Job Search

Lentz and Tranaes (2005) develop a model in which individuals move between employment and unemployment, jointly choosing job search effort and savings. With the assumption that utility is additively separable in consumption and search effort, their model generates the prediction that search effort declines as wealth increases. This model has been adapted and used by Card, Chetty, and Weber (2007) and Chetty (2008) in their studies of cash-on-hand and labor market behavior. I borrow liberally from these papers in the following description.

Consider an individual who becomes unemployed at time $t = 0$. He chooses job search intensity s_t, normalized so that s_t is equal to the probability of finding a job in period t. Searching has a cost of $\psi(s_t)$, assumed to be increasing and convex. If the individual finds a job, he starts work immediately and earns an exogenously fixed wage of w in that period. If employed in period t, his consumption is c_t^e. If he does not find a job, he receives a benefit of b from the unemployment insurance system and his consumption is c_t^u. His flow utility in period t is $u(c_t) - \psi(s_t)$. This individual has a subjective discount rate of δ and faces an interest rate of r. Let A_t denote the value of assets held at the beginning of period t.

If an individual has a job at time t, his value function conditional on having assets A_t at the beginning of the period is

$$V_t(A_t) = \max_{A_{t+1} \geq L} u\left(A_t - \frac{A_{t+1}}{1+r} + w\right) + \frac{1}{1+\delta} V_{t+1}(A_{t+1}) \tag{1}$$

where L is a lower bound on assets, consistent with facing a borrowing constraint. If a person has not found a job as of time t, his value function is

$$U_t(A_t) = \max_{A_{t+1} \geq L} u\left(A_t - \frac{A_{t+1}}{1+r} + b\right) + \frac{1}{1+\delta} J_{t+1}(A_{t+1}). \tag{2}$$

Here J_t is the expected value of entering period t without a job, defined as

$$J_t(A_t) = \max_{s_t} s_t \cdot V_t(A_t) + (1 - s_t) \cdot U_t(A_t) - \psi(s_t). \tag{3}$$

A person who is unemployed chooses search effort to maximize his expected utility. This yields the first order condition

$$\psi'(s_t^*) = V_t(A_t) - U_t(A_t). \tag{4}$$

Intuitively, a person exerts effort just until the marginal cost of search is equal to the marginal benefit of search, the difference between utility in the employed and unemployed states.

This first order condition can be differentiated to show the effect of changes in wealth on search effort. Doing so yields

$$\frac{\partial s_t^*}{\partial A_t} = \frac{u'(c_t^e) - u'(c_t^u)}{\psi''(s_t)}. \tag{5}$$

Lentz and Tranaes show that, with the assumption that utility is additively separable in consumption and search effort, the numerator of this term will be negative. Thus, search effort falls as wealth increases. This model can easily be applied to the case of tax refund receipt. I argue that the concentrated disbursement of EITC-related refunds in February generates temporarily higher values of A_t. I test whether search intensity is lower at this time, as measured by lower hazards of exiting from unemployment and longer unemployment durations.

1.2 Previous Empirical Evidence

The prediction that higher levels of liquid assets are associated with longer unemployment spells has been tested by two earlier papers. Card, Chetty, and Weber (2007) take advantage of a sharp discontinuity in eligibility for government-financed severance pay in Austria. Individuals who have been working for 36 consecutive months prior to job loss are eligible for a lump-sum severance payment equal to two months of pre-tax income. Individuals with slightly shorter job tenure are ineligible for severance pay. The authors find that the hazard rate of finding a new job in the first twenty weeks of unemployment is 8 to 12% lower for those who are just barely eligible for severance pay relative to those just barely ineligible.

Chetty (2008) uses two datasets to document the role of cash-on-hand in determining unemployment duration. Using data from the SIPP, he shows that the well-established positive relationship between state UI generosity and unemployment duration is much stronger in households with low cash-on-hand, as measured by net liquid wealth. Using a survey of job losers, he finds that recipients of severance payments have substantially longer average unemployment spells. Chetty uses this evidence to decompose the overall effect of UI generosity on unemployment spell length into a moral hazard effect and a liquidity effect. The moral hazard effect occurs when UI benefits lower an individual's private marginal cost of leisure to a level below the social marginal cost of leisure, and the individual chooses an unemployment duration longer than what is socially optimal. The liquidity effect can occur when borrowing constraints prevent an individual from perfectly smoothing consumption over a period of unemployment. If UI lengthens an unemployment spell by relaxing a borrowing constraint, the longer unemployment duration represents a socially beneficial response. Chetty finds that the non-distorting liquidity effect accounts for about 60% of the relationship between UI generosity and spell length.

Chetty acknowledges that variation in A_t stemming from either receipt of a severance payment or from differences in net liquid wealth is likely endogenous to unobserved individual characteristics, some of which may also affect unemployment duration. For example, individuals with high levels of impatience may accumulate lower net wealth and have also been shown to exert less search effort during an unemployment spell, leading to lower unemployment exit rates (DellaVigna and Paserman 2005). My paper adds to the existing literature by using variation in cash-on-hand that is plausibly exogenous to unobserved characteristics.

Examining the responsiveness of job search behavior to changes in cash-on-hand builds on the very large literature testing the permanent income hypothesis. Under this hypothesis, the arrival of an anticipated and transitory lump sum should not change an individual's level of consumption. Japelli and Pistaferri (2010) review papers testing this prediction. While the resulting estimates range over a wide spectrum, those that make use of tax-related changes in income typically find a substantial consumption response. Souleles (1999) relies on a distinctive temporal pattern of tax refund receipt similar to what I use in this paper. Using data from the Consumer Expenditure Survey (CEX) spanning years 1980 to 1991, Souleles estimates that between 34 and 64 cents of each dollar of tax refund is spent within a quarter, with the consumption response concentrated on durable goods.[1] This finding has an important

[1]Hsieh (2003) finds that residents of Alaska, who receive large and predictable annual payments from the Alaska Permanent Fund, do not adjust their consumption upon receipt of such payments. In contrast, the same households do display excess sensitivity of consumption upon receiving income tax refunds. Hsieh argues that the greater consumption response out of tax refunds may be due to the smaller size of these payments, and hence the lower utility cost associated with failing to smooth.

implication for my analysis. If refunds are spent down slowly, then an EITC recipient who loses her job two or three months after receiving a refund may have nearly as much cash on hand at the time of job loss as an EITC recipient who loses her job in February. The substantial MPC found by Souleles suggests instead that an individual who begins unemployment a few months after refund receipt will have already spent most of her refund.

There are several closely related papers that study the consumption response to one-time tax rebate programs designed to provide fiscal stimulus. Johnson, Parker, and Souleles (2006) find that the average household spent 20 to 40% of its 2001 tax rebate on non-durable goods in the 3-month period in which the payment arrived. Households with incomes in the bottom third of the distribution, earning less than about $34,000, spent down their rebates much more quickly. They spent about 76% of the rebate on non-durables in the 3-month period in which the rebate was received. Parker et al. (2011) perform a similar analysis of the 2008 tax rebates, finding that the average household spent between 50 and 90% of the rebate within a 3-month period. Agarwal, Liu, and Souleles (2007) use panel data on credit card accounts to study responses to the 2001 rebates. They find that the average credit card holder paid down debt shortly after rebate receipt but eventually increased spending. Nine months after receiving a rebate, credit card spending had increased by over 40% of the typical rebate amount.

Other research has focused more specifically on how the consumption of low-income families responds to tax refunds. Barrow and McGranahan (2000) use CEX data from 1982 to 1996 to investigate seasonal patterns of consumption among low-income individuals. They find that, in the month of February, EITC-eligible households spend about 3 percent more overall and about 9 percent more on durable goods than do non-EITC-eligible households. Adams, Einav, and Levin (2009) use data from an auto company on the loan applications of low-income individuals with poor credit histories. Among low income filers with two or more dependents, precisely the group receiving large EITC payments, the number of loan applications is twice as high in February as in other months. The number of new car purchases is about three times as high in February as in other months.

Behaviors other than consumption are also affected by the timing of income receipt. Dobkin and Puller (2007) show that drug-related hospital admissions increase at the beginning of each month. Using administrative data from California, they link patient records with information on government transfer payments. They find that the within-month cycle of drug admissions is concentrated among SSI and DI recipients, who receive their payments on the first and third days of the month. Foley (2011) investigates the relationship between welfare payments and crime. In cities where welfare payments are concentrated at the beginning of the month, crimes with some financial benefit (such as burglary and motor vehicle theft) are relatively less common in the first ten days of the month and increase later in the month. This pattern is not evident in cities with more staggered welfare payment schedules, or for crimes without an obvious financial motivation. Evans and Moore (forthcoming) find that death rates are about one percent above average in the first few days of a calendar month, and about one percent below average in the last week of a month. This monthly cycle in mortality is stronger among individuals with low levels of education, who are more likely to be credit-constrained. If cash-on-hand can affect the quite irreversible outcome of mortality, it seems plausible that it may affect lower-stakes outcomes such as job search behavior.

2 Tax Refunds and Cash-on-Hand

The empirical strategy I employ in this paper depends on tax refunds generating substantial and systematic differences in cash-on-hand across different months of the year. In this section I document three key facts about EITC recipients that motivate my empirical strategy. First, I show that EITC recipients receive tax refunds that are quite large relative to their annual income. In terms of the model of section 1.1, a large tax refund generates large variation in A_t. Second, I show that the refunds of EITC recipients are disbursed in a narrow and well-defined window of time. Third, I argue that EITC recipients spend down their refunds quickly. The second and third facts allow me to characterize the month of February as a time of temporarily high A_t relative to other months of the year.

2.1 Refunds are Large for EITC Recipients

Filers with earnings in the EITC range receive larger refunds than filers with slightly higher earnings, and low-income filers with children receive substantially larger refunds than do low-income filers without children. This reflects both a higher propensity to receive a refund at all and a larger dollar value conditional on refund receipt. Figure 1 documents this pattern using data from the 1993-2007 Statistics of Income cross-sectional samples of tax returns.[2] The sample is restricted to non-dependent filers with real adjusted gross income (AGI) between \$0 and \$33,000, measured in real 2007 dollars. This matches the income cutoff I later apply to my SIPP sample. On average, 91% of low-income filers with children receive a refund. In contrast, only 69% of low-income filers without children receive refunds. Averaging across those who receive a refund and those with a balance due, the mean real refund amount for filers with children steadily grows from \$1810 in 1993 to \$3582 in 2007. These dollar amounts include refundable EITC payments, any other refundable tax credits, and refunds of overwithheld taxes. On average the tax refund amount is equal to 30% of AGI for these filers, equivalent to roughly three and a half months of income. Figure 1 indicates that low-income filers without children receive much smaller refunds in all years. The gap between the average refund for filers with and without children is never less than \$1000, and averages \$2042 over the 15-year period.

[2] I am grateful to Laura Kawano of the Treasury Department's Office of Tax Analysis for providing these tabulations.

2.2 Most EITC Payments are Distributed in February

My empirical strategy assumes not only that low-income filers with children receive refunds that are large relative to their annual incomes, but that these refunds are distributed within a narrow window of time. Evidence of this pattern comes from various *Monthly Treasury Statements* published by the Treasury Department's Financial Management Service.[3] Figure 2 shows the share of annual refund payments made in each month of the year, averaging across years 1998 through 2007. Pooling refunds paid to filers of any income level, approximately 19% of all refund payments are made in February, 23% in each of March and April, and 17% in May. The pattern of payments is even more concentrated, and shifted somewhat earlier in the year, for returns that include a refundable EITC payment. About 54% of refundable EITC payments are made in February and 25% are made in March.[4]

The share of refund payments made in February has been increasing over the time period I consider, particularly for EITC returns. February's share of refundable EITC payments has increased from 46% in 1998 to 58% in 2007. Why are EITC-related refunds paid so early in the year? One explanation is that, regardless of income level, filers receiving a refund tend to file earlier than those with a balance due (Slemrod et al. 1997). A second explanation more specific to the EITC is that e-filing is associated with an earlier refund payment, and EITC returns have very high rates of e-filing. Kopczuk and Pop-Eleches (2007) show that even as early as 1999, 54% of EITC-claiming returns were e-filed. In contrast, the IRS Oversight Board (2008) shows that the national average e-filing rate in 1999 was around 25%.

Figure 2 documents the timing of IRS disbursements, but filers can receive cash a few weeks earlier through the use of a refund anticipation loan (RAL). A RAL is a financial product similar to a payday loan, and is widely offered by paid tax preparers. Berube et al. (2002) find that 39% of EITC recipients used a RAL in 1999, and that 47% of all EITC dollars were distributed through RALs. For a filer who otherwise would have used direct deposit, a RAL reduces the time between filing and refund receipt by about two weeks. For a filer who otherwise would have received a check in the mail, a RAL reduces wait time by about six weeks.

There are mechanisms through which a refund recipient could spread after-tax income more smoothly across the year. An EITC recipient could take up the Advance EITC option, and any filer can adjust the level of taxes withheld from her paycheck. Either of these options involves submitting paperwork to an employer. In practice, these options are very rarely used. Jones (2010a) shows that experimentally providing more information about the Advance EITC, simplifying the application process, and requiring employees to make an active decision to either opt in or opt out of the program increased Advance EITC participation rates by only a very

[3]Reports are available at http://www.fms.treas.gov/mts/backissues.html.

[4]The entirety of a person's refund is disbursed at one time, regardless of whether the funds represent an EITC payment or a refund of overwithholding. Thus, while I do not have information on the temporal pattern of all tax refunds made to EITC recipients, the pattern of refundable EITC payments is a very good proxy.

small amount, from 0.3 to 1.2 percentage points. Jones (2010b) investigates the extent to which taxpayers adjusted their withholding in response to the 1990s expansions in the EITC. He finds a very precisely estimated zero adjustment, and can rule out that EITC-eligible taxpayers adjust their withholding by more than 2 cents in response to a $1 increase in the EITC benefit level.

2.3 EITC Recipients Spend Refunds Quickly

Shefrin and Thaler (1988) posit that the marginal propensity to consume out of a large lump sum payment will be lower than the MPC out of an equivalent stream of smaller, periodic payments. If so, receipt of a large tax refund may facilitate saving among low-income households. Indeed, EITC recipients often report a desire to channel a portion of their refunds to savings. Smeeding, Ross, and O'Connor (2000) analyze the results of in-depth interviews with 650 EITC recipients from the Chicago area. About a third (32.5%) of recipients intend to save part of their refund. At the same time, it is quite common for recipients to anticipate spending a portion of the funds on short-term expenses. 36.8% of respondents planned to spend part of their refund on utilities, 34.0% intended to spend part on rent, and 20.8% planned to spend part on food. Beverly, Schneider, and Tufano (2006) surveyed low-income filers in Oklahoma who were given the opportunity to directly deposit part of their tax refund into a savings account and to receive a check for the remainder. About 27% expressed interest in participating in the refund-splitting program. However, a much smaller share (15%) actually used direct deposit to divert part of the refund into a savings account. Bronchetti et al. (2011) run a field experiment in which filers have the option of allocating part of their tax refund to buying U.S. savings bonds. They investigate the sensitivity of this decision to changes in the default. They find that very few low-income filers, about 9% of the sample, use part of their tax refund to buy savings bonds. In an interesting contrast to earlier work on default rules, they find that changing the default option has virtually no effect on this savings decision. This evidence on the EITC and savings, along with estimates of the short-term consumption response to refund receipt described in section 1.2, suggests that EITC payments are spent down fairly quickly. Thus, an individual who enters unemployment a few months after receiving a tax refund is unlikely to have much of that refund payment still tucked away.

It is likely that EITC recipients quickly spend down their refund payments because they face severe credit constraints. One piece of evidence consistent with this explanation is the willingness of EITC recipients to borrow at the high effective interest rates implicit in RAL pricing. Berube et al. (2002) estimate an annualized interest rate of 250% for a typical RAL purchased in the Washington D.C. area. Athreya, Reilly, and Simpson (2010) compare measures of credit constraints for EITC-eligible households and for all other households in the 2007 Survey of Consumer Finances. EITC-eligible households are about twice as likely to report being 60 days or more late with a debt payment (11.2% vs. 5.4%), four times as likely to report having no checking account (28% vs. 7%) and more likely to have been denied a checking account because of a poor credit history (2.3% vs. 0.5%).

3 Estimation Strategy

In order to test the hypothesis that unemployment spells beginning shortly after refund receipt are longer than unemployment spells beginning at other times of year, I estimate the hazard of exiting from an unemployment spell into a new job. Specifically, I estimate Cox proportional hazard models of the following form:

$$log(h_{it}) = \beta_1 Feb\ Start_i + \beta_2 WBA_i + \gamma X_{it} + \epsilon_{it} \tag{6}$$

where h is the hazard rate and *Feb Start* is a dummy equal to one if an unemployment spell begins in February. If in fact the extra cash-on-hand generated by tax refunds reduces job search effort as predicted by theory, the coefficient β_1 will be negative. The variable WBA_i represents the weekly benefit amount an individual can receive from his state's UI program. The vector X includes measures of age, race, marital status, number of children, pre-unemployment wage and job tenure, net liquid wealth, the monthly state unemployment rate, and a dummy for being on the seam between SIPP interviews.[5] In my preferred specifications I include fixed effects for state of residence, calendar month and year, and pre-unemployment industry. The calendar month variables control for other, non-tax-related seasonal patterns in unemployment duration. I estimate the above equation for a sample of individuals likely to experience particularly large tax-refund-related variation in cash-on-hand—parents with low incomes and with no more than a high school degree.

My empirical strategy does not require individual-level information about the amount of a person's tax refund or the exact time at which she receives it. While this information would be useful, it is also endogenous to behavior that is plausibly correlated with determinants of job search effort, including observable variables such as labor income and unobservable variables such as impatience. The exact amount of one's refund depends on income and taxes withheld throughout the year. The timing of refund receipt depends largely on when a person files.

As figure 2 shows, the dollar value of EITC-related refund payments is greater in February than in any other month, but is also quite high in March. In some specifications I replace the indicator for beginning an unemployment spell in February with an indicator for entering unemployment in either February or March.

[5] In a given SIPP interview, respondents report a number of variables at monthly frequency, corresponding to each of the last four months. The last month covered by an interview is considered to be "on the seam." There is a strong tendency for individuals to report the same value for each of the months covered by an interview. Thus, changes within the reference period are smoothed out, changes between interviews are exaggerated, and transitions of all sorts, including out of unemployment, are particularly high for observations on the seam.

There is a large body of literature, reviewed by Krueger and Meyer (2002), establishing that individuals receiving more generous UI benefits have longer unemployment durations. This motivates the inclusion of WBA_i, a measure of the UI weekly benefit amount potentially available to an individual. In most specifications, I use the weekly benefit amount that an individual could receive based on her state of residence and earnings history. As an alternative I use the average weekly benefit amount disbursed in a state within a given year. Details on state UI programs come from the Employment and Training Administration of the U.S. Department of Labor. When analyzing the behavior of low-income individuals, these measures of benefit generosity are preferable to the maximum weekly benefit amount, which has often been used in the UI literature. While different maximum values do account for a substantial amount of the cross-state heterogeneity in benefit generosity, EITC recipients are generally earning too little to qualify for the maximum benefit.

I do not control for the potential duration of UI benefit receipt, because there is very little variation in this parameter over the time period I consider. Almost all state UI benefit programs cap receipt at 26 weeks. The exceptions are Massachusetts (where the maximum duration of benefit receipt is 30 weeks in all years of my analysis), Montana (28 weeks beginning in 2004), and Washington (30 weeks prior to 2005). If a state's insured unemployment rate is above some threshold, a resident of that state can claim up to 13 weeks of extended benefits, funded jointly by the federal and state government, after exhausting the state-only benefits. The greatest variation in benefit duration during my analysis period comes from the Temporary Extended Unemployment Compensation Act, in effect from March of 2002 until March of 2004. Details on this program come from the Congressional Budget Office (2004). Under this policy, workers in all states could receive up to 13 weeks of federally-funded TEUC benefits after exhausting the state component. If workers in high-unemployment states exhausted the TEUC benefits, they could receive yet another 13 weeks of benefits, known as TEUC-X benefits. This variation in potential benefit duration, concentrated in a few years, will be absorbed by the year fixed effects.

Including state fixed effects mitigates concerns that other state-specific attributes or policies affect seasonal variation in unemployment duration. One such policy is the payroll tax states levy on employers to fund UI programs. These taxes are partially experience rated. A firm's marginal cost of laying off a worker is generally increasing with the firm's layoff rate, but the firm's cost of layoffs rises less quickly than the benefits paid to the firm's former employees. The degree of experience rating differs across states, and less complete experience rating has been shown to increase the rate of temporary layoffs.[6]

[6]Card and Levine (1994) show that imperfect experience rating increases rates of temporary unemployment more during times of low demand than during expansionary times. This is true regardless of whether low demand is attributable to a trough in the business cycle or to seasonal fluctuations within an industry. The implication of this for my analysis is that if February is a generally low-demand month, imperfect experience rating will result in more temporary layoffs at that time. This could result in longer duration for spells beginning

Longer durations of unemployment may be desirable if additional search time leads to higher-quality eventual matches. Previous research on whether longer unemployment durations are associated with better subsequent jobs has yielded mixed results. Addison and Blackburn (2000) find no evidence that, among UI recipients, more generous UI benefits are associated with a larger gain in wages. They find limited evidence that UI recipients have larger wage gains than non-recipients. Centeno (2004) finds that, for men, more generous state UI benefits are associated with longer post-unemployment job tenure. This relationship is stronger when unemployment rates are high. Card, Chetty, and Weber (2007) find that Austrian workers who are just eligible for severance pay or extended UI benefits do not have greater wage gains or longer duration on the next job, despite having longer spells of unemployment.

I test whether the wage gains associated with re-employment are higher for those who enter unemployment in February. I estimate OLS regressions in which the dependent variable is wage growth, defined as:

$$\text{Wage Growth}_i = log[\text{Post-Unemp Wage}_i] - log[\text{Pre-Unemp Wage}_i] \tag{7}$$

The controls used in these equations include an indicator for beginning an unemployment spell in February as well as most of the demographic controls included in the hazard models. I do not include the pre-unemployment wage as a control in these regressions. I also do not control for WBA_i in these regressions, as an individual's potential benefit amount is highly correlated with his pre-unemployment wage. I also investigate whether beginning an unemployment spell in February is associated with two other proxies for better job quality, being paid a salary rather than being paid on an hourly basis and working full-time rather than part-time. I measure pre-unemployment job characteristics in the last full calendar month preceding entry into unemployment and post-unemployment job characteristics in the first full month following re-employment.

4 SIPP Data

I use data from the 1993, 1996, 2001, and 2004 panels of the SIPP. Each of these SIPP panels is a longitudinal survey that follows respondents for up to three years (1993 and 2001 panels) or four years (1996 and 2004 panels). Interviews take place every four months. Respondents report weekly labor force status, allowing precise measurement of the times at which a person enters and exits a spell of unemployment.

My definition of unemployment spells follows earlier work such as Cullen and Gruber (2000) and Chetty (2008). An unemployment spell begins with a transition from having a job (either

in February for reasons unrelated to tax refund receipt. Excluding spells of temporary unemployment from my sample reduces this concern. Card and Levine find a much smaller relationship between the degree of experience rating and the unemployment rate excluding temporary layoffs.

working or temporarily absent without pay) to having no job. A person is considered to remain in a spell of unemployment until she reports having a job in which she subsequently works for at least four consecutive weeks. I drop unemployment spells that correspond to a temporary layoff and spells in which there is no active search for a new job. To focus on individuals with some demonstrated attachment to the labor force, I restrict the sample to individuals with at least twelve weeks of work history prior to the start of their first observed unemployment spell. To minimize the number of unemployment spells that end in a decision to retire, I restrict the sample to individuals ages 20 to 64.[7] As is common in this literature, I restrict the sample to unemployment spells lasting no more than one year. My sample includes spells of unemployment beginning in calendar years 1993 through 2007.

To construct a sample of EITC-eligible individuals, I sum earnings from the three calendar months preceding the month of entry into unemployment. I restrict the sample to those whose combined own and spouse's 3-month earnings are greater than zero and less than $8250, measured in real 2007 dollars. Scaled up to annual earnings of $33,000, this roughly corresponds to the top of the EITC-eligible income range for a family with one child in each year of my analysis.[8] I further restrict the sample to parents. My definition of a parent reflects the conditions under which a person can claim an EITC-qualifying child. I consider a person to be a parent if, in at least 6 months out of the preceding year, she was living with one or more of her own children under age 19. Finally, I restrict the sample to individuals whose highest level of completed education is a high school degree or less.

These sample restrictions result in a set of 4181 unemployment spells, 1717 experienced by men and 2464 by women. It is not uncommon for an individual to experience multiple spells of unemployment meeting the selection criteria.[9] Each of these spells is counted as a separate observation, and standard errors in all regressions are clustered at the person level.

Table 1 presents descriptive statistics for my sample, comparing unemployment spells beginning in February to spells beginning in other months.[10] Columns 1 and 2 show information for women and columns 4 and 5 are for men. By construction, this is a low-income sample. On

[7]Chan and Stevens (2001) find that only 70-75% of displaced workers in their 50s return to work within two years, and even fewer displaced workers in their 60s return. My results are robust to lowering the age cutoff to 59, 54, or 49.

[8]Scaling up three months of earnings yields a reasonable approximation of annual earnings. For people with 12 months of observed pre-unemployment earnings, and who meet all of my sample criteria other than the earnings restriction, 57% are income-eligible for my sample using actual 12-month earnings or using 3-month earnings multiplied by four. 32% are ineligible for my sample using either income measure, and 7% would be eligible based on 12-month earnings but are ineligible using the scaled up 3-month earnings measure.

[9]There are 3294 unique individuals in my sample, with 21% experiencing multiple included unemployment spells. Stevens (1997) analyzes the effect of multiple job losses on the earnings profiles of displaced workers, using PSID data from 1968-88. In her sample of displaced household heads, 41% experienced multiple job displacements, and the probability of a subsequent displacement was 10-12% in the two years following the first displacement.

[10]Grouping together unemployment spells beginning in February and March and comparing them to unemployment spells beginning in the remaining ten months generates very similar patterns. Results are available upon request.

average, women in the sample earned about \$3060 in the three months prior to unemployment and had an imputed hourly wage of \$8.02.[11] Men had average 3-month earnings of approximately \$4040 and imputed hourly wages of \$9.41. On average, individuals in my sample are eligible for about \$140 in weekly UI benefits, measured in real 2007 dollars.[12] This is lower than state-level average weekly benefit amounts, as expected for a low-income sample. Only about 22% of the unemployment spells in my sample involve receipt of UI benefits, and this value is similar across groups entering unemployment at different times of year. This figure is consistent with other estimates of UI takeup. The CBO (2004) shows that in recent years about 40% of all unemployed individuals collect UI benefits, and Levine (2005) documents a growing gap between the UI take-up rates of high-skilled and low-skilled individuals. Using 2003 CPS data, Levine finds a UI recipiency rate of 21% among unemployed individuals with less than a high school degree, relative to a UI recipiency rate of 35% among unemployed individuals with higher levels of education. Importantly for my empirical strategy, none of the income-related variables have statistically different means for February unemployment entrants and for others.

Not surprisingly, wealth levels are also low for this sample. Net liquid wealth is defined as total wealth minus home equity, business equity, vehicle equity, and unsecured debt. Asset and wealth variables are collected in periodic topical modules. The number of times each topical module is included varies across SIPP panels, from once in the 1993 panel to four times in the 1996 panel. In the case of multiple wealth observations, I use the measure that most closely pre-dates entry into unemployment. If there is no pre-unemployment measure available, I use the earliest observation following entry into unemployment. The infrequent collection of wealth data means that the available values for February unemployment entrants are not necessarily measured in February, and these wealth data are ill-suited for verifying that the liquid assets

[11] I impute hourly wage for all workers, because self-reported hourly wage is available only for those who are paid on an hourly basis. I use data from the last full calendar month before entry into unemployment in this calculation. I divide the monthly earnings associated with a particular job by the number of hours worked in that month and job. Each respondent can report earnings and hours information for up to two jobs per wave. I use data from job 1 in most cases, and use data from job 2 only if the job 1 calculation produces a zero or missing value. Workers report the typical number of hours worked per week, which I multiply by four to estimate monthly hours of work. (As an alternative I have used the actual number of weeks per month, which can be either four or five. However, using actual weeks per month produces larger mean differences between imputed and self-reported hourly wages for the approximately 84% of my sample paid hourly. It appears that most workers are assuming a 4-week month when they report monthly earnings from a job.) This calculation produces a handful of extremely high imputed hourly wage rates. To reduce the influence of outliers, I have topcoded imputed hourly wage at the 99th percentile of the wage distribution in each panel. Topcoding instead at the maximum allowed value of self-reported hourly wage has virtually no effect on the results.

[12] I have calculated *WBA* using earnings from the three months prior to unemployment entry, multiplied by four to approximate annual earnings. Most states use information on quarterly earnings from the first four of the five quarters before UI application to compute benefits. To check the accuracy of using scaled-up quarterly earnings to approximate annual earnings, I compute an alternative *WBA* amount for individuals observed for 12 months prior to unemployment entry. This alternative *WBA* calculation makes use of a full year of earnings history. For the 2629 spells with the necessary data, the initial *WBA* calculation yields a mean benefit amount of \$143 and the alternative calculation yields a mean benefit of \$166. The correlation between the two *WBA* amounts is 0.647.

of EITC recipients are higher in February than in other months. In fact net liquid wealth is only $1047 for women who enter unemployment in February and $3725 for women who enter unemployment in other months. The standard deviation is large, and this difference in means by month of unemployment entry is not significant. Median net wealth is zero for men and women entering unemployment in any month. These very low levels of net wealth are consistent with the view that this sample faces severe liquidity constraints.

It is possible that individuals entering unemployment in February are leaving short-term jobs associated with the holiday shopping season. If these individuals have generally lower levels of human capital than workers leaving more permanent jobs, they may have a longer average search time before reemployment. Restricting my sample to individuals who have worked for at least twelve weeks prior to their first observed unemployment spell makes it unlikely that holiday-season jobs are affecting the results. As shown in Table 1, the mean number of weeks worked prior to unemployment is not statistically different for women entering unemployment in February than for women entering unemployment at other times. Among men, unemployment spells beginning in February are preceded by *longer* working spells than are unemployment spells beginning in other months. It should be noted that the measure of pre-unemployment job tenure is left censored at the time a person first enters the SIPP.

My estimation strategy relies on the assumption that individuals who enter unemployment in February are similar to individuals who enter unemployment at other times of year, except for the fact that they receive tax refunds at approximately the start of their unemployment spells. The summary statistics in Table 1 provide some reassurance on this point, but it is important to rule out other possible differences between the two groups. It is quite plausible that both seasonal patterns of layoff and average unemployment duration differ across industries. Table 2 compares the pre-unemployment industry of sample members entering unemployment at different times of year. The industry mix is generally similar for people who begin unemployment spells around the time of tax refund receipt and for people who begin unemployment spells at other times, with February entrants somewhat less likely to have been employed in Administration. Overall, Table 2 suggests that longer unemployment durations among those entering unemployment in February are not a result of February entrants being disproportionately drawn from particular industries. Even so, I control for pre-unemployment industry in my preferred hazard model specification.

To further investigate the possibility that February entrants differ from others, I estimate linear probability models predicting that an unemployment spell begins in February rather than in some other month of the year. The lower the predictive power of these regressions, the more plausible the argument that recent tax refund receipt is responsible for any February effect on unemployment duration. The results of these regressions are shown in columns 3 and 6 of Table 1. In addition to the controls reported in the table, I include a set of year, state, month of entry into the SIPP, and industry fixed effects.[13] Reassuringly, demographics and income-related measures are very poor predictors of February unemployment entrance. However, among women, both the observed length of the pre-unemployment job and an indicator for whether that job tenure is censored are significant predictors of beginning an unemployment spell in February. The results are quite similar if I predict February unemployment entrances using a probit model rather than a linear probability model.

Before turning to results, I present one other piece of descriptive information. Figure 3 plots the average duration of unemployment spells for women in my sample, by month of entry into unemployment. Generally, spells beginning in the early part of the year last longer than spells beginning in the second half of the year. Spells beginning in February last longer than spells beginning in any other month of the year. A similar pattern appears if I restrict the sample to women included in the richest hazard models, those individuals with non-missing state of residence and wealth variables.

5 Results

5.1 Baseline Results

I first present graphical evidence on job-finding rates. Figures 4 and 5 plot Kaplan-Meier survival curves for men and women in my sample. Separate curves are plotted for individuals who begin their spells of unemployment in February (indicated by the more lightly shaded line) and for individuals who enter unemployment in some other month. Among women, those who enter unemployment in February have lower hazards of job-finding. The survival curve for February entrants is always above the survival curve for other entrants, showing that the probability of remaining in unemployment after t weeks is always higher for those who entered unemployment in February. Equality of the February and other-month survival curves for women is rejected with $p = 0.0049$. Among men, there is no evidence that the hazard of re-employment differs with the month of entry into unemployment.

Coefficients from hazard model estimates are shown in Tables 3 (women) and 4 (men). Columns 1 and 4 show results from the simplest hazard models, controlling only for the time of entry into unemployment and for being on the seam between interviews. Columns 2 and 5 add a set of demographic controls, the monthly state unemployment rate, and an individual's potential weekly UI benefit amount.[14] Columns 3 and 6 add year, month, state, and industry

[13] Because my sample selection rule requires 12 observed weeks of work prior to an unemployment spell, the month first observed in the SIPP affects the set of months in which any transition to unemployment can satisfy my sample criteria.

[14] The sample size falls when these controls are added because state of residence is missing for some observations. In the 1996 and 2001 panels, residents of Maine and Vermont are grouped together as are residents of North Dakota, South Dakota, and Wyoming. In the 1993 panel, there are three composite state categories. One includes Maine and Vermont, the second includes Iowa, North Dakota, and South Dakota, and the third

fixed effects. This is my preferred specification. Among women, there is evidence that spells beginning around the time of refund receipt last longer than spells beginning at other times of year. In all specifications, the hazard of re-employment is significantly lower for unemployment spells that begin in February. The *Feb Start* coefficient of -0.306 in column 3 indicates that the re-employment hazard rate of February entrants is $exp(-0.306) = 74\%$ of the re-employment hazard of those entering unemployment at other times of the year. The hazard of re-employment is about 13 percent lower for spells beginning in either February or March than for spells beginning at other times.

Table 4 shows no evidence that men's hazard of exiting from unemployment differs with month of entry. This is not simply because a smaller sample of low-income, low-education men with children generates less precise estimates. The size of the standard errors for men and women is similar, and the point estimates on the *Feb Start* term for men are quite close to zero. A specification that pools unemployment spells of men and women and that includes an interaction of the *Feb Start* and female variables confirms that the effect of starting an unemployment spell in February is statistically different for men and women.

It is perhaps not surprising that refund-related cash-on-hand appears to affect the search behavior of women only. Historically married women's labor supply has been more wage-elastic than men's (Blundell and MaCurdy 1999), although this gap narrowed substantially between 1980 and 2000 (Blau and Kahn 2007; Heim 2007). It seems plausible that women's job search effort would also be more sensitive to the level of cash-on-hand. To further investigate the difference, I have tried splitting the sample into groups of primary and secondary earners rather than into groups of men and women. I classify all unmarried individuals as primary earners. I classify a married individual as a primary earner if she earns more than her spouse in the three months prior to unemployment entry. I find that only among secondary earners are February unemployment entrances associated with longer spells.

I can separately identify the effect of *starting* an unemployment spell in February and of a particular week of a spell falling *within* the month of February, given that spells often persist beyond the month in which they start. I have done this by including a full set of calendar month dummies in the specifications shown in columns 3 and 6 of Tables 3 and 4. This helps to address concerns about non-tax-related seasonal patterns in re-employment hazard rates. December is a month of low re-employment hazards for women and the last quarter of the year is associated with lower re-employment hazards for men. The February coefficient is not significantly different from zero, for women or for men. This in combination with the *Feb Start* coefficient suggests that it is tax refund receipt at the beginning of an unemployment spell, rather than at any point during an unemployment spell, that has an important influence on spell length. One possible explanation for this pattern is that extra cash-on-hand may delay the

includes Alaska, Idaho, Montana, and Wyoming. Other observations with missing imputed hourly wage or missing wealth variables are also dropped.

commencement of active search at the beginning of an unemployment spell but that it is less likely to interrupt a period of active search already underway. This interpretation is supported by the rarity of an unemployment spell involving a pattern in which a week or more of looking for work is followed by a week or more of not working and not looking, followed in turn by a week or more of active search. Only 8.5% of women's unemployment spells and 6.1% of men's unemployment spells in my sample include this pattern. Spells that include at least one week in February are no more likely to include this pattern than are spells including at least one week in any other calendar month of the year. Next I consider the share of unemployment spells that include active search within the first week: 69% of female unemployment spells beginning in February and 76% of female unemployment spells beginning in other months. This comparison goes in the direction consistent with tax refund receipt delaying the start of active search, and the difference is significant at the 10% level.

The coefficients on other regressors are generally as expected. Among both men and women, unemployment spells are shorter for whites than for non-whites. Being married is associated with longer unemployment duration for women but has no effect on the unemployment duration of men. Being on the seam between interviews is always associated with a dramatically higher rate of exit from unemployment.[15] In some specifications for men, a more generous weekly benefit amount is associated with a higher hazard of exit from unemployment. This appears to contradict the conventional wisdom that more generous UI benefits result in longer spells of unemployment. However, Levine (1993) points out that if a more generous benefit level reduces the search intensity of UI recipients, it can shorten the unemployment duration of non-recipient searchers by essentially reducing their competition. This sort of spillover could be important in my sample in which only 22% of unemployment spells involve UI receipt.

5.2 Robustness Checks

By comparing unemployment entrances occurring in February to unemployment entrances occurring in all other months of the year taken together, the results in Tables 3 and 4 may miss other important seasonal variation in unemployment duration. I have estimated an alternative hazard model in which I include a set of 11 starting-month dummies, one for beginning an unemployment spell in each month of the year. (July is the omitted month.) The coefficients on these month dummies and the corresponding 95% confidence intervals, for women, are plotted in Figure 6. The job-finding hazard is lower for spells that begin in February than for spells beginning in any other month. One caveat about this figure is that because of the small number of observations in a given month, the precision of the estimated month coefficients is low. I have estimated a similar hazard model with 11 starting-month dummies for men. There is no evidence that beginning an unemployment spell around the time of refund receipt has

[15] The coefficients on this variable are similar to coefficients on a "last month of wave" dummy in unemployment duration models for disadvantaged single mothers estimated by Ham, Li, and Shore-Sheppard (2009).

any effect on the unemployment duration of men. None of the 11 starting-month coefficients is statistically different from zero.

While February is the modal month of refund receipt for EITC recipients, non-trivial EITC refund payments are made in January, March, and April. By essentially treating all unemployment entrances in months other than February as part of a control group, assumed to be unaffected by tax refund receipt, I may be biasing my estimates downwards. To address this point, I try dropping unemployment spells that begin in January, March, or April. In this case, I can be more confident that a comparison of February entrances to other entrances is a comparison of spells beginning with and without recent tax refund receipt. The results of this exercise are shown in columns 1 and 3 of Table 5. There is again evidence that beginning an unemployment spell around the time of refund receipt lengthens unemployment durations for women but not for men. For women, the coefficient of -0.361 indicates that the re-employment hazard for February entrants is only 70% of the re-employment hazard for other entrants.

To this point, I have restricted my sample to individuals who have no more than a high school education. Similar restrictions have been used in other papers studying EITC-eligible individuals, but are not part of the tax law determining EITC eligibility. In columns 2 and 5 of Table 5 I expand my sample to individuals of any education level, as long as they meet other selection criteria. This is still a low-income sample, as I maintain the selection criterion that real earnings (of the individual and his or her spouse) over the previous three months are not greater than $8250. In this specification, the *Feb Start* point estimate for women falls substantially, but is still statistically different from zero. The results for women indicate that not only are low levels of education associated with longer unemployment spells, but they are also associated with a greater sensitivity of duration to cash-on-hand. For men, evidence from the larger sample again indicates that beginning an unemployment spell around the time of tax refund receipt has no effect on unemployment duration.

If in fact the February effect on unemployment duration is driven by tax refund receipt, the effect should be larger for individuals receiving larger tax refunds. I use information on number of children and the earnings of sample members and their spouses to predict the size of EITC payments. I then add an interaction of the *Feb Start* dummy and the predicted EITC amount, measured in thousands of dollars, to investigate whether those eligible for larger EITC payments display a stronger seasonal pattern in unemployment duration. In these specifications I include individuals with and without children. Those without children are ineligible for the EITC before 1994, and are eligible for only a small EITC in later years. The mean predicted EITC value for childless women in the sample is $177, compared to a mean value of $2344 for women with children. The results in column 3 of Table 5 show that beginning an unemployment spell in February has an insignificant effect on duration among women ineligible for the EITC. The larger a woman's potential EITC, the more negative the effect of a February entrance on the re-employment hazard.[16] Column 6 shows that men's unemployment duration is unaffected by beginning a spell in February, regardless of the size of an individual's EITC payment. Although the sample sizes are very small, I have also estimated models relying on variation in EITC generosity for February entrants only. This specification also indicates that larger EITC values are associated with lower re-employment hazards for women, with a coefficient of -0.173 (0.107) on the predicted EITC value. This estimate comes from a sample of only 312 unemployment spells of women and thus should be treated with caution.

The pattern of results in Tables 3 and 4 is robust to alternative methods of identifying parents. There are some complications involved in identifying parents in the SIPP. In the 1996, 2001, and 2004 panels, both the mother and the father of each household member can be identified, if they live in the same household. In the 1993 panel, only one parent is identified. To compute the number of children a person has, I count up all the under-19 members of a household who list that person as a parent. For observations from the 1993 panel, I also assign to a married person the number of children listing his spouse as a parent, and to unmarried partners of the household reference person I assign the number of children listing the reference person as a parent. This method results in somewhat fewer identified parents in the 1993 panel than in later panels. As a robustness check, I apply the 1993 method of identifying parents to later panels. This change has virtually no effect on the results. To mirror the tax code's definition of an EITC-qualifying child, my baseline definition of a parent requires living with a child for at least six months before unemployment, or for all observed months for those with fewer than six months observed prior to unemployment entry. As a further robustness check, I count a person as a parent if he or she was living with an under-19 child at the time of entry into unemployment, ignoring information on household composition in the previous five months. In this specification, the *Feb Start* coefficient for women is -0.249 with a standard error of 0.107. The pattern of results is also robust to identifying EITC income-eligible households in different ways. My baseline approach uses earnings from the three months prior to entry into unemployment. If instead I use 12 months of earnings, my sample size falls by about 25% but the *Feb Start* coefficient is essentially the same. It varies between -0.246 and -0.248 across different parent definitions and is always significant at the 5% level.

I have tried a number of other specifications not reported in the tables. Replacing an individual's own weekly UI benefit amount with the average weekly amount paid in her state and year has virtually no effect on the results. I have looked for heterogeneity in the effect of entering unemployment in February. There is not a significant difference between the responses of single and married women, nor is the response significantly different for whites and non-whites. The evidence presented by Chetty (2008) suggests that the effect of cash-on-hand

[16] I have also estimated a specification including a full set of starting-month dummies and the interaction of each of these dummies with an individual's predicted EITC amount. For women the coefficient on the February interaction term is -0.086, with a standard error of 0.052. None of the other month interaction terms is close to significant at conventional levels.

should be largest for those with the lowest levels of liquid assets. I have tried interacting the *Feb Start* term with an indicator for having net liquid assets less than or equal to zero. Approximately 75% of the sample falls into this group. I do not find that the February effect is significantly larger among this group than among those with positive net liquid assets. However, nearly everyone in my sample could reasonably be considered liquidity constrained.

5.3 Falsification Tests

It is possible that unemployment spells beginning in February are longer than unemployment spells beginning at other times of year for reasons unrelated to tax refund receipt. While it is difficult to rule this out conclusively, the absence of a February effect for groups of individuals unlikely to be receiving large refunds in February makes such a story less plausible.

I consider three groups who are similar to my primary sample in many ways but who receive smaller average refunds. First I consider individuals who appear to have no EITC-qualifying children, but who otherwise meet my sample criteria. Next I consider individuals who have children but who are earning too much to be eligible for the EITC. I use a sample of individuals with real 3-month earnings of $8250 to $16500, corresponding to annual incomes of $33,000 to $66,000. Tax return data indicate that, over the years 1993 to 2007, filers in this group received refunds equal to 5.5% of their AGI on average. Finally, I apply my baseline sample definition to data from the 1984, 1985, and 1986 panels of the SIPP. The EITC existed during this time period but was substantially less generous than it became in later years. Results of hazard model estimates for each of these three groups are shown in Table 6. In these samples entering unemployment in February is not associated with significantly longer unemployment duration. Standard errors are large, though, and 95% confidence intervals generally include the point estimate for my primary sample.

5.4 Effects on Job Quality

Given that beginning an unemployment spell around the time of tax refund receipt is associated with longer unemployment spells for low-income women with children, I next investigate whether these longer searches result in better eventual search outcomes. Sample sizes are smaller here because not all spells in my sample end with re-employment, and because measures of job quality are missing for some employed respondents.

Results of wage growth regressions for women are shown in column 1 of Table 7. There is no evidence that unemployment spells beginning in February are associated with greater wage growth. In column 2 I use an indicator for whether a job pays an hourly wage as a measure of lower-quality employment. In this specification I also control for whether an individual's pre-unemployment job paid an hourly wage. There is substantial persistence in having employment compensated on an hourly basis. Entering an unemployment spell in February has no significant effect on whether one's post-unemployment compensation is hourly. In column 3 the dependent variable is a dummy equal to one if the usual number of hours worked per week in the post-unemployment job is greater than 40. Using this as a proxy for job quality is motivated by the fact that full-time jobs typically offer higher hourly pay than part-time jobs (Hirsch 2005), and that full-time jobs are more likely to provide health insurance benefits (Farber and Levy 2000). Unemployment spells beginning in February are no more likely to end with a full-time job than are unemployment spells beginning in other months of the year.

The absence of any positive effect on job quality measures is not particularly surprising, for at least three reasons. First, the extra search time associated with a February entrance into unemployment is only about four weeks. Second, this group of individuals with at most a high school degree faces generally limited labor market opportunities. Finally, other researchers have found little effect of longer unemployment spells on wage gains, even for samples drawn from a broader range of education levels (Addison and Blackburn 2000; Card, Chetty, and Weber 2007).

6 Conclusion

Low-income filers with children receive large tax refunds in a concentrated period of time. On average, the tax refunds of EITC-eligible parents are equivalent to about three and a half months of income. More than half of all refundable EITC payments are distributed in the month of February. This paper highlights a previously unexplored impact of this concentrated delivery of tax refunds. For mothers with low levels of income and education, the additional cash-on-hand from a tax refund lengthens unemployment spells. Among these women, unemployment spells beginning in February have a 26% lower hazard of re-employment and are about four weeks longer than unemployment spells beginning at other times of year.

Tax refund payments provide liquidity but, unlike UI benefits, do not change the return to a marginal unit of work. Although a filer's EITC payment is a function of her annual earnings, the amount is predetermined by the time the tax refund arrives. Thus, in the framework developed by Chetty (2008), tax refund payments lengthen unemployment spells not through moral hazard but by relaxing liquidity constraints. Given the evidence in this paper of a strong relationship between cash-on-hand and unemployment duration for low-income women with children, it is likely that making UI benefits more generous for this group would increase unemployment duration by further easing credit constraints. Such an increase in generosity would likely allow greater consumption smoothing for low-income mothers, which could have positive effects for their children.

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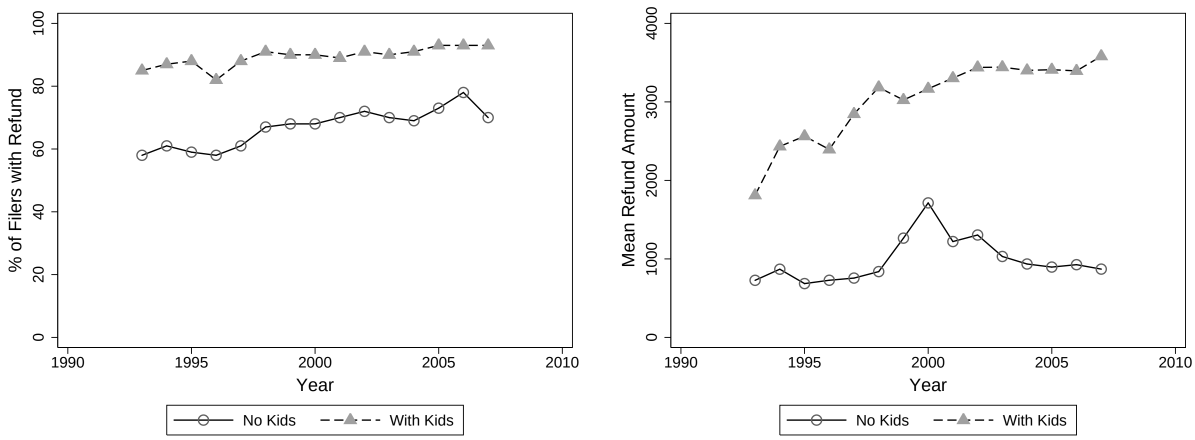
Figure 1: Refund Receipt, Low-Income Filers

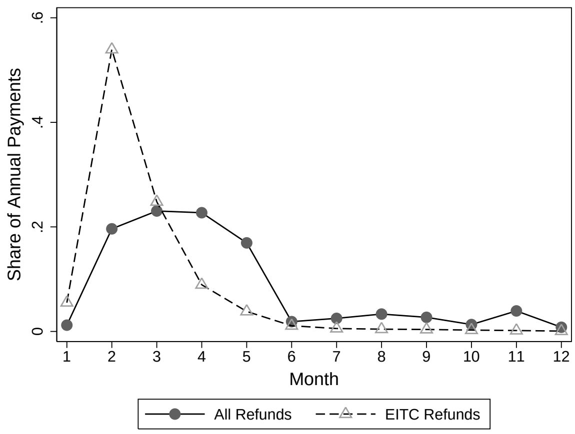


(a) Percent Receiving Refunds

(b) Mean Refund Amount

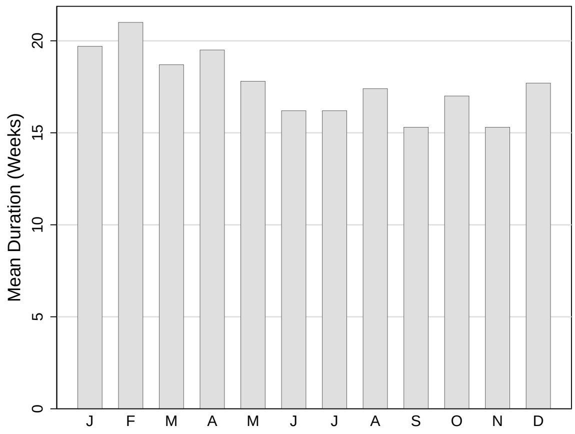
The sample is restricted to returns with AGI>0 and AGI<\$33,000, measured in real 2007 dollars. Mean refund amounts are reported in real 2007 dollars. A filing unit is classified as having kids if there are any dependent exemptions claimed for children living at home.

Figure 2: Tax Refund Payments by Month



This figure uses data from *Monthly Treasury Statements* covering years 1998 through 2007. For each year, I compute the share of annual refund payments disbursed in each calendar month. The figure shows the 10-year average of each month's share.

Figure 3: Mean Duration by Month of Unemployment Entry, Women



This figure shows mean unemployment duration, in weeks, for unemployment spells of women. Included individuals are ages 20 to 64, lived with one or more own children prior to unemployment, had earnings (own earnings or own plus spouse's earnings if married) less than $8250 in the three months prior to unemployment, and worked at least 12 weeks prior to unemployment. Unemployment spells that are temporary layoffs or that contain no weeks of active search for a new job are not included.

Figure 4: Survival Functions, Women

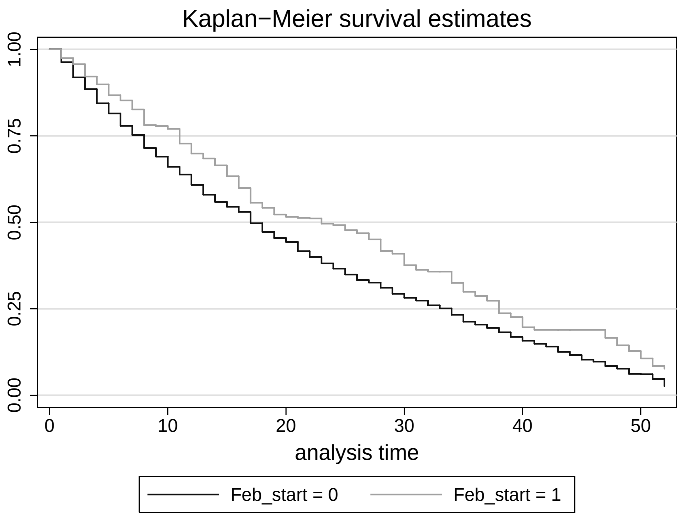


Figure 5: Survival Functions, Men

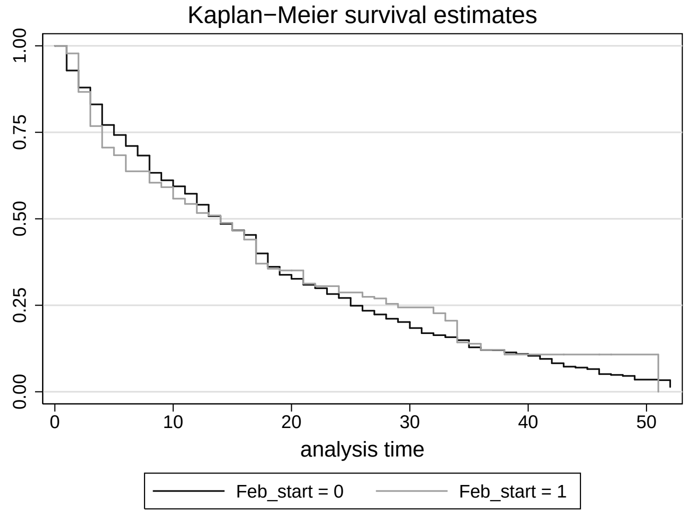
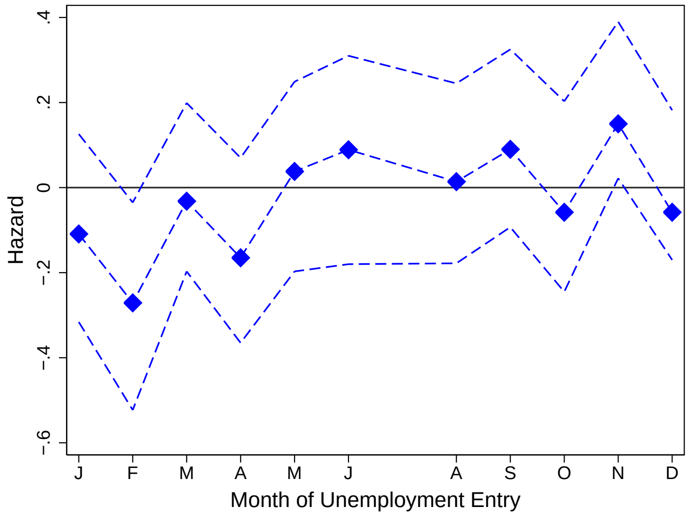


Figure 6: Month Coefficients from Hazard Model, Women



The figure plots the point estimates and 95% confidence intervals for variables indicating the month of entry into unemployment, from a hazard model including demographic controls and a set of year, state, industry, current calendar year and current calendar month dummies. July is the omitted month.

Table 1: Comparing Unemployment Spells by Month Of Entry

	Women			Men		
	Means		Predicting	Means		Predicting
	Feb	Other	Feb Start	Feb	Other	Feb Start
	(1)	(2)	(3)	(4)	(5)	(6)
Age	32.6	32.1	0.001	34.8	33.6	0.001
			(0.005)			(0.005)
Age Squared			-0.00002			$-6.30{\cdot}10^{-6}$
			(0.00007)			(0.00007)
% White	71.1	65.5	0.009	78.5	83.5	-0.027
			(0.012)			(0.021)
% Married	30.1	28.6	-0.007	78.6	76.0	-0.003
			(0.013)			(0.016)
Number of Kids	1.9	1.9	-0.002	2.2	2.0**	0.010
			(0.005)			(0.007)
Own Earnings, Previous 3 Months	3040	3064	$5.96{\cdot}10^{-6}$	4306	4016	$8.28{\cdot}10^{-7}$
			$(6.36{\cdot}10^{-6})$			$(9.02{\cdot}10^{-6})$
Imputed Hourly Wage	7.73	8.04	-0.00009	9.15	9.44	-0.001
			(0.001)			(0.001)
Potential Weekly UI Benefit	122	129	$3.88{\cdot}10^{-6}$	172	162	0.0002
			(0.0001)			(0.0002)
Pre-Unemp Job Tenure (Weeks)	38.7	39.4	-0.0008***	49.4	43.0*	-0.00006
			(0.0002)			(0.0002)
% with Job Tenure Censored	28.8	28.2	0.065***	31.9	34.3	0.005
			(0.016)			(0.017)
Annual Unemp Rate	5.4	5.3	0.014	5.6	5.5	0.025*
			(0.010)			(0.013)
Mean Net Liquid Wealth	1047	3725	$-4.91{\cdot}10^{-8}$	747	3088	$-3.60{\cdot}10^{-7}$*
			$(9.17{\cdot}10^{-8})$			$(2.13{\cdot}10^{-7})$
Median Net Liquid Wealth	0	0		0	0	
Spell Included UI Receipt	19.1	19.9		24.4	24.8	
Mean Duration (Weeks)	21.0	17.2***		15.0	14.6	
N	174	2290	2288	131	1586	1575
R^2			0.060			0.110

Stars in columns 2 and 5 indicate a significant difference in means relative to the previous column. Sample sizes are somewhat smaller for imputed wage, potential weekly UI benefit, net liquid wealth, and annual unemployment rate. Stars in columns 3 and 6 indicate statistical significance of a regression coefficient. The regressions in columns 3 and 6 also include fixed effects for year, state, pre-unemployment industry, and calendar month first observed in the SIPP. All dollar amounts are in real 2007 values.

Table 2: Pre-Unemployment Industry, by Month Of Entry

	Women		Men	
	Feb	Other	Feb	Other
Construction	1.0	0.9	25.6	22.4
Manufacturing	12.8	13.3	14.6	16.2
Wholesale Trade	4.4	3.0	8.1	3.4
Retail Trade	14.9	16.9	14.4	10.7
Transportation	1.7	1.8	2.3	4.4
Administration	3.6	7.5***	4.1	7.4*
Education Services	1.6	2.0	0.4	1.0
Health Services	14.7	13.0	1.0	1.8
Accommodation, Food Services	24.7	19.0	7.6	6.9
Other Services	3.0	4.3	5.6	5.5
Other Industry	17.5	18.4	16.4	20.2

Stars indicate a significant difference across the preceding two columns. Those with a missing value for pre-unemployment industry are placed in the other industry category.

Table 3: Hazard Model Estimates, Women

	(1)	(2)	(3)	(4)	(5)	(6)
Feb Start	-0.267***	-0.279***	-0.306***			
	(0.092)	(0.096)	(0.099)			
F/M Start				-0.137**	-0.124*	-0.129*
				(0.067)	(0.068)	(0.071)
On Seam	1.792***	1.775***	1.720***	1.794***	1.777***	1.722***
	(0.057)	(0.059)	(0.059)	(0.057)	(0.059)	(0.059)
Age		0.039*	0.041*		0.041*	0.043*
		(0.023)	(0.024)		(0.023)	(0.024)
Age Squared		-0.0006*	-0.0006*		-0.0006**	-0.0006*
		(0.0003)	(0.0004)		(0.0003)	(0.0004)
White		0.222***	0.216***		0.215***	0.206***
		(0.060)	(0.065)		(0.060)	(0.065)
Married		-0.076	-0.104*		-0.077	-0.107*
		(0.060)	(0.060)		(0.060)	(0.060)
Number of Kids		-0.018	-0.007		-0.018	-0.006
		(0.027)	(0.026)		(0.027)	(0.026)
Pre-Unemployment Wage		0.006	0.003		0.005	0.003
		(0.004)	(0.004)		(0.004)	(0.004)
WBA		0.0001	0.0001		0.0001	0.0001
		(0.0003)	(0.0004)		(0.0003)	(0.0004)
Pre-Unemp Job Tenure		-0.002*	0.001		-0.002*	0.001
		(0.001)	(0.001)		(0.001)	(0.001)
Pre-Unemp Job Censored		0.220***	0.002		0.222***	0.004
		(0.058)	(0.075)		(0.058)	(0.075)
Net Liquid Wealth		$-2.23 \cdot 10^{-7}$	$-4.06 \cdot 10^{-7}$		$-2.33 \cdot 10^{-7}$	$-4.20 \cdot 10^{-7}$
		$(4.54 \cdot 10^{-7})$	$(4.96 \cdot 10^{-7})$		$(4.55 \cdot 10^{-7})$	$(4.95 \cdot 10^{-7})$
Unemp Rate		-0.065***	-0.034		-0.065***	-0.033
		(0.022)	(0.039)		(0.022)	(0.039)
January			0.149			0.148
			(0.137)			(0.138)
February			0.103			0.078
			(0.127)			(0.126)
March			0.087			0.086
			(0.127)			(0.127)
April			0.019			0.017
			(0.127)			(0.127)
May			-0.053			-0.054
			(0.124)			(0.124)
June			-0.124			-0.122
			(0.120)			(0.120)
August			0.142			0.139
			(0.112)			(0.112)
September			-0.113			-0.115
			(0.121)			(0.120)
October			0.153			0.153
			(0.115)			(0.115)
November			-0.199			-0.200
			(0.130)			(0.130)
December			-0.367***			-0.370***
			(0.138)			(0.138)
Fixed Effects (Year, State, Industry)			Yes			Yes
Number of Spells	2464	2288	2288	2464	2288	2288

The table reports coefficients from hazard models. Standard errors, clustered at the person level, are in parentheses. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 4: Hazard Model Estimates, Men

	(1)	(2)	(3)	(4)	(5)	(6)
Feb Start	-0.067	-0.030	0.044			
	(0.119)	(0.120)	(0.114)			
F/M Start				-0.021	0.015	0.041
				(0.078)	(0.083)	(0.085)
On Seam	1.952***	1.876***	1.854***	1.951***	1.876***	1.854***
	(0.061)	(0.064)	(0.064)	(0.061)	(0.064)	(0.064)
Age		-0.054**	-0.043**		-0.054**	-0.044**
		(0.021)	(0.022)		(0.021)	(0.022)
Age Squared		0.0006**	0.0005*		0.0006**	0.0005*
		(0.0003)	(0.0003)		(0.0003)	(0.0003)
White		0.322***	0.386***		0.324***	0.385***
		(0.092)	(0.099)		(0.093)	(0.099)
Married		0.001	-0.010		0.0002	-0.009
		(0.074)	(0.076)		(0.075)	(0.076)
Number of Kids		0.038	0.003		0.037	0.003
		(0.027)	(0.029)		(0.027)	(0.029)
Pre-Unemployment Wage		-0.006	-0.004		-0.006	-0.004
		(0.004)	(0.004)		(0.004)	(0.004)
WBA		0.0007*	0.0003		0.0007*	0.0003
		(0.0004)	(0.0004)		(0.0004)	(0.0004)
Pre-Unemp Job Tenure		-0.003***	-0.001		-0.003***	-0.001
		(0.001)	(0.001)		(0.001)	(0.001)
Pre-Unemp Job Censored		-0.002	-0.138		-0.00004	-0.138
		(0.069)	(0.083)		(0.069)	(0.083)
Net Liquid Wealth		$-1.47 \cdot 10^{-6}$	$-8.51 \cdot 10^{-7}$		$-1.46 \cdot 10^{-6}$	$-8.39 \cdot 10^{-7}$
		$(1.32 \cdot 10^{-6})$	$(1.45 \cdot 10^{-6})$		$(1.32 \cdot 10^{-6})$	$(1.45 \cdot 10^{-6})$
Unemp Rate		-0.065**	-0.049		-0.065**	-0.049
		(0.027)	(0.047)		(0.027)	(0.047)
January			-0.140			-0.138
			(0.134)			(0.134)
February			-0.147			-0.143
			(0.143)			(0.142)
March			-0.269*			-0.275*
			(0.141)			(0.142)
April			-0.144			-0.148
			(0.135)			(0.136)
May			-0.150			-0.152
			(0.131)			(0.131)
June			-0.004			-0.006
			(0.122)			(0.122)
August			0.023			0.024
			(0.131)			(0.131)
September			-0.112			-0.109
			(0.137)			(0.137)
October			-0.277*			-0.274*
			(0.142)			(0.142)
November			-0.313**			-0.309**
			(0.140)			(0.141)
December			-0.369**			-0.366**
			(0.154)			(0.154)
Fixed Effects (Year, State, Industry)			Yes			Yes
Number of Spells	1717	1575	1575	1717	1575	1575

The table reports coefficients from hazard models. Standard errors, clustered at the person level, are in parentheses. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 5: Alternative Specifications

	Women			Men		
	Dropping Jan, March, April	All Ed Levels	EITC Dollar Value	Dropping Jan, March, April	All Ed Levels	EITC Dollar Value
	(1)	(2)	(3)	(4)	(5)	(6)
Feb Start	-0.361***	-0.161**	-0.080	-0.034	0.019	0.022
	(0.106)	(0.074)	(0.091)	(0.119)	(0.094)	(0.080)
HS Grad or Less		-0.160***			-0.209***	
		(0.044)			(0.056)	
Feb Start · EITC Value (1000s)			-0.084*			-0.013
			(0.046)			(0.044)
Number of Spells	1741	3708	4304	1191	2171	5436

The table reports coefficients from hazard models. Standard errors, clustered at the person level, are in parentheses. All specifications include demographic controls and a full set of year, month, state, and industry fixed effects.

Table 6: Hazard Model Estimates for Groups with Smaller Average Refunds

	Women			Men		
	No Kids	Higher Income	Earlier Years	No Kids	Higher Income	Earlier Years
	(1)	(2)	(3)	(4)	(5)	(6)
Feb Start	-0.093	-0.058	0.016	0.036	0.114	-0.140
	(0.105)	(0.145)	(0.220)	(0.083)	(0.132)	(0.184)
Number of Spells	1852	884	761	3617	1059	667

The table reports coefficients from hazard models. Standard errors, clustered at the person level, are in parentheses. All specifications include demographic controls and a full set of year, month, state, and industry fixed effects. Columns 2 and 5 include individuals whose combined real own and spouse's earnings in the three months prior to unemployment were between \$8250 and \$16500. Columns 3 and 6 include parents who satisfy the criteria for inclusion in the main sample, but who are observed in the 1984, 1985, or 1986 SIPP.

Table 7: Effects on Job Quality, Women

	Dependent Variable		
	Wage Growth	Paid Hourly	Full Time
	(1)	(2)	(3)
Feb Start	-0.068	-0.016	0.018
	(0.104)	(0.033)	(0.061)
Pre-Unemployment Controls			
Paid Hourly		0.252***	
		(0.035)	
Full Time			0.311***
			(0.028)
N	1596	1823	1747

The table reports coefficients from OLS regressions. Standard errors, clustered at the person level, are in parentheses. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.