Essays in Unemployment Insurance

by

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Abstract

This thesis consists of three essays that examine household responses to state unemployment insurance (UI) generosity across spells of unemployment, with a particular emphasis on the role of liquidity constraints.

Enacted in 1986, the Consolidated Omnibus Budget Reconciliation Act (COBRA) provides limited portability of employer-sponsored health insurance coverage amongst job separators. Separated workers are eligible to maintain their employer-sponsored health coverage at the point of separation for a period of typically 18 months, though are obligated to pay 102 percent of the full employer premium. The substantial cost to maintain continuation coverage relative to transitory income poses a potential barrier for the unemployed. Using Survey of Income and Program Participation (SIPP) panels spanning 1990-2003, Chapter One re-evaluates existing evidence of UI adequacy and the limited effectiveness of continuation of coverage mandates by assessing the role of UI in maintaining private health insurance coverage across employment status. I first establish the magnitude of the loss of private health insurance coverage associated with unemployment, separating the issue of duration dependence. I find that private coverage falls by approximately 19 percentage points, or 26 percent of pre-separation levels, across employment status. Exploiting plausibly exogenous spatial and temporal variation in UI generosity, I then employ a simulated instruments approach to estimate the effect of UI generosity on private health insurance coverage amongst the unemployed. I find that a 10 percentage points increase in the UI replacement rate increases private coverage amongst the unemployed by 3.0 percentage points, and that a $100 increase in weekly UI benefits increases private coverage amongst the unemployed by 7.6 percentage points. Although imprecise, these results imply that current UI generosity mitigates the loss of private health insurance coverage by roughly 41 to 44 percent. Stratification across proxies for liquidity constraint and consumption commitment reveals suggestive evidence of an associated liquidity effect.

The policy response to shortfalls in insurance coverage for job separators has been to enact continuation of coverage mandates, which allow job leavers to continue their employer-sponsored coverage without the typical direct cost subsidization provided to active employees. For the unemployed, this cost is incurred during a period of low transitory income, suggesting a plausibly important role for liquidity constraints in limiting take-up of continuation benefits. Incorporating SIPP panels spanning 1983-2003, Chapter Two first evaluates the effectiveness of continuation of coverage mandates in mitigating the fall in private health insurance coverage across spells of unemployment, identified by variation in state mandates and implementation of mandated federal coverage through COBRA. These results imply that 12 months of continuation of coverage eligibility mitigates the fall in private coverage amongst the unemployed with employer-sponsored health coverage prior to separation by approximately 18 percent. Exploiting plausibly exogenous spatial and temporal variation in state UI benefits

across the reference period, I then employ a simulated instruments approach to estimate the heterogeneous effect of continuation of coverage mandates across levels of transitory income. These results are consistent with the notion of excess sensitivity to cash-in-hand. Absent state UI, mandate eligibility mitigates only 6 percent of the fall in private coverage. Yet for every \$100 in eligible weekly UI benefits, private coverage is increased for mandate-eligible separators by 10 percentage points relative to mandate-ineligible separators. Policy makers must comprehensively address both access to group insurance markets as well as ability to pay for constrained households.

Chapter Three re-evaluates existing evidence of a spousal labor supply response to state UI generosity. Although Chetty (2008) documents an associated liquidity effect in the response of unemployment spell duration to UI generosity, there has been no comparable work investigating the importance of liquidity constraints in explaining the crowd-out of spousal labor supply by eligible UI benefits of the household's primary earner. Across such periods of low transitory income of the primary earner, the spousal labor supply of liquidity constrained households plausibly exhibits greater responsiveness to eligible UI benefits. Yet the spousal labor supply response to UI generosity is composed of both an indirect effect, driven by eligible UI benefits of the unemployed primary earner, and a direct effect, driven by own-eligibility of the spouse. The longitudinal nature of the SIPP allows for identification of UI-ineligible spouses, and corresponding sample restrictions purge estimates of the confounding direct effect of UI. Employing a simulated instruments approach that exploits variation within-states across the reference period 1983-2003, I find that each eligible dollar in UI benefits crowds-out spousal earnings by 33 cents across the unemployment spell of the household's primary earner. Despite the sizeable estimate of crowd-out, the predicted increase in spousal earnings absent UI would offset only 13 percent of the lost wages of the unemployed primary earner. Stratification across proxies for liquidity constraint and fixed consumption commitment yields suggestive evidence of an associated liquidity effect. In terms of average spousal earnings, couples proxied as liquidity unconstrained through consideration of net liquid wealth are only 26 percent as responsive to eligible UI benefits of the primary earner relative to couples proxied as liquidity constrained. These results rationalize of the large crowd-out estimates of Cullen and Gruber (2000).

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1.1 Motivation

The US health insurance market is distinguished by the relationship between employment and access to group insurance markets. As a result, job separation typically entails a loss of income as well as access to group insurance markets. One cited feature of health care reform is the ability to maintain health insurance coverage across job status (The White House 2009). The existing federal program established to facilitate this goal falls under the sweeping reform of the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA). Under this program, employees who separate from their jobs are generally able to continue their employer-sponsored coverage for up to 18 months. Yet, individuals are required to pay the full premium as well as a small administrative surcharge to continue their insurance through COBRA. Workers insured through employer-sponsored coverage typically directly pay only 16 percent of the cost of individual coverage and 27 percent of the cost of family coverage (Claxton et al. 2008).[1] In 2009, the full annual cost of employer-sponsored health insurance averaged $4,824 for an individual policy and $13,375 for a family policy (KFF 2009a). A recent Kaiser Family Foundation (2009b) survey found that 59 percent of adults with employer-sponsored coverage would find it very difficult to pay the full cost of their premiums if they were no longer employed.

[1] Although evidence supports the notion of cost-shifting to employees through wages (e.g. Baicker and Chandra 2006), non-wage income is not insured through UI. Thus loss of subsidized health insurance coverage through the employer represents both a large price distortion, even with access to continuation coverage, and a substantial loss of uninsured benefits.

Whereas consumption measures evaluated by the existing literature exhibit only moderate declines associated with unemployment, health insurance coverage exhibits a substantial drop across employment status, suggesting market imperfections and potential inadequacy of unemployment insurance (UI) benefits. Using data from the Survey of Income and Program Participation (SIPP) spanning 1990-2003, I re-evaluate existing evidence of UI adequacy and the limited effectiveness of continuation of coverage mandates by assessing the role of UI in maintaining health insurance coverage across employment status. By focusing exclusively on an observation window following the effective date of COBRA legislation, I avoid potentially confounding factors associated with state continuation mandates.[2]

Despite availability of continuation benefits under COBRA throughout the reference period, I find that private health insurance coverage falls amongst the unemployed by approximately 19 percentage points, or 26 percent of pre-separation levels of coverage. Investigating dynamics across spells of unemployment reveals limited evidence of duration dependence. Conditional upon completed duration, the severity of the fall in private coverage is amplified as a spell progresses. Exploiting plausibly exogenous spatial and temporal variation in UI generosity, I implement a simulated instruments approach to estimate the effect of UI generosity on private health insurance coverage amongst the unemployed, restricting to UI-eligible unemployment spells. I estimate that a 10 percentage points increase in the UI replacement rate increases private coverage amongst the unemployed by 3.0 percentage points, and that a $100 increase in weekly UI benefits increases private coverage amongst the unemployed by 7.6 percentage points. Although imprecisely measured, scaling these results implies that current UI generosity mitigates the loss of private health insurance coverage by roughly 41 to 45 percent.

Rising unemployment has heightened political interest in extending health insurance coverage amongst the unemployed. As of February 2009, the number of unemployed persons in the United States had risen by 5.0 million to 12.5 million, and the unemployment rate had grown by 3.3 percentage points to 8.1 percent over the previous 12 months (BLS 2009). On February 17, 2009, President Obama signed into law H.R. 1, the American Recovery and Reinvestment Act of 2009, which according to CBO (2009) estimates will result in $787 billion in additional government deficit over the 2009-2010 period. Of the $149 billion devoted to improving access to health, $24.7 billion was assigned to provide a 65 percent subsidy of health care insurance premiums for the unemployed under the COBRA program. An additional $40 billion was assigned to provide extended UI benefits through December 31, 2009 and increase benefits by $25 per week (DOL 2009a). A similar 65 percent subsidy was offered through the Health Coverage Tax Credit, a part of the Trade Act of 2002, for qualifying workers who lost their jobs due to international trade. In 2006 only an estimated 12 to 15 percent of the approximately 200,000 eligible households participated in the program (Dorn 2008). However, that program was structured as a refundable tax credit, which required that workers pay the full premium

[2] Brown (2010a) separately considers the phase-in of COBRA, incorporating earlier SIPP panels.

prior to reimbursement.

Stratification across proxies for access to credit reveals suggestive evidence of an associated liquidity effect, providing insight into the perceived failure of the Health Coverage Tax Credit and informing future policy considerations.[3] Specifically, I find parameter concentrations that reveal substantial responsiveness amongst liquidity constrained households relative to unconstrained households. I consider both pre-separation net liquid wealth and pre-separation gross liquid wealth as proxies of liquidity constraint. Alternatively, I consider mortgage status at the point of separation as a proxy for consumption commitment, with largely consistent inference. These results suggest that the responsive margin is driven by liquidity-constrained households, and that delayed tax credits may be of limited efficacy relative to policies that directly subsidize the purchase price of continuation coverage.

The rest of the paper proceeds as follows. A brief review of the existing literature is discussed in the next section. Section 1.3 details the data and core sample selection criteria. Section 1.4 evaluates the magnitude of the fall in private health insurance coverage amongst the unemployed, controlling for heterogeneity in the demand for insurance and separating the issue of duration dependence. Section 1.5 incorporates measures of individual UI generosity and implements a simulated instruments approach to estimate the effect of UI generosity in mitigating the loss of private health insurance coverage amongst the unemployed. Section 1.6 differentiates heterogeneity in the effect of UI generosity across proxies of liquidity constraint and consumption commitment. The last section concludes.

1.2 A Brief Literature Perspective

Health Insurance Coverage and Unemployment

The US health insurance market is distinguished by the relationship between employment and access to group insurance markets, though this relationship has eroded somewhat in recent history. Cutler (2003) finds that despite economic prosperity in the 1990s, increased premiums associated with employer-sponsored health insurance largely explains the decline in health insurance coverage across this period resulting from a reduction in take-up. In turn, Gruber and McKnight (2003) attribute this increase in premiums with rising costs, falling tax rates, expansions of public insurance, and increased managed care penetration. Despite this, approximately 88 percent of private health insurance coverage was acquired through the workplace in 2008 (DeNavas-Walt et al. 2009).

[3] In the current policy climate, it is reasonable to question the merit of such studies, as the United States faces radical health insurance reform. Yet in the early 1970s, financing and the impact of cost sharing took center stage in the national health care debate. To inform this debate over free, universal health care and whether the benefits would justify the cost, RAND researchers designed and conducted the Health Insurance Experiment (RAND Corporation 2009).

Yet, previous work detailing the loss of health insurance coverage associated with unemployment is largely inconsistent. Some evidence (Monheit et al. 1984, Klerman and Rahmna 1992) suggests that low rates of insurance amongst the unemployed is largely explained by a lack of coverage while on the previous job. This evidence contrasts with other studies (Berki et al. 1985, Podgursky and Swaim 1987, Bazzoli 1986), which report large declines in insurance coverage following job loss. Modeling underlying preference heterogeneity, Gruber and Madrian (1997) find that over the 1983-1989 period the likelihood of private insurance coverage drops by roughly 20 percentage points following a job separation.

Costs of Short-Term Uninsurance

Although health insurance coverage is not a direct measure of consumption, there is evidence to suggest that smoothing insurance coverage across employment status may proxy for smoothing medical care consumption. Adults and children uninsured for less than one year are less likely to receive recommended screenings and are more likely to have gone without a needed physician visit due to cost relative to the insured (Ayanian et al. 2000, Olson et al. 2005). Only emergency departments are required by federal law to stabilize all individuals irrespective of ability to pay, though the uninsured may be denied follow-up care for even urgent medical conditions if unable to pay in full. Health providers are not required to provide care to the uninsured, restricting access to needed care. The uninsured are typically billed for any care received and often face higher charges than the insured (Asplin et al. 2005). By forgoing care to avoid medical debt, the uninsured may worsen health problems (Schwartz 2007), heightening the difficulty of re-securing employment. To the extent that health care consumption is not perfectly substitutable over time, then, there are potential consequences to even relatively short spells of uninsurance.

The Health Insurance Portability and Accountability Act (HIPAA) of 1996 limits the exclusion of pre-existing conditions for individuals with uninterrupted coverage. Yet individuals uninsured for 63 days or more, defined as a break in 'creditable coverage', may have pre-existing condition exclusions imposed by a new employer-sponsored health plan for most health conditions for which treatment, advice, or diagnosis were received in the six months prior to enrolling in an employer-sponsored insurance plan (DOL 2009b). In 2005, nearly three in five adults who considered buying non-group coverage had difficulty finding a plan they could afford, and one in five were either turned down by an insurance carrier, charged a higher premium based on health status, or had a specific health condition excluded from coverage (Collins 2006). Again, even relatively shorts spells of uninsurance can have long-term implications that extend beyond the immediate duration of the unemployment spell.

Adequacy of UI Benefits

A central question for the design of the UI program is the adequacy of UI benefits, in terms of maintaining the standard of living of recipients while unemployed.[4] Yet, increased generosity of the UI system crowds out private insurance coverage along a number of dimensions: precautionary savings (Engen and Gruber 2001, Klein 2009), spousal labor supply (Cullen and Gruber 2000, Brown 2010b), and severance pay (Chetty and Saez 2010). Rather than simply the computation of benefit levels or wage replacement rates, then, this literature suggests the use of consumption behavior responses to UI benefits as a metric of adequacy. Hamermesh and Slesnick (1998) find that the consumption of households receiving UI benefits falls little relative to comparable households in the economy, suggesting that UI benefits are largely adequate. Similarly, Gruber (1997) demonstrates that increases in UI benefits are not translated directly into increased consumption, supporting the conclusion that the unemployed rely upon other resources to smooth consumption throughout an unemployment spell. Dissimilarly, health insurance coverage exhibits a dramatic decline following separation, suggesting potential inadequacy of current UI generosity. I investigate the implications of increased UI generosity in terms of mitigating the fall in private coverage.

Liquidity Effect

While the relationship between employment and access to group insurance markets suggests a potentially large role for continuation of coverage mandates in reducing the prevalence of uninsurance amongst the unemployed, existing work concerning the effectiveness of continuation benefits in decreasing uninsurance has found only modest effects (Klerman and Rahmna 1992, Gruber and Madrian 1997, Brown 2010a). Considering the substantial effective subsidization of the cost of insurance relative to the non-group market, these results imply a small price elasticity of insurance. This evidence is consistent with the low estimate of price elasticity of demand for insurance in Thorpe et al. (1992) and Marquis and Long (1995), yet inconsistent with the estimates of Leibowitz and Chernew (1992) and Gruber and Poterba (1994). However, this viewpoint ignores the plausible role of household liquidity constraint in restricting take-up of continuation benefits, given the high cost of continuation coverage relative to transitory income across spells of unemployment.

Browning and Lusardi (1996) provides an inconclusive review of existing studies evaluating whether households are liquidity constrained. However, given the extremely skewed asset distribution amongst workers prior to unemployment (Gruber 2001), a non-trivial fraction of the unemployed are plausibly unable to smooth transitory income shocks relative to permanent income. For these households, the liquidity effect may be considered a socially beneficial response to the correction of credit and insurance market failures. Several recent studies have used consumption data to investigate the importance of liquidity constraints and partial insurance (Johnson, Parker, and Souleles 2006; Blundell, Pistaferri, and Preston 2008). In the context of UI, Bloemen and Stancanelli (2005) find evidence that UI benefits help to smooth consumption for households without financial wealth at the time of job loss. This result is

[4] A review of the study of UI adequacy is contained in O'Leary (1996).

consistent with the findings of Browning and Crossley (2001) concerning concentration of the consumption-smoothing response to the Canadian UI system within a subset of households without liquid assets. Chetty (2008) differentiates moral hazard and liquidity effects in the endogenous duration of an unemployment spell to UI benefits. More recent evidence from Brown (2010a) finds that the effectiveness of continuation of coverage mandates is sensitive to eligible state UI benefits, suggesting that households are particularly responsive to available continuation benefits when UI benefits sufficiently bolsters a household's ability to pay.

1.3 Data

I incorporate data from the Survey of Income and Program Participation (SIPP) panels spanning the reference period October 1989 - December 2003.[5] Each SIPP panel surveys a national set of households at four month intervals (waves) for $2\frac{1}{2}$-4 years, with sample sizes ranging from approximately 14,000 to 36,700 interviewed households.[6] At each interview, households are asked questions in reference to the four month recall period. Data are collected regarding health insurance coverage, income and labor force participation, as well as a wide array of socioeconomic characteristics of each household member and of the household as a whole. The SIPP provides monthly data on income and health insurance coverage and weekly data on labor force status. Relative to other widely used sources, such as the CPS and PSID, the advantages of the SIPP are the availability of asset data and high-frequency data on individual and household income, employment status, detailed health insurance coverage, and UI benefit receipt. Deliberate over-sampling of the low-income population provides a suitably large sample of unemployment spells. I supplement the SIPP with (1) monthly national price indices and seasonally adjusted monthly state unemployment rates as reported by the US Bureau of Labor Statistics, and (2) state-year level annual employer individual and family health insurance premium data from the Medical Expenditure Panel Survey Insurance Component (MEPS-IC), spanning 1996-2003.[7]

Starting from the universe of job separations across the pooled SIPP panels, I retain spells of unemployment, defined as spells following a job separation during which individuals are either on layoff or are searching for a new job. Observations are not conditioned on duration.[8]

[5] US Census Bureau. Survey of Income and Program Participation Users' Guide. http://www.census.gov/sipp/usrguide.html (Accessed September 2009)

[6] The length of observation varies across panels. The 1990 and 1991 panels contain 8 waves. The 1992 and 1993 panels contain 9 waves. The 1996 panel contains 12 waves. The 2001 panel contains 9 waves. Owing to the overlapping design of the survey, observations are continuous across the reference period, excepting for March – September 2000, where a minor interruption arose over a funding shortfall and the subsequent cancellation of the in-progress 2000 panel, which was re-started as the 2001 panel.

[7] Available at http://www.meps.ahrq.gov

[8] I consider exclusions of spells of less than 1 month from the analysis, as: (1) spells of less than 1 month may be false transition in the SIPP, and (2) it is difficult to properly assign reported health insurance coverage, measured at the monthly level, across a transitional month. Results are robust to exclusion, though these spells

As labor force nonparticipation among this sample is often disguised long-term unemployment (Clark and Summers 1979), I do not exclude observations for workers who drop out of the labor force at some point during an unemployment spell. I measure the duration of a spell as consisting of all weeks of separation from work. For those who have some weeks of unemployment (either search or layoff), 17.4 percent of their spells are weeks out of the labor force. Spells begin with initial month of separation and end with the first full month of re-employment.[9]

I restrict attention to prime-age males, 25-54 years old, thus focusing on job-separators who have a high rate of attachment to the labor force. I include only unemployment spells once I have observed at least one quarter of employment experience.[10] This allows for the accurate measurement of non-employment spell duration, as well as characteristics of pre-separation jobs, most importantly imputation of pre-separation wages and pre-separation health insurance coverage.

The resulting left-censoring for unemployment spells in-progress at the start of the respective SIPP panel disproportionately omits the long-term unemployed, potentially skewing the composition of remaining spells.[11] For prime-age males, the above selection rule excludes only approximately 15 percent of the sample, whereas for women and younger/older men, the share of separations excluded would be approximately 48 percent. Thus, for prime-age males, this selection rule is less prohibitive in terms of generalization. This restriction also limits the impact of schooling and early retirement decisions, as well as childcare decisions, on the resulting pool of job separations. If married, I restrict to households where both the husband and wife are 25-54 years old due to inter-dependence of health insurance decisions within the household.

I further exclude separations for which I have missing individual or job characteristics or basic health insurance coverage. These restrictions leave 10,280 unemployment spells in the core analysis sample, consisting of 154,480 monthly observations, of which 30,208 are months unemployed.

Health Insurance Coverage

Health insurance coverage is a monthly measure, and I differentiate private, public, and uninsured coverage status.[12] Private insurance is defined as any health insurance coverage other

are included in baseline specifications to maintain focus on the representative spell.

[9] Alternatively, in results not reported I exclude the initial month of separation due to ambiguity of health insurance coverage assignment in a transitional month. Results are largely consistent. In results below, I include the initial month of separation in an attempt to maintain focus on the representative spell.

[10] This exclusion eliminates all monthly observations for individuals who never work in three consecutive months, as well as the initial months for workers whose first three months of work occurs later in the respective SIPP panel.

[11] To the extent that durations respond to state UI generosity, then, spells of unemployment for particularly responsive individuals are less likely to be included in the sample. Discussion of the 'dynamic sample-selection' bias is detailed in Diamond and Hausman (1984).

[12] As I am unable to observe generosity of insurance coverage, I cannot distinguish underinsurance. Underinsurance refers to individuals covered by health insurance, but the provisions of that insurance does not adequately protect the individual from high medical expenses. As of 2007 there were an estimated 25 million

than Medicare or Medicaid and includes employer-sponsored coverage, continuation coverage, and non-group coverage. Once per wave, the detailed source of an individual's health insurance coverage is revealed along a number of dimensions: whether the insurance is in the individual's name or some other family member's name and whether the policy is sponsored by an employer/union or acquired in a non-group setting. I impute monthly assignment respecting the nature of the survey design.[13] Baseline specifications below are constructed using person-month level observations to allow for evaluation of health insurance dynamics across employment status as well as throughout a given unemployment spell.[14]

Seam Bias

As individuals are required to recall information from the preceding four months of the reference period for each wave, it is unclear how much unique information is contained in monthly responses. Individuals have a tendency to propagate their status at the point of the interview backwards through the preceding months.[15] A disproportionately large number of labor force transitions are reported on the 'seam' between interviews, leading to artificial spikes in the hazard rate. This bias extends to transitions in insurance coverage (Klerman and Rahman 1991).

I use monthly observations as many spells of unemployment are less than four months in duration. In this context, seam bias will produce false classifications of labor participation, blurring the distinction between the employed and unemployed. Further, health insurance coverage may be falsely classified, dropping off prematurely leading up to a separation. Thus I draw information on prior employed health insurance coverage from the wave preceding the point of separation. Of course, coverage may still drop off with artificial abruptness following a separation and short spells of uninsurance may be underreported.[16]

underinsured adults in the US, a dramatic rise from the 16 million underinsured in 2003 (Schoen et al. 2008). To the extent that individuals experiencing layoff transition to less generous coverage (commonly cited with health insurance policies purchased on the non-group market), results to follow will understate the true fall in insurance coverage associated with unemployment.

[13] For waves in which individuals are employed throughout the wave, I assign employer-sponsored coverage if the worker reports private insurance in his own name through an employer at the end of the wave. In waves for which a worker is not employed for some part or entirety of the wave, I assume that the reported coverage refers to the fourth reference month (end of the wave), thus I impute monthly coverage from the current wave provided that the unemployment spell extends through the end of the wave. Else, for spells in-progress at the start of a wave, I impute coverage based upon reported coverage in the previous wave. I am unable to clearly assign source of insurance coverage during the spell for some subset of spells. This outlined approach is problematic, so I also consider restricting relevant results below to only observations for the fourth reference month, with largely consistent results. In all cases, I code individuals as covered by employer-sponsored coverage prior to separation based upon reported coverage in the wave prior to separation.

[14] Potential sample selection bias owing to 'overweighting' long spells in the baseline specifications is addressed by collapsing the data into person-spell level observations in Section 1.5.6. This produces results more typical of a representative spell, though at the loss of the ability to explore dynamics.

[15] See Klerman (1991) for a detailed discussion of the seam bias problem.

[16] Results are re-considered using only the fourth reference month, just prior to the interview in the unobserved fifth month. Although point estimates are largely comparable, statistical imprecision impedes inference.

1.4 Health Insurance Coverage Amongst the Unemployed

1.4.1 Descriptive Statistics

Descriptive statistics for the core sample relative to alternative sampling restrictions are presented in Table 1. As reported in column (1), 90.2 percent of employed prime-age males have some form of private health insurance coverage; the majority of this coverage, 85.6 percent, is provided through the worker's employer.[17] In contrast, only 60.7 percent of the non-employed in column (2) report private health insurance coverage. Similarly, only 59.0 percent of the unemployed in column (3) report private coverage.[18] These data reveal a raw private health insurance coverage gap of over 31.2 percentage points between the employed and the unemployed. However, prior to separation, only 71.8 percent of the unemployed report private health insurance coverage, suggesting significant population heterogeneity between workers employed throughout the respective panel and those experiencing unemployment. In terms of demographics, those experiencing job loss tend to disproportionately attain lower levels of education and earn significantly less compared to the employed sample. These factors are typically associated with low private insurance coverage and may partially explain the large raw coverage gap between the employed and the unemployed.

Comparison of the core sample of unemployed in column (3) relative to individuals experiencing non-employment in column (2) reveals no substantive differences across populations in terms of health insurance coverage or demographics. The unemployed and non-employed report average private coverage across relevant spells of 59.0 percent and 60.7 percent, respectively. Prior to separation, 71.8 percent of the unemployed report private coverage, compared to 72.8 percent of the non-employed. Similarly, the populations are comparable in terms of age, marital status, spousal work status, racial composition, family size, educational attainment and earnings. These similarities are striking in light of the unemployment restriction reducing the sample of 15,876 spells of non-employment to 10,280 spells of unemployment, an exclusion of more than 35 percent of the sample of non-employment spells. Presumably, then, results to follow provide insight not only in reference to spells of unemployment, but more generally with respect to spells of non-employment.[19]

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[17] Only 4.6 percent of the employed report private coverage in own name purchased in the non-group market (5.6 percent of private coverage in the worker's name). 8.4 percent of the employed (9.3 percent of private coverage) report coverage in the spouse's name, the majority of which is employer-sponsored coverage. In total, 93.3 percent of private coverage in derived through either the worker's employer or the spouse's employer.

[18] Less than 0.9 percent of the employed and approximately 3.1 percent of the unemployed report public insurance coverage, suggesting that evaluation of private coverage is the empirically-relevant health coverage margin.

[19] Similarly, UI monetary eligibility restrictions imposed in Section 1.5 yield a largely comparable population of UI-eligible unemployment spells in column (4), though as expected due to the nature of these restrictions, average annual earnings and private health insurance coverage prior to separation rise, while public health insurance coverage prior to separation falls.

1.4.2 Health Insurance Transitions

Prior to presenting the empirical model, I briefly consider the flow of workers across health insurance coverage status around the point of separation. Figure 1 details the transition paths for individuals experiencing unemployment, unconditional on health insurance coverage prior to separation.[20] As expected, there is a sharp rise in the rate of uninsurance and a muted increase in public insurance coverage associated with unemployment. In the month prior to separation, the uninsurance rate is 28.2 percent of workers. In the month of separation, this fraction rises to 34.9 percent, and by three months after the point of separation this fraction reaches 44.5 percent of the unemployed. Longer unemployment spell durations are associated with a more pronounced rise in uninsurance, though it is unclear from the figure whether this is attributed to population heterogeneity with respect to completed unemployment spell durations or duration dependence, resulting from either realization of a longer than expected spell, loss of temporary coverage measures, or realization of reduced means of smoothing across the spell. Figure 2 constructs a similar set of transition paths, restricting to workers with private health insurance coverage from any spouse prior to separation.[21] As expected, those with private coverage in the wave prior to separation experience more pronounced patterns, with uninsurance jumping from 5.72 percent in the month prior to separation to 15.6 percent in the month of separation, and further increasing to 29.3 percent three months after the point of separation. For workers unemployed twelve or more months, uninsurance approaches rates of 39 percent.

Figures 3-4 deconstruct private health insurance coverage into a number of detailed sources: private health insurance through the worker's employer, private health insurance in own name purchased in the non-group market, private health insurance through the spouse's employer, and private health insurance in the spouse's name purchased in the non-group market. Figure 3 details transition paths for workers experiencing unemployment conditional on private health insurance in own name through the employer prior to separation. At the point of separation, coverage through the previous employer falls to 62.5 percent. Three months past the point of separation, coverage through the previous employer is reported at 40.5 percent, and the coverage rate continues to decline with duration to a minimum of 9.1 percent twelve or more months past the point of separation.[22] In addition to a modest increase in reported public coverage,

[20]In results not reported, child records for the dependents of individuals experiencing unemployment exhibit particularly noisy health insurance coverage, yielding results inconsistent with expectations, motivating their exclusion from specifications below. Spousal records for wives exhibit comparable results to the husband.

[21]Private coverage begins to fall prior to the point of separation. Although some fraction of workers may legitimately lose coverage prior to separation, this more likely reflects the 'seam bias' inherent in the survey. Workers who lose coverage conditional on separation propagate the uninsured status at the point of the interview through the start of the wave, encompassing 0-3 months employed depending upon the sequencing of the separation in relation to the interview schedule.

[22]The initial large fraction of individuals continuing to report health insurance coverage through the former employer, presumably continuation coverage, is at odds with the results of Flynn (1992), who estimates that 19.3 percent of individuals who were terminated, laid off, or quit from firm that had health insurance chose to

there are sizeable movements towards both private insurance in own name purchased in the non-group market (7.9 percent three months past the point of separation) and private coverage through the spouse's employer (10.7 percent three months past the point of separation).[23] The majority of transitions, though, result in a rising rate of uninsurance, 24.3 percent at the point of separation and 55.9 percent three months past the point of separation. Yet, Figure 4 reveals that for individuals receiving coverage through a spouse's employer prior to separation, there is only a tempered rise in the rate of uninsurance. At the point of separation, 7.5 percent report uninsurance, and three months past the point of separation 8.3 percent report uninsurance. This suggests that the fall in private health insurance coverage associated with unemployment may be largely driven either by access to group insurance markets or the relative cost of group insurance through continuation coverage compared to directly subsidized rates while employed, rather than simply the associated income loss.

1.4.3 Estimating the Fall in Coverage

To present an estimate for the effect of unemployment on private health insurance coverage, I estimate multivariate linear probability models similar to Gruber and Madrian (1997) of the form:[24]

$$Private_{ist} = \beta Unemployed_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{1.1}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-month level. $Private_{ist}$ is an indicator for private health insurance coverage in a given month. $Unemployed_{it}$ is an indicator for whether an individual is unemployed in a given month. z_{it} is a vector of individual demographics and job characteristics. α_s is a vector of state indicators. δ_t is a vector of panel, year, and calendar month indicators.[25] Lagged private

take-up continuation benefits. However, as persuasively argued in Gruber and Madrian (1997), despite high out-of-pocket costs, realized take-up amongst certain sub-populations, such as workers who do not move to jobs in which they receive health insurance, is substantially higher.

[23] Although the movement towards the spouse's employer's coverage is amplified by duration, the effect is largely static for movement towards private coverage in own name purchased in the non-group market. The latter likely reflects reporting error, failure to distinguish continuation coverage from non-group coverage.

[24] Due to limitations of the linear probability model (LPM), specifically the failure of both the normality and the independence assumption arising from mechanical correlation between the outcome and the error term, as well as infeasible predicted values outside the 0-1 bounds, I alternatively consider binary-choice (probit) specifications. Given similar marginal effects and a high fraction of within-sample predictions, I estimate the LPM for ease of interpretation.

[25] Individual characteristics include: educational attainment indicators, a race indicator, a marriage indicator, a spousal work status indicator, age bins, and bins for number of children. Characteristics of the worker's job include: 10 Standard Industrial Classification System (SIC) major industry sectors indicators, and 23 Standard Occupational Classification (SOC) major groups occupation indicators. Wage controls include a flexible 7-knot cubic spline in base-period wages and household annual income and 4-knot cubic splines in highest-quarter wages. Alternatively, linear income spline knots are assigned at the 1st,5th,10th,...,90th,95th,99th percentiles of the real earnings distribution for the relevant sample under analysis. Log-linear splines are also considered. Results are consistent across these alternative wage controls. Year indicators control for any national time trends in health insurance coverage, and the month dummies control for seasonal trends in health insurance

coverage critically controls for pre-separation health insurance coverage.[26] Correlation in the behavior of an individual within a panel and across individuals within a state indicates that it is inappropriate to treat monthly observations on health insurance status as independent, thus standard errors are clustered to accommodate an arbitrary variance-covariance matrix within each state.

I use coverage by any private health insurance source as the dependent variable in the regression specification, rather than a detailed source of insurance for several reasons. First, private health insurance coverage is a monthly measure, whereas employer-sponsored health insurance and other detailed source measures are measured once per wave. Second, this measure nets the impact of unemployment on private insurance coverage, including potential switching to non-employer sources of coverage, such as reliance upon a spouse's employer-sponsored health insurance option. Third, classification of continuation coverage in the SIPP is not clearly assigned as employer-sponsored coverage rather than individually purchased coverage. Lastly, I consider private health insurance coverage, rather than any health insurance coverage, so as to distinguish movement from private to public insurance coverage. The coefficient β on the regressor of interest, $Unemployed_{it}$, measures the fall in private health insurance coverage associated with unemployment.

Results are presented in Table 2. In specification (1), unemployment is associated with a highly statistically significant drop in private insurance coverage of approximately 18.95 percentage points for the sample of workers who experience an unemployment spell at some point in the respective panel. This estimate, relying upon observable controls, remains roughly 61 percent as large as the raw private coverage gap between the employed and the unemployed. In specification (2), I incorporate an individual fixed effect to control for unobservable heterogeneity.[27] I estimate that unemployment is associated with a fall in private coverage of 16.10 percentage points, a magnitude approximately 85 percent as large as relying upon observable controls and 52 percent as large as the gap in the raw data.

Specifications (3)-(8) estimate the fall in private coverage associated with unemployment under a number of alternative sample selection criteria. Incorporating non-employment spells, rather than restricting to unemployment, yields comparable measures of the fall in private coverage associated with non-employment; private coverage falls by 17.06 percent relying upon observable controls in specification (5) and 14.4 percent relying upon the individual fixed effect

coverage, as might correspond to open enrollment periods. Panel indicators are included given the overlapping panels design of the SIPP. State indicators control for time-invariant differences across states in health insurance coverage.

[26] For employed observations, prior health insurance coverage is constructed as the individual's private health insurance status in the prior wave. For unemployed observations, prior private health insurance coverage and job characteristics are assigned at their pre-separation values for the entirety of the unemployment spell. The result is a different timing in variable construction for the employed, for whom these variables refer to the prior wave, and unemployed, for whom these variables refer to the wave prior to separation.

[27] I exclude the control for lagged private health insurance in this specification, as the introduction of fixed effects necessarily induces serial correlation in the error term.

to control for heterogeneity in specification (6). Incorporating workers employed throughout the respective panel as a control, I estimate a fall in private coverage associated with non-employment of 18.57 percent relying upon observable controls in specification (7) and 14.78 percent relying upon the individual fixed effect in specification (8). Consistency across specifications suggests that restrictions imposed to produce the core sample minimally distort the representativeness of these spells relative to the evaluation of non-employment spells.

1.4.4 Evaluating Duration Dependence

To model the effect of the duration of an unemployment spell on the magnitude of the fall in private coverage, I replace the $Unemployed_{it}$ indicator in Equation (1.1) with a series of leading and lagged terms relative to the point of separation, allowing for an evaluation of the dynamics of private coverage across the spell. Specifically, monthly indicators are constructed for each of the 12 months employed prior to job separation, the month of job separation, and each of the 11 months unemployed following the month of separation, with an additional collapsed indicator for 12 or more months unemployed past the point of separation. These results, with covariates suppressed, are presented in Table 3 specifications (1)-(2), and the graphical representation is presented in Figure 5.[28]

Similarity of the results across the specifications suggests that observable controls perform reasonably well relative to individual fixed effects when restricting to the core sample of individuals who experience an unemployment spell at some point in the respective panel. Comparing across observable controls in specification (1) and the individual fixed effect in specification (2), I find that the months of separation is associated with a fall in private coverage of 11.12 percentage points and 10.15 percentage points, respectively. This differential widens as the spells progress, and unemployment six months beyond the point of separation is associated with declines in private coverage of 26.75 percentage points, relying upon observable controls, and 23.55 percentage points, relying upon the individual fixed effect. Referencing Figure 5, the fall in private coverage associated with unemployment is largely realized in the first month of the unemployed spell, though coverage continues to decline sharply through the seventh month of an unemployment spell, at which point private coverage appears to largely stabilize. This is suggestive of duration dependence, though these results are plausibly generated by population heterogeneity across completed spell durations.[29] Statistically significant declines in private coverage in the three months preceding the point of separation, relying upon observable controls or incorporating the individual fixed effect, are evident either graphically in Figure 5 or

[28]Even incorporating individual fixed effects, the negative impact of the three months prior to job separation remain statistically significant. This may be the result of misreporting or seam bias, but restricting to only the seam month yields similar results, with a prominent drop the month prior to separation, suggesting misreporting.

[29]That is, if the impact of unemployment is greatest for individuals who ultimately experience a longer unemployment spell duration, then the decline in private coverage associated with longer durations may result from heterogeneity or expectations over completed duration, rather than duration dependence.

numerically in Table 3 and reflect the 'seam bias' inherent to the sampling design.

To separate the concept of duration dependence, I estimate the individual fixed effects specification stratified across groups of clustered completed spell duration: less than 1 month, 1-3 months, 4-6 months, and 7-12 months. These results are presented in Table 3 specifications (3)-(6), and the graphical representation is presented in Figure 6. The unemployed who experience longer completed spell durations are associated with a more severe fall in private health insurance coverage relative to shorter final durations at the same point within a spell, suggesting heterogeneity across spell type. Relative to periods of sustained employment, in the month of separation private coverage falls by 6.24 percentage points for unemployment spells less than one month in completed duration, 11.16 percentage points for unemployment spells 1-3 months in completed duration, 12.30 percentage points for unemployment spells 4-6 months in completed duration, and by 14.57 percentage points for unemployment spells 7-12 months in completed duration. This monotonic relationship holds at every relevant comparison point across spells. However, the continued fall in private coverage as the spell progresses within the same cluster of final spell durations suggests a modest role for duration dependence, as well, though it is unclear whether this results from an income effect, information revelation, or some other effect.

1.5 Evaluating the Role of UI

1.5.1 Incorporating UI Generosity

Information on the regulations regarding UI eligibility criteria and benefit schedules across states is reported semiannually by the United States Department of Labor. The basis for both the monetary eligibility calculator and benefits calculator come from the initial calculators developed by Gruber (1997) and later updated by Chetty (2008). This paper improves upon the inherited calculators by incorporating more recent data, enhancing accuracy through consultation of legislative documentation, and extending eligibility criteria to include distributional considerations.

Eligibility

Eligibility for UI is multi-dimensional. Monetary eligibility is established through qualifying wages, often paired with a required wage distribution across the 'base period', defined as the first four of the past five calendar months.[30] The worker must not have exhausted available benefits within a given benefits period. Recipients must additionally demonstrate nonmonetary eligibility, generally consisting of: (1) unemployed through no fault of their own, (2) able and

[30] Example: In January 1999, Texas required that the applicant: (1) earn base period wages at least 37 times that of the computed weekly benefit amount and (2) document wages in at least 2 quarters.

available to work full-time, and (3) actively seeking full-time work.[31]

A job separation is excluded if prior work history appears to make the worker ineligible for UI. This motivates exclusion of self-employed workers from the sample, as they cannot avail themselves to the UI system.[32] I restrict to spells in which the individual reports looking for work in at least some months in order to focus on unemployment and not strictly labor force exit. However, while UI eligibility requires continuing demonstration of labor force attachment, in specifications reported below observations are included if individuals report stopping search effort, effectively dropping out of the labor force, to account for the discouraged worker effect.

I include temporary layoffs, despite concerns of potentially different information about probabilities of layoff and recall, as well as potential ex-ante arrangements with the employer, for two reasons: (1) temporary layoffs consist of approximately 20 percent of all unemployment spells in my core sample and exclusion may result in non-representative spells, and (2) these individuals may constitute a particularly responsive margin as they are plausibly more aware of the UI system's parameters.[33]

Reason for Separation

The SIPP asks respondents whether an individual experiencing job separation was discharged, laid off, left because the job was temporary, or voluntarily quit.[34] Amongst the core sample of unemployment spells satisfying monetary eligibility requirements, 54 percent of separations are coded as voluntary, 13 percent as involuntary discharge, and only 33 percent as involuntary layoff. However, non-trivial UI receipt reported at some point during the spell for those who report voluntary separation (25 percent) or involuntary discharge (40 percent) compared to those reporting involuntary layoff (66 percent) indicates that individuals inconsistently report the reason for job separation. Thus I retain all qualifying unemployment spells regardless of self-reported reason for separation.[35] Although restricting to spells of unemployment in which the worker is actively searching for work plausibly focuses on involuntary separation, some

[31] A waiting period is also imposed, typically one week. Given person-month observations, I ignore the impact of this provision, though this will understate the impact of actual eligible UI benefits in the first month of unemployment.

[32] An indicator for self-employment status was removed starting with the 1996 panel. I rely upon the BLS definition of self-employed as workers for their own, unincorporated businesses, including those who worked for profit or fees in their own unincorporated business or professional practice. If self-employed as workers for their own, incorporated businesses, then these workers are not classified as self-employed because they are paid employees of their own companies. A small fraction of workers report self-employment income less than full wage income. For this group, I classify the individual as self-employed if self-employment income constitutes at least 50 percent of total wage income. For consistency, this measure is applied across all panels.

[33] Temporary layoffs are documented to be endogenous to the level of UI generosity. Thus if a disproportionately large number of temporary layoffs are included as a result of high UI generosity and if those spells are documented to have natural smoothing properties, perhaps through ex-ante arrangements with the employer, then UI generosity may have a spurious positive relationship with health insurance coverage across the unemployment spell. Existing empirical results are conflicting. Nonetheless, results are largely robust to the exclusion of temporary layoffs.

[34] This measure is expanded as part of the 1996 SIPP restructuring. The author's calculations re-structure responses from the 1996 and 2001 panels to be consistent with the 1990-1993 panels.

[35] In alternative baseline specifications not reported, separations are classified as having experienced 'invol-

fraction of the sample reflect UI-ineligible voluntary separations. Estimated coefficients will then tend to understate the effect of UI generosity in mitigating the fall in private coverage amongst the UI-eligible unemployed population.

Benefits

Receipt of UI benefits is not automatically provided, rather an individual satisfying state-defined eligibility criteria must apply for benefits. Among eligibles, take-up is much less than full. Blank and Card (1991) estimate take-up rates of roughly 67 percent among eligibles. An alternative to the use of eligibility is the actual UI benefits receipt amongst the unemployed. However, this poses a potentially serious selection bias, as take-up of UI may be endogenous to benefit level, thus I do not condition on receipt of UI benefits.[36] Also, receipt of public assistance is generally noisily measured in survey data. While this may call for use of eligibility as an instrument for actual UI receipt, Gruber (1997) persuasively argues that UI benefit eligibility, rather than actual UI receipt, is of direct policy relevance.

Eligible UI weekly benefit amounts are constructed as non-linear, and in some states complex, functions of wage levels and distribution in the base period.[37] Accurate benefits estimation requires five calendar quarters of earnings history, which is not available for a non-trivial subset of the sample. Instead, I impute an individual's earnings history as completely as the data allow, requiring a minimum of one quarter of wage data. Additional inputs used in determining weekly benefit amounts vary by state-year and include: annual earnings, number of children, spousal work status, and average tax rates. State-specific rules for minimum and maximum weekly benefit amounts are then imposed and vary greatly across states. The worker's replacement rate is constructed as the ratio of the UI weekly benefit amount to the weekly wage level across the base period.[38] Individuals from small states are excluded as the SIPP clusters these states, citing confidentiality concerns, preventing the assignment of state laws for these individuals.[39] These additional restrictions leave a sample of 7,669 UI-eligible unemployment spells.

untary separation', distinguished by layoff and discharge. Restricting to the set of job separators satisfying non-monetary eligibility for UI yields point estimates of larger magnitude, though the restricted sample size yields considerable inaccuracy that prohibits statistical inference.

[36] Restricting the sample to those who take-up UI could lead to selection bias due to the endogenous nature of the take-up decision with respect to the benefit level (Anderson and Meyer 1997). If factors determining UI take-up are correlated with the change in an individual's private health insurance coverage associated with unemployment, this will tend to overstate the effect of UI on private insurance coverage of the unemployed. There is some 'option value' to individuals who do not take-up benefits, but derive value from the availability of UI resources should the individual encounter a longer-than-expected unemployment duration.

[37] Example: In January 2001, Texas weekly benefit amounts (wba) are assigned as 1/25 of high quarter wages, subject to a minimum wba of $48 and a maximum wba of $294. With 13 weeks per quarter, this is designed to replace approximately 50 percent of a recipient's weekly wage.

[38] If unemployment is expected to increase earnings, such that the eligible weekly benefit amount exceeds weekly earnings over the base period, I exclude the spell. Point estimates are minimally affected by this exclusion restriction, though precision is improved.

[39] These states are Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont, and Wyoming.

A summary of the source of variation in UI generosity across states is presented in Table 4. Although there is benefit generosity variation with each state over time, variation is largely drawn from a cross-state comparison. Across UI-eligible spells, the average UI weekly benefit amount is $172, with state-specific values ranging from a minimum of $123 in Nebraska to a high of $239 in Hawaii. The average UI replacement rate across all UI-eligible spells is 50.1 percent, with state-specific values ranging from a minimum of 40.4 percent in Louisiana to a maximum of 67.4 percent in Washington DC.

Duration of Benefits

Typically, individuals are eligible for UI benefits for a maximum entitlement of 26 weeks. The maximum is limited by past earnings as well as state-imposed caps on the fraction of base period wages that total unemployment benefits may replace within a 52 week period.[40] As the cap is hit earlier for individuals with higher replacement rates, more generous UI entitlement durations may be misleading as a margin of UI generosity across individuals within a state. Further, the maximum entitlement period may be extended through 'trigger' levels of state unemployment during recessionary periods or through a federal extended benefits program. Endogeneity of these extensions with respect to labor market opportunities suggests exclusion of this factor from estimation. In the analysis below, I consider the entire duration of the spell, regardless of expiration of UI eligibility, noting the endogeneity concerns above. This decision further relates to the notion of momentum in the health insurance decision.[41]

UI Generosity and Endogenous Spell Durations

As UI distorts the relative prices of leisure and consumption, reducing the marginal incentive to search for a job, increased generosity of the social insurance program is expected to reduce labor supply. Moffitt (1985) and Meyer (1990) demonstrate that a 10 percent increase in UI benefits results in a 4-8 percent increase in unemployment durations in the US. As discussed in Section 1.4, longer completed unemployment spell durations are associated with lower health insurance coverage rates at all points in a spell. Thus, endogeneity of duration to UI generosity will tend to underestimate the impact of the income effect associated with greater UI generosity on private health insurance coverage during the unemployment spell, though this bias is second-order.[42]

A related issue arises from the potential endogeneity of layoff to the generosity of the UI system, though the existing literature is inconsistent. Feldstein (1978) and Topel (1983) find supporting evidence in the context of temporary layoffs, in contrast to the inconsistent relationship detailed by Anderson and Meyer (1994). It is unlikely that such selection drives the results below, as

[40] Example: In January 2001, Texas capped total unemployment benefits at 27 percent of base period wages.

[41] A natural experiment to consider is the break in unemployment benefits occurring at the point of benefits exhaustion. However, the limited sample size of unemployment durations exceeding eligibility provides too little statistical power. Coefficients of approximately 0 suggest momentum in the health insurance decision, in some cases contractually through open enrollment periods in the event of 'switching' to a spouse's insurance coverage.

[42] This issue is partially addressed by the collapsed specification is Section 1.5.6.

modeling the probability of layoff as a function of UI generosity and a set of demographic control reveals a small and insignificant effect of UI generosity on the probability of layoff.

Graphical Evidence

Figure 7 presents graphical evidence of the heterogeneous effect of UI generosity on the fall in private health insurance coverage by estimating the extended version of Equation (1.1), stratified by eligible UI generosity for the state-year. Constructing the generosity measure at the state-year level removes confounding individual characteristics, such as income, from driving the results.[43] Visual inspection reveals a more dramatic fall in private health insurance coverage for individuals experiencing separation in below-median generosity state-year pairs relative to above-median generosity state-year pairs. A more rigorous treatment is detailed below.

1.5.2 Baseline Specification

In the baseline specification, I restrict to months unemployed within a UI-eligible unemployment spell.[44] To evaluate the effect of eligible UI generosity on health insurance coverage, I estimate multivariate linear probability models of the form:

$$Private_{ist} = \beta RR_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{1.2}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-month level. $Private_{ist}$ is an indicator for private health insurance coverage in a given month. RR_{it} is a measure of an individual's eligible wage replacement rate under the relevant state UI system and varies continuously between 0 and 1. z_{it} is a vector of individual demographics and job characteristics.[45] α_s is a vector of state indicators. δ_t is a vector of panel, year, and calendar month indicators. State indicators control for time-invariant heterogeneity across states correlated with UI generosity, such as risk aversion. Time indicators control for common trends in private health insurance coverage amongst the unemployed. Additionally, I incorporate seasonally adjusted state unemployment levels to account for potential legislative endogeneity.[46]

[43] The measure employed is the simulated instrument of the state-year UI replacement rate, detailed below. This measure is purged of individual characteristics and state distributions.

[44] This specification has the appeal of removing the confounding factor of false anticipation effects relative to incorporating employed observations. I re-consider an interacted model incorporating both months unemployed and months employed in Section 1.5.5.

[45] Controls include those under Equation (1.1).

[46] Seasonally adjusted unemployment rates are used in place of non-seasonally adjusted unemployment rates as the discussion of legislative endogeneity of the form above would suggest policy deviations from atypical fluctuation in unemployment. One dimension of endogeneity captured by this approach is the 'trigger' of extended UI benefits resulting from sufficiently high state unemployment.

The coefficient β on the regressor of interest, RR_{it}, measures the effect of UI generosity on the probability that an individual reports private health insurance coverage. The inclusion of state and time fixed effects results in a model effectively identified from higher order interactions of wage, state, and time, assumed to be legitimately excluded form an individual's health insurance coverage decision.[47] Effectively, β is identified by the differential private health insurance coverage rates of high- and low-earning unemployed workers across states that provide these earnings levels with different relative UI benefits.

As RR_{it} is a measure of eligible benefits, rather than received benefits, the estimated parameter β captures the differential effect of eligible UI generosity on private health insurance coverage rather than the effect of an increase in actual UI benefits received. The individual replacement rate, rather than weekly benefits level, is chosen as the measure of UI generosity in the baseline specification for interpretation and comparability across years.[48] These results are reported in Table 5.

However, it is not obvious that behavior responds to the replacement rate, or fraction of wage income replaced by the state UI program, rather than the weekly benefit amount, measured in dollar terms. This issue is one of relative versus absolute generosity. As the replacement rate is a mechanically decreasing function of income within a state-year, owing to the progressive benefits structure, and the weekly benefit amount is a weakly increasing function of income within a given state-year, comparing results across both specifications is a reasonable check for proper income controls, as well as consistency in responsiveness along a margin of generosity classification. These alternative specifications are reported in Table 6.[49]

Perhaps the most compelling index is the anticipated cost of COBRA coverage within a given state-year. As approximately 75 percent of the unemployed sample is married, I consider the average employer cost of family health insurance premiums by state-year as reported by the MEPS-IC.[50] The alternative generosity measure, then, is the nominal individual eligible UI weekly benefit amount scaled by the average employer-sponsored family coverage premium. These data are only available across the 1996-2003 period, so I only briefly consider this alternative generosity measure in Table 7. I re-construct results incorporating both the replacement rate and the real weekly benefit amount measures of UI generosity using this restricted reference

[47] A potential violation is state trends in private health insurance coverage correlated with the evolution of state UI generosity over time. Inclusion of the lagged private coverage from the wave prior to separation attempts to control for such a spurious correlation.

[48] Use of the replacement rate does not necessitate explicit use of a price index to generate comparability of the generosity measure across panels. However, a wage index is implicity applied.

[49] Weekly benefit amounts are discounted to January 1990 dollars using the Medical Care component of the Consumer Price Index (CPI), motivated by the fact that the rate of increase in health insurance premiums outpaces the aggregate CPI. Regrettably, the Medical Component of the Producer Price Index, perhaps a more reasonable adjustment, is not available for the duration of the study. Alternative indexes are considered in specifications not reported, using the aggregate CPI and the Prescription Drug component of the CPI. Inference across these alternative specifications is comparable.

[50] I refrain from adjusting the generosity measure index based upon family composition to prevent population heterogeneity from driving the results.

period to demonstrate consistency of results across the panels and maintain comparability.

Simulated Instrument Approach

Motivation for the implementation of a simulated instruments instrumental variables strategy is drawn from Meyer (1990), noting that the UI replacement rate for an individual is a function of the legislative environment in a given state-year, but also of an individual's characteristics. Even with flexible controls, relative state UI generosity may reflect differences in the distribution of incomes and other individual characteristics across states, thus confounding inference of the effect of UI on private health insurance coverage. I therefore instrument for predicted eligible UI benefits using 'simulated eligibility', a strategy developed in Currie and Gruber (1996) and detailed in application to UI generosity in Levine (1993) and Gruber (1997). A related two-step procedure is proposed and implemented in Chetty (2008).

Using the national sample of individuals in each six month period, given the frequency of reported policy updates, I assign that sample to each state in that period. I calculate each individual's eligible weekly benefit amount and determine the eligible replacement rate. I then average the resulting replacement rates across the simulated sample for each state-year. The resulting instrument is purged of potentially confounding individual characteristics of the individuals in that state-year and is a function of only the legislative environment in that state-year.[51] This simulated instrument is then incorporated as an excluded instrument.[52]

A second motivation for constructing the simulated instruments is related to the inherent measurement error of the UI benefits calculator. Although a noisy proxy for eligible benefits for a given individual, as a result of imputation and imprecise measurement of income distribution throughout the base period, the estimated UI weekly benefits amounts should be correct on average. This noise component, however, will drive the estimated coefficients towards zero in the classical errors-in-variables construction. Although the simulated instrument is, of course, a noisy measure as well, I can reasonably assume that the measurement error is uncorrelated across the measures, provided no systematic over- or under-estimation of weekly benefits amounts. The simulated instruments approach will then produce consistent estimates of the effect of eligible UI benefits in mitigating the fall in private coverage under this assumption.

One limitation to this approach, however, is a restriction in the variance of the UI generosity measures, as the instruments are fixed at a point in time across all individuals within the state. As detailed in Table 4, the majority of variation in the simulated instrument is driven by dif-

[51] As the SIPP panels sample potentially systematically different populations over time, I have alternatively constructed the simulated instrument using a fixed national sample from 1996, with wage data inflated by the Employment Cost Index for wages and salaries. These results, not reported, provide similar inference.

[52] The power of the simulated instrument is confirmed by the Kleibergen-Paap statistic, which is well beyond reasonable thresholds, rejecting weak instruments. Similarly, a partial R^2 of excluded instruments is approximately .28 for specifications with demographics. However, with state FE, the partial R^2 is reduced to .07, though the Kleibergen-Paap statistic remains suitably high. The Anderson-Rubin confidence set, robust to weak instruments, yields comparable inference to the standard inference reported below.

ferences across states, rather than within-states over time.[53] Results of simulated instruments approach (2SLS) are reported alongside the OLS results in Tables 5 and 6.

1.5.3 Baseline Results

Replacement Rate as a Measure of UI Generosity

Table 5 presents the baseline results using the replacement rate as the measure of eligible UI generosity. Specifications (1)-(4) consider OLS estimation, ignoring the potential endogeneity issue and measurement issue discussed above. In specification (1), I report a model for private health insurance coverage, excluding all demographics. The mechanical relationship between replacement rate and income results in a negative association between the UI replacement rate coefficient and private health insurance coverage. In specification (2), I incorporate demographic controls, including wage splines. The coefficient on the UI replacement rate flips sign, and the result suggests that a 1 percentage point increase in the UI replacement rate increases private coverage by .11 percentage points. This sign reversal on the replacement rate suggest adequacy of the wage splines. Specification (3) incorporates a control for legislative endogeneity, the state seasonally adjusted unemployment rate, which enters insignificantly and yields a similar coefficient on our regressor of interest, suggesting that a 1 percentage point increase in the UI replacement rate increases private coverage by .11 percentage points. Specification (4) incorporates state fixed effects, producing a similar point estimate, though the result is marginally significant. These results suggest that a 1 percentage point increase in the UI replacement rate increases private coverage amongst the unemployed by .13 percentage points.

Scaled by the average UI replacement rate in the sample of UI-eligible spells of unemployment, 50.1 percent, these results suggest that existing UI generosity mitigates the fall in private coverage by 6.5 percentage points. Specification (3) of Table 2 provides an estimate for the fall in private coverage associated with unemployment amongst UI-eligible separation. Relying upon observable controls, I estimate a fall in private coverage of 19.13 percentage points, given existing levels of UI generosity.[54] Together, these results suggest that absent UI, unemployment would result in a fall in private coverage of approximately 25.6 percentage points. Thus, existing UI generosity may alternatively be interpreted as mitigating roughly 25.4 percent of the fall in private coverage that would occur absent UI.

Specifications (5)-(8) of Table 5 consider the simulated instruments approach. Consistent with expectations, there is no sign flip under this approach when incorporating demographics, as

[53] This relationship holds for both real weekly benefit amounts as well as the replacement rates.

[54] For comparison, incorporating an individual fixed effect in specification (4) yields a tempered fall in private coverage of 16.11 percentage points associated with unemployment. Alternative scaling based upon this result would yield somewhat larger estimates of the effect of UI generosity in mitigating the fall in private coverage amongst the unemployed.

the instrument, by construction, is uncorrelated with individual characteristics. Relative to the OLS specifications, the 2SLS approach yields larger estimates of the effect of marginal increases in the eligible UI replacement rate on private coverage amongst the unemployed, reflecting either imperfect controls for individual characteristics or the inherent measurement error discussed above. Specification (7), which excludes state fixed effects, suggests that a 1 percentage point increase in the UI replacement rate increases private coverage by .30 percentage points. Scaled by the sample average UI replacement rate, this corresponds to an increase in private coverage resulting from existing levels of UI generosity of approximately 15.0 percentage points. Scaled by the implied fall in private coverage absent UI, these results suggest that existing levels of UI generosity mitigate roughly 43.9 percent of the fall in private coverage absent UI. Incorporating state fixed effects in specification (8) yields a point estimate of comparable magnitude, yet the robust standard error is inflated as the instrument's variation is largely across-states rather than within-states across-time, resulting in a statistically insignificant point estimate.

Weekly Benefit Amount as a Measure of UI Generosity

Table 6 presents analogous results incorporating the eligible real UI weekly benefit amount as the measure of generosity. The measure is scaled by $\frac{1}{100}$, such that point estimates correspond to the effect of a \$100 increase in the UI weekly benefit amount on private health coverage amongst the unemployed. Results are largely consistent with those of the baseline specification. The simulated instruments approach in specification (7) suggests that for every \$100 in eligible UI benefits, the private coverage increases by 7.64 percentage points amongst the unemployed. Scaled by an average UI weekly benefit amount of \$172, this results implies that existing UI generosity levels mitigate the fall in private coverage across employment status by 13.1 percentage points. Given a fall in private coverage across employment status of 19.13 percentage points evaluated at existing levels of UI generosity, these results suggest that absent UI private coverage would fall by approximately 32.2 percentage points. Thus, existing levels of UI generosity mitigate roughly 40.6 percent of the fall in private coverage absent UI. As above, though the OLS specification is robust to inclusion of state fixed effects, by restricting to within-state across-time variation in the instrument, results of the simulated instruments approach are too noisy to draw proper inference when state fixed effects are included in specification (8), though the point estimate is again similar.

Fraction of Continuation Coverage Costs as a Measure of UI Generosity

Table 7 restricts to the 1996-2003 reference period, owing to the data limitations of the MEPS-IC. Specifications (4) and (5) re-construct results of the simulated instruments approach, incorporating the eligible UI replacement rate and the eligible UI weekly benefit amount as measures of UI generosity, respectively. These point estimates are comparable to those of the extended reference period. Specifically, a 1 percentage point increase in the UI replacement rate is estimated to have increased private coverage amongst the unemployed by .43 percentage points, relative to an estimate of .30 in the baseline specification. Similarly, a \$100 increase in UI weekly benefit amount is estimated to increase private coverage amongst the unemployed by 9.51 percentage points, compared to an estimate of 7.64 in the baseline specification. These results are particularly encouraging given passage of HIPAA in 1996, easing concerns that the pooled effect may not apply across panels. Specifications (3) and (6) consider the alternative generosity measure of UI nominal weekly benefit amount scaled by the state-year average cost of employer-sponsored family coverage. Under OLS estimation in specification (3), I find that as the UI weekly benefit amount is increased by the average cost of continuation coverage, private coverage amongst the unemployed increases by 4.0 percentage points. Under the simulated instruments approach in specification (6), I find a larger effect of 9.8 percentage points. These results closely mirror those of the UI weekly benefit amount measure of UI generosity, scaled by the CPI, suggesting adequacy of that metric.

1.5.4 Imperfect Controls

Although state fixed effects are accommodated in the OLS specification above, the simulated instruments approach loses significance, resulting from limited variation in UI generosity within-states across-time. Thus the data are unable to accommodate state trends. This raises the concern that heterogeneous trends in private health insurance coverage across employment status, such as implementation of pooling mechanisms in the non-group market, may spuriously drive the results if correlated with UI generosity. As a result, an array of imperfect controls are considered: employed observations for individuals experiencing UI-eligible unemployment within the respective panel, employed observations for individuals continuously employed throughout the panel, and non-employed observations for individuals experiencing UI-ineligible separations, failing to meet either monetary or non-monetary restriction criteria.

Clearly, each control group is far from ideal. The employed observations for individuals experiencing UI-eligible unemployment at some point in the panel present a less-than-ideal control as: (1) false anticipation effects, as documented in Section 1.4.4, may create a false effect of UI generosity on private health insurance coverage while approaching separation if health insurance coverage is falsely back-coded through the wave, and (2) the effect of UI while employed will not contain other contemporaneous effects of unemployment on private coverage correlated with state UI generosity. Both the continuously employed and UI-ineligible separators consist of fundamentally dissimilar populations in comparison with UI-eligible separators, thus distinctions may be driven by population heterogeneity.

Regardless, each group is expected to demonstrate limited sensitivity to simulated eligible UI generosity if the baseline model is properly identified, as each control group is ineligible for UI receipt. I estimate modifications of Equation (1.2) for each imperfect control group. Results are presented in Table 8 alongside baseline results for the replacement rate measure of UI generosity and in Table 9 for the weekly benefit amount measure of UI generosity. Results are encouraging and support interpretation of the baseline specification as causal. Compared to the baseline specifications, the control groups exhibit diminutive sensitivity to UI generosity. Considering the simulated instrument approach in specification (5) or Table 8, a 1 percentage point increase in the UI replacement rate is estimated to increase private coverage amongst the UI-eligible unemployed by .30 percentage points in the baseline specification. By comparison, months employed are only 9.8 percent as responsive to UI generosity as months unemployed for the same set of separators, and this may reflect in part the 'seam bias' discussed above. The always employed population are only 1.0 percent as responsive as the UI-eligible unemployed. Perhaps most compelling, the UI-ineligible experiencing spells of non-employment are only 13.2 percent as responsive compared to the core sample, and the estimate is wrong-signed, that is a 1 percentage point increase in the UI replacement rate is estimated to decrease private coverage amongst the UI-ineligible by .04 percentage points. Similar inference is drawn from comparison across specifications in Table 9; relative to the core sample in specification (5), months employed are only 14.4 percent as responsive to UI generosity as measured by the weekly benefit amount in specification (6), the always employed are 4.7 percent as responsive in specification (7), and though the UI-ineligible population is 23.8 percent as responsive in specification (8), the estimate is again wrong-signed. These results are encouraging, as across the specifications point estimates for the control groups are comparatively modest, often wrong-signed, and generally insignificant.

1.5.5 Partially Interacted Model

To determine the significance of the estimated effect of eligible UI generosity on health insurance coverage for the UI-eligible unemployed sample relative to the employed observations for these same individuals, I estimate partially interacted multivariate linear probability models modified from Equation (1.1) of the form:

$$Private_{ist} = \beta_1 Unemployed_{it} + \beta_2 RR_{it} + \beta_3 Unemployed_{it} \bullet RR_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \quad (1.3)$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-month level, and include months unemployed and months employed for workers who experience UI-eligible unemployment at some point within the respective panel. $Private_{ist}$ is an indicator for private health insurance coverage in a given month. $Unemployed_{it}$ is an indicator for whether an individual is unemployed in a given month. RR_{it} is a measure of an individual's eligible wage replacement rate under the relevant state UI system and varies continuously between 0 and 1. z_{it} is a vector of individual and job characteristics.[55] α_s is a vector of state indicators. δ_t is a vector of panel, year, and calendar month indicators.

[55] Controls are equivalent to Equation (1.2). Controls are drawn from the wave prior to separation for months unemployed and from the prior wave for months employed.

β_1 provides an estimate of the fall in private insurance coverage absent state UI. β_2 measures the effect of UI generosity on the probability that an individual reports private health insurance coverage across months employed and months unemployed. β_3 estimates the sensitivity of the effect of UI generosity on the probability that an individual reports private health insurance coverage across months unemployed relative to months employed. Results of Section 1.5.4 suggest that eligible UI generosity significantly increases the probability of private insurance coverage amongst the unemployed, but not amongst months employed for workers experiencing UI-eligible unemployment at some point within the respective panel. Results above suggest that $\beta_1 < 0$ as unemployment is associated with a dramatic fall in private coverage in Section 1.4.3, $\beta_2 \approx 0$ given limited sensitivity of the employed observations to UI generosity in Section 1.5.4, and that $\beta_3 > 0$ given sensitivity of private coverage to UI generosity amongst the unemployed in Section 1.5.2.

Results for the eligible replacement rate measure of UI generosity are presented in Table 10. Under the simulated instruments approach, there is a dramatic decline in private coverage associated with unemployment, a highly statistically significant fall of 20.1 percentage points in specification (8). Although increased generosity of UI as captured through the replacement rate is associated with an increase in private coverage, the result is diminutive in comparison to the effect of increased generosity conditional upon unemployment. Specifically, a 1 percentage point increase in the UI replacement rate is estimated to increase private coverage by .04 percentage points irrespective of employment status, though this result is statistically insignificant. By comparison, a 1 percentage point increase in the UI replacement rate is estimated to increase private coverage by .21 percentage points for the unemployed relative to the employed, and the result is marginally significant. Thus the employed are only approximately 16 percent as responsive to UI generosity as the unemployed, consistent with the results of Section 1.5.4. Scaled by the implied fall in private coverage absent UI, these results suggest that existing levels of UI generosity mitigate roughly 52.7 percent of the fall in private coverage.[56]

Comparable inference is drawn from the results for the eligible weekly benefit amount alternative measure of UI generosity presented in Table 11. Absent UI, private coverage is estimated to fall by a highly statistically significant 16.6 percentage points under the simulated instruments approach in specification (8). A \$100 increase in the UI weekly benefit amount increases private coverage by 1.62 percentage points irrespective of employment status, though this result is insignificant. By comparison, conditional upon unemployment, a \$100 increase in the UI weekly benefit amount increases the probability of private coverage by 5.46 percentage points relative to months employed, and this result is highly statistically significant. Scaled by the implied fall in private coverage across employment status absent UI, these results suggest that existing UI generosity levels mitigate roughly 56.7 percent of the fall in private coverage,

[56] Specifically, $\frac{.2114 \cdot .5011}{|-.2009|} \approx .527$. This calculation only accounts for the heterogeneous effect of UI generosity across employment status, as the effect of UI generosity irrespective of employment status does not differentially affect the unemployed population, thus cannot offset the fall in private coverage associated with unemployment.

similar to the 53.8 percent estimated under the replacement rate measure of UI generosity.

1.5.6 Collapsed Spells

Documentation of duration endogeneity to UI generosity (Meyer 1990, Chetty 2008) suggests a potential sample selection bias inherent to the person-month analysis above. To avoid over-weighting long spells, which presumably appear disproportionately in response to increased UI generosity, spells of unemployment are collapsed to a single observation. Similarly, spells are weighted by the reciprocal number of spells per individual, such that the sum of an individual's weights equals 1, to avoid over-weighting short, repeated spells. This approach is appealing as variation in UI generosity is naturally drawn from across spells, as benefits are fixed within a spell, conditional on take-up. Collapsed models are of the form:

$$\overline{Private_{ist}} = \beta RR_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{1.4}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-spell level. $\overline{Private_{ist}}$ is the average private health insurance coverage rate for an individual across the unemployment spell, constructed as the fraction of months insured across the spell. R_{it} is a measure of an individual's eligible wage replacement rate under the relevant state UI system and varies continuously between 0 and 1. z_{it} is a vector of individual and job characteristics.[57] α_s is a vector of state indicators. δ_t is a vector of panel, year, and calendar month indicators. Standard errors are clustered to accommodate an arbitrary variance-covariance matrix within each state, given correlation across repeated spells as well as across couples residing within a state.

Results are of the collapsed model are reported in Table 12 alongside baseline results. Although point estimates are somewhat smaller in magnitude than the baseline specifications, patterns are consistent and significance is retained. Evaluating the simulated instruments approach, the collapsed model in specification (6) reports an increase in average private coverage across the spell of .22 percentage points associated with a 1 percentage point increase in the eligible UI replacement rate, compared to an increase of .30 percentage points in the baseline specification re-produced in specification (5). This suggests that the effect of UI generosity in mitigating the fall in private coverage is approximately 73.3 percent as large under the collapsed specification with respect to the replacement rate measure of UI generosity. Similarly, an increase of $100 in the eligible UI weekly benefit amount is associated with an increase in average private coverage across the spell of 5.53 percentage points, compared to an increase of 7.64 percentage points in the baseline specification re-produced in specification (7). This suggests that the effect of UI generosity in mitigating the fall in private coverage is approximately 72.4 percent as large

[57] Controls are equivalent to Equation (1.2). Controls are drawn from the wave prior to separation.

under the collapsed specification with respect to the weekly benefit amount measure of UI generosity. This diminished magnitude can be explained in part by the re-weighting of spells away from long spells of unemployment as under person-month observations and towards the representative spell.

1.6 Asset Heterogeneity

1.6.1 Identifying Liquidity Constrained Households

Browning and Crossley (2001), Bloemen and Stancanelli (2005), and Sullivan (2008) detail consumption drops during unemployment mitigated by UI generosity for households with little financial assets, though limited sensitivity amongst households with higher asset holdings. Chetty (2008) finds similar evidence with respect to hazard rates for leaving unemployment. In related work, Blundell, Pistaferri, and Preston (2008) find that consumption-income comovement is pronounced for households with low asset holding. A natural extension, then, of the above framework is to evaluate heterogeneous sensitivity of private insurance coverage across spells of unemployment to UI generosity amongst plausibly liquidity constrained households relative to liquidity unconstrained households.

The SIPP is designed to provide a broader context for analysis by incorporating supplemental data contained within 'topical modules', uniquely matched to individuals within the 'core' dataset. Though the SIPP contains no direct measure of a household's access to credit markets, SIPP respondents are interviewed about detailed household wealth holdings at a single interview point in the 1990-1993 panels, and once annually in the 1996 and 2001 panels. As a result, pre-separation asset data are available for approximately 50 percent of these unemployment spells. I include only observations for which I observe asset holdings prior to the point of separation, to avoid issues of asset draw-down during an unemployment spell, which may respond to the level of UI generosity. Although the SIPP imputation methodology has been criticized (Curtin et al. 1989, Hoynes et al. 1998), non-random imputation suggests potential bias from the exclusion of imputed values, thus I retain these observations.[58]

I focus on net liquid wealth as the primary proxy for liquidity constraint. Following Chetty (2008), I define net liquid wealth as gross liquid wealth less unsecured debt.[59] Although substantial unsecured debt may limit a household's ability to finance an unemployment spell, unsecured debt may alternatively reflect a household's access to unsecured borrowing. This motivates alternative consideration of gross financial wealth as a proxy for liquidity constraint.

[58] Gruber (2001) finds no systematic difference in results owing to exclusion of imputed results, though wealth adequacy is approximately 50 percent lower, reflecting the non-random imputation assignment.

[59] Liquid wealth is defined as total wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debts.

A secondary proxy for liquidity constraint is mortgage status prior to separation. Gruber (1998) finds that less than 5 percent of the unemployed sell their homes during an unemployment spell, in contrast to high mobility amongst renters. A household burdened with mortgage payments prior to job loss has a fixed consumption obligation, limiting the household's ability to smooth other forms of consumption (Chetty and Szeidl 2007). Yet this consumption commitment results in heightened risk aversion over moderate losses, thus a mortgage may result in additional valuation of health insurance for a household. For both imperfect proxies of liquidity constraint, misclassification will bias the sensitivity differential towards zero, underestimating the potential liquidity effect.

Descriptive Statistics

Descriptive statistics across these heterogeneous groups are presented in Table 13.[60] The most striking statistic is that the median unemployed household has only \$175 in net liquid wealth prior to separation, though the distribution is heavily skewed, with mean reporting of \$19,450. Net liquid wealth is non-monotonic in gross liquid wealth, as the first quartile reports \$422 in median liquid wealth relative to \$6,923 in unsecured debt, compared to the second quartile with \$0 in median liquid wealth, but only \$80 in median unsecured debt. In spite of heterogeneity in asset holdings of the household, the average UI replacement rates and average real weekly benefit amounts are similar across the net liquid wealth quartiles. Comparison of the second and fourth quartiles reveals the largest disparities; the average UI replacement rate ranges from 46.1 percent within quartile 4 to 53.7 percent within quartile 2, and corresponding average UI weekly benefits amounts are reported as \$203 and \$146, respectively.

With respect to health insurance coverage, the first quartile of net liquid wealth closely mirrors the pooled sample, reporting an average private coverage across spells of 61.5 percent in comparison to the pooled sample's average private coverage of 62.5 percent. Although the second quartile is roughly 38 percent less likely to have private health insurance across an unemployment spell compared to the pooled sample, these individuals are similarly 32 percent less likely to report private health insurance coverage prior to separation. Correspondingly, public insurance coverage amongst the second quartile of 7.2 percent prior to separation is more than twice that of the pooled sample, 3.0 percent. As quartiles of net liquid wealth vary substantively along more dimensions than simply asset holdings, I therefore stratify across proxies for liquidity constraint and consumption commitment rather than estimate a joint model with commonly identified covariates.

Graphical Evidence

[60] Gruber (2001) finds that the median worker holds sufficient gross financial assets to cover roughly two-thirds of income loss from an unemployment spell, though the extremely skewed distribution suggests that one-third of workers are unable to replace even 10 percent of the income loss. The implication of incorporating net liquid assets suggests further inadequacy of private savings at the median, though the distribution remains highly skewed.

Figures 8-10 present graphical evidence of the heterogeneous effect of UI generosity in mitigating the fall in private health insurance coverage, stratifying across each proxy for liquidity constraint or consumption commitment. Figure 8 estimates the extended version of Equation (1.1) separately by above- and below-median UI state-year generosity and above- and below-median net liquid wealth prior to separation. These results suggest that the differential in private coverage with respect to UI generosity is amplified for separations proxied as liquidity constrained, those with below-median net liquid wealth, and muted for separation proxied as liquidity unconstrained, those with above-median net liquid wealth. Population heterogeneity is expressed through the mitigated fall in private coverage for both high and low generosity proxied liquidity unconstrained households in comparison to both high and low generosity proxied liquidity constrained households. A similar relationship holds for the gross liquid wealth proxy of liquidity constraint in Figure 9.

Figure 10 estimates the extended version of Equation (1.1) separately by above- and below-median UI state-year generosity and across mortgage status prior to separation. Unlike the asset proxies, under the mortgage proxy of consumption commitment separations proxied as constrained exhibit a mitigated fall in private coverage in comparison to the unconstrained. This reflects in part the higher average educational attainment and earnings of the constrained group. As above, the constrained group exhibits amplified sensitivity to state-year UI generosity in comparison to the unconstrained group, though the comparison is somewhat less clear in the context of the mortgage proxy. A more rigorous treatment is detailed below.

1.6.2 Stratification Across Proxies

To evaluate heterogeneity in the sensitivity of private coverage to UI generosity, I stratify across proxies for liquidity constraint and consumption commitment, estimating collapsed models modified from Equation (1.4). I further incorporate a linear control for total (illiquid and liquid) household wealth. Table 14 presents results stratified by above- or below-median net liquid wealth level prior to separation. Along both measures of UI generosity, the UI replacement rate and the UI weekly benefit amount, the effect of UI generosity on private health insurance coverage during an unemployment spell is concentrated within the constrained group, with positive, significant coefficients, while coefficients for the unconstrained group are of diminutive magnitude and insignificant. Specifically, under the simulated instruments approach, a 1 percentage point increase in the UI replacement rate is estimated to increase private insurance coverage amongst the unemployed by .23 percentage points for the constrained group in specification (2) and .06 percentage points for the unconstrained group in specification (6). Thus, the unconstrained separators are estimated to be approximately 26.1 percent as responsive as the constrained separators. Similarly, in terms of the increase in private coverage associated with a \$100 increase in the UI weekly benefit amount in specifications (4) and (8), the unconstrained group is approximately 21.3 percent as responsive as the constrained group.

Alternatively, I consider stratifying across net liquid wealth quartile, rather than above- and below-median distinction.[61] Results are presented in Table 15 and provide suggestive evidence of a liquidity effect concentrated amongst the first quartile of net liquid wealth. Point estimates are monotonically decreasing across net liquid wealth quartiles for both measures of UI generosity, across both OLS and 2SLS specifications. As quartile 1 closely mirrors the pooled sample with exception of substantial unsecured debts, it is unlikely that this effect is driven by population heterogeneity, supporting interpretation of the results above as suggestive evidence of an associated liquidity effect.

As noted above, it is possible that negative net liquid wealth proxies not for liquidity constraint, but rather for access to unsecured borrowing, as might allow an individual to smooth across transitory income shocks such as unemployment. As verification, then, I alternatively stratify across the gross liquid wealth proxy, which produces comparable inference as presented in Table 16. Under the simulated instruments approach, a 1 percentage point increase in the UI replacement rate is associated with an increase in private coverage amongst the unemployed by .28 percentage points for the constrained group in specification (2) and .07 percentage points for the unconstrained group in specification (6), a sensitivity ratio of approximately 26.0 percent. Similarly, in terms of the increase in private coverage associated with a $100 increase in the UI weekly benefit amount in specifications (4) and (8), the unconstrained group is approximately 20.9 percent as responsive as the constrained group.

Similarity of results under the net liquid wealth proxy in Table 17 and the gross liquid wealth proxy in Table 15 is not surprising, as the majority of separations are consistently coded as above- or below-median under either proxy of liquidity constraint. Dissimilarly, the constrained types under the mortgage proxy, resulting from consumption commitment, have higher income, educational attainment, and private health insurance coverage than the unconstrained types, who are primarily renters. Separations with a mortgage face a fixed consumption obligation, limiting the household's ability to smooth health insurance coverage as the household faces plausibly more costly choices while experiencing low transitory income. Results of stratification across mortgage status prior to separation are presented in Table 16. These results are consistent with use of net liquid wealth as the proxy for liquidity constraint. In terms of the simulated instruments approach, a 1 percentage point increase in the UI replacement rate is associated with a .29 percentage points increase in private coverage amongst the constrained group in specification (2), compared to an increase of .14 percentage points for the unconstrained group in specification (6). These results imply that the unconstrained group is approximately 46.6 percent as responsive as the constrained group. Similarly, comparison of the effect of a $100 increase in the UI weekly benefit amount in specifications (4) and (8) suggest that the unconstrained group is approximately 35.2 percent as responsive as the constrained group. This provides a reasonable cross-check that the heterogeneous effects of UI generosity

[61] Stratification across finer units places too large a strain on the data.

on private health insurance coverage is not spuriously driven by other distinctions across the groups. These results conform to the graphical evidence, where net liquid wealth and gross liquid wealth proxies provide a more clear distinction in sensitivity across the unconstrained and constrained groups in relation to the mortgage proxy.

These results demonstrate that the interaction effect of UI generosity and liquidity constraint or consumption commitment in terms of maintaining private health insurance coverage amongst the unemployed is negative, illustrating a potential excess sensitivity to cash-in-hand for constrained sub-populations. As UI generosity affects private coverage solely through an income effect, I am therefore observing the liquidity effect directly through the heterogeneity in responsiveness detailed above. However, statistical imprecision renders these results merely suggestive of an associated liquidity effect.

1.7 Conclusion

Although previous work concerning the effectiveness of continuation mandates have found at most modest effects with respect to increasing private health insurance coverage amongst the unemployed, these studies have failed to address the issue of ability to pay for a household with an unemployed member experiencing low transitory income. I find a dramatic fall in private health insurance coverage for prime-age males experiencing unemployment, despite federally mandated continuation benefits throughout the reference period. However, introduction of UI generosity into the framework reveals that current UI generosity mitigates approximately 41 to 44 percent of the fall in private health insurance coverage that would occur absent UI benefits. Stratification across proxies for a household's access to credit reveals concentration patterns consistent with an associated liquidity effect. Separations proxied as liquidity unconstrained exhibit limited sensitivity to UI generosity, with a measured response only 21-26 percent as large as for the proxied liquidity constrained separation.

These results suggest that policies relying upon non-advanceable tax subsidies, requiring enrollees to pay up-front for coverage and only later receive a tax credit, will likely have limited efficacy relative to policies that directly subsidize the purchase price of health insurance for the unemployed. A recent study (Hewitt Associates Inc. 2009) found that the percentage of involuntarily terminated employees opting for COBRA coverage increased from an average of 19 percent between September 2008 through February 2009 to 38 percent between March 2009 through June 2009, effectively doubling the program participation rate. Eligible workers receive a nine-month 65 percent subsidy, effectively decreasing COBRA continuation costs for the typical worker from \$8,800 annually to only \$3,000 annually, similar to the \$1,900 annual directly subsidized cost incurred by the typical worker with employer-sponsored health coverage. This participation rate ignores workers seeking coverage with a new employer or through a spouse's employer, thus under-reporting take-up of the targeted population. Regardless, this suggests that the American Recovery and Reinvestment Act of 2009 subsidy, which reduced the immediate purchase price for eligible job separators, may have invoked a substantially larger behavioral response than the 2002 Health Coverage Tax Credit, which provided a refundable, but not advanceable, tax credit.

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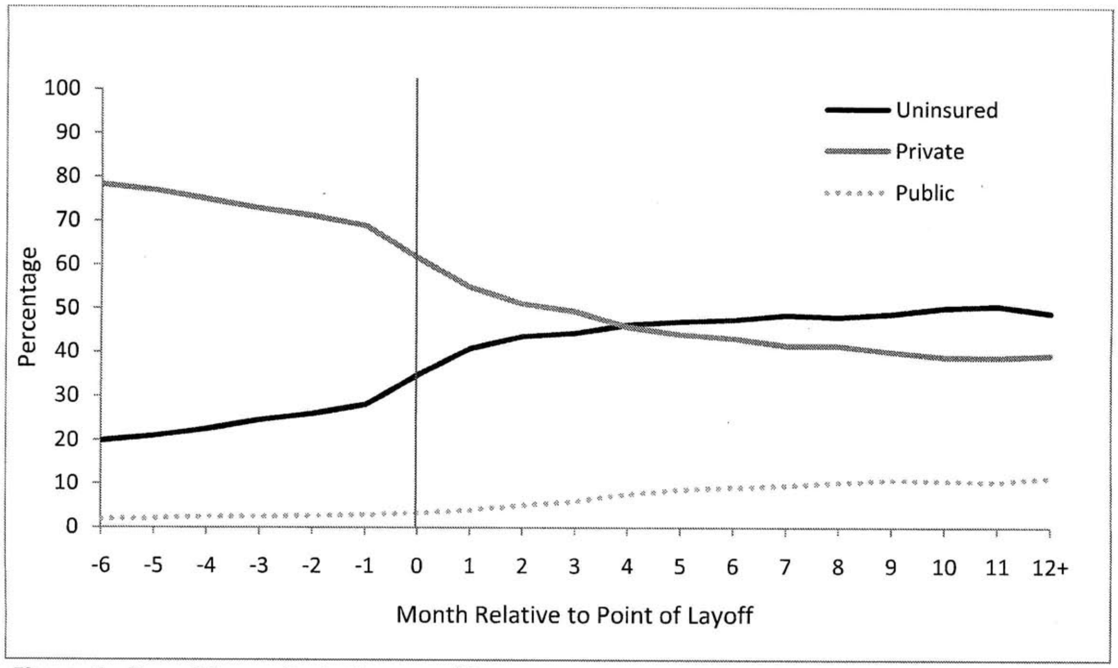

Figure 1. Transitions - Basic Source of Coverage.

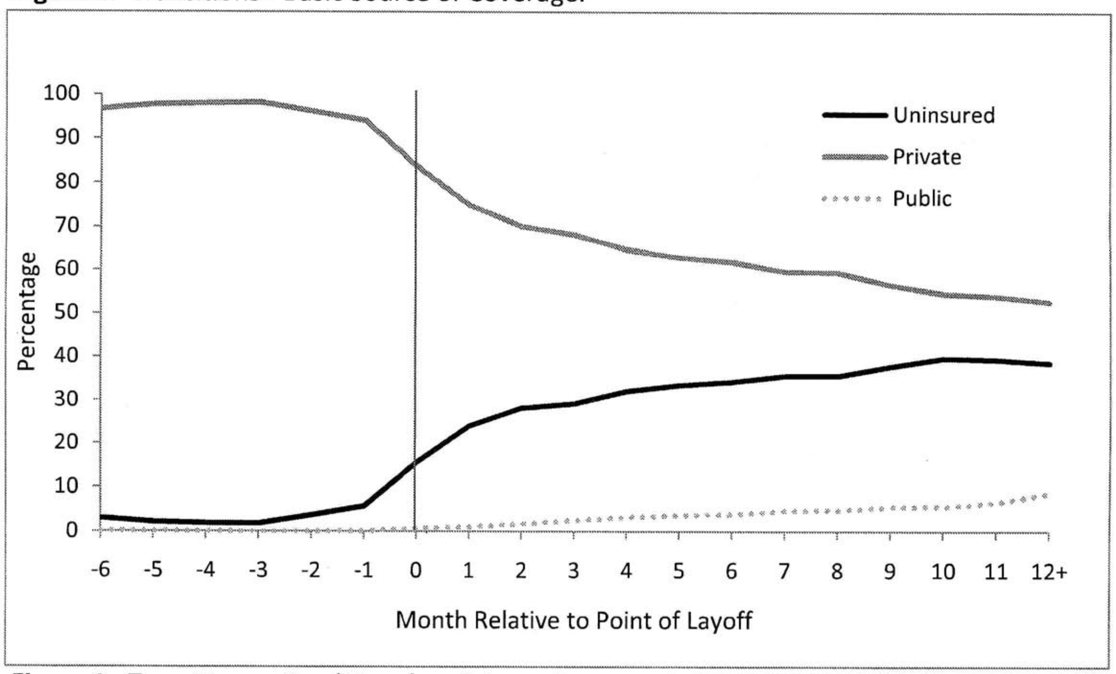

Figure 2. Transitions - Conditional on Private Coverage in Wave Prior to Separation.

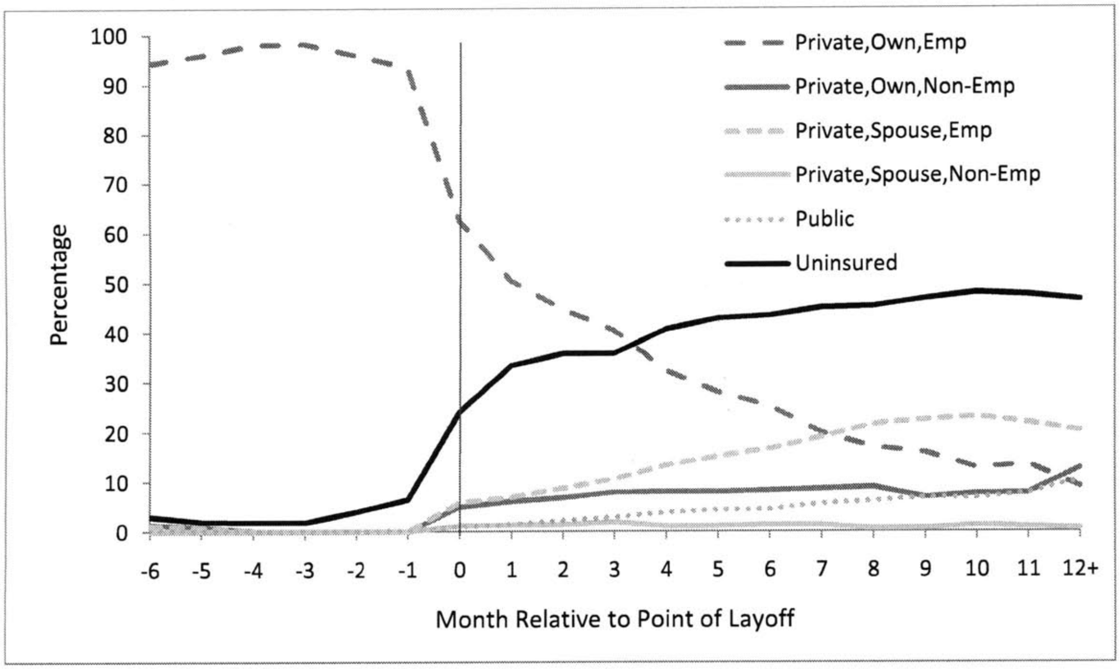

Figure 3. Transitions - Own Employer Coverage Prior to Separation.

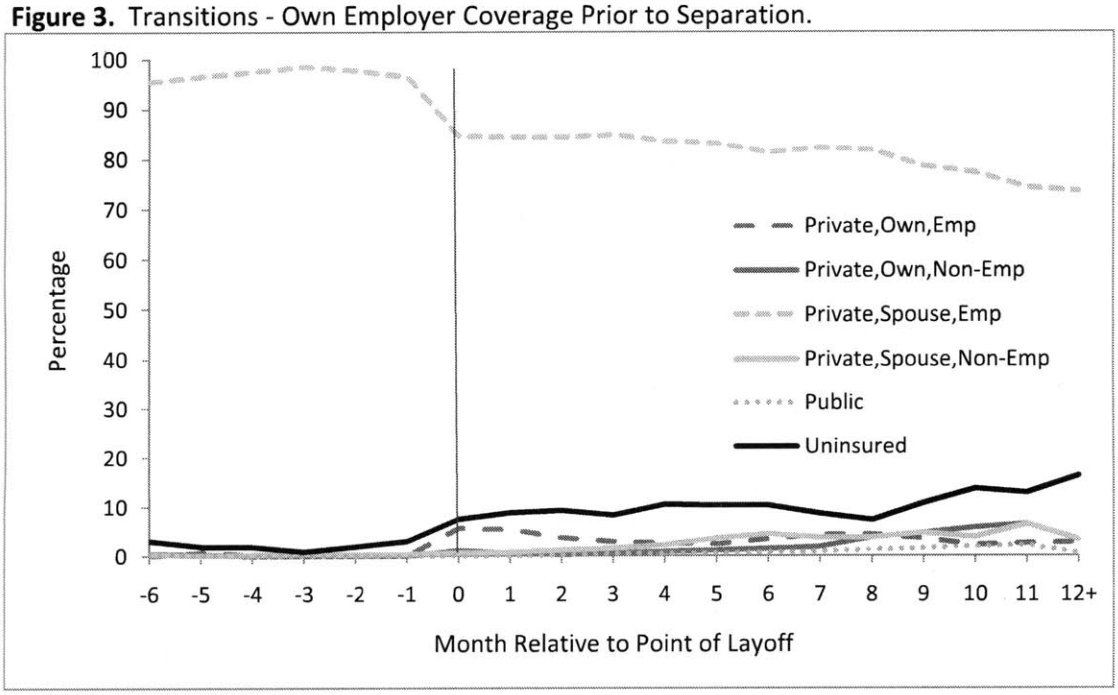

Figure 4. Transitions - Spousal Employer Coverage Prior to Separation.

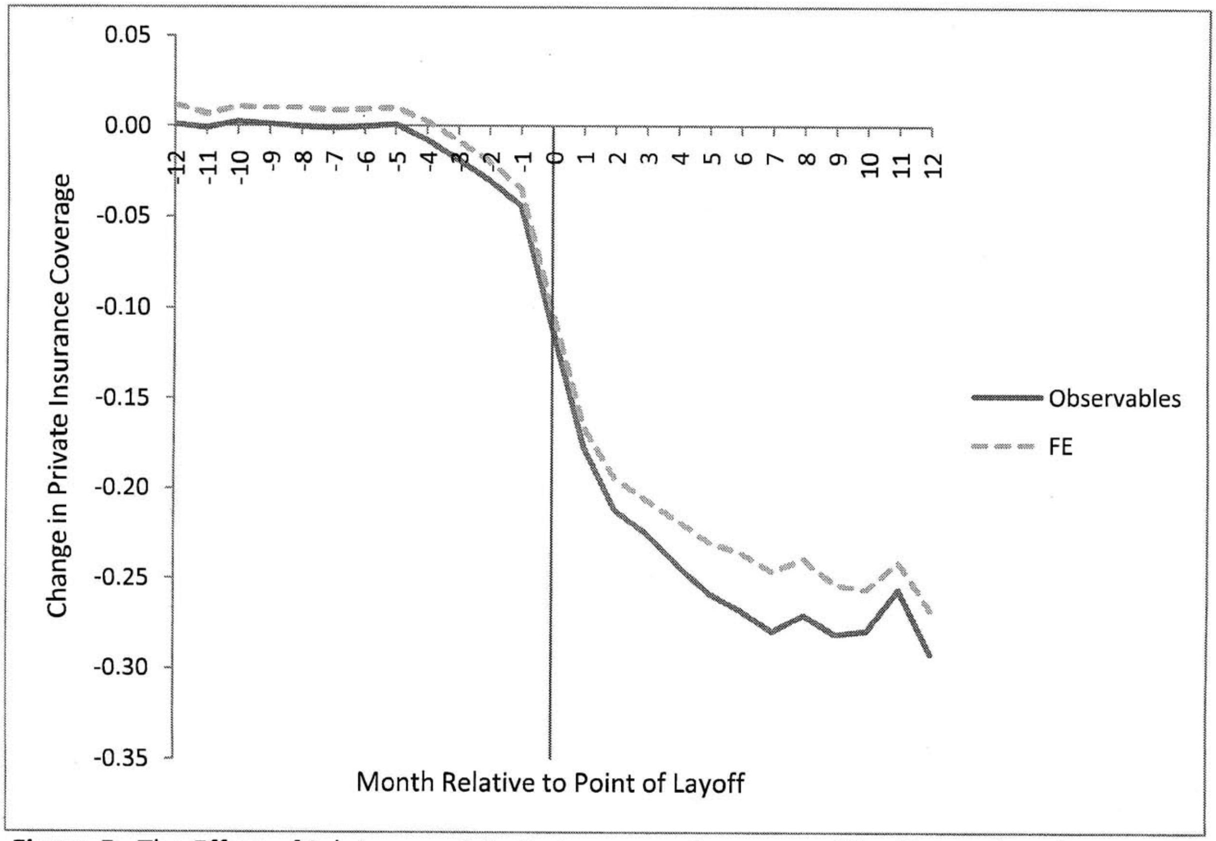

Figure 5. The Effect of Job Loss on Private Insurance Coverage of the Unemployed.

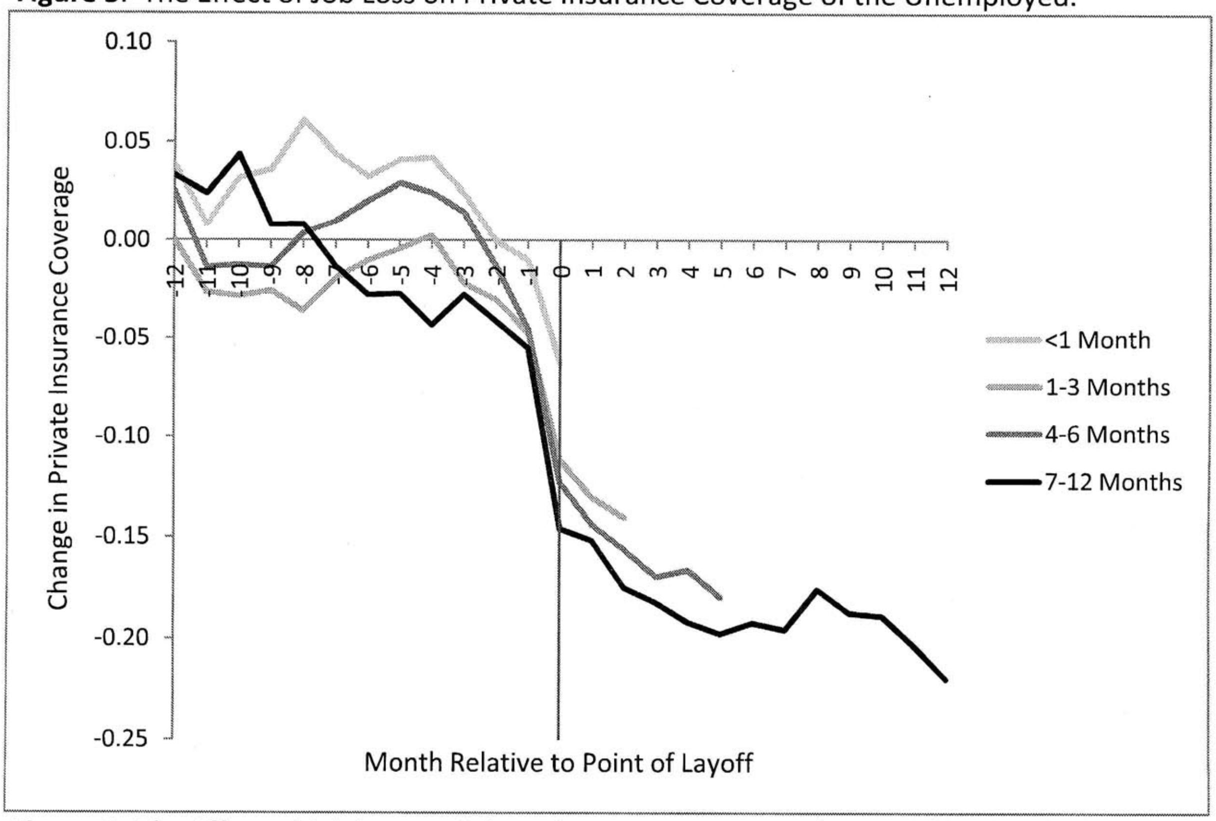

Figure 6. The Effect of Job Loss on Private Insurance Coverage by Completed Duration.

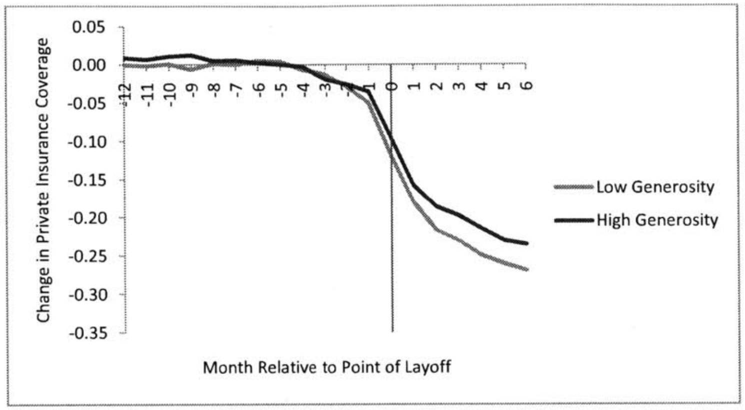

Figure 7. Heterogeneous Effect of Unemployment by UI Generosity.

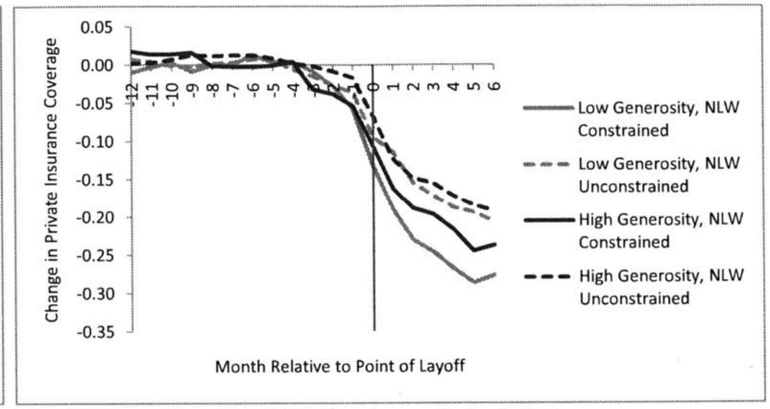

Figure 8. Heterogeneous Effect of UI Generosity by NLW Proxy.

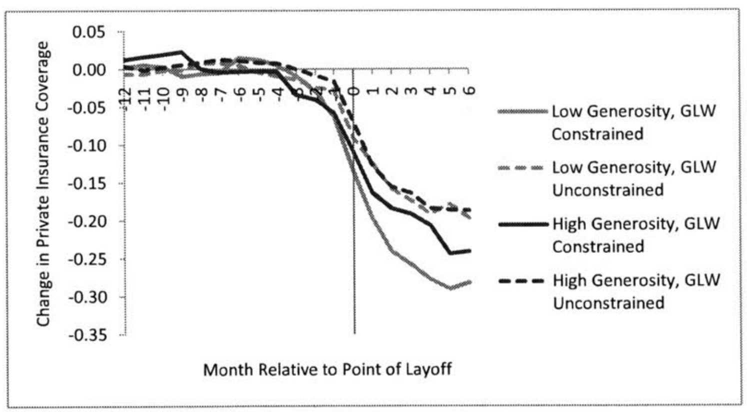

Figure 9. Heterogeneous Effect of UI Generosity by GLW Proxy.

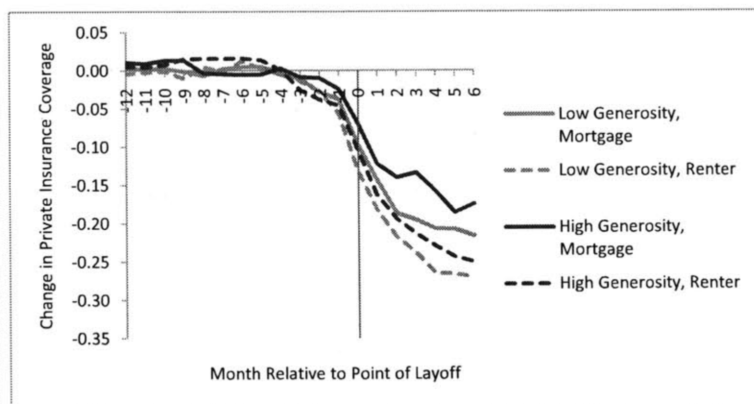

Figure 10. Heterogeneous Effect of UI Generosity by Mortgage Proxy.

Table 1

Summary Statistics Across Samples of Interest

	Employed	Job Separation Sample: Non-Employed	Unemployed	UI-Eligible
	(1)	(2)	(3)	(4)
Health Insurance Coverage				
Mean (Private HI) Across Spell	--	60.7	59.0	60.6
Private HI	90.2	72.8	71.8	75.8
Private HI in Own Name	81.8	61.1	60.2	62.6
Public HI	0.9	3.2	3.1	2.6
Employer-Provided HI	77.2	54.9	54.4	57.0
Demographics				
Age	39.3	38.2	38.2	38.3
Married	81.7	73.2	74.6	74.9
Working Spouse	57.2	48.4	48.6	48.7
Non-White	15.8	22.1	22.4	21.7
Number of Children	1.2	1.1	1.1	1.1
Education: Less than HS	7.6	15.4	16.8	16.3
Education: HS Graduate	29.9	36.2	38.4	38.4
Education: Some College	27.1	26.0	25.8	26.2
Education: 4+ Years of College	35.3	22.4	19.0	19.2
Annual Earnings	$29,767	$20,046	$19,326	$20,572
Median Annual Earnings	$25,396	$16,549	$16,279	$17,338
Completed Spell Durations (Weeks)				
Duration	--	17.0	14.6	14.4
Median Duration	--	10.0	9.0	8.0
UI Benefits				
UI Replacement Rate	--	--	--	50.1
Real WBA	--	--	--	$172
Spells	72,965	15,876	10,280	7,669

Table entries are mean values unless noted otherwise. Statistics are based upon the wave prior to separation unless noted otherwise, with exception of the employed sample, for which values are based upon the first wave in which the individual establishes sufficient earnings history to impute earnings. Data are drawn from the 1990, 1991, 1992, 1993, 1996, and 2001 SIPP panels. Left-censored spells of non-employment and unemployment (in-progress at the start of the panel), are excluded. UI weekly benefit amount is simulated per discussion in Section 5.1. UI replacement rate is constructed as the eligible weekly benefit amount divided by weekly pre-separation wage. Duration is defined as weeks elapsed from end of last job to start of next job and does not adjust for right-censoring (spells in-progress at the end of the panel). All monetary values are in real 1990 values.

Table 2

Effect of Unemployment on Private Health Insurance Coverage

Population	Experience Unemployment		Experience UI-Eligible Unemployment		Experience Non-Employment		Employed and Non-Employed	
Mean Dependent	.731	.731	.737	.737	.746	.746	.887	.887
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployed	-.1895***	-.1610***	-.1913***	-.1611***	-.1706***	-.1444***	-.1857***	-.1478***
	(.0066)	(.0018)	(.0070)	(.0018)	(.0054)	(.0014)	(.0062)	(.0009)
Age 30-34	.0102**	--	.0093**	--	.0088**	--	.0063***	--
	(.0043)	--	(.0042)	--	(.0037)	--	(.0014)	--
Age 35-39	.0101**	--	.0093*	--	.0114***	--	.0102***	--
	(.0049)	--	(.0050)	--	(.0033)	--	(.0019)	--
Age 40-44	.0153**	--	.0150**	--	.0161***	--	.0111***	--
	(.0064)	--	(.0066)	--	(.0052)	--	(.0024)	--
Age 45-49	.0086	--	.0090	--	.0111**	--	.0098***	--
	(.0055)	--	(.0057)	--	(.0051)	--	(.0019)	--
Age 50-54	.0221***	--	.0222***	--	.0284***	--	.0164***	--
	(.0060)	--	(.0058)	--	(.0051)	--	(.0020)	--
Non-White	-.0231***	--	-.0250***	--	-.0326***	--	-.0143***	--
	(.0047)	--	(.0048)	--	(.0048)	--	(.0019)	--
Married	.0238***	-.0055	.0238***	.0010	.0304***	.0033	.0058***	.0026
	(.0072)	(.0097)	(.0078)	(.0099)	(.0056)	(.0074)	(.0027)	(.0025)
Spouse Works	.0697***	.0404***	.0693***	.0408***	.0658***	.0331***	.0256***	.0162***
	(.0047)	(.0027)	(.0049)	(.0028)	(.0036)	(.0022)	(.0023)	(.0007)
Kids: 1	-.0054	--	-.0040	--	-.0044	--	-.0027**	--
	(.0044)	--	(.0045)	--	(.0048)	--	(.0012)	--
Kids: 2	-.0166***	--	-.0156***	--	-.0136***	--	-.0054***	--
	(.0042)	--	(.0044)	--	(.0039)	--	(.0010)	--
Kids: 3	-.0318***	--	-.0303***	--	-.0298***	--	-.0107***	--
	(.0052)	--	(.0054)	--	(.0047)	--	(.0016)	--
Kids: 4+	-.0531***	--	-.0520***	--	-.0507***	--	-.0218***	--
	(.0083)	--	(.0085)	--	(.0079)	--	(.0028)	--
HS Graduate	.0267***	--	.0274***	--	.0314***	--	.0260***	--
	(.0056)	--	(.0061)	--	(.0063)	--	(.0039)	--
Some College	.0364***	--	.0362***	--	.0450***	--	.0333***	--
	(.0063)	--	(.0067)	--	(.0059)	--	(.0044)	--
College Graduate	.0593***	--	.0592***	--	.0699***	--	.0394***	--
	(.0075)	--	(.0079)	--	(.0073)	--	(.0043)	--
Prior Private HI	.6411***	--	.6394***	--	.6323***	--	.7073***	--
	(.0073)	--	(.0072)	--	(.0066)	--	(.0058)	--
Wage Splines	X	X	X	X	X	X	X	X
Year/Month Fixed Effects	X	X	X	X	X	X	X	X
State Fixed Effects	X		X		X		X	
Occupation/Industry Effects	X		X		X		X	
Individual Fixed Effects		X		X		X		X
Observations	163,918	163,918	154,502	154,502	248,082	248,082	973,714	973,714

The dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (1). Observations are person-month level. Sample restrictions for each specification are detailed in Section 4 and Section 5. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001. Omitted categories: Age 25-29, White, No Kids, Unmarried, Non-Working Spouse, Less than HS.

Table 3
Evaluating Duration Dependence

	Experience Unemployment		Completed Duration of Unemployment Spell			
	Observables	Individual FE	<1 Month	1-3 Months	4-6 Months	7-12 Months
Mean Dependent	.731	.731	.664	.631	.594	.475
Specification	(1)	(2)	(3)	(4)	(5)	(6)
12 Months Before	.0011	.0121***	.0387**	.0005	.0250	.0331
	(.0028)	(.0035)	(.0166)	(.0157)	(.0150)	(.0257)
11 Months Before	-.0008	.0069*	.0082	-.0265	-.0137	.0238
	(.0040)	(.0042)	(.0196)	(.0193)	(.0172)	(.0323)
10 Months Before	.0027	.0108***	.0319*	-.0285	-.0126	.0436
	(.0038)	(.0040)	(.0193)	(.0187)	(.0171)	(.0304)
9 Months Before	.0013	.0101**	.0359*	-.0258	-.0135	.0082
	(.0043)	(.0042)	(.0185)	(.0179)	(.0167)	(.0297)
8 Months Before	-.0001	.0101***	.0610***	-.0360**	.0038	.0086
	(.0035)	(.0037)	(.0182)	(.0171)	(.0163)	(.0262)
7 Months Before	-.0009	.0089**	.0436**	-.0191	.0102	-.0133
	(.0042)	(.0036)	(.0171)	(.0166)	(.0155)	(.0256)
6 Months Before	-.0001	.0094***	.0324**	-.0101	.0201	-.0279
	(.0029)	(.0030)	(.0149)	(.0134)	(.0142)	(.0223)
5 Months Before	.0011	.0102***	.0410**	-.0041	.0292**	-.0272
	(.0032)	(.0034)	0.0161	(.0153)	(.0146)	(.0250)
4 Months Before	-.0076**	.0029	.0419***	.0027	.0241*	-.0432**
	(.0036)	(.0032)	(.0149)	(.0142)	(.0130)	(.0214)
3 Months Before	-.0186***	-.0081**	.0237	-.0218	.0144	-.0276
	(.0041)	(.0034)	(.0145)	(.0137)	(.0123)	(.0207)
2 Months Before	-.0301***	-.0194***	.0121	-.0303**	-.0129	-.0412**
	(.0036)	(.0030)	(.0137)	(.0132)	(.0118)	(.0195)
1 Months Before	-.0441***	-.0348***	-.0100	-.0471***	-.0451***	-.0549***
	(.0034)	(.0029)	(.0131)	(.0125)	(.0113)	(.0179)
Month of JobSeparation	-.1112***	-.1015***	-.0624***	-.1116***	-.1230***	-.1457***
	(.0045)	(.0029)	(.0210)	(.0151)	(.0127)	(.0231)
1 Month After	-.1766***	-.1656***	--	-.1303***	-.1439***	-.1516***
	(.0073)	(.0034)	--	(.0151)	(.0127)	(.0230)
2 Months After	-.2118***	-.1941***	--	-.1403***	-.1558***	-.1746***
	(.0069)	(.0039)	--	(.0162)	(.0126)	(.0230)
3 Months After	-.2248***	-.2060***	--	--	-.1692***	-.1820***
	(.0076)	(.0045)	--	--	(.0142)	(.0230)
4 Months After	-.2427***	-.2178***	--	--	-.1659***	-.1918***
	(.0104)	(.0056)	--	--	(.0164)	(.0230)
5 Months After	-.2582***	-.2297***	--	--	-.1792***	-.1976***
	(.0113)	(.0062)	--	--	(.0201)	(.0230)
6 Months After	-.2675***	-.2355***	--	--	--	-.1922***
	(.0112)	(.0070)	--	--	--	(.0230)
7 Months After	-.2790***	-.2458***	--	--	--	-.1955***
	(.0122)	(.0078)	--	--	--	(.0235)
8 Months After	-.2701***	-.2390***	--	--	--	-.1754***
	(.0126)	(.0091)	--	--	--	(.02483)
9 Months After	-.2807***	-.2532***	--	--	--	-.1871***
	(.0144)	(.0098)	--	--	--	(.0259)
10 Months After	-.2787***	-.2560***	--	--	--	-.1886***
	(.0167)	(.0106)	--	--	--	(.0279)
11 Months After	-.2560***	-.2409***	--	--	--	-.2032***
	(.0238)	(.0059)	--	--	--	(.0274)
12+ Months After	-.2914***	-.2672***	--	--	--	--
	(.0326)	(.0149)	--	--	--	--
Observations	163,918	163,918	16,416	21,836	30,704	20,044

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (1). Observations are person-month level. The population includes individuals experiencing at least one unemployment spell. Subpopulation totals do not sum to the pooled sample as a result of category exclusion (12+ Months) and non-assignment of observations following an unemployment spell. Except for the first column, all specifications estimate the indivial fixed effect model. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 4
Unemployment Insurance Generosity by State

State	Number of Spells	Mean Weekly Benefit Amount	Standard Deviation of WBA	Mean Simulated Instrument (WBA)	Standard Deviation of SI (WBA)	Mean Replacement Rate	Standard Deviation of RR	Mean of Simulated Instrument (RR)	Standard Deviation of SI (RR)
Alabama	121	126.97	29.24	127.95	4.16	46.74	17.41	42.37	2.32
Arizona	181	137.37	26.35	138.93	5.39	48.63	20.75	43.67	2.65
Arkansas	53	156.73	63.60	174.08	9.04	55.57	13.67	50.99	3.51
California	1123	152.03	49.91	151.42	9.97	44.51	18.79	44.67	3.42
Colorado	109	159.98	74.61	162.47	14.35	45.79	13.21	46.02	4.28
Connecticut	115	224.78	72.67	201.36	7.63	51.98	16.50	55.43	3.10
Delaware	31	181.14	53.35	175.03	5.29	51.71	12.75	52.41	2.44
D.C.	18	184.09	66.80	205.97	14.65	67.40	17.17	57.44	3.98
Florida	392	151.44	53.32	162.40	7.48	49.25	13.00	46.04	3.29
Georgia	200	151.91	41.16	149.36	11.33	46.91	17.21	47.35	2.92
Hawaii	7	239.39	72.50	219.88	8.30	60.69	12.04	62.60	1.88
Illinois	458	185.86	71.80	174.27	9.53	49.64	16.90	51.29	3.31
Indiana	211	153.53	43.86	150.12	28.53	43.26	19.72	45.53	6.16
Kansas	72	181.84	52.48	184.30	8.08	53.55	16.84	52.25	2.36
Kentucky	123	154.55	63.39	165.01	16.01	50.25	14.87	48.81	1.76
Louisiana	137	133.03	42.42	130.91	7.17	40.43	11.88	40.11	0.76
Maryland	125	185.49	37.90	173.25	4.72	45.21	18.17	51.51	2.11
Massachusetts	184	233.98	85.64	224.76	12.59	57.30	15.68	59.45	2.73
Minnesota	253	196.71	67.34	193.65	12.38	56.40	17.23	54.13	3.28
Mississippi	152	130.60	27.92	133.26	4.15	46.52	16.23	42.17	2.03
Missouri	254	145.18	27.96	145.90	9.35	48.22	20.28	44.84	1.99
Nebraska	48	124.35	38.13	130.47	16.97	47.60	16.56	40.76	3.19
Nevada	34	165.06	47.66	165.26	7.09	56.10	16.29	49.94	3.05
New Hampshire	48	157.01	52.90	146.85	19.26	40.82	12.68	41.93	1.77
New Jersey	298	230.25	80.41	209.69	9.71	55.48	13.49	58.77	1.10
New Mexico	24	146.48	42.10	155.45	10.37	53.42	14.10	46.68	2.69
New York	574	180.39	74.11	172.60	16.00	47.40	10.68	47.91	2.65
North Carolina	272	173.26	60.64	183.75	14.76	52.07	12.73	52.26	3.62
Ohio	480	159.64	62.19	157.18	5.06	46.61	9.08	45.17	1.04
Oklahoma	95	170.29	43.20	176.03	9.62	50.49	17.66	49.94	2.86
Oregon	132	191.00	71.18	187.99	10.48	56.02	12.88	53.90	0.87
Pennsylvania	475	221.85	72.89	215.38	8.46	59.91	17.04	57.94	2.84
Rhode Island	37	277.14	49.52	230.37	9.11	59.37	16.15	62.07	2.77
South Carolina	87	150.85	39.80	153.40	6.79	48.64	16.32	47.33	2.74
Tennessee	203	140.59	42.15	149.78	12.97	49.99	17.85	45.51	3.03
Texas	659	162.41	57.46	176.81	6.69	54.83	18.08	51.46	2.51
Utah	31	177.81	62.58	179.51	12.52	55.23	16.41	51.47	2.93
Virginia	132	149.70	40.34	149.89	10.40	47.03	17.76	48.20	5.09
Washington	241	214.23	79.26	195.49	15.93	56.54	16.78	55.79	3.97
West Virginia	118	148.25	64.24	169.20	4.75	51.54	6.58	47.73	1.05
Wisconsin	220	187.30	46.32	173.58	12.24	50.35	15.21	50.41	3.99
United States	8,527	171.61	65.86	170.07	25.89	50.11	16.79	49.24	5.81

Spell counts indicate the number of UI-eligible unemployment spells observed within each uniquely identified state across the 1990, 1991, 1992, 1993, 1996, and 2001 SIPP panels. Eligibility includes both monetary and non-monetary components. Calculated weekly benefit amounts and replacement rates are constructed through simulation at the individual level, building upon the benefits/eligibility calculators developed in Gruber (1997) and modified by Chetty (2008). Discussion regarding construction of the simulated instruments is presented in Section 5.2. All monetary values are in real 1990 values.

Table 5

Unemployed Specification (RR)

	OLS				2SLS			
Mean Dependent	.535	.535	.535	.535	.535	.535	.535	.535
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	-.2286***	.1126**	.1060**	.1345*	.4798***	.3152***	.3008***	.2714
	(.0581)	(.0530)	(.0508)	(.0727)	(.1322)	(.1110)	(.1086)	(.2374)
Prior Private HI	.5796***	.4699***	.4692***	.4593***	.6600***	.4721***	.4715***	.4583***
	(.0188)	(.0148)	(.0147)	(.0148)	(.0202)	(.0142)	(.0141)	(.0151)
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Wage Splines		X	X	X		X	X	X
Demographics		X	X	X		X	X	X
Unemployment Rate			X	X			X	X
State Effects				X				X
Observations	30,208	30,208	30,208	30,208	30,208	30,208	30,208	30,208

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (2). Observations are person-month level and include only UI-eligible unemployed observations. The UI weekly benefit amount is simulated per discussion in Section 5.1. The UI replacement rate is constructed as the individual nominal weekly benefit amount divided by weekly pre-unemployment wage. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 6

Unemployed Specification (WBA)

	OLS				2SLS			
Mean Dependent	.535	.535	.535	.535	.535	.535	.535	.535
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Real UI WBA/100	.1012***	.0459**	.0445**	.0587*	.0982***	.0782***	.0764***	.0837
	(.0097)	(.0200)	(.0206)	(.0305)	(.0278)	(.0270)	(.0265)	(.0714)
Prior Private HI	.5460***	.4680***	.4675***	.4592***	.5478***	.4676***	.4671***	.4585***
	(.0142)	(.0145)	(.0144)	(.0146)	(.0165)	(.0141)	(.0140)	(.0145)
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Wage Splines		X	X	X		X	X	X
Demographics		X	X	X		X	X	X
Unemployment Rate			X	X			X	X
State Effects				X				X
Observations	30,208	30,208	30,208	30,208	30,208	30,208	30,208	30,208

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (2). Observations are person-month level and include only UI-eligible unemployed observations. The UI weekly benefit amount is simulated per discussion in Section 5.1. The UI wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on private health insurance coverage of the unemployed. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-.05, ***-0.001.

Table 7

Alternative Generosity Measure Comparison

	OLS			2SLS		
Mean Dependent	.557	.557	.557	.557	.557	.557
Specification	(1)	(2)	(3)	(4)	(5)	(6)
UI RR	.1121	--	--	.4341**	--	--
	(.0756)	--	--	(.1685)	--	--
Real UI WBA/100	--	.0487**	--	--	.0951**	--
	--	(.0242)	--	--	(.0393)	--
Nominal UI WBA/	--	--	.0401*	--	--	.0977**
Family Premium	--	--	(.0223)	--	--	(.0473)
Prior Private HI	.5318***	.5304***	.5307***	.5352***	.5302***	.5318***
	(.0230)	(.0225)	(.0225)	(.0233)	(.0223)	(.0224)
Year/Month Effects	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X
State Effects						
Observations	12,568	12,568	12,568	12,568	12,568	12,568

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (2). Observations are person-month level and include only UI-eligible unemployed observations. Supplemental data from the MEPS-IC are available only for the 1996-2003 period, thus all estimation is performed with the restricted reference period. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 8

Imperfect Controls (RR)

	OLS				2SLS			
		Controls				Controls		
	UI-Eligible Unemployed	Employed	Always Employed	UI-Ineligible	UI-Eligible Unemployed	Employed	Always Employed	UI-Ineligible
Mean Dependent	.535	.794	.935	.407	.535	.794	.935	.407
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	.1060**	.0135	0.0017	.0254	.3008***	.0296	-.00289	-.0398
	(.0508)	(.0130)	(.0015)	(.0842)	(.1086)	(.0310)	(.0030)	(.1634)
Prior Private HI	.4692***	.6928***	.7558***	.4394***	.4715***	.6929***	.7637***	.4378***
	(.0147)	(.0063)	(.0066)	(.0211)	(.0141)	(.0062)	(.0092)	(.0204)
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X			X	X			X
Wage Splines	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
State Effects								
Observations	30,208	124,294	725,582	19,990	30,208	124,294	725,582	19,990

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (2). Observations are at the person-month level. Prior Private HI refers to the individuals status in the previous wave if employed and to the wave prior to separation if unemployed or UI-ineligible. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly wages. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 9

Imperfect Controls (WBA)

	OLS				2SLS			
		Controls				Controls		
Sample	UI-Eligible Unemployed	Employed	Always Employed	UI-Ineligible	UI-Eligible Unemployed	Employed	Always Employed	UI-Ineligible
Mean Dependent	.535	.794	.935	.407	.535	.794	.935	.407
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Real UI WBA/100	.0445**	.0078***	.0028**	.0080	.0764***	.0110*	.0036	-.0182
	(.0206)	(.0025)	(.0011)	(.0199)	(.0265)	(.0062)	(.0022)	(.0368)
Prior Private HI	.4675***	.6925***	.7557***	.4387***	.4671***	.6924***	.7557***	.4389***
	(.0144)	(.0063)	(.0066)	(.0209)	(.0140)	(.0063)	(.0065)	(.0204)
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X			X	X			X
Wage Splines	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
State Effects								
Observations	30,208	124,294	725,582	19,990	30,208	124,294	725,582	19,990

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (2). Observations are at the person-month level. Prior Private HI refers to the individuals status in the previous wave if employed and to the wave prior to separation if unemployed or UI-ineligible. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. The UI wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a real $100 increase in UI wba on private health insurance coverage of the unemployed. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 10
Partially Interacted Specification (RR)

	OLS				2SLS			
Mean Dependent	.743	.743	.743	.743	.743	.743	.743	.743
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployed	-.2313***	-.2301***	-.2292***	-.2283***	-.2231***	-.2056***	-.2038***	-.2009***
	(.0350)	(.0309)	(.0307)	(.0304)	(.0779)	(.0718)	(.0718)	(.0723)
UI RR	-.1143***	.0498***	.0468***	.0303**	.1370***	.0691*	.0623*	.0400
	(.0117)	(.0134)	(.0134)	(.0151)	(.0400)	(.0362)	(.0343)	(.0900)
Unemployed*UI RR	.1084**	.0940**	.0948**	.0977**	.2226**	.2099**	.2074**	.2114*
	(.0464)	(.0405)	(.0403)	(.0487)	(.0919)	(.0820)	(.0821)	(.1129)
Prior Private HI	.7489***	.6799***	.6794***	.6740***	.7770***	.6730***	.6726***	.6673***
	(.0075)	(.0067)	(.0068)	(.0075)	(.0079)	(.0071)	(.0072)	(.0080)
Year/Month Effects	X	X	X	X	X	X	X	X
Wage Splines		X	X	X		X	X	X
Demographics		X	X	X		X	X	X
Unemployment Rate			X	X			X	X
State Effects				X				X
Observations	154,502	154,502	154,502	154,502	154,502	154,502	154,502	154,502

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (3). Observations are at the person-month level and include both employed and unemployed observations for individuals who experience at least one unemployment spell within the panel. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The coefficient on the interaction term is a fairly standard difference-in-difference estimator. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 11
Partially Interacted Specification (WBA)

	OLS				2SLS			
Mean Dependent	.743	.743	.743	.743	.743	.743	.743	.743
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployed	-.1837***	-.1711***	-.1706***	-.1713***	-.1724***	-.1700***	-.1688***	-.1655***
	(.0123)	(.0121)	(.0121)	(.0121)	(.0340)	(.0334)	(.0332)	(.0333)
Real UI WBA/100	.0560***	.0074	.0070	-.0038	.0221***	.0119	.0114	-.0162
	(.0052)	(.0048)	(.0050)	(.0054)	(.0060)	(.0073)	(.0071)	(.0266)
Unemployed*Real UI WBA/100	.0498***	.0338***	.0329***	.0336***	.0626**	.0583**	.0578**	.0546***
	(.0089)	(.0083)	(.0083)	(.0086)	(.0260)	(.0249)	(.0247)	(.0250)
Prior Private HI	.7293***	.6830***	.6825***	.6774***	.7489***	.6847***	.6842***	.6789***
	(.0067)	(.0064)	(.0064)	(.0070)	(.0088)	(.0074)	(.0075)	(.0080)
Year/Month Effects	X	X	X	X	X	X	X	X
Wage Splines		X	X	X		X	X	X
Demographics		X	X	X		X	X	X
Unemployment Rate			X	X			X	X
State Effects				X				X
Observations	154,502	154,502	154,502	154,502	154,502	154,502	154,502	154,502

Dependent variable is private health insurance coverage. Results correspond to estimating modifications of Equation (3). Observations are at the person-month level and include both employed and unemployed observations for individuals who experience at least one unemployment spell within the panel. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. The wba has been expressed in real 1990 dollards and scaled by 1/100, thus the coefficient on the interaction term corresponds to the effect of a real $100 increase in wba on private health insurance coverage of the unemployed relative to the employed. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 12

Collapsed Specification

	OLS				2SLS			
	Baseline	Collapsed	Baseline	Collapsed	Baseline	Collapsed	Baseline	Collapsed
Mean Dependent	.535	.606	.535	.606	.535	.606	.535	.606
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	.1060**	.0693**	--	--	.3008***	.2206***	--	--
	(.0508)	(.0318)	--	--	(.1086)	(.0778)	--	--
UI Real WBA/100	--	--	.0445**	.0273**	--	--	.0764***	.0553***
	--	--	(.0206)	(.0132)	--	--	(.0265)	(.0166)
Lagged Private HI	.4692***	.5629***	.4675***	.5617***	.4715***	.5637***	.4671***	.5608***
	(.0147)	(.0163)	(.0144)	(.0161)	(.0141)	(.0154)	(.0140)	(.0156)
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
State Effects								
Observations	30,208	7,669	30,208	7,669	30,208	7,669	30,208	7,669

Under collapsed specifications, the dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (4). Observations are person-spell level. Spells have been weighted by the reciprocal of the individual's number of spells, such that an individual's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on average private health insurance coverage of the unemployed. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, ** 0.05, ***-0.001.

Table 13
Summary Statistics by Net Liquid Wealth Quartile

		Net Liquid Wealth Quartile			
Sample	Pooled	1	2	3	4
Net Liquid Wealth Range	--	<($1510)	($1510)-$174	$175-$10,199	>$10,199
Health Insurance Coverage					
Mean (Private HI) Across Spell	62.5	61.5	39.0	69.0	80.4
Private HI	76.9	74.0	51.6	79.9	90.1
Private HI in Own Name	61.8	61.1	45.7	65.5	74.4
Public HI	3.0	2.5	7.2	2.0	0.4
Employer-Provided HI Before Job Loss	55.6	54.8	42.3	58.7	66.2
Demographics					
Age	38.6	37.5	37.5	37.9	41.5
Married	74.7	82.0	71.0	71.4	74.6
Working Spouse	49.8	54.2	37.7	50.0	57.2
Non-White	22.3	22.7	36.5	20.8	9.3
Number of Children	1.1	1.3	1.3	1.0	0.9
Education: Less than HS	15.3	13.9	29.2	12.9	5.6
Education: HS Graduate	36.6	37.7	40.3	39.8	28.5
Education: Some College	27.8	30.5	25.1	28.5	26.9
Education: 4+ Years of College	20.3	17.9	5.5	18.9	38.9
Annual Earnings	$20,544	$18,841	$14,184	$21,095	$28,059
Median Annual Earnings	$17,268	$16,881	$12,458	$18,619	$24,292
Completed Spell Durations (Weeks)					
Duration	13.0	12.2	14.4	12.6	14.0
Median Duration	8.0	7.0	9.0	8.0	9.0
UI Benefits					
UI Replacement Rate	52.0	52.1	53.7	51.0	46.1
Real WBA	$177	$175	$146	$183	$203
Assets					
Net Liquid Wealth	$19,450	-$11,824	-$291	$3,163	$86,792
Median Net Liquid Wealth	$175	-$5,394	$0	$2,339	$38,268
Liquid Wealth	$24,313	$1,899	$409	$4,832	$90,146
Median Liquid Wealth	$1,637	$422	$0	$3,579	$41,685
Unsecured Debt	$4,862	$13,723	$700	$1,669	$3,353
Median Unsecured Debt	$1,090	$6,923	$80	$250	$827
Home Equity	$29,441	$19,838	$13,124	$26,616	$58,195
Median Home Equity	$8,190	$2,424	$0	$10,674	$41,827
Mortgage Indicator	50.0	47.5	33.3	50.1	67.7
Renter Indicator	33.8	38.3	50.2	33.1	13.6
Spells	4,258	1,065	1,065	1,064	1,064

Table entries are means unless otherwise noted. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. Unemployment duration is defined as time elapsed from end of last job to start of next job and does not adjust for right-censoring. Asset and liability data is collected once per panel prior to the 1996 panel, twice per panel thereafter. Eligible unemployment spells without sufficient asset data are excluded, including spells for which asset data are observed following a job separation. This restriction excludes approximately 50 percent of the UI-eligible sample. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debt. All monetary values are in real 1990 dollars.

Table 14

Heterogeneous Effect by Household NLW

	Household Net Liquid Wealth							
	Below Median				Above Median			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	50.3	50.3	50.3	50.3	74.7	74.7	74.7	74.7
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	.1262**	.2261**	--	--	-.0409	.0619	--	--
	(.0607)	(.1098)	--	--	(.0882)	(.1229)	--	--
UI WBA	--	--	.0541**	.0891***	--	--	.0127	.0190
	--	--	(.0239)	(.0341)	--	--	(.0227)	(.0339)
Prior Private HI	X	X	X	X	X	X	X	X
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
Observations	2,130	2,130	2,130	2,130	2,128	2,128	2,128	2,128

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (4). Observations are at the individual-spell level. Spells have been weighted by the reciprocal of the individual's number of spells, such that an individual's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on private health insurance coverage of the unemployed. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debt. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 15
Heterogeneous Effect Across Net Liquid Wealth Quartiles

	Household Net Liquid Wealth Quartile															
	Q1				Q2				Q3				Q4			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	61.5	61.5	61.5	61.5	39.0	39.0	39.0	39.0	69.0	69.0	69.0	69.0	80.4	80.4	80.4	80.4
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
UI RR	.1547*	.3809**	--	--	0.0834	.1057	--	--	.0085	.0477	--	--	-.1811	.0029	--	--
	(.0806)	(.1871)	--	--	(.0678)	(.1924)	--	--	(.1330)	(.2221)	--	--	(.1044)	(.1727)	--	--
UI WBA	--	--	.0754**	.1149**	--	--	.0279	.0724	--	--	.0105	.0262	--	--	.0041	.0185
	--	--	(.0287)	(.0450)	--	--	(.0345)	(.0507)	--	--	(.0322)	(.0579)	--	--	(.0228)	(.0285)
Wage Splines	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Employed HI Control	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
State Effects																
Observations	1,065	1,065	1,065	1,065	1,065	1,065	1,065	1,065	1,064	1,064	1,064	1,064	1,064	1,064	1,064	1,064

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (4). Observations are at the individual-spell level. Spells have been weighted by the reciprocal of the individual's number of spells, such that an individual's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on private health insurance coverage of the unemployed. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debt. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 16

Heterogeneous Effect by Household GLW

	Household Gross Liquid Wealth							
	Below Median				Above Median			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	47.2	47.2	47.2	47.2	77.8	77.8	77.8	77.8
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	.1787**	.2769**	--	--	-.0223	.0720	--	--
	(.0696)	(.1190)	--	--	(.0797)	(.1283)	--	--
UI WBA	--	--	.0658**	.0872***	--	--	.0176	.0182
	--	--	(.0271)	(.0327)	--	--	(.0184)	(.0255)
Prior Private HI	X	X	X	X	X	X	X	X
Year/Month Effects	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
Observations	2,129	2,129	2,129	2,129	2,129	2,129	2,129	2,129

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (4). Observations are at the individual-spell level. Spells have been weighted by the reciprocal of the individual's number of spells, such that an individual's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on private health insurance coverage of the unemployed. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 17

Heterogeneous Effect by Household Mortgage Status

	Household Mortgage Status at Point of Separation							
	Mortgage				No Mortgage			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	72.6	72.6	72.6	72.6	52.4	52.4	52.4	52.4
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI RR	.0905*	.2919***	--	--	.0404	.1359	--	--
	(.0502)	(.0861)	--	--	(.0627)	(.1988)	--	--
UI WBA	--	--	.0461***	.0597***	--	--	.0100	.0210
	--	--	(.0126)	(.0167)	--	--	(.0340)	(.0459)
Wage Splines	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X
Employed HI Control	X	X	X	X	X	X	X	X
Duration Controls	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X
Observations	2,132	2,132	2,132	2,132	2,126	2,126	2,126	2,126

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (4). Observations are at the individual-spell level. Spells have been weighted by the reciprocal of the individual's number of spells, such that an individual's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 5.1. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coeffient corresponds to the effect of a $100 increase in wba on private health insurance coverage of the unemployed. Mortgage proxy indicates household status at the point of separation. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Chapter 2

Maintaining Health Insurance Coverage for the Unemployed: The Role of Continuation of Coverage Mandates

2.1 Motivation

In 2008, more than 88 percent of private health insurance coverage was acquired through the workplace (DeNavas-Walt et al. 2009). Limited health insurance portability across employment status in the US health insurance market typically entails a loss of access to group insurance markets associated with job separation. The policy response to shortfalls in insurance coverage across employment status has been to enact continuation of coverage mandates. Uniform coverage was implemented under the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA), which allows job separators to continue their employer-sponsored coverage for up to 18 months.[1] Workers who take-up these continuation benefits are required to pay the full employer premium as well as a small administrative surcharge. Yet while employed, workers insured through employer-sponsored coverage typically directly pay only 16 percent of the cost of their individual coverage and 27 percent of the cost of family coverage while employed (Claxton et al. 2008). In 2009, the full annual cost of employer-sponsored health insurance averaged $4,824 for an individual policy and $13,375 for a family policy (KFF 2009a). For the unemployed, this cost is incurred during a period of low transitory income, suggesting a plausibly important role for liquidity constraints in limiting take-up of continuation benefits for households with insufficient assets to self-finance the unemployment spell and limited access

[1] COBRA is phased-in over the July 1986 - June 1987 period, at the start of the employer's next benefit year.

to credit markets. A recent Kaiser Family Foundation (2009b) survey found that 59 percent of adults with employer-sponsored coverage would find it very difficult to pay the full cost of their premiums if they were no longer employed.

This paper incorporates Survey of Income and Program Participation (SIPP) panels spanning the reference period 1983-2003 and evaluates the effectiveness of continuation of coverage mandates in mitigating the fall in private coverage associated with unemployment. Through simulation of eligible state unemployment insurance (UI) benefits, I investigate excess sensitivity of implied take-up of continuation benefits to cash-in-hand. The effect of mandate eligibility is identified primarily through heterogenous implementation of the federal mandate given pre-existing state mandates.[2] A related analysis from Gruber and Madrian (1997) finds limited effectiveness of continuation of coverage mandates in maintaining private coverage for the sample of all non-employment spells, without direct application to unemployment or interactions with UI benefits. This paper, then, is able to answer a number of questions of interest related to the effectiveness of continuation mandates and specifically the heterogeneous response by cash-in-hand.

First, how effective are state continuation of coverage mandates in mitigating the fall in private coverage for the unemployed? I evaluate the effect of mandated eligibility on the probability that an eligible individual reports private coverage across an unemployment spell, identified by changes in state mandates over time. Although eligibility is nearly universal following the phase-in of COBRA, the impact of the federal mandate is heterogeneous across states due to pre-existing state mandates.[3] I find that eligibility for 12 months of continuation benefits increases the probability that an unemployed individual with previous employer-sponsored coverage reports private coverage by 7.2 percentage points. To scale this result, I estimate the fall in private coverage associated with unemployment as varies across continuation of coverage eligibility. These results suggest that mandate eligibility, despite the high cost of continuation coverage relative to transitory income, mitigates the fall in private coverage by approximately 18 percent.

A series of robustness checks follows. I consider potential sample selection bias resulting from endogeneity of pre-separation health coverage to mandate eligibility and find trivial implications. Further, I construct a falsification test incorporating an early phase-in of COBRA to test for heterogeneous underlying state trends. These results validate the baseline empirical approach. I also consider contemporaneous effects of unobserved state action by analyzing a sample of unemployed with private, non-employer health coverage prior to separation. This control group exhibits modest sensitivity to state-year variation in mandate eligibility, again

[2] Additionally, there are a number of states that either implement continuation of coverage mandates or extend the eligible months under pre-existing mandates relative to the start of the reference period. Reference Table 1.

[3] Identification is similar to the approach of Card and Krueger (2000), where implementation of a federal minimum wage is binding only for states with a lower pre-existing mimum wage.

validating the baseline empirical approach.

Second, is the responsiveness to mandate eligibility heterogeneously distributed across sub-populations? I consider stratifications by household composition, specifically single workers, workers married to employed spouses, and workers married to non-employed spouses. I find a responsiveness range associated with 12 months of mandate eligibility in terms of increased private coverage that varies from 4.2 percentage points for workers married to working spouses to 15.1 percentage points for workers married to non-employed spouses. Presumably, these heterogeneous responses are driven, at least in part, by varying outside options available to the household. Alternatively, I stratify across self-reported health status prior to separation, given heterogeneous costs of, or even access to, non-group private coverage. I find that unemployed classified in the worst health categories are almost three times as responsive to 12 months of mandate eligibility relative to unemployed classified in the best health categories, and that results are monotonic increasing in terms of health deterioration.

Third, to what extent does the effectiveness of mandate eligibility vary with state UI generosity? This presumably reflects the extent to which responsiveness to mandate eligibility is affected by cash-in-hand. Given endogeneity concerns regarding the predicted eligible UI benefits, I employ a simulated instruments approach. Absent state UI, mandate eligibility is estimated to increase private coverage by only 2.4 percentage points amongst unemployed workers with employer-sponsored coverage prior to separation. By comparison, evaluated at the sample average eligible weekly UI benefits, mandate eligibility is estimated to increase maintenance of private coverage by 21.3 percentage points. Thus, continuation of coverage mandates are most effective when the unemployed worker has sufficient transitory income, as provided through UI benefits, suggesting an important role of liquidity constraints in explaining observed take-up rates.[4]

The rest of the paper proceeds as follows. A brief review of the existing literature is discussed in the next section. Section 2.3 details the data and core sample selection criteria. Section 2.4 evaluates the effectiveness of continuation of coverage mandates in mitigating the fall in private coverage for the unemployed. Section 2.5 incorporates a measure of individual eligible UI benefits and implements a simulated instruments approach to estimate the heterogeneous effect of mandates eligibility across UI benefit levels. The last section concludes.

2.2 A Brief Literature Perspective

Health Insurance Coverage and Unemployment

[4]Investigating the role of liquidity constraints would be a valuable addition to the analysis. Yet, household asset data as might substantiate such an analysis are sporadically collected throughout the period preceding COBRA implementation, rendering such analysis infeasible.

The US health insurance market is distinguished by the relationship between employment and access to group insurance markets. Approximately 88 percent of private health insurance coverage was acquired through the workplace in 2008 (DeNavas-Walt et al. 2009). As a result, health insurance coverage is limited in terms of mobility across employment status. Modeling underlying preference heterogeneity, Gruber and Madrian (1997) find that the likelihood of insurance coverage drops by roughly 20 percentage points following a job separation, evaluated over the 1983-1989 period.[5] More recently, Brown (2010) documents a 19 percentage points fall in private coverage amongst the unemployed across the 1990-2003 period. Shortfalls in insurance coverage across employment status is a persistent issue, and one that affects the more than 14.6 million unemployed persons and their families in the United States (BLS 2010), 45.5 percent of which are long-term unemployed, those jobless for 27 or more weeks.

Costs of Short-Term Uninsurance

Although insurance is not a direct measure of consumption, there is evidence to suggest that smoothing insurance coverage across employment status may proxy for smoothing medical care consumption. Adults and children uninsured for less than one year are less likely to receive recommended screenings and are more likely to have gone without a needed physician visit due to cost relative to the insured (Ayanian et al. 2000, Olson et al. 2005). Only emergency departments are required by federal law to stabilize all individuals irrespective of ability to pay, though the uninsured may be denied follow-up care for even urgent medical conditions if unable to pay in full. Health providers are not required to provide care to the uninsured, restricting access to needed care. Further, the uninsured are typically billed for any care received and often face higher charges than the insured (Asplin et al. 2005). By forgoing care to avoid medical debt, the uninsured may worsen health problems (Schwartz 2007), heightening the difficulty of re-securing employment. To the extent that health care consumption is not perfectly substitutable over time, and to the extent that pre-existing conditions preclude individuals from later obtaining coverage for necessary medical care, even relatively short spells of uninsurance may have potentially serious welfare costs.

Continuation of Coverage Mandates

Although the literature suggests a potentially large role for continuation of coverage mandates in reducing the prevalence of uninsurance amongst the unemployed, existing work concerning the role of continuation mandates in decreasing uninsurance (Klerman and Rahmna 1992, Gruber and Madrian 1997) has found only modest effects.[6] Considering the substantial effective subsidization of the cost of insurance relative to the non-group market, these results imply a

[5] Previous work is conflicting. Some evidence (Monheit et al. 1984, Klerman and Rahmna 1992) suggests that low rates of insurance coverage amongst the unemployed is largely explained by a lack of coverage while on the previous job. This evidence contrasts with other studies (Berki et al. 1985, Podgursky and Swaim 1987, Bazzoli 1986), which report large declines in insurance coverage following job loss.

[6] A more substantial literature has considered the issue of 'job lock' and the implied fluidity of the U.S. labor market. If job leaving is associated with loss of health coverage, then a worker who might otherwise optimally drop out of the labor force or seek new employment might be locked into a suboptimal job to retain coverage

small price elasticity of insurance. Yet, these studies have ignored the potential importance of liquidity constraints in explaining the low implied take-up of continuation benefits.

This paper broadly re-considers existing evidence regarding the limited effectiveness of continuation of coverage mandates in terms of mitigating the fall in private coverage associate with unemployment by considering interaction of mandate eligibility with cash-in-hand through state UI benefits. Plausibly, continuation of coverage mandates are of limited effectiveness absent sufficient transitory income to purchase continuation coverage in the presence of potential credit constraints or consumption commitments.

2.3 Data

I use data from the Survey of Income and Program Participation (SIPP) panels spanning the reference period October 1983 - December 2003.[7] Each SIPP panel surveys a national set of households at four month intervals (waves) for $2\frac{1}{2}$-4 years, with sample sizes ranging from approximately 14,000 to 36,700 interviewed households.[8] At each interview, households are asked questions in reference to the four month recall period. Data are collected regarding health insurance coverage, income and labor force participation, as well as a wide array of socioeconomic characteristics of each household member and of the household as a whole. The SIPP provides monthly data on income and health insurance coverage and weekly data on labor force status. Relative to other widely used data, such as the CPS and PSID, the advantages of the SIPP are the availability of high-frequency data on individual and household income, employment status, detailed health insurance coverage, and UI benefit receipt. Deliberate over-sampling of the low-income population provides a suitably large sample of unemployment spells. I supplement the SIPP with monthly national price indices and seasonally adjusted monthly state unemployment rates as reported by the US Bureau of Labor Statistics.

Starting from the universe of job separations in the pooled SIPP panels, I retain spells of unemployment, defined as spells following a job separation during which individuals are either on layoff or are searching for a new job. Observations are not conditioned on duration.[9] As labor

by an employer's group plan. Gruber and Madrian (1994, 1997) provide evidence that continuation of coverage mandates reduce 'job lock', largely through effective subsidization of transitions to non-employment.

[7] US Census Bureau. Survey of Income and Program Participation Users' Guide. http://www.census.gov/sipp/usrguide.html (Accessed September 2009)

[8] The length of observation varies across panels. The 1984 panel contains 9 waves. The 1985 panel contains 8 waves. The 1986 and 1987 panels contain 7 waves. The 1988 panel contains 6 waves. The 1990 and 1991 panels contain 8 waves. The 1992 and 1993 panels contain 9 waves. The 1996 panel contains 12 waves. The 2001 panel contains 9 waves. Owing to the overlapping design of the survey, observations are continuous across the reference period, excepting for January 1990, due to the incomplete nature of the omitted 1989 panel, and March – September 2000, where a minor interruption arose over a funding shortfall and the subsequent cancellation of the in-progress 2000 panel, which was re-started as the 2001 panel.

[9] As spells of less than 1 month may be false transition in the SIPP and given that inference of health insurance coverage, measured at the monthly level, is perhaps inconsistently reported across individuals, I

force nonparticipation amongst this sample is often disguised long-term unemployment (Clark and Summers, 1979), I do not exclude observations for workers who drop out of the labor force at some point during an unemployment spell. I measure the duration of a spell as consisting of all weeks of separation from work. For those who have some weeks of unemployment (either search or layoff), 17.3 percent of their spells are weeks out of the labor force. Spells begin with the initial month of separation and end with the first full month of re-employment.[10]

I restrict attention to prime-age males, 25-54 years old, thus focusing on job-separators who have a high rate of attachment to the labor force. I include only unemployment spells for which I observe at least one quarter of employment experience prior to separation.[11] This allows for the accurate measurement of non-employment spell duration, as well as characteristics of pre-separation jobs, most importantly imputation of pre-separation wages and pre-separation health insurance coverage.

The resulting left-censoring for unemployment spells in-progress at the start of the respective SIPP panel disproportionately omits the long-term unemployed, potentially skewing the composition of remaining spells.[12] For prime-age males, the above selection rule excludes only approximately 12 percent of the sample, whereas for women and younger/older men, the share of separations excluded would be approximately 52 percent. Thus, for prime-age males, this selection rule is less prohibitive in terms of generalization. This restriction also limits the impact of schooling and early retirement decisions, as well as childcare decisions, on the resulting pool of job separations.[13] If married, I restrict to households where both the husband and wife are 25-54 years old due to inter-dependence of health insurance decisions within the household.

A job separation is excluded if prior work history appears to make the worker ineligible for UI, as discussed at length within Section 2.5 below. I restrict to spells in which the individual reports looking for work in at least some months in order to focus on unemployment and not strictly labor force leavings. However, while UI eligibility requires continuing demonstration of labor force attachment, in specifications reported below, observations are included if individuals report stopping search effort.

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consider exclusions of spells of less than 1 month from the analysis. Results are robust to exclusion. These spells are included in baseline specifications to maintain focus on the representative spell.

[10]Alternatively, in results not reported, I exclude the initial month of separation due to ambiguity of health insurance coverage reporting in a transitional month. Results are largely consistent. In results below, I include the initial month of separation as a non-trivial fraction (22.7 percent) of spells are reported to last less than four weeks.

[11]This exclusion eliminates all monthly observations for individuals who never work in three consecutive months, as well as the initial months of observation for workers whose first three months of work occurs later in the panel.

[12]To the extent that continuation of coverage mandates incentivize longer unemployment spell durations, then spells of unemployment for particularly responsive individuals are less likely to be included in the sample. Given that the duration response is only relevant conditional upon take-up of continuation benefits, this potentially biases downward the estimated effect of mandate eligibility. Discussion of the 'dynamic sample-selection' bias is detailed in Diamond and Hausman (1984).

[13]Early retirement decisions would be particularly problematic for the analysis. A review of endogoneity of the retirement decision to continuation of coverage mandates is contained within Gruber and Madrian (2004).

Individuals from small states are excluded as the SIPP clusters these states, citing confidentiality concerns, preventing the assignment of relevant state mandates and imputation of eligible UI benefits for these individuals.[14]. I further exclude separations for which I have missing individual or job characteristics or missing basic health insurance coverage. These restrictions leave 11,095 unemployment spells, of which 6,301 spells report employer-sponsored coverage prior to separation and constitute the core sample. 5,820 spells are mandate-eligible and 481 spells are mandate-ineligible for either state or federal continuation of coverage protection as detailed in Section 2.4.

Health Insurance Coverage

Health insurance coverage is a monthly measure, and I differentiate private, public, and uninsured coverage status. Private insurance is defined as any health insurance coverage other than Medicare or Medicaid and includes employer-sponsored coverage, continuation coverage, spousal coverage, and non-group coverage. Once per wave, the detailed source of an individual's health insurance coverage is revealed along a number of dimensions: whether the insurance is in the individual's name or some other family member's name, and whether the policy is sponsored by an employer/union or acquired in a non-group setting. In the analysis to follow, the basic monthly source of coverage is considered for accuracy, though this will mask potential transitions from employer-sponsored coverage to spousal coverage or non-group coverage.[15] As I am unable to observe generosity of insurance coverage, I cannot distinguish underinsurance.[16]

Seam Bias

As individuals are required to recall information from the preceding four months of the reference period for each wave, it is unclear how much unique information is contained in monthly responses. Individuals have a tendency to propagate their status at the point of the interview backwards through the preceding months.[17] A disproportionately large number of labor force transitions are reported on the 'seam' between interviews, leading to artificial spikes in the hazard rate. This bias extends to transitions in insurance coverage (Klerman and Rahman 1992). In this context, seam bias will produce false classifications of labor participation, blurring the distinction between the employed and unemployed, and between insured and uninsured.[18] Thus I draw information on prior employed health insurance coverage from the wave preceding

[14]These states are Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont, and Wyoming. West Virginia separations are additionally excluded prior to completed COBRA implementation as the state mandate could not be decisively dated by Gruber and Madrian (1997).

[15]I code individuals as covered by employer-provided health insurance coverage prior to separation based on whether or not they report such coverage in the wave preceding separation.

[16]As continuation of coverage mandates presumably operate through the purchase of eligible group insurance, the results to follow may then underestimate the true impact of continuation of coverage mandates, as I am unable to differentiate the crowd-out of underinsurance in the non-group market with more comprehensive group market coverage.

[17]This issue is evident in Figure 2. See Klerman (1991) for a detailed discussion of the seam bias problem.

[18]Results are re-considered using only the fourth reference month just prior to the interview in the unobserved fifth month. Although point estimates are largely comparable, statistical imprecision impedes inference. An alternative would be the use of wavely observations. However, as spell of unemployment are often less that four

the job separation. Of course, coverage may still drop off with artificial abruptness following a separation and short spells of uninsurance may be underreported.

2.4 Continuation of Coverage Mandates

Insurance coverage shortfalls resulting from loss of employer-sponsored insurance have prompted continuation of coverage mandates as a policy response to limited portability of private insurance coverage. Common to these mandates are the requirement that the employer's group health insurance provider offer separating employees the option to continue health coverage through the employer's plan for some limited duration. These mandates effectively reduce the cost of insurance to job leavers relative to the non-group insurance market.[19] Beginning with Minnesota implementation in 1974, a total of 27 states had created a patchwork of mandate eligibility prior to the phase-in of uniform worker protections under COBRA at a national level starting in July 1986.

A summary of legislative changes at the state and federal level as adapted from Gruber and Madrian (1997) are presented in Table 1.[20] A graphical representation of the distribution of state mandates at the start of the reference period and prior to COBRA implementation is presented in Figure 1. At the start of the reference period, 18 states had continuation of coverage mandates in-place, though it is important to note that eligible durations vary substantially, from 1 month in Oklahoma to 18 months in Wisconsin. Prior to federal implementation of COBRA in 1986, an additional 9 states implemented new protections for workers and 1 state increased the number of eligible months, bringing the total number of pre-existing state mandates to 27.[21] Uniform protection under COBRA is phased-in starting July 1986, with firms required to offer continuation benefits starting with the next plan year. Absent information regarding the firm-specific phase-in of the federal mandate, I phase-in this eligibility by 1.5 months for each month over the July 1986 - June 1987 period, to the legislative maximum of 18 months.[22]

months in duration, this would exclude a large fraction of spells and shift emphasis away from the representative spell.

[19] As the premium is not directly subsidized, it is unclear the extent to which adverse selection in the continuation coverage take-up decision may generate costs for the employer and remaining employees in terms of higher future premiums.

[20] State laws come from Hewitt (1985), Thompson Publishing Group (1992) and cross-checked against state statues as cited in Gruber and Madrian (1997).

[21] Given the restriction to spells of unemployment for which the individual is actively looking for work, the focus is presumably upon involuntary separations. As a result, I incorporate all state mandates, irrespective of eligibility restriction to involuntary separations.

[22] An alternative approach, as taken by Klerman (1991) and Gruber and Madrian (1997) is to assign the full 18 months of coverage in January 1987, under assumption that the majority of plan years start in January. Results presented below are largely robust to this alternative implementation of COBRA.

2.4.1 Descriptive Statistics

Descriptive statistics across populations of interest are presented in Table 2. Column (1) considers the full sample of unemployment spells without conditioning on pre-separation health insurance coverage. Approximately 56.8 percent of separations occur with employer-sponsored coverage in the worker's name prior to separation. This represents 78.2 percent of all pre-separation private coverage and 91.6 percent of all pre-separation private coverage in the worker's name, consistent with the notion that the vast majority of private coverage is derived through the employer.[23] Comparing columns (1) and (2), however, it is apparent that workers with employer-sponsored coverage are dissimilar along some dimensions relative to the aggregate of separators. Specifically, separators with employer-sponsored coverage are more likely to be white, better educated, and report substantially higher pre-separation earnings. Yet, the populations are similar in terms of age, marital status, and spouse's work status.

Comparing separators with employer-sponsored coverage residing within states with and without state mandates prior to COBRA implementation, columns (6) and (8) respectively, reveals a distinction in average private coverage across the unemployment spell, 65.2 percent relative to 59.8 percent. Thus, for workers with employer-sponsored coverage prior to separation, state mandate-eligibility appears to mitigate the fall in private coverage by approximately 5.4 percentage points, or 13.4 percent of the raw fall in private coverage for state mandate-ineligible separators. Dissimilarly, comparing the raw fall in private coverage for the aggregate of separators across this same period reveals a relatively smaller effect of mandate eligibility, a difference of approximately 3.6 percentage points. This is consistent with the expectation that state mandates should not mitigate the fall in private coverage for workers without employer-sponsored coverage prior to separation.[24]

Yet, comparing the population composition across mandate eligibility suggests a few potentially confounding factors. Specifically, separators in mandate-eligible states are somewhat older, more likely to be married, to have a working spouse, and to be white, all factors associated with higher rates of private coverage (Brown 2010). Further, mandate eligible separators typically experience shorter unemployment durations. Pre-separation earnings and UI benefits, in terms of the weekly benefit amount and the earnings replacement rate, however, are largely comparable across the groups.

Although the descriptive statistics suggest a potential role for continuation of coverage mandates to mitigate the fall in private coverage across employment status, these comparisons also provide reason for pause. Even in the presence of uniform mandate eligibility under COBRA,

[23] These statistics do not include coverage through the spouse's employer, which effectively raises the fraction of private coverage obtained through any employer to 89.2 percent.

[24] Scaling the 5.4 percentage points increase in average private coverage across the spell for workers with employer-sponsored coverage prior to separation by the fraction of these workers within the pool of separators suggests an aggregate effect of 3.0 percentage points owing exclusively to the effect of mandate eligibility for workers with employer-sponsored coverage prior to separation.

only approximately 56.8 percent of unemployment spells are eligible to take-up continuation benefits, as pre-separation coverage through the employer is a necessary condition. Further, demographic distinctions between states with and without mandates in-place prior to federal COBRA implementation highlights the need to control for potentially confounding factors given population heterogeneity in the regression framework. These cross-state comparisons indicate the need to incorporate state fixed effects in the specifications to follow. Increasing average private coverage rates across unemployment spells for mandate-eligible separators over time, as illustrated in columns (4) and (6), emphasizes the need for time fixed effects to control for common trends.[25]

2.4.2 Estimating the Fall in Private Coverage

Before considering the effectiveness of continuation of coverage mandate, I first estimate the fall in private coverage associated with unemployment for workers with employer-sponsored coverage prior to separation.[26] Specifically, I estimate multivariate linear probability models of the form:

$$Private_{ist} = \beta Unemployed_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.1}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are person-month level and include months unemployed as well as months employed for those who experience separation at some point within the respective panel. Only those spells with employer-sponsored coverage prior to separation or pre-separation employed observations for workers who experience a qualifying separation within the respective panel are included. $Private_{ist}$ is an indicator for private health insurance coverage within the month. $Unemployed_{it}$ is an indicator for whether an individual is unemployed in a given month. z_{it} is a vector of individual demographics and job characteristics. α_s is a vector of state indicators. δ_t is a vector of panel and year indicators.[27] Correlation in the behavior of an individual within a panel and across individuals within a state indicates that it is inappropriate to treat monthly observations on health insurance status as independent, thus standard errors are clustered to accommodate an arbitrary variance-covariance matrix within each state. The coefficient β on the regressor of

[25] It is also possible that this discrepancy is in-part explained by the extension in months of coverage under COBRA relative to pre-existing state mandates. Thus, one would expect average private coverage rates to rise, ceteris paribus.

[26] Although Brown (2010) investigates the fall in private coverage associated with unemployment, the results are not restricted to workers with employer-sponsored coverage prior to separation.

[27] Individual characteristics include age bins, a marriage indicator, a spousal work status indicator, a race indicator, educational attainment bins, and bins for number of children. Characteristics of the worker's job include 10 Standard Industrial Classification System (SIC) major industry sectors indicators and 23 Standard Occupational Classification (SOC) major groups occupation indicators. Wage controls include a flexible 5-knot cubic spline in base-period wages and household annual income and 3-knot cubic splines in highest-quarter wages. Year indicators control for any national trends in health insurance coverage. Panel indicators are included given the overlapping panels design of the SIPP. State indicators control for time-invariant differences across states in health insurance coverage.

interest, $Unemployed_{it}$, measures the fall in private health insurance coverage resulting from unemployment. In alternative specifications, I incorporate an individual fixed effect to control for unobservable heterogeneity.

Fall in Private Coverage Results - Table 3

For purposes of comparison, specifications (1) and (2) estimate the fall in private coverage for all separations, regardless of pre-separation health coverage.[28] Unemployment is associated with a highly statistically significant fall in private coverage of 20.6 percentage points, relying upon observable controls, and 17.2 percentage points incorporating an individual fixed effect.[29] Specifications (3) and (4) present results for separators with private coverage not through the employer prior to separation, including purchase in the non-group market or coverage through a spouse's employer. For these separators, unemployment is associated with a highly statistically significant fall of 9.1 percentage points relying upon observed controls, and 7.6 percentage points incorporating an individual fixed effect.[30] Specifications (5) and (6) present results for separators with employer-sponsored coverage prior to separation, regardless of mandate eligibility. For workers with employer-sponsored coverage prior to separation, unemployment is associated with a highly statistically significant fall in private coverage of 30.5 percentage points relying upon observable controls, and 23.7 percentage points incorporating an individual fixed effect. Thus, separators with employer-sponsored coverage experience a more dramatic fall in private insurance coverage.[31]

I then stratify the sample of separations with employer-sponsored coverage prior to separation along mandate eligibility, as a preliminary check to whether mandate eligibility is associated with a mitigated fall in private coverage. Specifications (7) and (8) present results restricted to separators with employer-sponsored coverage prior to separation who are mandate eligible. Relative to the mandate ineligible results, presented in specifications (9) and (10), mandate eligible separators experience a tempered fall in private coverage, 30.5 percentage points compared to 39.5 percentage points relying upon observable controls, and 22.9 percentage points compared to 32.3 percentage points incorporating an individual fixed effect. Thus, for mandate-eligible separations, which includes separations occurring within states with mandates prior to COBRA implementation and all separations following the start of COBRA phase-in in

[28] Specification (1) additionally incorporates a lagged control for pre-separation private coverage, drawn from the wave prior to separation, or simply drawn from the prior wave for employed observations.

[29] These results are largely consistent with existing estimates (Gruber and Madrian 1997, Brown 2010), albeit with somewhat different sample restrictions in place.

[30] In results not reported, the sample of all workers without employer-sponsored coverage prior to separation, including these separations in addition to the uninsured, exhibit an aggregate fall in private coverage of 5.2 percentage points across employment status relying upon observable controls, and 4.3 percentage points incorporating an individual fixed effect.

[31] This is hardly surprising as a separators with non-group coverage or coverage through a spouse's employer prior to separation experience a negative transitory income shock, but not a price distortion as experienced by separators with employer-sponsored coverage. Similarly, separators uninsured or insured through public provision are not expected to purchase coverage in the non-group market conditional upon unemployement.

July 1986, unemployment is associated with a less severe fall in private coverage relative to mandate-ineligible separations.[32]

2.4.3 Mandate Effectiveness

As continuation of coverage is only available to workers with employer-sponsored coverage prior to separation, I restrict the sample to unemployed observations for individuals with private health insurance coverage through the employer in the wave prior to separation.[33] To determine the effectiveness of mandate eligibility in terms of mitigating the fall in private coverage, I estimate the multivariate linear probability model:[34]

$$Private_{ist} = \beta Eligible_{st} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.2}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-month level. $Private_{ist}$ is an indicator for private health insurance coverage in a given month.[35] $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. z_{it} is a vector of individual and job characteristics.[36] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. Alternatively, I replace $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate-eligible months. Individuals are assigned the maximum number of months of continuation benefits available under either state provisions or the federal law in place at the time of separation.[37] Correlation in the behavior of an individual within a panel and across individuals within a state indicates that it is inappropriate to treat monthly observations on health insurance status as independent, thus standard errors are clustered to accommodate an arbitrary variance-covariance matrix within each state.

[32] In alternative results not presented, results are stratified by months of continuation benefits (none, 1-3, 4-6, 7-12, and 13-18 months). Results are monotonic in relation to eligible months, with higher months of continuation benefits associated with a less severe fall in private coverage across the unemployment spell, though the relationship is non-linear. Limited sample size in some specifications impedes statistical inference.

[33] Classifying job separators using the detailed source of health coverage in the wave prior to separation circumvents classification issues arising from the seam bias discussed above. This necessarily introduces misclassification as some fraction of separators would have actually dropped employer-sponsored coverage prior to separation, though this issue is minor given the wave-to-wave turnover in employer-sponsored coverage amongst the empoyed.

[34] A probit specification is alternatively considered to address concerns regarding limitations of the linear probability model.

[35] I focus on private coverage, rather than detailed source of coverage, as private coverage is reported monthly and the typical unemployment spell is shorter than a SIPP wave (4 months). Further, the focus of the analysis is the effect of continuation of coverage mandates on private coverage amongst the unemployed, including crowd-out of alternative sources. Lastly, respondents may misclassify continuation coverage, as individually-purchased coverage as the coverage is no longer a fringe benefit of employment.

[36] Controls are equivalent to Equation (2.1) with the additional of the monthly state unemployment rate to proxy for economic conditions.

[37] Some state laws require that an employee must have been covered under the employer's insurance for 3-6 months prior to obtaining eligibility for continuation benefits. This restriction is approximated by the restriction to employer-sponsored coverage in the wave prior to separation.

As the estimating equation includes state and time fixed effects, the effect of mandate eligibility or months of mandate eligibility is identified by changes in state laws over time, most prominently through COBRA implementation beginning July 1986. Although eligibility is nearly universal following the phase-in of COBRA, the impact is heterogeneous across states due to pre-existing state mandates. Figure 1 depicts a point-in-time summary of state mandates at the start of the reference period (October 1983) and again prior to the phase-in of COBRA (June 1986).[38] 18 states had some protection in place at the start of the reference period, though prior to COBRA implementation a total of 27 states had implemented some form of protection.[39] While the $Eligible_{st}$ specification captures only implementation of new state mandates or federal coverage through COBRA for states without pre-existing mandates, the $Months_{st}$ specification further incorporates the effect of extended eligible months under COBRA, as state mandates typically provide a limited duration of benefits relative to COBRA.

In terms of interpretation of the baseline specification, there are a number of confounding factors that may bias the results as identification relies critically upon the assumption that maintenance of private coverage trends similarly across states with varying levels of months of continuation benefits absent implementation of COBRA. Duration of unemployment spells may respond to mandate eligibility, through subsidization of the cost of health insurance coverage in the unemployed state. Further, the potential endogeneity of pre-separation coverage, resulting from continuation coverage eligibility criteria, may distort the distribution of spells following implementation of a continuation mandate or extension of an existing mandate. Both issues may result in a sample selection bias. Further, as state legislation prior to federal implementation of universal coverage under COBRA is not randomly assigned, heterogeneous state trends associated with implementation of state mandates may create a spurious relationship between mandate eligibility and maintenance of private coverage. Lastly, there may be contemporaneous state effects around COBRA implementation that affect maintenance of private coverage. These concerns are addressed in specification checks below and largely support interpretation of the baseline result as indeed causal.

Results - Table 4

The role of mandate eligibility in mitigating the fall in private coverage amongst the unemployed is presented in Panel 1 of Table 4. Mandate eligibility is estimated to increase the probability of private coverage in the unemployed state by 5.6 percentage points, incorporating the full control set in specification (3), though the result is marginally significant. Scaled by the 39.5 percentage points fall in private coverage for the mandate ineligible as presented in Table 3, this result suggests that mandate eligibility mitigates approximately 14.2 percent

[38] Although the majority of changes are movements are from 0 to non-0 eligible months, there are also extensions of eligible months for previously enacted state mandates across this period.

[39] However, 3 of the 9 state mandates implemented after the start of the reference period and prior to COBRA phase-in are dated within 6 months of the effective COBRA date.

of the fall in private coverage across employment status.[40] The marginal effect of probit estimation in specification (4) yields a similar result, that mandate eligibility increases private coverage by 6.8 percentage points. Both results are similar to the estimated effect of mandate eligibility absent any covariates in specification (1), 7.2 percentage points.

Alternatively, 12 months of mandate eligibility is predicted to increase the probability of private coverage by 7.2 percentage points, and the result is statistically significant as reported in specification (7). Scaled by the fall in private coverage for the mandate ineligible, this result suggests that 12 months of mandate eligibility mitigates approximately 18.2 percent of the fall in private coverage across employment status. Again, the marginal effect of the probit estimation in specification (8) is similar, an increase in private coverage of 9.2 percentage points. Note that these results are driven by distinct comparisons; specifically, the months of mandate eligibility results incorporate increased eligible months under COBRA relative to pre-existing state mandates. This may explain why the mandate eligibility results are less precisely estimated relative to the months of mandate eligibility results.[41]

These results imply COBRA take-up rates well below commonly cited estimates of approximately 20 percent (Flynn 1992, KFF 1999). However, the results above may under-estimate take-up rates for a number of reasons. First, COBRA enrollment may crowd-out other forms of health coverage available to the unemployed, such as insurance through a spouse's employer or purchase of coverage in the non-group market.[42] Second, individuals may misrepresent their insurance coverage, as eligible separators have 60 days to elect coverage. Third, given inconsistent reporting of an employer's size in the SIPP, it is not feasible to exclude separators ineligible for mandate eligibility, thus potentially understating the responsiveness to mandate eligibility amongst eligible separations.

Yet it is not entirely clear that private health insurance coverage is the appropriate dependent variable. A distinct policy question is to what extent continuation of coverage mandates mitigate the loss of health insurance from any source? Panel 2 of Table 4 reports comparable specifications where the dependent variable is a monthly measure of health insurance coverage through any source. Results are comparable to those considering only private coverage. This is unsurprising given the low rate of public insurance coverage amongst separators with employer-sponsored coverage prior to separation, 2.9 percent of spells or 3.5 percent of

[40] Alternatively, scaled by the predicted fall in private coverage for mandate eligible workers absent the mandate, the result is approximately 15.5 percent of the fall in coverage. By way of comparison, Gruber and Madrian (1997) report that 12 months of mandate eligibility mitigates the fall in private coverage of all non-employment spells by only 6.7 percent.

[41] It is not entirely clear why months of mandated eligibility, scaled by 12 months (substantially shorter than the 18 months of COBRA coverage), implies larger effects than mandate eligibility. This may in part be explained by state implementation between the start of the panel and the phase-in of COBRA, in which states mandated between 3 and 9 months.

[42] It would be of interest to estimate this crowd-out effect directly. However, given that source of insurance coverage is revealed wavely (every 4 months) and the typically unemployement spell is less than three months in duration, this approach is infeasible.

months unemployed.[43] Similarly, it is interesting to consider the extent to which continuation of coverage mandates might crowd-out public insurance take-up. Panel 3 of Table 4 reports on the effect of mandate eligibility on public insurance coverage amongst separations with employer-sponsored coverage prior to separation. Unsurprising in light of the above analysis, estimated effects are of small magnitude and often inconsistent sign, though probit estimation in specification (4) suggests that mandate eligibility decreases the probability of public insurance coverage by 0.25 percentage points. Similarly, an additional month of mandate eligibility decreases the probability of public coverage by 0.01 percentage points, as reported in specification (8). Neither results is statistically significant.

Collapsed Specification

Figure 2 presents the distribution of unemployment spell durations in the sample. The distribution is heavily skewed towards relative short durations; the majority of spells are shorter than 3 months. Note that Equation (2.2) weights each unemployed person-month equally, potentially over-weighting longer unemployment spells relative to the representative spell. Thus, I alternatively consider the collapsed model:[44]

$$\overline{Private}_{ist} = \beta Eligible_{st} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.3}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-spell level. $\overline{Private}_{ist}$ is a measure of the average number of months across the spell in which the unemployed worker reports private health insurance coverage from any source. $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[45] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. To avoid overweighting repeated short spells relative to the representative spell, I construct an individual weight equal to the reciprocal of the individual's count of unemployment spells, such that an individual's weights sum to one. In alternative specifications, I replace $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate eligible months.

Comparison of the collapsed specification relative to the linear probability model are presented in Table 5. The weighted spell-based approach of the collapsed model is employed to estimate the effect of mandate eligibility in specification (2), resulting in a point estimate of 5.1 percentage points, compared to the linear probability model estimate of 5.6 percentage points in specification (1). A similar comparison can be made between specifications (5) and (4) in

[43] This distinction suggests that individuals who switch to public insurance typically experience above-average unemployment spell durations.

[44] The collapsed model is used as the baseline specification for subsequent analysis unless noted otherwise.

[45] Controls are equivalent to Equation (2.2).

terms of the effect of each month of mandate eligibility, with point estimates of 0.7 percentage points and 0.6 percentage points, respectively. Collapsing spells yields comparable inference.

Endogenous Durations

Mandate eligibility decreases the cost of continued unemployment by effectively subsidizing the cost of insurance in the unemployed state. Thus, spell duration is potentially endogenous to mandate eligibility. This suggests the use of an alternative framework that does not condition on continued unemployment. Rather than collapsing to the person-spell level, I instead construct observation windows of varying length following the UI-eligible separation:

$$\overline{Private}_{ist}^{m} = \beta Eligible_{st} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.4}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-separation level. $\overline{Private}_{ist}^{m}$ measures the average private health insurance coverage across the m months of the observation window, inclusive of both months unemployed as well as months re-employed. $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[46] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. Right-censored observations, those separations with insufficient post-separation observational months, are excluded from the specifications. To maintain focus on the representative spell, I consider $m = 3$, that is 3 months of collapsed observation inclusive of the month of separation.[47] Equation (2.4) estimates the effect of mandate eligibility on average private coverage over the three-month period following a separation resulting in UI-eligible unemployment irrespective of spell duration. Alternatively, I replace $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate-eligible months.

Comparison of the fixed observation window model to the linear probability model and the collapsed spell model is presented in Table 5. The effect of mandate eligibility, specification (3), is estimated as a 5.8 percentage points increase in private coverage across the 3-month window following separations. Similarly, each month of mandate eligibility is estimated in specification (6) to increase the average private coverage across the 3-month window by 0.8 percentage points. All models report similar point estimates and significance, allowing for consistent inference.

[46] Controls are equivalent to Equation (2.2).

[47] Alternative lengths were considered. Choice of $m = 1$ results in weak, imprecisely measured effects of mandate eligibility, due in part to the ambiguous coding of private coverage in the month of separation. Choice of m in excess of 6 similarly results in weak, imprecisely measured effects of mandate eligibility, due in part to a rising fraction of months re-employed as the observation window is extended.

Specification Checks

Endogenous Pre-Separation Coverage

Continuation of coverage eligibility may distort the sample of unemployed workers with employer-sponsored coverage prior to separation, as this is a condition for eligibility. That is, mandate eligibility may increase valuation of offered employer-sponsored insurance amongst the employed, increasing take-up. If these marginal workers who take-up coverage while working anticipate continuing coverage across an unemployment spell, then the resulting sample selection bias may overestimate the effectiveness of continuation of coverage mandates in mitigating the fall in private coverage across employment status.[48] To evaluate the importance of this potential source of bias, I model the composition of employed health coverage for workers who experience unemployment within the respective panel:[49]

$$Employer_{ist} = \beta Eligible_{st} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.5}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-separation level, without conditioning on source of pre-separation coverage. $Employer_{ist}$ is an indicator for whether a worker had employer-sponsored coverage in the wave prior to separation. $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[50] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. This specification is estimated for all separations, investigating an absolute change in employer-sponsored coverage. Alternatively, I restrict to those with private coverage prior to separation, to evaluate the change in the composition of private coverage. As above, consider replacing $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate-eligible months.

Results are presented in Table 6. Both mandate eligibility and months of eligibility have little empirical relevance in terms of distorting the pre-separation coverage of separators. Mandate eligibility is estimated to actually decrease employer-sponsored coverage prior to separation by 0.2 percentage points, though the result is statistically insignificant. Similar inference is reached in considering the composition of pre-separation private coverage. These results are consistent with comparison of summary statistics in Table 2. Comparing columns (3) and (5), mandate-eligible separations throughout the reference period and restricting to pre-COBRA separations respectively, the percentage of separations with employer-sponsored coverage is similar, 56.9 percent and 56.3 percent. Further, comparing columns (5) and (7), pre-COBRA separations for

[48] This issue could alternatively be addressed by estimating the effect of mandate eligibility on the aggregate sample of separations, without conditioning on pre-separation employer-sponsored coverage.

[49] The sample consists of all job separations resulting in UI-eligible unemployment, irrespective of pre-separation health coverage.

[50] Controls are equivalent to Equation (2.2).

mandate-eligible and mandate-ineligible separations respectively, the percentage of separations with employer-sponsored coverage is again similar, 56.3 percent and 56.1 percent. Thus, this concern appears empirically unimportant.

Heterogeneous State Trends

Although Equation (2.3) incorporates state and time fixed effects, the estimated effect of mandate eligibility may be the spurious result of heterogeneous underlying trends across state. To investigate the relevance of this concern, I re-estimate Equation (2.3) for the restricted sample of spells prior to COBRA implementation for states which do not experience modification of state mandates between the start of the reference period and the phase-in of COBRA. For this sample, states are assumed to trend identically across the period prior to COBRA implementation. To test this assumption, I assign falsified 'early' COBRA legislation, phased-in between July 1985 and June 1986. Violation of the identifying assumption would suggest incorporating state-specific trends into the baseline specification.[51]

Results of this falsification test are reported in Table 7. Specifications (1) and (4) re-produce the baseline results for the reference period 1983-2003, incorporating all states uniquely identified in the SIPP, using the true phase-in period of COBRA spanning July 1986 - June 1987. Specification (2) and (5) identify the effect of mandate eligibility and months of mandate eligibility using a restricted sample. I include only spells between October 1983 and June 1987 for states that do not experience relevant modification of state mandates over this period, and all spells following implementation of COBRA. In this way, I produce an estimate of the effect of mandate eligibility identified exclusively from variation in pre-existing state mandates interacted with implementation of COBRA. The results suggest that the effect of mandate eligibility is consistently estimated using this restricted sample. Mandate eligibility increases average private coverage across spells by 7.6 percentage points, and each month of mandate eligibility increases average private coverage by 0.6 percentage points, similar to the baseline specification results of 5.1 percentage points and 0.7 percentage points presented in specification (1) and (4), respectively. Thus, large and statistically significant results of mandate eligibility can be identified exclusively through COBRA implementation.

The falsification exercise is presented in specifications (3) and (6). Restricting to the set of spells prior to COBRA implementation, I phase-in a false federal mandate one year prior to actual implementation, using the same relative sequencing as the true phase-in of COBRA. I exclude spells for states that introduce or extend existing state mandates across this period. For this restricted sample, the estimated effect of the false federal mandate should be approximately 0 if the baseline specification is properly identified. The results presented are consistent with the expectation, as I find that mandate eligibility is estimated to actually decrease private coverage

[51] Previous results are largely robust to the inclusion of state-specific trends. However, statistical power is weakened and reliance upon state-year variation in the simulated instruments approach discussed below is impeded.

across the spell by 0.78 percentage points in specification (3), and each month of mandate eligibility is estimated to decrease private coverage by 0.04 percentage points in specification (6). As the baseline results persist with similar sample restrictions to the falsification test, failure of the falsification test to pick up spurious effects of falsified COBRA implementation did not result strictly from these necessary exclusions. Thus, these results present evidence that heterogeneous state trends are not driving the baseline results. Further, the small magnitude of the estimates suggest that heterogeneous state trends are not a serious confounding factor.

Contemporaneous Effects

Other factors, besides changes in eligibility for continuation of coverage may differ across states over time, such as improvements in non-group insurance market pooling that coincide with changes in continuation of coverage mandates. To address this issue, I re-estimate Equation (2.3) for separators with private coverage prior to separation not through the employer (such as through a spouse's employer or purchase of non-group coverage). Continuation of coverage mandates are inapplicable to this population.

Results for this 'control' set are reported in Table 8. Although identification is equivalent to that of the baseline specification, separators with non-employer private coverage exhibit limited sensitivity to mandate eligibility, estimated to decrease average private coverage across the spell by 1.4 percentage points in specification (3). Conversely, 12 months of mandate eligibility is estimated to increase average private coverage across the spell by 0.4 percentage points in specification (4). These results are inconsistent and diminutive in relation to the baseline results.

Heterogeneity Across Household Composition

To the extent that mandate eligibility represents a price distortion relative to the cost of securing private insurance in the non-group market, there will be substantial heterogeneity in this benefit associated with marital status and spousal work status. To investigate plausibly heterogeneous responsiveness to mandate eligibility, I stratify my sample of separators with employer-sponsored coverage prior to separation into three mutually exclusive categories: single, married with an employed spouse, and married with a non-employed spouse. A single worker will have no private insurance option outside of the non-group market absent mandate eligibility. Dissimilarly, a married worker with an employed spouse potentially has access to group rates directly subsidized through the spouse's employer.[52] A married worker with a non-employed spouse has a dependent spouse and has no private insurance option outside the non-group market absent mandate eligibility. I estimate Equation (2.3) separately for each stratified group.

[52]The SIPP does not contain a consistent measure of health insurance options available to the individual, so it is unclear whether a spouse's employer offers health insurance as a fringe benefit or not.

Results are presented in Table 9. Consistent with expectation, married separators with an employed spouse are the least responsive to mandate eligibility, estimated to increase average private coverage across the spell by 2.5 percentage points in specification (3), or 4.2 percentage points for 12 months of mandate eligibility in specification (4), though neither result is statistically significant. Single separators are more responsive, as mandate eligibility increases the average private coverage across the spell by 8.2 percentage points in specification (1), or 10.1 percentage points for 12 months of mandate eligibility in specification (2). The most responsive group, however, are married separators with a non-employed spouse. For this group, mandate eligibility increases average private coverage across the spell by 11.3 percentage points in specification (5), or 15.1 percentage points for 12 months of mandate eligibility in specification (6). These results are consistent with the notion that responsiveness to mandate eligibility varies across household composition and outside options.

Heterogeneity Across Pre-Separation Health Status

As discussed in Gruber and Madrian (1994), non-group coverage often excludes pre-existing medical conditions for some period of time after enrollment, non-group policies typically have higher out-of-pocket expenses relative to group policies, and particularly unhealthy individuals may be unable to obtain coverage at any price. In 2005, nearly three in five adults who considered buying non-group coverage had difficulty finding a plan they could afford, and one in five were either turned down by an insurance carrier, charged a higher premium based on health status, or had a specific health condition excluded from coverage (Collins et al. 2006). Thus, workers in particularly poor health may be more sensitive to mandate eligibility. Incorporating periodic SIPP 'topical modules' provides pre-separation, self-reported health status for approximately 71.8 percent of spells. I construct three mutually exclusive health classifications: respondents reporting either 'Excellent' or 'Very Good' health, respondents reporting 'Good' health, and respondents reporting either 'Fair' or 'Poor' health. I estimate Equation (2.3) separately for each stratified group.

Results are presented in Table 10. Interestingly, results are monotonic decreasing in order of worsening health status.[53] In order of worsening health status, the estimated effect of mandate eligibility in terms of increasing average private coverage across the spell varies considerably: point estimates are 4.0 percentage points in specification (1), 6.7 percentage points in specification (3), and 21.24 percentage points in specification (5), though the result for separators in 'Excellent' or 'Very Good' health is not statistically significant. Similarly, the estimated effect of 12 months of mandate eligibility in terms of increasing average private coverage across the spell vary consistently: points estimates are 7.1 percentage points in specification (2), 9.0

[53]These results suggest the potential for adverse selection in the take-up decision of continuation benefits. That is, a disproportionately high fraction of separators taking up benefits are high-cost enrollees. The welfare implications are unclear.

percentage points in specification (4), and 19.0 percentage points in specification (6), and each estimate is at least marginally statistically significant. These results are suggestive of heterogeneous responses by health status, and consistent with the notion that responsiveness to mandate eligibility is positively related to the net benefit of continuation coverage relative to the non-group market.[54]

2.5 The Role of UI

2.5.1 Incorporating UI Generosity

Information on the regulations regarding UI eligibility criteria and benefit schedules across states is reported semiannually by the United States Department of Labor. The basis for both the monetary eligibility calculator and benefits calculator come from the initial calculators developed by Gruber (1997) and later updated by Chetty (2008) and Brown (2010). This paper improves upon the inherited calculators, incorporating earlier panels relative to Brown (2010), while enhancing accuracy through consultation of legislative documentation and extended eligibility criteria to include distributional considerations relative to Chetty (2008).

Eligibility

Eligibility for UI is multi-dimensional. Monetary eligibility is established through qualifying wages, often paired with a required wage distribution across the 'base period', defined as the first four of the past five calendar months.[55] The worker must not have exhausted available benefits within a given benefits period. Recipients must additionally demonstrate nonmonetary eligibility, generally consisting of: (1) unemployed through no fault of their own, (2) able and available to work full-time, and (3) actively seeking full-time work.[56]

A job separation is excluded if prior work history appears to make the worker ineligible for UI.[57] This motivates exclusion of self-employed workers from the sample, as they cannot avail themselves to the UI system.[58] I restrict to spells in which the individual reports looking for work in at least some months in order to focus on unemployment and not strictly labor force

[54] Comparison of point estimates between the sample of separators in 'Excellent' or 'Very Good' health relative to those in 'Fair' or 'Poor' health status through generalized Hausman tests reveals a difference in the effect of mandate eligibility that is statistically significant at the 10% level. The difference in the effect of months of mandate eligibility is statistically insignificant, as is every comparison involving separators in 'Good' health. Thus, results are merely suggestive.

[55] Example: In January 1999, Texas required that the applicant: (1) earn base period wages at least 37 times that of the computed weekly benefit amount and (2) document wages in at least 2 quarters.

[56] A waiting period is also imposed, typically one week. Given person-month observations, I ignore the impact of this provision, though this will understate the impact of actual UI eligible benefits in the first month of unemployment.

[57] Absent a consistent measure of reason for separation within the SIPP, I potentially include some voluntary separations, though the restriction to periods of unemployment where the individual reports looking for work should exclude at least a fraction of voluntary separations.

[58] An indicator for self-employment status was removed starting with the 1996 panel. I rely upon the BLS

leavings. However, while UI eligibility requires continuing eligibility through demonstration of labor force attachment, in specifications reported below observations are included if individuals report stopping search effort, effectively dropping out of the labor force, to account for the discouraged worker effect.

I include temporary layoffs, despite concerns of potentially different information about probabilities of layoff and recall, as well as potential ex-ante arrangements with the employer, for two reasons: (1) temporary layoffs consist of approximately 13 percent of all unemployment spells in my core sample and exclusion may result in non-representative spells, and (2) these individuals may constitute a particularly responsive margin as they are plausibly more aware of the UI system's parameters.[59]

Benefits

Receipt of UI benefits is not automatically provided, rather an individual satisfying state-defined eligibility criteria must apply for benefits. Among eligibles, take-up is much less than full. Blank and Card (1991) estimate take-up rates of roughly 67 percent among eligibles. An alternative to the use of eligibility is the actual UI benefits receipt amongst the unemployed. However, this poses a potentially serious selection bias, as take-up of UI may be endogenous to benefit level, thus I do not condition on receipt of UI benefits.[60] Also, receipt of public assistance is generally noisily measured in survey data. While this may call for use of eligibility as an instrument for actual UI receipt, Gruber (1997) persuasively argues that UI benefit eligibility, rather than actual UI receipt, is of direct policy relevance.

Weekly UI benefits are constructed as non-linear, and in some states complex, functions of wage levels and distribution in the base period.[61] Accurate benefits estimation requires five calendar quarters of earnings history, which is not available for a non-trivial subset of the

definition of self-employed as workers for their own, unincorporated businesses, including those who worked for profit or fees in their own unincorporated business or professional practice. If self-employed as workers for their own, incorporated businesses, then these workers are not classified as self-employed because they are paid employees of their own companies. A small fraction of workers report self-employment income less than full wage income. For this group, I classify the individual as self-employed if self-employment income composes at least 50 percent of total wage income. For consistency, this measure is applied across all panels.

[59] Temporary layoffs are documented to be endogenous to the level of UI generosity. Thus if a disproportionately large number of temporary layoffs are included as a result of high UI generosity and if those spells are documented to have natural smoothing properties, perhaps through ex-ante arrangements with the employer, then UI generosity may have a spurious positive relationship with health insurance coverage across the unemployment spell. However, it is unclear that this bias would result in a non-trivial interaction term with mandate eligibility. Results are largely robust to the exclusion of temporary layoffs.

[60] Restricting the sample to those who take-up UI could lead to selection bias due to the endogenous nature of the take-up decision with respect to the benefit level (Anderson and Meyer 1997). If factors determining UI take-up are correlated with the change in an individual's private health insurance coverage associated with unemployment, then the effect of UI on private insurance coverage of the unemployed will tend to overstate the effect. There is some "option value" to individuals who do not take-up benefits, but derive value from the availability of UI resources should the individual encounter a longer-than-expected unemployment duration.

[61] Example: In January 2001, Texas weekly benefit amounts are assigned as 1/25 of high quarter wages, subject to a minimum wba of \$48 and a maximum wba of \$294. With 13 weeks per quarter, this is designed to replace approximately 50 percent of a recipient's weekly wage.

sample. Instead, I impute an individual's earnings history as completely as the data allow, requiring a minimum of one quarter of wage data. Additional inputs used in determining weekly benefit amounts vary by state-year and include: annual earnings, number of children, spousal work status, and average tax rate. State-specific rules for minimum and maximum weekly benefit amounts are then imposed and vary greatly across states. The worker's replacement rate is constructed as the ratio of weekly UI benefits to the weekly wage level in the base period.[62]

A summary of the variation in UI generosity across states is presented in Table 11. Although there is benefit generosity variation with each state over time, variation is largely drawn from a cross-state comparison. Average real weekly UI benefits range from a minimum of \$133.40 in Mississippi to a high of \$255.57 in Massachusetts.

2.5.2 Excess Sensitivity to Cash-In-Hand

To identify the heterogenous effect of mandate eligibility by cash-in-hand of the separated worker, I consider a modified version of Equation (2.3):[63]

$$\overline{Private}_{ist} = \beta_1 Eligible_{it} + \beta_2 WBA_{it} + \beta_3 Eligible_{it} \bullet WBA_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \quad (2.6)$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-spell level. $\overline{Private}_{ist}$ is a measure of the average number of months across the spell in which the unemployed worker reports private health insurance coverage from any source. $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. WBA_{it} is an individual's level of eligible (real) UI weekly benefit amount.[64] z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[65] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. To avoid overweighting repeated short spells relative to the representative spell, I construct an individual weight equal to the reciprocal of the individual's

[62] If unemployment is expected to increase earnings, such that the eligible weekly benefit amount exceeds weekly earnings over the base period, I exclude an individual. Point estimates are minimally affected by this exclusion restriction, though precision is improved.

[63] Prior to the Tax Reform Act of 1986, which made all UI benefits taxable after December 31, 1986, UI benefits were tax subsidized for households with income below \$12.000 and \$18,000 for single filers and joint filers respectively. Although potentially problematic, binary assignment of eligibility occurs universally in July 1986 and results are similar to the months of eligibility results scaled by one year. Further, exclusion of impacted households yields similar, though less precisely estimated results. In terms of excess sensitivity to cash-in-hand, tax withholdings on UI benefits are voluntary, thus only households with minimal senstivity to UI benefits should be directly affected in the immediate term.

[64] Weekly benefit amounts are discounted to January 1990 dollars using the CPI. Alternatively I consider discounting by the Medical Care component of the Consumer Price Index, motivated by the fact that rate of increase in health insurance premiums outpaces the CPI. Inference is comparable.

[65] Controls are equivalent to Equation (2.2).

count of unemployment spells, such that an individual's weights sum to one. In alternative specifications, I replace $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate-eligible months.

β_1 provides an estimate of the effect of continuation mandates for workers absent state UI benefits. β_2 estimates the effect of an increase in eligible UI benefits absent mandate eligibility.[66] β_3 estimates the sensitivity of the effect of mandate eligibility to increased cash-in-hand through the state UI system.

To account for potential legislative endogeneity, such as a spurious correlation resulting from state UI generosity increasing during peak economic periods associated with a limited fall in private health insurance coverage – as perhaps through the spouse's employer, I include seasonally adjusted state unemployment levels.[67] State indicators control for time-invariant heterogeneity across states correlated with UI generosity, such as risk aversion. The inclusion of state and time fixed effects results in a model effectively identified from higher order interactions of wage, state, and time, assumed to be legitimately excluded from an individual's health insurance decision. Results are presented in Table 12.

Simulated Instrument Approach

Motivation for the implementation of a simulated instruments instrumental variables strategy is drawn from Meyer (1990), noting that the UI replacement rate for an individual is a function of the legislative environment in a given state-year, but also of an individual's characteristics. Even with flexible controls, relative state UI generosity may reflect differences in the distribution of incomes and other individual characteristics across states, thus confounding inference of the effect of UI on private health insurance coverage. I therefore instrument for predicted UI benefit eligibility using 'simulated eligibility', a strategy developed in Currie and Gruber (1996) and detailed in application to UI generosity in Levine (1993) and Gruber (1997). A related two-step procedure is proposed and implemented in Chetty (2008).

Using the national sample of individuals in each six month period, given the frequency of reported policy updates, I assign that sample to each state in that period. I calculate each individual's eligible weekly benefit amount. I then average the resulting weekly benefit amounts across the simulated sample for each state-year. The resulting instrument is purged of potentially confounding characteristics of the individuals in that state-year and is a function of only the legislative environment in that state-year.[68] This simulated instrument is then

[66] In results not reported, I alternatively consider the replacement rate as a measure of state UI generosity. Results are similar once scaled by sample means, though the replacement rate results are too noisy to draw proper inference. The replacement rate is constructed as $\left(\frac{\text{nominal wba}}{\text{pre-separation weekly earnings}}\right)$.

[67] Seasonally adjusted unemployment rates are used in place of non-seasonally adjusted unemployment rates as the discussion of legislative endogeneity of the form above would suggest policy deviations from atypical fluctuation in unemployment. One dimension of endogeneity captured by this approach is the "trigger" of extended unemployment benefits resulting from sufficiently high state unemployment.

[68] As the SIPP panels sample potentially systematically different populations over time, I have alternatively

incorporated as an excluded instrument.[69]

A second motivation for constructing the simulated instruments is related to inherent measurement error of the UI benefits calculator. Although a noisy proxy for eligible benefits for a given individual, owing to imputation and imprecise measurement of the income distribution throughout the base period, the estimated UI weekly benefits amounts should be correct on average. However, this noise component will drive the estimated coefficients towards zero in the classical errors-in-variables construction. Although the simulated instrument is, of course, a noisy measure as well, I can reasonably assume that the measurement error is uncorrelated across the measures, provided no systematic over- or under-estimation of weekly benefits amounts. The simulated instruments instrumental variables approach will then produce consistent estimates of the effect of eligible UI benefits under this assumption.

One limitation to this approach is a restriction in the variance of the UI generosity measure, as the measure is fixed at a point in time across all individuals within the state. As detailed in Table 11, the majority of variation in the simulated instrument is driven by differences across states, rather than within-states over time. Results of simulated instruments instrumental variables specifications (2SLS) are reported alongside the OLS results in Table 12.

Endogenous Durations

Given well-documented endogeneity of the duration of the unemployment spell to state UI generosity (Moffitt 1985, Meyer 1990, Chetty 2008), I alternatively consider a modified version of Equation (2.4):

$$\overline{Private}^{m}_{ist} = \beta_1 Eligible_{it} + \beta_2 WBA_{it} + \beta_3 Eligible_{it} \bullet WBA_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \quad (2.7)$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-spell level. $\overline{Private}^{m}_{ist}$ measures the average private health insurance coverage across the m months of the observation window, inclusive of both months unemployed as well as months re-employed. $Eligible_{st}$ is an indicator for non-zero months of mandate eligibility. WBA_{it} is an individual's level of eligible (real) UI weekly benefit amount. z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[70] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. In alternative specifications, I replace $Eligible_{st}$ with $Months_{st}$, which is treated as a continuous measure of mandate eligible months.

Results of both OLS and 2SLS specifications are reported alongside those of the collapsed spell

constructed the simulated instrument using a fixed national sample from 1990, with wage data inflated by the Employment Cost Index for wages and salaries. These results, not reported, produce similar results.

[69] Both the WBA_{it} and the $Eligible_{it} \bullet WBA_{it}$ term are endogenous regressors. The simulated instrument and its interaction with mandate eligibility are incorporated as excluded instruments.

[70] Controls are equivalent to Equation (2.2).

model in Table 12.

2.5.3 Interacted Results

Table 12 reports interacted results of excess sensitivity to cash-in-hand. OLS specifications are suggestive of a positive interaction, though only the simulated instruments approach yields statistically significant interactions between mandate eligibility and eligible UI benefits. Point estimates under the simulated instruments approach are substantially larger than their OLS analogue, perhaps reflecting the issue of measurement error noted above, though the overall pattern of coefficients under either estimation is consistent with the notion of excess sensitivity to cash-in-hand.

Mandate eligibility, evaluated in the absence of UI, is associated with a highly statistically significant increase in average private coverage across the spell, though the magnitude is diminished relative to baseline specifications. In specification (2), mandate eligibility is associated with an increase in private coverage of 2.4 percentage points evaluated at an eligible UI benefits level of 0, and in specification (4), 12 months of mandate eligibility evaluated at an eligible benefits level of 0 is associated with an increase in private coverage of 2.2 percentage points. Absent state UI, then, mandate eligibility is estimated to mitigate approximately 6.1 percent of the fall in private coverage, relative to a predicted fall of 39.5 percentage points presented in Table 3, and 12 months of mandate eligibility is estimated to mitigate approximately 5.6 percent of the fall in private coverage. These estimates are approximately 42.9 percent and 30.6 percent of the respective baseline results, suggesting that mandate eligibility is minimally effective in mitigating the fall in private coverage absent means of bolstering household ability to pay, such as through state UI.

The coefficient on the interaction term is a measure of excess sensitivity to cash-in-hand with respect to the responsiveness to mandate eligibility. These results imply that responsiveness to mandate eligibility increases with eligible UI benefits. Specification (2) suggests that the probability of maintaining private coverage is increased by approximately 10.0 percentage points for every \$100 in eligible weekly UI benefits in addition to the pure effect of mandate eligibility reported above. Similarly, results of specification (4) suggest that 12 months of mandate eligibility increase the probability of maintaining private coverage by 7.4 percentage points for every \$100 in eligible weekly UI benefits in addition to the pure effect of 12 months of mandate eligibility reported above. In total, then, mandate eligibility is estimated to increase the probability of private coverage by 21.3 percentage points evaluated at the sample average of \$190 in eligible weekly UI benefits. Similarly, 12 months of mandate eligibility is estimated to increase maintenance of private coverage by 16.3 percentage points when evaluate at the sample average of \$190 in eligible weekly UI benefits. Comparing specifications (1)-(4) to analogous specifications (5)-(8), inference is consistent across the collapsed spell model and the 3-month fixed window model, reducing concerns over duration endogeneity. Thus mandate eligibility appears to be substantially more effective in mitigating the fall in private coverage at higher eligible UI benefit levels, presumably by expanding the household's ability to pay to take-up continuation benefits.[71]

Stratified Results Along Mandate Eligibility

Since Equations (2.6) and (2.7) are partially interacted models, there is a concern that heterogeneous factors across the groups correlated with state UI generosity may spuriously drive the interaction term. In response, I estimate stratified models of the form:

$$\overline{Private}_{ist} = \beta WBA_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{2.8}$$

where i indexes individuals, s indexes states, and t indexes time. Observations are at the person-spell level. $\overline{Private}_{ist}$ is a measure of the average number of months across the spell in which the unemployed worker reports private health insurance coverage from any source. WBA_{it} is an individual's level of eligible (real) UI weekly benefit amount. z_{it} is a vector of individual and job characteristics taken from the wave prior to separation.[72] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. To avoid overweighting repeated short spells relative to the representative spell, I construct an individual weight equal to the reciprocal of the individual's count of unemployment spells, such that an individual's weights sum to one. Specifications are separately estimated for mandate-eligible spells and mandate-ineligible spells.

Results are presented in Table 13. These results suggest that the large interaction terms identified in Table 12 are not spurious. Rather, average private coverage over mandate-ineligible spells exhibits limited sensitivity to eligible state UI generosity relative to mandate-eligible spells. Comparing OLS results, the subset of mandate-ineligible spells are estimated to report an increase in average private coverage of 7.3 percentage points per \$100 in eligible weekly UI benefits in specification (1), compared to 17.5 percentage points for the mandate-eligible subset in specification (3). A similar relationship is observed for the 2SLS estimates, both are more pronounced at 9.5 percentage points and 22.9 percentage points respectively. These results roughly translate to the interacted model results. That is, for a mandate-ineligible spell, the interacted 2SLS results suggest that every \$100 in eligible weekly UI benefits increases private coverage by 12.75 percentage points, compared to 9.5 percentage points in the stratified specification. Similarly, for a mandate-eligible spell, the interacted 2SLS results suggest that every \$100 in eligible weekly UI benefits increases private coverage by a total of 22.7 percentage

[71] Note that these results are based on out-of-sample predictions and may not yield meaningful comparisons. Specifically, if individuals are sensitive to small changes in UI around the sample average, perhaps as basic subsistence is assured and health coverage may be a secondary concern for the unemployed, then these results may overstate the true effect of continuation of coverage mandates.

[72] Controls are equivalent to Equation (2.2).

points, compared to 22.9 percentage points from the stratified results.[73] Thus, the results of the interacted model appear valid, and not driven spuriously by improperly controlled group heterogeneity across the subsets of spells mandate-eligible and mandate-ineligible.

2.6 Conclusion

Consistent with the existing literature, I find a robust result that continuation of coverage mandates are only modestly effective in mitigating the fall of private coverage across employment status, reducing the loss of private coverage by approximately 18 percent. Yet responsiveness to mandate eligibility is heterogeneously distributed. I find concentration patterns suggestive of the importance of family composition and outside insurance options, as well evidence of a potential adverse selection issue regarding take-up of continuation benefits. Amongst these responsive populations, however, average private coverage rates are below those of less responsive populations, suggesting that mandate eligibility alone is an insufficient policy response to the issue of shortfalls in insurance coverage associated with unemployment.

Extending the analysis through simulation of eligible UI benefits, I find that absent state UI, mandate eligibility is estimated to increase private coverage by only 2.4 percentage points amongst unemployed workers. By comparison, evaluated at the sample average eligible weekly UI benefits, mandate eligibility is estimated to increase the maintenance of private coverage by 21.3 percentage points.

Excess sensitivity to cash-in-hand suggests an important role of liquidity constraints in limiting take-up of continuation benefits. Policy aimed at improving portability of health coverage across employment status must therefore address not only access to coverage at group market rates, but also address ability to pay for households experiencing low transitory income and, despite the effective subsidy relative to non-group rates, substantially higher out-of-pocket costs of insurance coverage through continuation coverage. Given persistent, dramatic declines in private coverage amongst the unemployed, even those eligible for continuation benefits, these results suggest potential inadequacy of current UI benefits or the need for direct subsidization of the purchase price of continuation coverage.

[73] These results are substantially greater in magnitude than the results of Brown (2010). This may be partially explained by sample restrictions to employer-sponsored coverage prior to separation, while Brown (2010) incorporated spells irrespective of pre-separation coverage. Further, though point estimates are statistically significant, the confidence intervals on the estimates are wide. Lastly, these estimates are out-of-sample predictions, as Table 11 demonstrates that state UI benefits do not vary dramatically within a state across the reference period.

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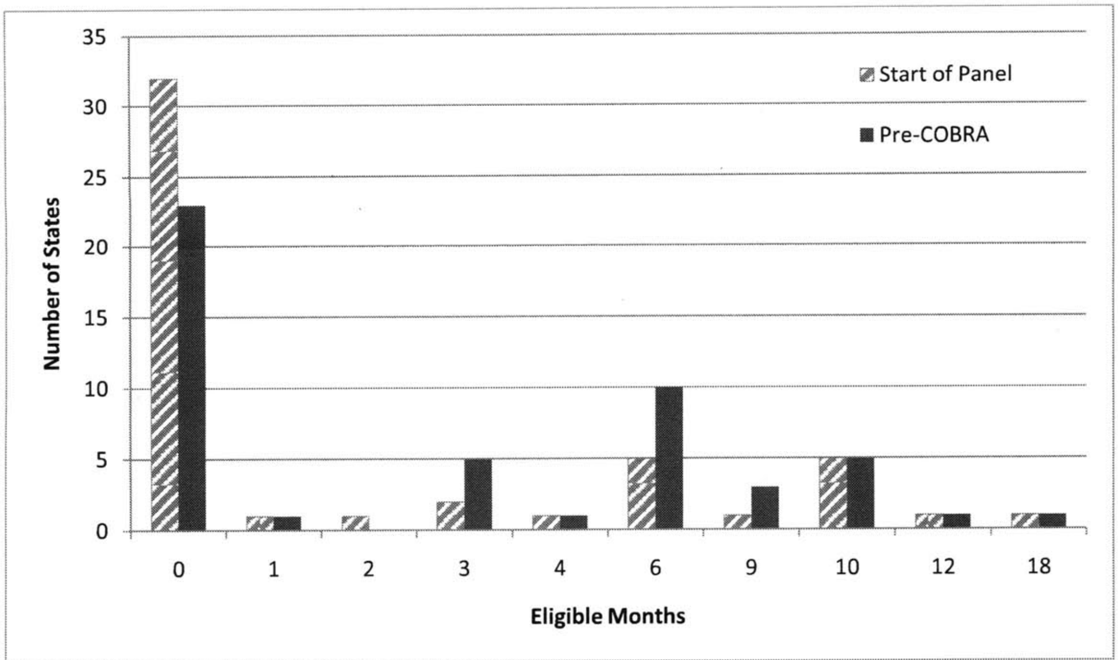

Figure 1. Distribution of State Mandates.

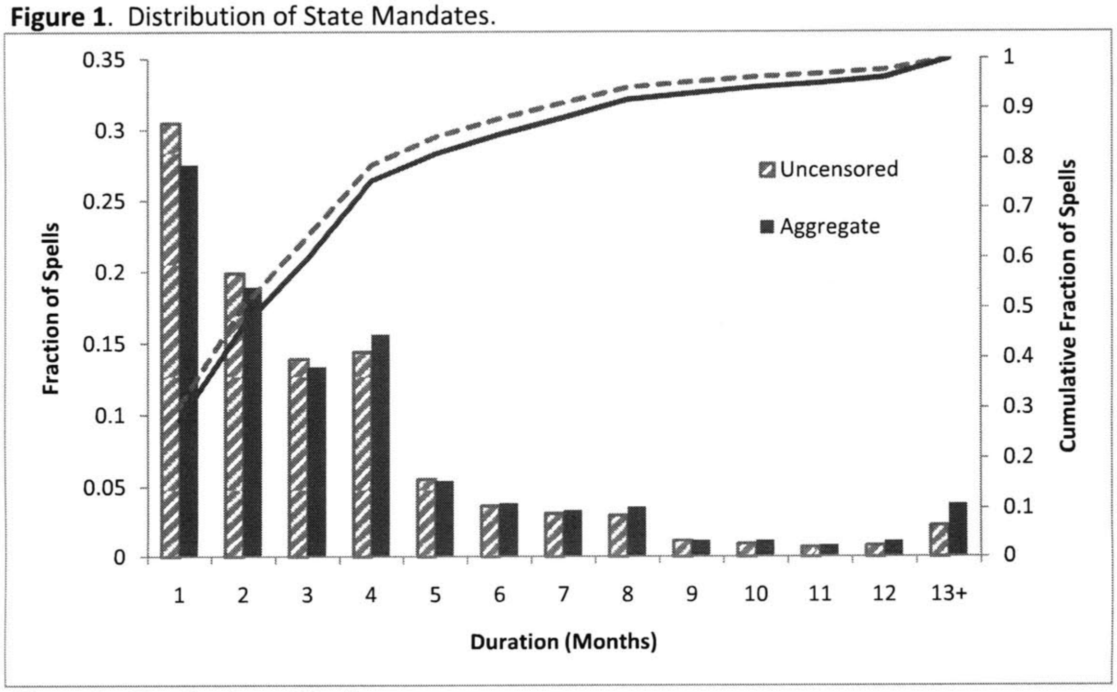

Figure 2. Distribution of Spells.
Aggregate spell count is 6,301 of which 5,257 (83.4%) are uncensored.

Table 1
Continuation of Coverage Laws

State	Date	Months	Voluntary	State	Date	Months	Voluntary
Arkansas	07/20/1979	4	Y	New Hampshire	08/22/1981	10	Y
California	01/01/1985	3	Y	New Mexico	07/01/1983	6	Y
Colorado	07/01/1986	3	Y	North Carolina	01/01/1982	3	Y
Connecticut	10/01/1975	10	Y	North Dakota	07/01/1983	10	Y
	01/01/1987	20	Y	New York	01/01/1986	6	Y
Georgia	07/01/1986	3	Y	Ohio	07/01/1984	6	N
Illinois	01/01/1984	6	Y	Oklahoma	01/01/1976	1	Y
	08/23/1985	9	Y	Oregon	01/01/1982	6	Y
Iowa	06/01/1984	6	N	Rhode Island	09/01/1977	10	N
	07/01/1987	9	Y	South Carolina	01/01/1979	2	Y
Kansas	01/01/1978	6	Y		01/01/1990	6	Y
Kentucky	07/15/1980	9	Y	South Dakota	07/01/1984	3	Y
Maryland	07/01/1986	18	N		03/03/1988	18	Y
Massachusetts	01/01/1977	10	N	Tennessee	01/01/1981	3	Y
Minnesota	08/01/1974	6	Y	Texas	01/01/1981	6	Y
	03/19/1983	12	Y	Utah	07/01/1986	2	Y
	06/01/1987	18	Y	Vermont	05/14/1986	6	Y
Missouri	09/28/1985	9	Y	Virginia	04/17/1986	3	Y
Nebraska	01/01/1978	6	N	Wisonsin	05/14/1980	18	Y
United States	07/01/1986	18	Y				

Sources: Hewitt (1985), Thompson Publishing Group (1992), and state statutes as cited in Gruber and Madrian (1997). Implementation date and mandated months of eligibility are indicated for state-mandated continuation of coverage laws preceding and immediately following implementation of federally-mandated eligibility through COBRA, phased-in between July 1986 and June 1987. Voluntary identifies whether mandate eligibility is extended to voluntary separations or only to involuntary separations.

Table 2

Summary Statistics Across Samples of Interest

Observation Period	October 1983 - December 2003				Oct 1983 - June 1986			
Mandate Eligibility	Unrestricted		Eligible (State/Federal)		Eligible (State)		Ineligible	
Source of Pre-Separation Coverage	Unrestricted	Employer	Unrestricted	Employer	Unrestricted	Employer	Unrestricted	Employer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Health Insurance Coverage								
Mean (Private HI) Across Spell	57.5	70.4	58.1	71.3	51.4	65.2	50.1	59.8
Private HI	72.6	--	72.6	--	69.0	--	71.3	--
Private HI in Own Name	62.0	--	62.1	--	59.7	--	61.3	--
Public HI	2.5	--	2.6	--	2.2	--	2.1	--
Employer-Provided HI	56.8	--	56.9	--	56.3	--	56.1	--
Demographics								
Age	37.4	38.0	37.6	38.2	35.7	36.6	35.1	35.5
Married	71.6	72.2	72.3	72.7	67.9	70.6	63.3	66.1
Working Spouse	45.1	45.2	46.0	46.0	39.9	40.3	34.3	34.7
Non-White	11.5	10.1	11.1	9.7	13.1	11.0	16.0	14.3
Number of Children	1.3	1.2	1.3	1.2	1.9	1.8	1.8	1.7
Education: Less than HS	17.8	13.0	17.6	12.8	24.0	18.3	20.5	14.9
Education: HS Graduate	39.5	39.1	39.2	38.7	39.0	38.5	43.3	44.5
Education: Some College	24.5	25.2	24.9	25.6	21.4	24.4	20.5	19.8
Education: 4+ Years of College	18.1	22.7	18.3	22.9	15.6	18.8	15.8	20.7
Annual Earnings	$20,036	$24,996	$20,140	$25,108	$18,786	$23,949	$18,798	$23,640
Median Annual Earnings	$16,949	$21,804	$17,004	$21,852	$16,328	$21,756	$16,230	$20,981
Completed Spell Durations (Weeks)								
Duration	14.5	14.5	14.3	14.2	14.7	15.4	16.9	17.9
Median Duration	9.0	9.0	9.0	9.0	9.0	11.0	11.0	12.0
UI Benefits								
Real WBA	$168	$190	$169	$191	$157	$183	$159	$180
UI Replacement Rate	50.4	45.0	50.4	45.0	50.3	44.5	50.2	45.2
Number of Spells	11,095	6,301	10,237	5,820	824	464	858	481

Table entries are mean values unless noted otherwise. Statistics are based upon the wave prior to separation unless noted otherwise. Data are drawn from the 1984-1988, 1990-1993, 1996, and 2001 SIPP panels. Left-censored spells of non-employment and unemployment (in-progress at the start of the panel), are excluded. Eligible/Ineligible refer to in-place state/federal mandates; only separators with employer-sponsored coverage prior to separation can opt to continue coverage through the former employer. UI weekly benefit amount is simulated per discussion in Section 5.1. UI replacement rate is constructed as the eligible weekly benefit amount divided by weekly pre-separation wage. Duration is defined as weeks elapsed from end of last job to start of next job and does not adjust for right-censoring (spells in-progress at the end of the panel). All monetary values are in real 1990 values.

Table 3
Private Health Insurance Coverage Across Employment Status

					Employer-Sponsored Coverage					
Sample Restriction	Experience UI-Eligible Unemployment		Private Coverage, Non-Employer		Pooled		Continuation of Coverage Eligible		Continuation of Coverage Ineligible	
Mean Dependent	.7220	.7220	.9131	.9131	.8497	.8497	.8526	.8526	.8067	.8067
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployed	-.2060***	-.1720***	-.0910***	-.0760***	-.3130***	-.2370***	-.3050***	-.2290***	-.3950***	-.3230***
	(.0051)	(.0040)	(.0088)	(.0069)	(.0073)	(.0060)	(.0076)	(.0062)	(.0236)	(.0221)
Age 30-34	.0030	--	.0010	--	-.0010	--	.0020	--	-.0320	--
	(.0047)	--	(.0108)	--	(.0065)	--	(.0067)	--	(.0229)	--
Age 35-39	.0040	--	.0060	--	.0050	--	.0060	--	-.0010	--
	(.0049)	--	(.0112)	--	(.0066)	--	(.0069)	--	(.0237)	--
Age 40-44	.0110**	--	.0060	--	.0120*	--	.0120*	--	.0310	--
	(.0051)	--	(.0113)	--	(.0068)	--	(.0070)	--	(.0276)	--
Age 45-49	.0050	--	.0250**	--	.0050	--	.0060	--	.0090	--
	(.0055)	--	(.0111)	--	(.0075)	--	(.0076)	--	(.0352)	--
Age 50-54	.0220***	--	.0180	--	.0260***	--	.0240***	--	.0840**	--
	(.0062)	--	(.0118)	--	(.0086)	--	(.0088)	--	(.0347)	--
Non-White	-.0080	--	-.0290**	--	-.0140*	--	-.0170**	--	.0260	--
	(.0056)	--	(.0123)	--	(.0080)	--	(.0084)	--	(.0254)	--
Married	.0270***	.0330*	-.0070	.0330	.0430***	.0180	.0400***	.0050	.0830***	.0730
	(.0051)	(.0183)	(.0186)	(.0721)	(.0068)	(.0229)	(.0070)	(.0255)	(.0235)	(.0529)
Spouse Works	.0760***	.0430***	.1240***	.1560***	.0460***	.0210***	.0460***	.0230***	.0510***	-.0050
	(.0037)	(.0059)	(.0124)	(.0185)	(.0047)	(.0072)	(.0048)	(.0077)	(.0189)	(.0228)
Kids: 1	-.0090**	.0080	-.0070	.0200	-.0170***	.0050	-.0140**	.0120	-.0630***	-.0900**
	(.0042)	(.0094)	(.0076)	(.0177)	(.0056)	(.0121)	(.0057)	(.0126)	(.0230)	(.0460)
Kids: 2	-.0170***	.0040	-.0180**	.0050	-.0180***	.0010	-.0160***	-.0020	-.0400*	-.0300
	(.0044)	(.0131)	(.0087)	(.0245)	(.0057)	(.0167)	(.0059)	(.0175)	(.0225)	(.0558)
Kids: 3	-.0300***	.0050	-.0440***	-.0350	-.0370***	-.0060	-.0320***	-.0050	-.0940***	.0010
	(.0056)	(.0166)	(.0134)	(.0316)	(.0077)	(.0218)	(.0078)	(.0233)	(.0302)	(.0678)
Kids: 4+	-.0470***	-.0120	-.0460**	-.0430	-.0530***	-.0030	-.0540***	-.0160	-.0560*	.0210
	(.0072)	(.0218)	(.0182)	(.0480)	(.0111)	(.0298)	(.0116)	(.0331)	(.0310)	(.0723)
HS Graduate	.0350***	--	.0250**	--	.0330***	--	.0320***	--	.0470	--
	(.0052)	--	(.0123)	--	(.0081)	--	(.0084)	--	(.0284)	--
Some College	.0440***	--	.0310**	--	.0330***	--	.0310***	--	.0760**	--
	(.0056)	--	(.0129)	--	(.0085)	--	(.0088)	--	(.0340)	--
College Graduate	.0670***	--	.0470***	--	.0540***	--	.0520***	--	.0850***	--
	(.0057)	--	(.0125)	--	(.0085)	--	(.0088)	--	(.0318)	--
Prior Private HI	.6010***	--	--	--	--	--	--	--	--	--
	(.0057)	--	--	--	--	--	--	--	--	--
Wage Splines	X	X	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X	X	X
State Effects	X		X		X		X		X	
Job Characteristics	X		X		X		X		X	
Individual Fixed Effect		X		X		X		X		X
Observations	218,215	218,215	34,227	34,227	118,168	118,168	110,536	110,536	7,632	7,632

The dependent variable is private health insurance coverage. Results correspond to estimating Equation (1). Observations are person-month level. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001. Omitted categories: Age 25-29, White, No Kids, Unmarried, Non-Working Spouse, Less than HS.

Table 4

Continuation of Coverage Eligiblity

Panel 1: Private Health Insurance Coverage

	Measure of Mandate Eligibility									
	Eligible					Eligible Months				
Estimation Model	LPM			Probit		LPM			Probit	
Mean Dependent	.5946	.5946	.5946	.5946		.5946	.5946	.5946	.5946	
Specification	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Eligibility	.0716**	.0915**	.0560*	.1756*	[.0682]	.0043**	.0096***	.0060**	.0202**	[.0077]
	(.0379)	(.0376)	(.0323)	(.1037)	--	(.0017)	(.0034)	(.0025)	(.0081)	--
Demographics			X	X				X	X	
Wage Splines			X	X				X	X	
Job Characteristics			X	X				X	X	
State Effects		X	X	X			X	X	X	
Year Effects		X	X	X			X	X	X	
Observations	20,971	20,971	20,971	20,971		20,971	20,971	20,971	20,971	

Panel 2: Health Insurnace Coverage (Any Source)

	Measure of Mandate Eligibility									
	Eligible					Eligible Months				
Estimation Model	LPM			Probit		LPM			Probit	
Mean Dependent	.6224	.6224	.6224	.6224		.6224	.6224	.6224	.6224	
Specification	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Eligibility	.0765*	.0756*	.0555*	.1756*	[.0682]	.0039**	.0100**	.0053**	.0165**	[.0061]
	(.0395)	(.0418)	(.0322)	(.1038)	--	(.0015)	(.0041)	(.0023)	(.0073)	--
Demographics			X	X				X	X	
Wage Splines			X	X				X	X	
Job Characteristics			X	X				X	X	
State Effects		X	X	X			X	X	X	
Year Effects		X	X	X			X	X	X	
Observations	20,971	20,971	20,971	20,971		20,971	20,971	20,971	20,971	

Panel 3: Public Health Insurnace Coverage

	Measure of Mandate Eligibility									
	Eligible					Eligible Months				
Estimation Model	LPM			Probit		LPM			Probit	
Mean Dependent	.0349	.0349	.0349	.0349		.0349	.0349	.0349	.0349	
Specification	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)	
Eligibility	-.0037	-.0104	.0002	-.0716	[-.0025]	-.0005	-.0011	-.0001	-.0038	[-.0001]
	(.0157)	(.0207)	(.0177)	(.1915)	--	(.0007)	(.0014)	(.0012)	(.0119)	--
Demographics			X	X				X	X	
Wage Splines			X	X				X	X	
Job Characteristics			X	X				X	X	
State Effects		X	X	X			X	X	X	
Year Effects		X	X	X			X	X	X	
Observations	20,971	20,971	20,971	20,971		20,971	20,971	20,971	20,971	

Dependent variable is specified for each panel. Results correspond to estimating modifications of Equation (2). Observations are person-month level and include only UI-eligible unemployed observations for separators with employer-sponsored coverage prior to separation. Eligibility measures the effect of any mandate eligibility or the effect of one additional month of mandate eligibility. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001. Marginal effects of the probit specifications are calculated at the mean and are reported in brackets alongside the relevant point estimate.

Table 5

Continuation of Coverage Eligiblity - Alternative Models

	Measure of Mandate Eligibility					
	Eligible			Eligible Months		
Estimation Model	LPM	Spell	3-Month	LPM	Spell	3-Month
Mean Dependent	.5946	.7038	.7427	.5946	.7038	.7427
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility	.0560*	.0512*	.0581*	.0060**	.0071***	.0081***
	(.0323)	(.0301)	(.0320)	(.0025)	(.0021)	(.0023)
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Observations	20,971	6,301	5,706	20,971	6,301	5,706

Dependent variable varies across estimation models: LPM observations are person-month level and the dependent variable is private coverage across a given month of unemployment, estimating Equation (2); Spell observations are collapsed person-spell level and the dependent variable is average months of private coverage across the entirety of the unemployment spell, estimating Equation (3); 3-Month observations are collapsed to the person-separation level and the dependent variable is average months of private coverage across the three months following a separation, inclusive of both months unemployed and potentially re-employed, estimating Equation (4). All models include only UI-eligible unemployment separations for separators with employer-sponsored coverage prior to separation. Eligibility measures the effect of any mandate eligibility or the effect of one additional month of mandate eligibility. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 6
Endogeneity of Pre-Separation Coverage to Continuation of Coverage Mandates

	Measure of Mandate Eligibility					
	Eligible			Eligible Months		
Pre-Separation HI Restriction	Unrestricted	Unrestricted	Any Private	Unrestricted	Unrestricted	Any Private
Dependent Variable	Any Private	Employer	Employer	Any Private	Employer	Employer
Mean Dependent	.7255	.5679	.7828	.7255	.5679	.7828
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility	-.0038	-.0018	.0014	-.0002	-.0001	.0001
	(.0031)	(.0024)	(.0026)	(.0002)	(.0002)	(.0002)
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Observations	11,095	11,095	8,055	11,095	11,095	8,055

Dependent variable is a measure of health insurance coverage pre-separation. Results correspond to estimating Equation (5). Observations are person-spell level and include only UI-eligible unemployment spells. Specifications (1) and (4) estimate the change in probability that a separation occurs with private coverage in the wave prior to separation, and the sample includes all UI-eligible unemployment spells. Specifications (2) and (5) estimate the change in probability that a separation occurs with employer-sponsored coverage in the wave prior to separation, and again the sample includes all UI-eligible unemployement spells. Specifications (3) and (6) estimate the change in probability that a separation occurs with employer-sponsored coverage in the wave prior to separation, conditional on some form of private coverage prior to separation, thus investigating a change in the composition of private coverage. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 7

Falsification Check - Early COBRA Phase-In

	Measure of Mandate Eligibility					
	Eligible			Eligible Months		
Observation Window	1983-2003	1983-2003	1983-1986	1983-2003	1983-2003	1983-1986
State Mandates	Unrestricted	Restricted	Restricted	Unrestricted	Restricted	Restricted
COBRA Phase-In	1986-1987	1986-1987	1985-1986	1986-1987	1986-1987	1985-1986
Mean Dependent	.7038	.6946	.6721	.7038	.6946	.6721
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility	.0512*	.0756**	-.0078	.0071***	.0060***	-.0004
	(.0301)	(.0309)	(.0157)	(.0021)	(.0023)	(.0014)
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Observations	6,301	5,712	779	6,301	5,712	779

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (3). Observations are person-spell level and include only UI-eligible separations. Restricted state mandates are those occuring between the start of the panel and the phase-in of COBRA starting July 1986, including: CA, IL, IA, IA, MO, NY, OH, SD, VT, and VA. Note that IA, SD, and VT are excluded in all specifications due to a non-unique SIPP identifier. Separations occuring between the start of the panel and the end of COBRA phase-in in June 1987 are excluded where noted. The observation window 1983-1986 more precisely includes October 1983-June 1986, preceding phase-in of COBRA. COBRA phase-in 1986-1987 follows details of the text, while phase-in 1985-1986 falsely begins the phase-in in July 1985 and completes phase-in in June 1986. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 8

Stratified Results Across Source of Pre-Separation Coverage

	Source of Pre-Separation Coverage			
	Employer		Private, Non-Employer	
Measure of Mandate Eligiblity	Eligible	Eligible Months	Eligible	Eligible Months
Mean Dependent	.7038	.7038	.8972	.8972
Specification	(1)	(2)	(3)	(4)
Eligibility	.0512*	.0071***	-.0143	.0004
	(.0301)	(.0021)	(.0120)	(.0027)
Demographics	X	X	X	X
Wage Splines	X	X	X	X
Job Characteristics	X	X	X	X
State Effects	X	X	X	X
Year Effects	X	X	X	X
Observations	6,301	6,301	1,753	1,753

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (3). Observations are person-spell level and include only UI-eligible separations. Source of pre-separation coverage identifies whether a separator had employer-sponsored coverage prior to separation or private coverage not through the employer, typically coverage through a spouse's employer or purchase in the individual market. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 9

Stratified Results Across Houshold Composition

	Pre-Separation Household Composition					
	Single		Married Spouse Employed		Married Spouse Non-Employed	
Measure of Mandate Eligiblity	Eligible	Eligible Months	Eligible	Eligible Months	Eligible	Eligible Months
Mean Dependent	.5463	.5463	.7679	.7679	.6252	.6252
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility	.0816*	.0084**	.0253	.0035	.1131**	.0126**
	(.0433)	(.0038)	(.0401)	(.0040)	(.0519)	(.0049)
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Observations	1,752	1,752	2,848	2,848	1,701	1,701

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (3). Observations are person-spell level and include only UI-eligible separations. Pre-separation spousal dependence identifies whether a separator was unmarried at the point of separationn, married with an employed spouse, or married with a non-employed spouse. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 10

Stratified Results Across Pre-Separation Health Status

	Pre-Separation Health Status					
	Excellent, Very Good		Good		Fair, Poor	
Measure of Mandate Eligiblity	Eligible	Eligible Months	Eligible	Eligible Months	Eligible	Eligible Months
Mean Dependent	.6799	.6799	.6010	.6010	.5600	.5600
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Eligibility	.0402	.0059**	.0673*	.0075*	.2124**	.0158*
	(.0352)	(.0026)	(.0382)	(.0041)	(.1017)	(.0083)
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Observations	3,162	3,162	1,083	1,083	282	282

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating modifications of Equation (3). Observations are person-spell level and include only UI-eligible separations. Pre-separation health status is self-identifed; respondents indicate excellent, very good, good, fair, or poor health. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 11
Unemployment Insurance Generosity by State

State	Number of Spells	Mean Weekly Benefit Amount	Standard Deviation of WBA	Mean Simulated Instrument (WBA)	Standard Deviation of SI (WBA)
Alabama	83	134.88	21.82	125.85	5.44
Arizona	101	144.65	16.70	137.69	5.53
Arkansas	31	189.99	36.40	168.60	15.33
California	763	169.84	38.68	149.23	8.86
Colorado	82	205.24	52.68	168.51	14.01
Connecticut	92	236.02	57.19	198.13	8.86
Delaware	21	204.46	28.36	172.75	11.89
D.C.	6	190.70	60.03	200.77	16.67
Florida	253	171.29	42.10	156.37	11.80
Georgia	167	158.78	32.99	145.41	13.18
Hawaii	13	192.39	94.79	193.04	21.30
Illinois	375	202.88	59.63	168.10	10.25
Indiana	188	148.98	38.40	139.26	26.13
Kansas	63	197.11	40.43	182.65	8.59
Kentucky	95	170.23	48.98	154.08	18.78
Louisiana	118	176.58	52.53	153.26	29.77
Maryland	95	195.65	23.64	171.58	5.81
Massachusetts	148	255.57	72.12	219.08	17.25
Minnesota	213	214.72	60.13	184.76	17.69
Mississippi	86	133.40	22.77	131.81	4.63
Missouri	190	149.31	16.41	141.02	10.53
Nebraska	36	137.74	21.72	129.94	11.11
Nevada	15	184.26	24.22	164.77	6.96
New Hampshire	28	173.98	35.34	143.14	19.01
New Jersey	211	245.56	63.74	198.90	16.37
New Mexico	10	147.82	38.69	152.24	8.92
New York	412	196.49	63.12	162.88	20.02
North Carolina	203	185.53	54.92	178.57	18.39
Ohio	351	181.70	55.24	154.82	6.60
Oklahoma	78	195.98	32.13	178.70	9.13
Oregon	131	224.40	47.89	180.31	12.11
Pennsylvania	386	236.53	58.75	212.28	8.45
Rhode Island	28	270.55	63.81	221.67	21.90
South Carolina	68	152.28	30.04	144.17	12.96
Tennessee	134	150.37	31.07	143.12	15.30
Texas	446	194.66	41.31	176.03	5.84
Utah	27	194.26	43.06	176.21	13.19
Virginia	101	161.05	33.86	151.15	8.64
Washington	188	227.10	61.07	185.60	19.93
West Virginia	70	184.53	54.21	166.97	5.41
Wisconsin	195	194.67	39.85	166.25	15.48
United States	6,301	190.03	57.11	166.78	26.11

Spell counts indicate the number of UI eligible unemployment spells observed within each uniquely identified state across the 1984-1988, 1990-1993, 1996, and 2001 SIPP panels. Eligibility includes both monetary and non-monetary components. Calculated weekly benefit amounts are constructed through simulation at the individual level, building upon the benefits/eligibility calculators developed in Gruber (1997) and modified by Chetty (2008). Discussion regarding construction of the simulated instrument is presented in Section 5.2. All monetary values are in real 1990 values.

Table 12

Excess Sensitivity to Cash-In-Hand

Estimation Model	Unemployment Spell				3-Month Window			
Measure of Mandate Eligiblity	Eligible		Eligible Months		Eligible		Eligible Months	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	.7038	.7038	.7038	.7038	.7427	.7427	.7427	.7427
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligibility	.0103	.0240***	.0010***	.0018***	.0130*	.0335***	.0012***	.0015**
	(.0072)	(.0087)	(.0004)	(.0006)	(.0077)	(.0081)	(.0004)	(.0007)
UI WBA	.0918***	.1275*	.0791***	.1164*	.0999***	.1006**	.0839**	.1109*
	(.0271)	(.0741)	(.0289)	(.0685)	(.0362)	(.0466)	(.0370)	(.0652)
Interaction	.0430	.0996**	.0016	.0062**	.0513	.1018**	.0018	.0049**
	(.0296)	(.0416)	(.0016)	(.0029)	(.0352)	(.0518)	(.0019)	(.0024)
Demographics	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X	X	X
State Effects	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X
Observations	6,301	6,301	6,301	6,301	5,706	5,706	5,706	5,706

Dependent variable varies across estimation models: Spell observations are collapsed person-spell level and the dependent variable is average months of private coverage across the entirety of the unemployment spell, estimating Equation (6); 3-Month observations are collapsed to the person-separation level and the dependent variable is average months of private coverage across the three months following a separation, inclusive of both months unemployed and potentially re-employed, estimating Equation (7). All models include only UI-eligible unemployment separations for separators with employer-sponsored coverage prior to separation. Eligibility measures the effect of any mandate eligibility or the effect of one additional month of mandate eligibility. The UI weekly benefit amount is simulated per discussion in Section 5.1. Construction and motivation of the simulated instruments approach is discussed in Section 5.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a real $100 increase in UI benefits on maintaining private coverage. Standard errors, reported in parentheses beneath the estimated coëfficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 13

Excess Sensitivity to Cash-In-Hand: Stratified by Mandate Eligibility

Mandate Eligibility	Ineligible		Eligible	
	OLS	2SLS	OLS	2SLS
Mean Dependent	.5984	.5984	.7125	.7125
Specification	(1)	(2)	(3)	(4)
UI WBA	.0726*	.0951*	.1748***	.2285**
	(.0414)	(.0499)	(.0460)	(.0909)
Demographics	X	X	X	X
Wage Splines	X	X	X	X
Job Characteristics	X	X	X	X
State Effects	X	X	X	X
Year Effects	X	X	X	X
Observations	481	481	5,820	5,820

Dependent variable is average private health insurance coverage across the unemployment spell. Results correspond to estimating Equation (8). Observations are person-spell level and include only UI-eligible separations. The UI weekly benefit amount is simulated per discussion in Section 5.1. Construction and motivation of the simulated instruments approach is discussed in Section 5.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a real $100 increase in UI benefits on maintaining private coverage. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Chapter 3

Re-evaluating Unemployment Insurance and the Crowd-Out of Spousal Labor Supply

3.1 Motivation

The notion that spousal labor supply may act as a form of insurance against unemployment of the household's primary earner, the 'added worker effect' (AWE), dates to Woytinsky (1942). Surveys by Hamermesh (1989) and Fallick (1996) document negative earnings effects of job displacements, as might induce spousal labor force participation. Yet, the existing literature has somewhat surprisingly produced inconsistent estimation of the AWE. By partially insuring against the adverse event of job loss of the primary earner, unemployment insurance (UI) plausibly mitigates private insurance in the form of spousal labor supply. An important question, then, is to what extent private arrangements would insure against these losses absent government intervention, recognizing that current private insurance provisions are endogenously selected in the presence of social insurance arrangements. Indeed, the generosity of the UI system has been documented to crowd-out private insurance along other margins, specifically precautionary savings (Engen and Gruber 2001, Klein 2009) and severance pay (Chetty and Saez 2010). Further, heterogeneity of this crowd-out effect with respect to liquidity constraints is of particular interest, given a recent empirical literature documenting the importance of such constraints in rationalizing excess sensitivity to transitory income shocks.

In contrast to the widely documented endogenous durations of unemployment spells to UI benefits (e.g. Moffitt 1985, Meyer 1990, Chetty 2008), work investigating the crowd-out of spousal labor supply resulting from provision of UI is limited. Cullen and Gruber (2000) investigate the role for social insurance, in the form of UI, to provide a state-contingent transitory income stream that partially offsets the negative income effect from unemployment. The authors estimate that eligible UI benefits crowd-out spousal earnings at a rate of 36 percent.[1] The magnitudes of crowd-out reported by Cullen and Gruber (2000) are suggestive of a highly responsive margin, though the analysis exhibits some notable omissions and methodological issues addressed in this paper. Most critically, the inclusion of UI-eligible spouses in the estimation sample produces a potentially spurious negative relationship between the husband's eligible UI benefits and spousal labor supply.[2] To the extent that the eligible UI benefits of husbands and wives are correlated through state generosity, spouses may exhibit reduced labor supply in response to own-eligibility, as through extended spousal unemployment spells.

To address this limitation of existing evidence of the crowd-out of spousal labor supply, I restrict to the set of spells for which the spouse is identified as ineligible for UI benefits, exploiting the longitudinal nature of the Survey of Income and Program Participation (SIPP) spanning the reference period 1983-2003. These restrictions provide plausibly more accurate, though less representative, estimates of crowd-out, purged of the direct effect of UI resulting from own-eligibility of the spouse.[3] Employing a simulated instrument approach, I find that eligible UI benefits crowd-out spousal earnings at a rate of 33 percent. Thus, the indirect effect of UI through eligible benefits of the unemployed primary earner plays an important role in the spousal labor supply decision. Despite evidence of responsive spousal earnings, estimates of the increase in spousal labor supply absent UI imply that increased spousal earnings would only offset roughly 13 percent of lost transitory income of the unemployed primary earner. This indicates that even in the absence of social insurance, spousal labor supply would only imperfectly insure against unemployment of the primary earner. This result reflects the partial replacement of wages under UI and comparatively lower earnings potential for the spouse relative to the unemployed primary earner.[4]

These results suggest a surprising extent of responsiveness in the context of a life cycle model, where there is limited scope for a transitory income shock such as unemployment to substantively affect spousal labor supply. Liquidity constraints rationalize the results above, as spousal labor supply would be more responsive to transitory income fluctuations of the primary earner. In the presence of credit market imperfections, then, UI benefits may result in a socially beneficial crowd-out of spousal labor supply operating through a liquidity effect. To investigate the importance of liquidity constraints, I stratify across proxies for a couple's liquidity constrained

[1]That is, for every dollar of eligible UI benefits, spousal earning are reduced by 36 cents.

[2]Although Cullen and Gruber (2000) present some evidence of a comparatively diminutive relationship between UI generosity and spousal labor supply for wives of employed husbands, results discussed below suggest correlated unemployment risk across partners, thus this evidence may underestimate the implied bias.

[3]As this sub-population is comprised largely of non-working spouses and those with weak attachment to the labor force, the results will not generalize to the aggregate of separations. I exclude the trivial number of working, UI-ineligible spouses and focus exclusively on non-working, UI-ineligible spouses at the point of separation.

[4]Across the 1983-2003 reference period, the average UI replacement rate for unemployed primary earners is 47.6 percent of pre-separation weekly wages.

status at the point of separation.[5] Without conditioning on a UI-ineligible spouse, heterogenous estimates of crowd-out cannot be clearly attributed to the indirect eligible UI benefits of the primary earner (solely an income effect), given the confounding factor of the direct effect of spousal eligibility (composed of both income and substitution effects).[6] Restricting to UI-ineligible spouses, I find that couples proxied as liquidity unconstrained are only 26.2 percent as responsive as couples proxied as liquidity constrained in terms of spousal average weekly earnings, stratifying by above and below median net liquid wealth at the point of separation. Similarly, stratification across mortgage status at the point of separation suggests that relative to couples with a fixed consumption commitment, the spousal average weekly earnings of couples without a fixed consumption commitment are only 17.6 percent as responsive. Although estimates for the restricted sample of UI-ineligible spouses are imprecise, I find parameter concentrations suggestive of an associated liquidity effect.

The rest of the paper proceeds as follows. A brief review of the existing literature is discussed in the next section. Section 3.3 details the data and core sample selection criteria. Section 3.4 re-evaluates existing estimates of the extent to which UI crowds-out spousal labor supply across unemployment spells of the primary earner. Section 3.5 introduces proxies for a couple's liquidity constrained status at the point of separation and differentiates heterogeneous spousal labor supply crowd-out by eligible UI benefits. The last section concludes.

3.2 A Brief Literature Perspective

Documenting an Added Worker Effect

The extent to which spousal labor supply is predicted to respond to unemployment of the household's primary earner, and specifically the sequencing of this response, is unclear. Surveys by Hamermesh (1989) and Fallick (1996) document negative earnings effects of job displacements, as might induce spousal labor force participation. Yet to the extent that the transitory income shock of unemployment is relatively minor in comparison to the total negative earnings effect associated with job loss, the responsiveness of spouses across the unemployment spell will be limited. Topel (1990) and Stevens (1997) document annual earnings losses in the year of displacement that range from 25 percent to 40 percent. Instantaneous wage losses are comparatively limited in relation to permanent earnings losses (Stevens 1997, Topel 1990, Ruhm 1991). While there appears to be a case for a non-trivial spousal labor supply response, it is entirely unclear that the response in the immediate term of the unemployment spell will be

[5] Specifically, I follow Chetty (2008) in considering net liquid wealth of the household as a proxy of liquidity constraint and the mortgage status of the household as a proxy of consumption commitment. Although Engen and Gruber (2001) suggest endogeneity of household assets to the generosity of the UI system, that work is critiqued in a recent evaluation by Klein (2009), suggesting a non-robust relationship.

[6] This is a particular concern given strong evidence of a heterogeneous response to UI generosity with respect to extending unemployment durations (Chetty 2008).

dramatic relative to a longer-term response.

Some studies that have attempted to document a contemporaneous AWE have found modest effects (Mincer 1962; Bowen and Finegan 1968; Heckman and Macurdy 1980, 1982; Lundberg 1985; Spletzer 1991).[7] Others have found no effect (Layard, Barton, and Zabalza 1980; Maloney 1987, 1991; Cullen and Gruber 1996). A recent investigation by Stephens (2002) investigates the dynamics of the AWE and documents small pre-displacement effects and larger, persistent post-displacement effects for wives of husbands who experience displacement. This is consistent with notion that the transitory income shock of unemployment is modest relative to the long-term permanent earnings losses. It is, therefore, unclear that UI provision substantively affects the spousal labor supply response, particularly given evidence of persistent effects of unemployment on spousal labor supply beyond the immediate duration of the unemployment spell.

The Role of Unemployment Insurance

Yet, these studies fail to address the presence of social insurance that protects against precisely the sort of transitory income loss insured through the AWE. UI provides a state-contingent transitory income stream that partially counteracts the negative income effect associated with unemployment of the primary earner. Thus, increased UI benefits may mitigate the spousal labor supply response, and may be partially responsible for masking the AWE. Then absent UI, the AWE would be more prominent, as the household would be less insulated from the negative transitory income shock associated with unemployment. Cullen and Gruber (2000) investigate the role of public insurance in the form of UI; the authors estimate that eligible UI benefits crowd-out spousal earnings at a rate of 36 percent, thus providing a partial explanation for the failure of the literature to document a substantial AWE.[8] As previously asserted, there are numerous methodological issues that confound inference of the specifications reported in Cullen and Gruber (2000), thus the question of spousal labor supply crowd-out by UI benefits remains indeterminate.

Potential Liquidity Effects

As noted in Heckman and MaCurdy (1980), to the extent that unemployment is a negative transitory income shock, it should not distort the intertemporal allocation of the spouse's labor supply, consistent with the failure of the literature to identify a sizeable AWE. Yet Mincer (1962) and Lundberg (1985) suggest that liquidity constrained couples may be unable to smooth consumption across the unemployment spell, resulting in a substantive AWE. Thus

[7] Earlier work used aggregate geographic data and documented evidence of a 'discouraged worker effect' (DWE), whereby high area male unemployment is associated with a reduction in the labor force participation of wives (Long 1958; Mincer 1962; Bowen and Finegan 1965, 1968; Cain 1966).

[8] An alternative specification of financial crowd-out resulting from actual UI receipt suggests a crowd-out rate of 73 percent, though this specification likely overstates the effect given the 'option-value' of UI for non-recipients.

the sizeable estimates of crowd-out of spousal labor supply resulting from eligible UI benefits of the primary earner in Cullen and Gruber (2000) may be rationalized within the framework of liquidity constraints. Browning and Lusardi (1996) provides an inconclusive review of existing studies evaluating whether households are liquidity constrained. However, given the extremely skewed asset distribution amongst workers prior to unemployment (Gruber 2001), a non-trivial number of the unemployed are plausibly unable to smooth transitory income shocks relative to permanent income. It stands to reason that the crowd-out of spousal labor supply resulting from UI benefits may be heterogeneously distributed across households by liquidity constrained status at the point of separation.

This idea is consistent with several recent studies that have used consumption data to investigate the importance of liquidity constraints and partial insurance (Johnson, Parker, and Souleles 2006; Blundell, Pistaferri, and Preston 2008). In the context of UI, Chetty (2008) differentiates moral hazard and liquidity effects in the endogenous duration of an unemployment spell to UI benefits, finding sizeable liquidity effects that suggest optimal UI benefits exceed current levels. Bloemen and Stancanelli (2005) find evidence that UI benefits help to smooth consumption for households without financial wealth at the time of job loss, a result consistent with the findings of Browning and Crossley (2001) concerning concentration of the consumption-smoothing response to the Canadian UI system within a subset of households without liquid assets. This paper then re-interprets the Cullen and Gruber (2000) estimates of spousal labor supply crowd-out by UI in considering how estimated crowd-out measures interact with a couple's liquidity constrained status or fixed consumption commitments at the point of separation.

3.3 Data

I incorporate Survey of Income and Program Participation (SIPP) panels spanning the reference period October 1983 - December 2003.[9] Each SIPP panel surveys a national set of households at four month intervals (waves) for $2\frac{1}{2}$-4 years, with sample sizes ranging from approximately 14,000 to 36,700 interviewed households.[10] At each interview, households are asked questions in reference to the four month recall period. Data are collected regarding income and labor force participation, as well as a wide array of socioeconomic characteristics

[9] US Census Bureau. Survey of Income and Program Participation Users' Guide. http://www.census.gov/sipp/usrguide.html (Accessed September 2009)

[10] The length of observation varies across panels. The 1984 panel contains 9 waves. The 1985 panel contains 8 waves. The 1986 and 1987 panels contain 7 waves. The 1988 panel contains 6 waves. The 1990 and 1991 panels contain 8 waves. The 1992 and 1993 panels contain 9 waves. The 1996 panel contains 12 waves. The 2001 panel contains 9 waves. Owing to the overlapping design of the survey, observations are continuous across the reference period, excepting for January 1990, due to the incomplete nature of the omitted 1989 panel, and March – September 2000, where a minor interruption arose over a funding shortfall and the subsequent cancellation of the in-progress 2000 panel, which was re-started as the 2001 panel.

of each household member and of the household as a whole. The SIPP provides monthly data on income, including a direct measure of UI receipt, and weekly data on labor force status. Deliberate over-sampling of the low-income population provides a suitably large sample of unemployment spells. I supplement the SIPP with monthly national price indices and seasonally adjusted monthly state unemployment rates as reported by the US Bureau of Labor Statistics.

Starting from the universe of job separations in the pooled SIPP panels, I retain spells of unemployment, defined as spells following a job separation during which individuals are either on layoff or are searching for a new job. Spells begin with the first full month of non-employment and end with the first full month of re-employment.[11] As labor force nonparticipation among this sample is often disguised long-term unemployment (Clark and Summers, 1979), I do not exclude observations for workers who drop out of the labor force at some point during an unemployment spell. I measure the duration of a spell as consisting of all weeks of separation from work.

I restrict the analysis to couples continuously married throughout the relevant panel with at least two years of observation. I further restrict attention to couples for which both partners are between 25-54 years old, thus focusing on job-separators who have a high rate of attachment to the labor force. This restriction also limits the impact of schooling and early retirement decisions on the resulting pool of job separations, as well as spousal labor supply decisions. I include only unemployment spells for which I observe at least one quarter of employment experience prior to separation.[12] This allows for the accurate measurement of non-employment spell duration, as well as characteristics of pre-separation jobs, most importantly imputation of pre-separation wages and determination of pre-separation spousal labor supply. I also eliminate spells for which the pattern of earnings in the base period appear to make the unemployed worker ineligible for the relevant state UI system, discussed below.

I restrict to spells of unemployment for the household's primary earner, defined as either the sole earner or the consistently highest-earner throughout the preceding wave, irrespective of gender. Implicit, then, is the assumption that the spouse acts as the secondary earner of the household. By focusing on spousal labor supply across separations of empirically-identified primary earners, I deviate from the gender assumption of the husband as the primary earner common to the AWE literature.

Individuals from small states clustered in the SIPP are excluded, as unique identification is necessary to accurately impute eligible UI benefits.[13] I further exclude separations for which

[11] Spells lasting less than one calendar month are difficult to scale, and may be false empoyment transitions.

[12] This exclusion eliminates all monthly observations for individuals who never work in three consecutive months, as well as the initial months of observation for workers whose first three consecutive months of work occurs later in the panel.

[13] These states are Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont, and Wyoming.

I have missing individual or job characteristics for the primary earner or the spouse.[14] These restrictions leave 3,591 unemployment spells, of which 605 (approximately 17 percent) are separations with wives as the primary earner.

Unemployment Insurance Eligibility

Information on the regulations regarding UI eligibility criteria and benefit schedules across states is reported semiannually by the United States Department of Labor. The basis for both the monetary eligibility calculator and benefits calculator come from the initial calculators developed by Gruber (1997) and later updated by Chetty (2008) and Brown (2010). Eligibility for UI is multi-dimensional. Monetary eligibility is established through qualifying wages, often paired with a required wage distribution across the 'base period', defined as the first four of the past five calendar months. Further, the worker must not have exhausted available benefits within a given benefits period. Recipients must additionally demonstrate nonmonetary eligibility, generally consisting of: (1) unemployed through no fault of their own, (2) able and available to work full-time, and (3) actively seeking full-time work.

I impose restrictions approximating these eligibility criteria. A job separation for a primary earner is excluded from the sample if prior work history appears to make the worker ineligible for UI. Self-employed workers are excluded, as they cannot avail themselves to the UI system.[15] I restrict to spells in which the individual reports looking for work in at least some months in order to focus on unemployment and not strictly labor force exit.[16]

I exclude temporary layoffs, spells that include months where the primary earner is with a job and on layoff, given concerns of potentially different information about probabilities of layoff and recall. Spousal labor supply may react differently to temporary as opposed to permanent unemployment.[17] Further, endogeneity of layoff to state UI benefits are potentially problematic in terms of interpretation, though the literature is conflicted on this point.[18] Restricting temporary layoffs, the resulting sample demonstrates minimal sensitivity of layoff

[14] I also exclude outlying observations with spousal hourly wages below \$1/hour and above \$100/hour, and weekly hours of more than 90 hours/week.

[15] An indicator for self-employment status was removed starting with the 1996 panel. I rely upon the BLS definition of self-employed as workers for their own, unincorporated businesses, including those who worked for profit or fees in their own unincorporated business or professional practice. If self-employed as workers for their own, incorporated businesses, then these workers are not classified as self-employed because they are paid employees of their own companies. A small fraction of workers report self-employment income less than full wage income. For this group, I classify the individual as self-employed if self-employment income composes at least 50 percent of total wage income. For consistency, this measure is applied across all panels.

[16] The SIPP does not contain a consistent measure of reason for separation, so restricting to spells in which individuals report looking for work also plausibly focuses the analysis on involuntary separations. As verification, the take-up rates of UI benefits, presented in Table 1, are consistent with empirical estimates of the take-up rate for the UI-eligible population (Blank and Card 1991).

[17] Alterntively, these couples may constitute a particulary responsive margin as they are plausibly more aware of the UI system's parameters. Results are largely robust to inclusion of temporary layoffs, though interpretation of these inclusive results is unclear.

[18] Feldstein (1978) and Topel (1983) document a relationship between the probability of layoff and the generosity of UI benefits, though Anderson and Meyer (1994) find an inconsistent relationship.

to state UI generosity, suggesting that results are not driven by selection.[19]

3.4 Crowd-Out of Spousal LS

3.4.1 Descriptive Statistics

Descriptive statistics across populations of interest are presented in Table 1. Most strikingly, the average spousal labor participation rate amongst employed primary earners in column (1) is 64.3 percent, compared to 57.3 percent amongst unemployed primary earners in column (2). A casual comparison of participation rates then suggests that spousal labor supply is lower during period of unemployment of the primary earner. This observation is consistent with conflicting empirical estimates of the AWE, and is often attributed to the DWE. Yet prior to separation, these primary earners report a working spouse in only 51.5 percent of spells. Failing to account for population heterogeneity amongst workers who do and do not experience unemployment, then, would imply a spurious negative relationship between unemployment of the primary earner and spousal labor supply. However, even a simple difference of spousal labor supply across unemployment spells relative to pre-separation spousal labor supply suggests a raw increase in labor force participation of 5.8 percentage points, or 11.3 percent of the pre-separation participation rate.[20]

Consideration of other measures of spousal labor supply supports the notion of population heterogeneity. Although the unemployed sample of primary earners report average spousal weekly earnings below that of the employed sample, \$190 compared to \$211, average weekly earnings increase across the unemployment spell relative to pre-separation earnings by \$38 or 25.0 percent of pre-separation earnings. Conditional upon non-zero weekly earnings, the average weekly earnings increase by \$11 across employment status, or 3.7 percent of pre-separation conditional weekly earnings. These comparisons are suggestive of an intensive response in addition to an extensive participation response of spousal labor supply resulting from unemployment of the primary earner.[21] A similar comparison can be made in terms of hours, which exhibit an increase across employment status of the primary earner of 1.0 hours or 4.9 percent of pre-separation hours.[22]

[19] Following Cullen and Gruber (2000), I model the probability of unemployment at any point during the panel as a function of eligible UI benefits. Consistent with their finding, I document an insignificant relationship between eligible UI benefits and the probability of permanent unemployment for the sample of primary earners.

[20] Cullen and Gruber (1996) utilize the longitudinal nature of the SIPP to incorporate a rich control set in an attempt to tease out an AWE distinct from a DWE. Their finding against an AWE may be attributed to the failure to recognize the inherent population heterogeneity amongst always employed husbands and ever unemployed husbands in their analysis. In results not reported, I identify some support for an AWE, yet these results are difficult to interpret in consideration of local economic shocks.

[21] Under assumption that the marginal induced working spouse earns less on average than a pre-separation working spouse.

[22] It is unclear in the SIPP that hours are as fluidly reported as earned income, as hours are reported in discrete amounts and exhibit considerable bunching.

In terms of composition, the husband is identified as the primary earner in 83.2 percent of spells. This is somewhat higher than the 80.3 percent of employed spells where the husband is identified as the primary earner. Compared to the employed sample of primary earners, those that experience unemployment are less educated, are married to less educated spouses, and earn considerably less on average ($23,130 compared to $31,755), as well as at the median ($19,541 compared to $28,057). Consistent with these observations, working spouses of unemployed primary earners earn an hourly wage of $8.23 compared to $9.15 for spouses of employed primary earners. Additionally, spousal receipt of UI at any point across the spell is considerably higher, 10.7 percent compared to 7.4 percent, for unemployed primary earners relative to the employed, suggestive of correlated unemployment risk. Population heterogeneity underscores some of the difficulties encountered in the empirical literature in terms of identifying a positive AWE, in addition to plausible crowd-out of the AWE by UI benefits.

3.4.2 Baseline Specification

To evaluate the role of UI benefits in crowding-out spousal labor supply, I estimate models modified from Cullen and Gruber (2000). Collapsed models are of the form:[23]

$$\overline{LS}_{ist} = \beta WBA_{it} + z_{it}\gamma + \alpha_s + \delta_t + \epsilon_{ist} \tag{3.1}$$

where i indexes couples, s indexes states, and t indexes time. Observations are at the couple-spell level. Documentation of duration endogeneity to UI generosity (Meyer 1990, Chetty 2008) suggests a selection bias inherent to a couple-month analysis. To avoid over-weighting long spells, which presumably appear disproportionately in response to increased UI generosity, spells are collapsed to a single observation. Similarly, spells are weighted by the reciprocal number of spells per couple, such that the sum of a couple's weight equals 1, to avoid over-weighting short, repeated spells. Variation in UI is naturally drawn from across spells, as weekly benefit levels are fixed within a spell, conditional on take-up. Standard errors are clustered to accommodate an arbitrary variance-covariance matrix within each state, given correlation across repeated spells as well as across couples residing within a state.

$\overline{LS}_{ist}$ is a measure of the average spousal labor supply across the unemployment spell of the primary earner. WBA_{it} is a measure of the eligible UI weekly benefit amount for the unemployed primary earner.[24] z_{it} is a vector of individual demographics and job characteristics of

[23] This baseline specification varies in several important ways from the analysis of Cullen and Gruber (2000). Table 3 presents sequential impacts of these modifications, evolving the approximate replication specification to the baseline model detailed here.

[24] The eligible state UI weekly benefit amount is scaled by $\frac{1}{100}$. In specifications not reported, I alternatively consider crowd-out of spousal labor supply resulting from UI generosity as measured by the replacement rate $\left(\frac{\text{nominal weekly benefits}}{\text{pre-separation weekly earnings}}\right)$. Scaled by population averages, the results are largely consistent, though specifications utilizing the replacement rate are less precisely measured.

the unemployed primary earner and the spouse.[25] α_s is a vector of state indicators. δ_t is a vector of panel and year indicators. I additionally include measures of employment opportunities available to the spouse. The monthly state unemployment rate proxies for economic downturn. The average spousal wage by gender, educational attainment, state, and year is constructed from the aggregate sample of the spouses of employed primary earners.[26] Covariates are evaluated at the point of separation, excepting for incorporation of a lagged spousal labor supply measure.[27] This measure is drawn from the wave prior to the primary earner's separation, respecting the seam bias inherent in the survey design.[28] Given the inclusion of state and year effects, the model is identified from higher-order interactions of the primary earner's wage, state, and time, assumed to be legitimately excluded from the spousal labor supply decision.[29] Effectively, β is identified by the differential spousal labor supply of high- and low-earning unemployed across states that provide these earnings levels with different relative UI benefits.

A coefficient $\beta < 0$ is consistent with the notion of UI benefits crowding-out spousal labor supply. As labor supply is multi-dimensional, I consider three measures of spousal labor supply.[30] $\overline{Employed}_{ist}$ measures spousal labor participation and is constructed as the fraction of months across the unemployment spell of the primary earner that the spouse is employed and reports non-zero earned income. $\overline{Hours}_{ist}$ is a measure of average spousal weekly hours worked.[31] $\overline{Earnings}_{ist}$ is a measure of the spousal average weekly earned income.[32]

[25] Individual characteristics include: gender of the spouse, age bins for each partner, educational attainment bins for each partner, race of each partner, and bins for number of children. Characteristics of the primary earner's job, drawn from the wave prior to separation, include: 10 Standard Industrial Classification System (SIC) major industry sector indicators, and 23 Standard Occupational Classification (SOC) major groups occupation indicators. Wage splines include a 21-knot spline in base-period wages of the primary earner, with knots located at the (1st, 5th, 10th, ..., 90th, 95th, 99th) percentiles, for the relevant sample under analysis. Alternative consideration of log-linear and cubic splines yield indistinguishable results.

[26] I do not model a predicted wage for the spouse, as identification would be solely based on functional form assumptions, given inclusion of predictive variables into the model. Results are indistinguishable if a predicted wage is additionally incorporated.

[27] This lagged value is potentially problematic, if there are substantial pre-separation effects. Stephens (2002) documents only small pre-separation effects, mitigating this concern.

[28] As individuals are required to recall information from the preceding four months of the reference period in each wave, it is unclear how much unique information is contained in monthly responses. Individuals have a tendency to propagate their status at the point of the interview backwards through the preceding months. As a result, a disproportionately large number of labor force transitions are reported on the 'seam' between interviews.

[29] A potential violation is state trends in spousal labor supply correlation with the evolution of state UI generosity over time. Inclusion of the lagged spousal labor supply measure attempts to control for such a spurious correlation.

[30] Each specification is modified with an appropriate measure of pre-separation spousal labor supply. $\overline{Employed}_{ist}$ specifications incorporate an indicator for spousal work in the wave prior to separation. $\overline{Hours}_{ist}$ specifications incorporate a measure of average weekly hours in the wave prior to separation. $\overline{Earnings}_{ist}$ specifications incorporate a measure of average weekly earnings in the wave prior to separation.

[31] Both hours worked and earned income are reported monthly. Each measure is scaled by the number of weeks in the relevant month to create an average weekly hours and average weekly earnings measure for each month of the spell. These monthly values are then averaged over the duration of the unemployment spell of the primary earner. Results are comparable if monthly hours and earnings are used and the coefficients scaled by $\frac{1}{4.3}$, given 4.3 weeks on average per month.

[32] Cullen and Gruber (2000) scale their crowd-out estimate of hours to derive a measure of earnings crowd-out

Estimation of the responsiveness of $\overline{Employed}_{ist}$ to UI benefits is conducted through ordinary least squares specifications (ignoring the limited dependent variable), as well as alternative probit specifications. Estimation of the responsiveness of $\overline{Hours}_{ist}$ and $\overline{Earnings}_{ist}$ to UI benefits is conducted through ordinary least squares specifications (ignoring the limited dependent variable), as well as alternative Tobit specifications.[33] As Tobits model the 'latent' hours or earnings decision, I additionally report marginal effects on observable spousal hours and spousal earnings.[34]

Spousal UI Eligibility

To the extent that both partners' eligible UI benefits are correlated through the relative generosity of the state UI regime, then β will capture both the indirect effect of UI through the eligible UI benefits of the unemployed primary earner, as well as the direct effect of UI resulting from own-eligibility of the spouse. Highlighting this concern, Table 1 reports a rate of spousal UI receipt of 10.7 percent across spells of unemployment for the primary earner. Thus, a coefficient $\beta < 0$ may spuriously capture the direct effect of UI generosity through spousal eligibility, or at least reflect a downward bias on the estimates presented below, overstating the extent of crowd-out resulting from the indirect effect of UI. In an alternative approach, I restrict to UI-ineligible spouses in Section 4.4 and find these spouses exhibit diminished sensitivity to the eligible UI benefits of the primary earner, providing evidence that the direct effect of UI through spousal eligibility is empirically relevant. Specifically, I find that the ratio of responsiveness of UI-ineligible spouses relative to the aggregate is approximately 82 percent with respect to spousal average monthly labor force participation, 71 percent with respect to spousal average weekly hours worked, and only 54 percent with respect to spousal average weekly earnings.[35] In order to maintain consistency with the existing literature and focus on the representative spell, my baseline specification does not impose these restrictions. Though evidence of a substantive indirect effect of UI generosity in crowding-out spousal labor supply persists efforts to purge estimates of the direct effect of spousal eligibility, the reader is cautioned to interpret these specifications with due scrutiny.

Interpretation

Receipt of UI benefits is not automatically provided; rather, an individual satisfying state-defined eligibility criteria must apply for benefits. Amongst eligibles, take-up is much less than

under an average earnings assumption. Yet it is the change in marginal earnings resulting from UI benefits that is of interest. There is no reason ex-ante to expect the marginal labor response to occur uniformly throughout the wage distribution.

[33] Estimating conditional hours of work or conditional earnings using a 'Heckit'-type model (Heckman 1979) is an appealing notion. Yet absent an excluded instrument, selection would be identified solely from functional form assumptions.

[34] Further, given a heavily skewed earnings distribution amongst employed spouses, I alternatively consider a least absolute deviation estimator to ensure that the mean regression is not driven exclusively by top-earning spouses.

[35] Motivation for the comparison and a discussion of the relevant restrictions are contained within Section 4.4. Results of the comparison are presented in Table 6.

full. Blank and Card (1991) estimate take-up rates of roughly 67 percent amongst eligibles, similar to the take-up rate of 64.7 percent amongst unemployed primary earners. An alternative to the use of eligible benefits is to incorporate actual UI benefits receipt amongst the unemployed. However, this poses a potentially serious selection bias, as take-up of UI may be endogenous to benefit level. Thus I do not condition on receipt of UI benefits.[36] Additionally, receipt of public assistance is noisily measured in survey data. While this may call for use of eligibility as an instrument for actual UI receipt, Gruber (1997) persuasively argues that UI benefit eligibility, rather than actual UI receipt, is of direct policy relevance. Results are then interpreted as the crowd-out of spousal labor supply, along multiple dimensions, resulting from a $100 increase in eligible UI weekly benefits of the unemployed primary earner.

Eligible Unemployment Insurance Benefits[37]

Eligible weekly UI benefits of the primary earner are constructed as non-linear, and in some states complex, functions of wage levels and earnings distribution in the base period.[38] Accurate benefits estimation requires five calendar quarters of earnings history, which is not available for a non-trivial subset of the sample. Instead, I impute an individual's earnings history as completely as the data allow, requiring a minimum of one quarter of wage data.[39] Additional inputs used in determining weekly benefit amounts vary by state-year and include: annual earnings, number of children, spousal work status, and average tax rates. State-specific rules for minimum and maximum weekly benefit amounts are then imposed and vary greatly across states.[40]

A summary of the source of variation in UI generosity across states is presented in Table 2. Although there is benefit generosity variation with each state over time, variation is largely drawn from a cross-state comparison. Average eligible UI weekly benefits by state range from a minimum of $122 in Alabama to a high of $274 in Massachusetts, though for the core sample

[36] Restricting the sample to those who take-up UI could lead to selection bias due to the endogenous nature of the take-up decision with respect to the benefit level (Anderson and Meyer 1997). If factors determining UI take-up are correlated with the change in spousal labor supply, then the estimated crowd-out effect of UI on spousal labor supply will be biased. Further, there is some 'option value' to couples that do not take-up benefits, but derive value from the availability of UI resources should the couple experience a longer-than-expected unemployment duration.

[37] Prior to the Tax Reform Act of 1986, which made all UI benefits taxable after December 31, 1986, UI benefits were tax subsidized for households with income below $12,000 for single filers and $18,000 for joint filers. Although potentially problematic, I do not model the subsidy as household income is obviously endogenous to the spousal labor supply decision. Exclusion of potentially impacted years results in comparable inference, albeit at a reduction in precision.

[38] Example: In January 2001, Texas weekly benefit amounts are assigned as 1/25 of high quarter wages, subject to a minimum wba of $48 and a maximum wba of $294. With 13 weeks per quarter, this is designed to replace approximately 50 percent of a recipient's weekly wage.

[39] An alternative approach would impute a complete earnings history for each spell using the previous quarter, regardless of data availability. This would provide a consistent treatment of data limitations, yet discards relevant information. Results are robust to this uniform imputation procedure.

[40] If unemployment is expected to increase earnings, such that the eligible weekly benefit amount exceeds weekly earnings over the base period, I exclude an individual. Point estimates are minimally affected by this exclusion restriction, though precision is improved.

average UI weekly benefits are $179.[41]

Simulated Instrument Approach

Motivation for the implementation of a simulated instruments instrumental variables strategy is drawn from Meyer (1990), noting that a primary earner's eligible UI weekly benefit amount is a function of the legislative environment in a given state-year, but also of individual characteristics. Even with flexible controls, relative state UI generosity may reflect differences in the distribution of incomes and other individual characteristics across states, thus confounding inference. I therefore instrument for predicted eligible weekly UI benefits of the primary earner using 'simulated eligibility', a strategy developed in Currie and Gruber (1996) and detailed in application to UI generosity in Levine (1993) and Gruber (1997). A related two-step procedure is proposed and implemented in Chetty (2008).

Using the national sample of primary earners in each six month period, given the frequency of reported policy updates, I assign that sample to each state in that period. I calculate each primary earner's eligible weekly benefit amount. I then average the resulting weekly benefit amounts across the simulated sample for each state-year. The resulting instrument is purged of potentially confounding individual characteristics of the individuals in that state-year and is a function of only the legislative environment in that state-year.[42] This simulated instrument is then incorporated as an excluded instrument.

A second motivation for constructing the simulated instrument is related to inherent measurement error of the UI benefits calculator. Although a noisy proxy for eligible benefits for a given individual, due to imputation and imprecise measurement of income distribution throughout the base period, the estimated weekly UI benefit amounts of the primary earners should be correct on average. This noise component, however, will drive the estimated coefficients towards zero in the classical errors-in-variables construction. Although the simulated instrument is, of course, a noisy measure as well, I can reasonably assume that the measurement error is uncorrelated across the measures, provided no systematic over- or under-estimation of weekly benefits amounts. The simulated instruments instrumental variables approach will then produce consistent estimates of the crowd-out of spousal labor supply resulting from eligible weekly UI benefits of the unemployed primary earner under this assumption.[43]

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[41] Nominal benefits are discounted by the CPI to Januray 1990 dollars, allowing for comparison of real benefits across states over time.

[42] As the SIPP panels sample potentially systematically different populations over time, I have alternatively constructed the simulated instrument using a fixed national sample from 1990, with wage data inflated by the Employment Cost Index for wages and salaries. These results, not reported, produce similar results.

[43] One limitation of this approach is the restricted variance of the simulated instrument measure as documented in Table 2. As the instrument is fixed at a point-in-time across all separations within a given state, identification of the baseline model is driven by changes within-state across-time in state UI generosity.

3.4.3 *Spousal Labor Supply Response*

Approximate Replication

As the baseline specification and sample selection is distinct from the analysis of Cullen and Gruber (2000), I perform an approximate replication of their results, reported in Table 3, specification (1). The sample of spells is re-constructed to incorporate all UI-eligible unemployment spells of prime-age males with a sufficient earnings history across the 1984-1988 and 1990-1992 SIPP panels.[44] The estimation model is comparable to Equation (3.1), though no control for pre-separation spousal labor supply is included, and clearly no control for spousal gender is required. Despite dissimilarities in the monetary eligibility and benefits calculators, as well as minor modifications to the estimating equation, point estimates achieved through this approximate replication closely resemble those presented by Cullen and Gruber (2000). I find that a \$100 increase in eligible weekly UI benefits of the primary earner is associated with a fall in average labor force participation for wives of unemployed husbands of 11.7 percentage points, compared to the existing estimate of 12.6 percentage points. As discussed above, these results are only suggestive of a relationship between spousal labor supply and state UI generosity as measured by the eligible benefits of the primary earner. Inability to separate the indirect effect of UI through the eligible UI benefits of the primary earner from the direct effect of UI through spousal eligibility is problematic in terms of interpreting these results as measures of crowd-out.

Extension

Extensions to the approximate replication are presented alongside those results in Table 3. Specifications (2) and (3) maintain focus on the wife's labor supply decision. Through incorporation of the 1993, 1996, and 2001 SIPP panels, the reference period effectively spans 1983-2003. The extended sample produces a reduced estimated measure of responsiveness with respect to the labor participation decision of the wife. \$100 in eligible weekly benefits is associated with a fall in participation of only 7.5 percentage points in specification (2), though the result retains statistical significance. This differential relative to the approximate replication may be explained, at least in part, by a rise in aggregate female labor force participation across this period.[45] Specification (3) then incorporates an explicit control for the wife's pre-separation labor supply with an indicator for participation in the wave prior to the husband's separation. Although the results suggests that \$100 in eligible weekly benefits is associated with a 3.3 percentage points decline in labor participation amongst wives, the estimate is not

[44] Cullen and Gruber (2000) follow the literature norm of assigning the husband as the household's primary earner. Across their reference period, in 87 percent of couples the husbard earned more over a 2-year period. A similar relationship holds true for only 83 percent of couples in the extended dataset, suggesting increasing prevalence of the wife as the primary earner.

[45] Across the extended reference period, the labor force participation of wives in the aggregate employed sample increased from 56.2 percent in 1984 to 67.7 percent in 2003. Plausibly, as wives become more strongly attached to the workforce, the spousal labor supply response along a participation margin is reduced.

statistically significant. The substantial discrepancy is point estimates across specifications casts some doubt upon the estimates of Cullen and Gruber (2000), in terms of their relevance in the presence of increasing female labor participation, but also in terms of the identification strategy.

Primary Earners

The baseline model restricts to UI-eligible unemployment spells of the primary earner, empirically identified rather than relying upon gender assignment.[46] Failing to account for the direct effect of UI through spousal eligibility, the results presented below are likely to overstate the effect of eligible weekly UI benefits on average spousal labor supply acting through the eligible UI benefits of the primary earner.

Responsiveness of spousal labor supply along the participation margin is presented in Table 3, specifications (4)-(7). OLS estimates are reported in specification (4) and suggest a marginally significant decline in spousal participation of 4.4 percentage points for every \$100 in eligible weekly UI benefits. By comparison, the simulated instruments approach results suggests a decline of 8.4 percentage points, reported in specification (5). Given an average participation rate of 57.3 percent across spells and average eligible weekly UI benefits of \$179, this result suggests that absent UI, spousal participation would rise by 15.0 percentage points, or 26.2 percent. Alternatively, this suggests that spousal non-employment rates would fall by 35.1 percent.[47] Probit estimation, incorporating a spousal binary measure of any months employed across the spell, yields similar results. Implementing the simulated instrument approach in specification (7), the marginal effect suggests that every \$100 in eligible weekly benefits reduces participation at any point across the spell by 10.2 percentage points.

Estimates of the responsiveness of spousal labor supply with respect to total hours worked are presented in Table 4. Results of OLS estimation in specification (1) suggest that \$100 in eligible weekly UI benefits reduces average weekly spousal hours worked across the spell by 2.21 hours. The simulated instruments approach in specification (2) suggests a larger effect, every \$100 in eligible weekly UI benefits reduces average weekly spousal hours by 4.69 hours. Given an average weekly spousal hours worked across spells of 21.5 hours and average eligible weekly UI benefits of \$179, this result suggests that absent UI average weekly spousal hours would increase by 8.4 hours, or 39 percent.[48] Tobit specifications are largely consistent with the linear specifications. Implementing the simulated instruments approach in specification (4), the computed marginal effect on observed hours suggest that every \$100 in eligible weekly UI benefits reduces average weekly spousal hours worked by 4.96 hours.

[46] Appoximately 16.8 percent of spells are separations where the wife is identifed as the primary earner. However, results below are similar restricting to only husbands as the primary earner.

[47] By comparison, Cullen and Gruber (2000) construct a similar measure suggesting that absent UI the spousal non-participation rate would fall by 45 percent.

[48] By comparison, Cullen and Gruber (2000) estimate that absent UI average weekly spousal hours worked would rise by 30%.

Direct estimates of the spousal earnings response to eligible UI benefits of the primary earner are presented in Table 5.[49] Results of OLS estimation in specification (1) suggest that every \$100 in eligible weekly UI benefits reduces average spousal weekly earned income by \$15.42, though the result is marginally significant. The simulated instrument approach in specification (2) suggest a greater responsiveness, that is every \$100 in eligible weekly UI benefits of the primary earner reduces average spousal weekly earned income by \$29.26. This result can be interpreted as a reduction in spousal earnings in response to eligible UI benefits of the primary earner at a rate of approximately 29.3 percent.[50] Alternatively, given average weekly spousal earned income of \$190 and average eligible weekly UI benefits of \$179, then absent UI average spousal weekly earned income would increase by approximately \$52, or 27.4 percent. Tobit specifications are largely consistent. Implementing the simulated instruments approach in specification (4), the computed marginal effect on observed average weekly spousal earnings suggests that every \$100 in eligible weekly UI benefits reduces average weekly spousal earnings by \$30.77, though this result is marginally significant. Similarly, LAD estimation suggests an earned income response that is substantial, yet magnitudes are diminished in relation to mean regression results. Specifically, the simulated instruments approach estimates that every \$100 in eligible weekly UI benefits reduces average spousal weekly earnings by \$20.83.

Although these estimates of responsiveness of spousal labor supply to eligible UI benefits of the primary earner are large, the implied increase in average weekly spousal earned income under specification (2) would replace only approximately 11.7 percent of lost income resulting directly from the primary earner's unemployment, an average weekly income loss of \$443.[51] This implied result is small owing primarily to only partial replacement of wages under UI and diminished earnings potential of the spouse relative to the primary earner.

3.4.4 Restricting to UI-Ineligible Spouses

As previously discussed, interpretation of β in Equation (3.1) as a measure of crowd-out of spousal labor supply resulting from the indirect effect of UI through the eligible UI benefits of the primary earner is potentially problematic. Specifically, the state UI regime may have a direct effect on spousal labor supply through own-eligibility of the spouse. As reported in Table 1, column (2), 10.7 percent of spells of unemployment for the UI-eligible primary earner

[49] I could plausibly estimate the earnings response in terms of actual UI benefits received by the primary earner by scaling these results by the inverted benefit take-up rate in my sample, 64.7 percent. This suggests that spousal earnings would fall by 45.2 percent of UI benefits received, relying upon the simulated instruments approach in specification (2). However, in light of selection bias and 'option-value' of UI for non-recipients, this may only be interpreted as an upper-bound.

[50] Cullen and Gruber (2000) do not directly estimate the spousal earnings response. Scaling their total hours of work result by the average spousal wage, the authors arrive at an estimated reduction in spousal earnings of 36 percent of eligible UI benefits of the primary earner.

[51] By comparison, Cullen and Gruber (2000) calculate that spousal labor supply would increase sufficiently to offset 13 percent of the husband's lost earnings absent UI.

result in take-up of UI benefits for the spouse. By under-estimating the total household eligible UI benefits, the magnitude of the β coefficient is biased upwards. Further, own-eligibility for UI benefits creates a substitution effect for the spouse, subsidizing leisure and discouraging labor supply, imposing a negative bias on β. Both sources of bias will tend to overstate the spouse's responsiveness.[52] This approach is particularly problematic in terms of interpreting heterogeneous crowd-out of spousal labor supply, detailed in Section 3.5, as heterogeneity in the total effect may be driven by heterogeneity in either the indirect or direct effect of UI generosity.

In response, I impose additional sample exclusion criteria to produce a sample of unemployment spells for which spouses are identified as UI-ineligible at the point of separation. Based upon spousal earnings history within the respective panel prior to the separation of the primary earner, I exclude spells for which the spouse is identified as UI-eligible. Further, spells for which the spouse is reportedly on layoff or looking for work are excluded if there is an insufficient observation period prior to separation as to determine spousal UI eligibility. In total 1,440 spells satisfy these criteria, of which 232 are spells of unemployment of the primary earner with a working spouse prior to separation, typically with weak labor force attachment. Thus, I exclude spells with a working spouse prior to separation, and focus the analysis exclusively upon the 1,208 spells with a non-working, UI-ineligible spouse prior to separation.[53]

Table 1 presents descriptive statistics for the aggregate sample of all spells with a non-working spouse prior to separation in column (3), and spells satisfying the additional exclusion criteria detailed above in column (4). As expected, the spousal take-up of UI benefits at some point within the spell falls from 12.9 percent of spells for the aggregate sample to 0.9 percent of spells satisfying the additional exclusion criteria. This is consistent with the notion that the exclusion criteria eliminates major threats to interpretability poised by the direct effect of UI benefits on spousal labor supply decisions.[54] These exclusions do not, however, impact the UI take-up rate for the unemployed primary earner, 62.2 percent for the aggregate sample and 62.4 percent for the restricted sample of UI-ineligible spouses. Further, the two populations are very similar with respect to eligible weekly UI benefits of the primary earner, \$169 compared to \$170.

[52] As a specification check, I estimate models of the same form as Equation (3.1) for the sample of *employed spells* for primary earners detailed in Table 1, column (1). Reassuringly, none of the estimates are significant. Implementing the simulated instruments approach, each \$100 in eligible weekly UI benefits is associated with a decline in average spousal labor participation of 1.9 percentage points, a decline in average spousal weekly hours of 0.95 hours, and a decline in average spousal weekly earnings of \$6.68. Although not statistically significant, these estimates are sizeable, and cause for concern.

An alternative interpretation of these results is as rational, tempered declines in spousal labor supply while the primary earner is employed in anticipation of increased smoothing opportunities presented by more generous UI.

[53] The sample of spells with a working, UI-ineligible spouse prior to separation is too small to warrant a separate investigation and potentially impedes interpretation of results in a jointly estimated model, particularly in the context of heterogeneous responses.

[54] The implied take-up rate amongst excluded spells is roughly 40 percent, suggesting that the exclusion criteria rejects the majority of UI-eligible spouses as well as some UI-ineligible spouses.

Average spousal labor participation across spells, however, is greater for the aggregate sample, 25.0 percent, in comparison to the restricted sample of UI-ineligible spouses, 16.8 percent.[55] These restrictions additionally vary the composition of spells with respect to gender; husbands as the primary earner constitute 83.8 percent of the aggregate sample of spells with a non-working spouse prior to separation, compared to 88.7 percent of spells additional satisfying the criteria of a UI-ineligible spouse.[56]

To determine the implications of these restrictions in terms of evaluating the crowd-out of spousal labor supply, I estimate modified specifications of the form of Equation (3.1).[57] These results are presented in Table 6. Across all specifications, the aggregate sample of spells with a non-working spouse prior to separation exhibits increased responsiveness to eligible UI benefits of the primary earner relative to the restricted sample of UI-ineligible spouses. The aggregate sample demonstrates a fall in average spousal labor participation of 13.77 percentage points in response to $100 in eligible weekly UI benefits of the primary earner, as reported in specification (2). This estimate exceeds the 11.24 percentage points estimate in specification (4) for the restricted sample of UI-ineligible spouses. These results imply that the restricted sample of UI-ineligible spouses, through which the direct effect of UI cannot operate, are only approximately 82 percent as responsive as the aggregate sample of spells with non-working spouses prior to separation. Amongst UI-ineligible spouses, this result suggests that average spousal participation absent UI would increase by 19.1 percentage points, scaled by average eligible weekly UI benefits of $179.

In terms of spousal average weekly hours, each $100 in eligible UI benefits of the primary earner decreases spousal weekly hours by 6.62 hours for the aggregate sample, compared to 4.70 hours for the restricted sample of UI-ineligible spouses. These results imply that the sample of UI-ineligible spouses, through which the direct effect of UI cannot operate, are only approximately 71 percent as responsive as the aggregate sample of spells with non-working spouses prior to separation. Amongst UI-ineligible spouses, this result corresponds to an expected increase in average weekly hours absent UI of 7.99 hours, scaled by average eligible weekly UI benefits of $170.

Similarly, the crowd-out rate estimate of spousal average weekly earnings in response to eligible weekly UI benefits of the primary earner is 61.55 percent for the aggregate sample in specification (10), compared to a crowd-out rate of 32.95 percent for the restricted sample of UI-ineligible spouses in specification (12). These results imply that the restricted sample of

[55] The higher rate of average labor participation for the aggregate sample does not suggest that concerns above are unwarranted. Rather, it is still entirely possible that the exit hazard from the unemployed state is impacted meaningfully by the state UI regime.

[56] It would be interesting to re-evaluate the heterogeneity of crowd-out measures with respect to the gender of the spouse after restricting to UI-ineligible spouses. Regrettably, an already modest sample of wives as primary earners is reduced to an unusable size.

[57] As both the aggregate sample and the sample of UI-ineligible spouses are conditioned on a non-working spouse prior to separation of the primary earner, I exclude the pre-separation spousal labor supply control.

UI-ineligible spouses, through which the direct effect of UI cannot operate, are only approximately 54 percent as responsive as the aggregate sample. These results re-affirm the notion that UI-eligible spouses respond to both the indirect effect of UI through eligible UI benefits of the primary earner and the direct effect of UI through own-eligibility. Although spousal eligibility for UI poses a serious endogeneity problem for the aggregate sample of non-working spouses, the data restrictions imposed above ensure that estimates of crowd-out for the restricted sample of UI-ineligible spouses are purged of the direct effect of UI. Thus β identifies the crowd-out of spousal labor supply resulting from the indirect effect of UI through the eligible UI benefits of the unemployed primary earner. For the restricted sample of UI-ineligible spouses, these estimates correspond to increased spousal weekly earnings absent UI of \$56 when scaled by average eligible weekly UI benefits of the primary earner, \$170 as reported in Table 1. Yet these increased earnings would only offset roughly 13 percent of lost transitory income of the primary earner, scaled by average weekly pre-separation earnings of \$424, suggesting that spousal labor supply would only imperfectly insure against unemployment of the primary earner absent social insurance provision through UI.

3.5 Asset Heterogeneity

3.5.1 Identifying Liquidity Constrained Households

A recent literature identifies heterogeneous effects of UI generosity across liquidity constrained groups along a number of behavioral dimensions. Browning and Crossley (2001), Bloemen and Stancanelli (2005), and Sullivan (2008) detail that consumption falls across employment status are mitigated by UI generosity for households with little financial assets, though households with higher asset holdings exhibit limited sensitivity. Chetty (2008) finds similar evidence with respect to hazard rates for leaving unemployment. In related work, Blundell, Pistaferri, and Preston (2008) find that consumption-income co-movement is pronounced for households with low asset holdings. A natural extension, then, of the above framework is to evaluate heterogeneous sensitivity of spousal labor supply with respect to eligible UI benefits of the primary earner amongst plausibly liquidity constrained households relative to liquidity unconstrained households.

The SIPP is designed to provide a broader context for analysis by incorporating supplemental data contained within 'topical modules', uniquely matched to individuals within the 'core' dataset. Though the SIPP contains no direct measure of a household's access to credit markets, SIPP respondents are interviewed about detailed household holdings at a single interview point in the 1985, 1987, and 1990-1993 panels, at two points in the 1986 panel, and once annually in the 1996 and 2001 panels. As a result, pre-separation asset data are available for approximately 55 percent of these spells. Although the SIPP imputation methodology has been criticized (Curtin et al. 1989, Hoynes et al. 1998), non-random imputation suggests potential bias from the exclusion of imputed values, thus I retain these observations.[58] I focus on net liquid wealth as the primary proxy for a couple's access to credit markets.[59] Following Chetty (2008), I define net liquid wealth as gross liquid assets less unsecured debt.[60] I include only observations for which I observe asset holdings prior to the point of separation, to avoid issues of asset draw-down during an unemployment spell, which may respond to the level of UI generosity.

A secondary proxy for liquidity constrained couples is mortgage status prior to separation.[61] Gruber (1998) finds that less than 5 percent of the unemployed sell their homes during an unemployment spell, in contrast to high mobility amongst renters. A couple burdened with mortgage payments prior to separation has a fixed consumption obligation, limiting the couple's ability to smooth other forms of consumption (Chetty and Szeidl 2007).[62] For both imperfect proxies of liquidity constraint status, misclassification will bias the differential across the stratified results towards zero.

Given compelling evidence of a heterogeneous unemployment duration response across household liquidity constrained status (Chetty 2008), interpreting stratified results of the form of Equation (3.1) is hindered by an inability to separate: (1) heterogeneous responses to the direct effect of spousal eligibility for UI benefits from (2) heterogenous responses to the indirect effects of eligible UI benefits of the primary earner. Therefore, I restrict to consider only spells where the spouse is identified as UI-ineligible, as detailed previously in Section 4.4. Absent this restriction, investigation of the role of household liquidity constraints would be rendered uninterpretable.[63]

Descriptive Statistics

Descriptive statistics for the pooled sample and across quartiles of net liquid wealth are presented in Table 7.[64] The most striking statistic is that the median separation occurs with \$0 in net liquid wealth, though the distribution is skewed, with mean reporting of \$11,676.[65] Also,

[58] Gruber (2001) finds no systematic difference in results owing to exclusion of imputed results, though wealth adequacy is approximately 50 percent lower, reflecting the non-random imputation assignment.

[59] Cullen and Gruber (2000) note that asset proxies for liquidity constraint may be inappropriate in light of endogeneity of household assets to UI generosity, as documented in Engen and Gruber (2001). However, recent analysis from Klein (2009) casts suspicion over the robustness of these results.

[60] Liquid wealth is defined as total wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debts. Substantial unsecured debt may limit a household's ability to finance an unemployment spell.

[61] Net liquid wealth may not proxy a couple's access to credit, as couples with negative net liquid wealth may instead proxy for access to unsecured borrowing.

[62] This consumption commitment results in heightened risk aversion over moderate losses. Thus a mortgage may result in additional value of spousal labor supply to protect the couple against further financial shocks.

[63] Stratification across proxies of liquidity constraint with the pooled sample of spells yields similar parameter concentrations. As mentioned, it is difficult to interpret these heterogeneous responses, so I present only the results for the restricted sample of UI-ineligible spouses.

[64] Quartiles do not contain equal numbers of spells, as a non-trivial number of separators report no household wealth accumulation. As a result, the respective spell counts across quartiles are: 165, 184, 145, and 165.

[65] Gruber (2001) finds that the median worker holds sufficient gross financial assets to cover roughly two-thirds

net liquid wealth is non-monotonic in liquid wealth, as the first quartile has \$70 in median liquid wealth relative to \$4,895 in median unsecured debt, compared to the second quartile with \$0 in median liquid wealth, but also \$0 in median unsecured debt. In the pooled sample, 46.9 percent of spells occur amongst couples with a mortgage prior to separation. Although it is unsurprising that quartile 4 contains the highest concentration of spells with a mortgage, 64.0 percent, the next highest concentration occurs within quartile 1, with 57.6 percent.[66]

Average spousal labor participation across spells in the pooled sample is 17.5 percent. Conditional upon participation, the average spousal weekly hours is 27.1 hours. Both average spousal participation rates and average spousal weekly hours vary across the quartiles of net liquid wealth, ranging from 13.7 percent participation in quartile 2 to 21.3 percent participation in quartile 3, and from 21.1 conditional hours in quartile 4 to 31.4 conditional hours in quartile 1. Similarly, observed wages vary across the quartiles, from \$6.21 in quartile 2 to \$7.46 in quartile 4. As a result of relatively low spousal participation rates and relatively low conditional hours, unconditional average spousal weekly earnings range from \$20 for quartile 2 to \$43 for quartile 4. Conditional upon participation, average spousal weekly earnings range from \$106 for quartile 2 to \$184 for quartile 1.

3.5.2 Stratification Across Proxies

To evaluate the potentially heterogeneous sensitivity of liquidity constrained households to eligible UI benefits, I stratify across proxies for household liquidity constraint and estimate modified specifications of the form of Equation (3.1), incorporating a further linear control for total (illiquid and liquid) wealth.[67] Stratification, rather than joint estimation with commonly identified controls, is important in light of demographic heterogeneity across quartiles of net liquid wealth.

Results of stratification across spells with net liquid wealth levels below and above the median pooled net liquid wealth level are presented in Table 9.[68] Although the results are imprecisely estimated, concentration patterns are nonetheless suggestive of an associated liquidity effect. Average spousal participation for couples proxied as liquidity constrained falls by 38.75 percentage points for each \$100 in eligible weekly UI benefits in specification (2), more responsive

of income loss from an unemployment spell, though the extremely skewed distribution suggests that one-third of workers are unable to replace even 10 percent of the income loss. The implication of incorporating net liquid assets suggests further inadequacy of private savings at the median, though the distribution remains highly skewed.

[66] This is re-assuring in light of the evidence to follow. The results suggest that this subset of spells with both negative net liquid wealth and a consumption commitment in the form of a mortgage may be most responsive to eligible UI benefits.

[67] As noted above, the sample of UI-eligible spells of unemployment for the primary earner with a UI-ineligible spouse imposes a non-working restriction on the spouse prior to separation of the primary earner. As a result, I omit the control for pre-separation spousal labor supply.

[68] Stratification across quartiles places too high a demand on the data.

that the 6.99 percentage points decline for proxied liquidity unconstrained households in specification (8).[69] Thus couples proxied as liquidity unconstrained are approximately only 18.0 percent as responsive as couples proxied as liquidity constrained. This is similar to the results for average spousal hours worked in specifications (4) and (10), and average spousal earnings in specifications (6) and (12), which suggest that couples proxied as liquidity unconstrained are only 17.2 percent and 26.2 percent as responsive, respectively, as couples proxied as liquidity constrained.

Results of stratification across the mortgage proxy of liquidity constraint are presented in Table 9. These results are largely consistent with the use of the net liquid wealth proxy. Yet unlike the net liquid wealth proxy, the constrained couples under the mortgage proxy typically report higher wages and higher educational attainment of both the primary earner and the spouse relative to the unconstrained couples, primarily renters. This provides a reasonable cross-check that heterogeneous effects of the crowd-out of spousal labor supply are not driven spuriously by other demographic factors. Recall that couples with a mortgage at the point of separation face a fixed consumption obligation across a period of low transitory income. Supplemental income through eligible UI benefits of the primary earner relaxes this constraint. Average spousal participation for couples proxied as liquidity constrained under the mortgage proxy falls by 35.06 percentage points for each $100 in eligible weekly UI benefits in specification (2), more responsive that the 4.12 percentage points decline for proxied liquidity unconstrained households in specification (8). Thus, couples proxied as liquidity unconstrained under the mortgage proxy are only approximately 11.8 percent as responsive as couples proxied as liquidity constrained. This is similar to the results for average spousal hours worked in specifications (4) and (10), and average spousal earnings in specifications (6) and (12), which suggest that couples proxied as liquidity unconstrained are 13.3 percent and 17.6 percent as responsive, respectively, as couples proxied as liquidity constrained under the mortgage proxy.

These results are consistent with a negative interaction effect of eligible UI benefits and liquidity constraint on spousal labor supply, illustrating excess sensitivity to cash-in-hand for groups likely to experience a liquidity effect. Note that for this restricted sample, UI generosity is assumed to operate only indirectly through the eligible UI benefits of the primary earner, given restriction to UI-ineligible spouses. These results are suggestive of an associated liquidity effect, and are not driven through heterogeneity in the direct effect of UI.

3.6 Conclusion

UI plays a theoretically important role in mitigating the extent of the spousal labor supply response to unemployment of the household's primary earner. Existing empirical evidence

[69] OLS results are statistically insignificant across all specifications, though magnitudes are consistently greater for couples with below-median net liquid wealth levels relative to those with above-median levels.

of crowd-out of spousal earnings by eligible UI benefits of the unemployed primary earner suggests a spousal earnings crowd-out rate of 36 percent (Cullen and Gruber 2000). However, issues concerning identification of the crowd-out of spousal labor supply render this previous work difficult to interpret. Across all measures of spousal labor supply, the aggregate sample of non-working spouses exhibits increased responsiveness to eligible UI benefits of the primary earner relative to the sample of UI-ineligible spouses, plausibly due to the confounding factor of spousal eligibility. Within a restricted sample of UI-ineligible spouses, I find a comparable spousal earnings crowd-out rate of 33 percent. In spite of this responsiveness, the implied increase in spousal earnings absent UI would only offset roughly 13 percent of the lost transitory income of the unemployed primary earner. This indicates that even absent social insurance in the form of UI, spousal labor supply would only imperfectly insure against unemployment of the primary earner.

Extending the analysis to consideration of a couple's liquidity constrained status at the point of separation reveals parameter concentrations consistent with an associated liquidity effect. In the context of a life cycle model, there is limited scope for a transitory income shock such as unemployment to substantively affect the spousal labor supply. Liquidity constraints, proxied by low levels of net liquid wealth, or consumption commitments, such as the mortgage proxy, rationalize this behavior. In the presence of such constraints, the spousal labor supply would be relatively more responsive to transitory income fluctuations of the primary earner. Identification of a liquidity effect suggests that UI partially corrects for imperfections in credit markets.

In the context of current public policy debate, persistently high levels of unemployment have placed strain on state and federal budgets to continue providing benefits under statutory UI provisions. The role of UI in crowding-out spousal labor supply as insurance against the adverse event of unemployment of the household's primary earner provides powerful insight into the counterfactual policy where social insurance, in the form of UI, does not provide a state-contingent transitory income stream. Although estimates of crowd-out are high, suggestive evidence of an associated liquidity effect indicates a socially beneficial role of UI in crowding-out spousal labor supply. Future work should prioritize evaluation of the crowd-out of spousal labor supply with respect to extended time horizons, as unemployment has prolonged negative earnings effects not captured in this analysis.

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Table 1

Summary Statistics Across Samples of Interest

			Spousal Restrictions at Separation	
			Non-Working	Non-Working, UI-Ineligible
Employment Status of the Primary Earner	Employed	Unemployed	Unemployed	Unemployed
	(1)	(2)	(3)	(4)
Spousal Labor Supply				
Employed	64.3	51.5	--	--
Mean (Employed) Across Spell	--	57.3	25.0	16.8
Weekly Hours	22.5	20.5	--	--
Mean (Weekly Hours) Across Spell	--	21.5	9.9	6.1
Weekly Hours, Hours>0	34.9	35.4	--	--
Mean (Weekly Hours) Across Spell, Hours>0	--	35.9	32.3	30.0
Weekly Earnings	$211	$152	--	--
Mean (Weekly Earnings) Across Spell	--	$190	$73	$39
Weekly Earnings, Earnings>0	$328	$296	--	--
Mean (Weekly Earnings) Across Spell, Earnings>0	--	$307	$240	$189
Mean Hourly Wage	$9.15	$8.23	$7.18	$6.41
Demographics				
Spouse Female	80.3	83.2	83.8	88.7
Age	39.6	39.0	38.5	38.2
Spouse Age	37.8	37.1	37.0	36.8
Non-White	8.3	10.2	8.9	7.5
Spouse Non-White	10.9	13.1	11.8	10.5
Number of Children	1.3	1.6	1.9	2.0
Education: Less than HS	7.9	19.4	25.6	28.3
Education: HS Graduate	30.8	37.0	37.0	35.0
Education: Some College	25.6	22.9	20.0	20.2
Education: 4+ Years of College	35.7	20.7	17.4	16.5
Spouse Education: Less than HS	9.3	17.8	25.2	27.2
Spouse Education: HS Graduate	36.8	40.3	39.8	40.6
Spouse Education: Some College	28.1	24.9	21.3	20.0
Spouse Education: 4+ Years of College	25.8	16.9	13.6	12.2
Annual Earnings	$31,755	$23,130	$21,328	$22,052
Median Annual Earnings	$28,057	$19,541	$17,149	$17,439
Mean Spousal Hourly Wage by Gender/Education/State/Year	$9.17	$8.53	$8.23	$7.98
State Unemployment Rate	6.1	6.5	6.7	6.7
Completed Spell Durations (Weeks)				
Duration	--	21.4	21.8	22.1
Median Duration	--	16.0	16.0	17.0
UI Benefits				
Eligible Real WBA	--	$179	$169	$170
Eligible UI Replacement Rate	--	47.6	49.7	48.7
UI Take-Up Rate	--	64.7	62.2	62.4
Spousal Receipt	7.4	10.7	12.9	0.9
Number of Spells	44,614	3,591	1,743	1,208

Table entries are mean values unless noted otherwise. Unemployment spells are restricted to UI-eligible separations of the empirically-identified household primary earner. Spells are restricted to couples continuously married throughout the respective panel. The employed sample includes pre-separation employed spells for individuals experiecning UI-eligible unemployment spells as the primary earner at some point within the panel, as well as the sample of continuously employed individuals identified as the household's primary earner. Statistics are based upon the wave prior to separation unless noted otherwise, with exception of the employed sample, for which values are averaged across the spell of employment. Data are drawn from the 1984-1988, 1990-1993, 1996, and 2001 SIPP panels. Left-censored spells of unemployment (in-progress at the start of the panel), are excluded. UI weekly benefit amount is simulated per discussion in Section 4.2. UI replacement rate is constructed as the eligible weekly benefit amount divided by weekly pre-separation wage. Duration is defined as weeks elapsed from end of last job to start of next job and does not adjust for right-censoring (spells in-progress at the end of the panel). All monetary values are in real 1990 values.

Table 2
Unemployment Insurance Generosity by State

State	Number of Spells	Mean Weekly Benefit Amount	Standard Deviation of WBA	Mean Simulated Instrument (WBA)	Standard Deviation of SI (WBA)
Alabama	76	122.37	32.67	121.44	10.07
Arizona	67	140.82	22.68	134.03	8.61
Arkansas	30	166.35	57.20	161.86	16.67
California	490	159.81	47.30	145.72	14.66
Colorado	44	199.85	61.46	159.16	24.73
Connecticut	56	236.24	58.97	189.52	22.00
Delaware	12	158.99	53.28	159.36	18.91
D.C.	5	130.10	75.78	204.00	12.59
Florida	178	162.71	48.16	151.43	17.34
Georgia	87	154.91	37.57	142.77	17.88
Hawaii	4	227.46	64.87	149.48	21.36
Illinois	188	196.76	68.66	162.76	19.52
Indiana	74	145.32	37.49	133.81	24.41
Kansas	42	192.70	47.22	172.69	16.66
Kentucky	47	154.33	58.87	149.33	19.93
Louisiana	82	157.12	57.58	142.83	30.47
Maryland	54	195.34	21.62	167.70	12.31
Massachusetts	89	274.11	73.09	208.98	27.15
Minnesota	91	203.18	73.66	174.47	24.33
Mississippi	58	130.95	29.96	130.34	7.73
Missouri	94	146.16	20.93	137.51	11.80
Nebraska	22	124.55	34.28	128.50	10.91
Nevada	8	181.10	30.82	157.54	14.97
New Hampshire	10	156.31	56.65	134.78	22.48
New Jersey	132	236.84	72.65	191.63	24.48
New Mexico	6	140.53	41.13	143.99	20.92
New York	220	181.04	66.39	155.67	23.12
North Carolina	95	189.93	56.86	170.82	24.70
Ohio	186	169.50	62.11	147.87	15.79
Oklahoma	58	173.59	45.79	171.76	14.77
Oregon	63	208.24	58.97	167.68	18.29
Pennsylvania	167	238.03	62.10	205.33	20.77
Rhode Island	13	286.94	68.91	207.77	31.36
South Carolina	42	151.50	37.52	141.25	16.32
Tennessee	84	135.82	35.35	135.95	17.55
Texas	326	175.87	56.65	169.92	15.37
Utah	14	196.91	54.15	173.15	19.87
Virginia	53	164.37	34.01	147.30	12.52
Washington	99	206.41	68.27	176.87	23.62
West Virginia	57	162.32	71.70	161.11	15.37
Wisconsin	68	181.14	53.00	159.44	21.55
United States	3,591	178.93	63.84	159.52	27.82

Spell counts indicate the number of UI eligible unemployment spells observed within each uniquely identified state across the 1984-1988, 1990-1993, 1996, and 2001 SIPP panels. Eligibility includes both monetary and non-monetary components. Calculated weekly benefit amounts and replacement rates are constructed through simulation at the individual level, building upon the benefits/eligibility calculators developed in Gruber (1997) and modified by Chetty (2008). Discussion regarding construction of the simulated instruments is presented in Section 4.2. All monetary values are in real 1990 values.

Table 3

Spousal Labor Supply - Participation Response

Sample Restrictions	Cullen-Guber:[1] Males, 1983-1993	Males, 1983-2003[2]		Primary Earners, 1983-2003[3]					
	OLS	OLS	OLS	OLS	2SLS	Probit		IVProbit	
Mean Dependent	0.614	0.648	0.648	.573	.573	.573		.573	
Specification	(1)	(2)	(3)	(4)	(5)	(6)		(7)	
UI WBA	-0.1174**	-0.0753**	-.0334	-0.0441*	-0.0842**	-0.0970*	[-0.0332]	-0.3299***	[-0.1017]
	(0.0531)	(0.0375)	(.0308)	(0.0236)	(0.0348)	(0.0497)	--	(0.0905)	--
Spouse Female	--	--	--	-0.1190***	-0.1190***	-0.5260***	[-0.1541]	-0.5250***	[-0.1541]
	--	--	--	(0.0280)	(0.0274)	(0.1151)	--	(0.1155)	--
Average Spousal Wage	0.0100	0.0150**	0.0100**	0.0090	0.0090	0.0280	[0.0091]	0.0280	[0.0092]
	(0.0092)	(0.0066)	(0.0047)	(0.0065)	(0.0062)	(0.0218)	--	(0.0218)	--
State Unemployment Rate	-0.0120*	-0.0110*	-0.0010	-0.0130	-0.0110	-0.0320	[-0.0108]	-0.0270	[-0.0091]
	(0.0067)	(0.0058)	(0.0044)	(0.0093)	(0.0088)	(0.0286)	--	(0.0259)	--
Spouse Age 30-34	0.0110	0.0060	-0.0010	0.0170	0.0560	0.2090	[0.0664]	0.2040	[0.0648]
	(0.0231)	(0.0185)	(0.0217)	(0.0373)	(0.0464)	(0.1757)	--	(0.1777)	--
Spouse Age 35-39	0.0832**	0.0670***	0.0110	0.0130	0.0540	0.1680	[0.0541]	0.1650	[0.0533]
	(0.0362)	(0.0219)	(0.0213)	(0.0322)	(0.0395)	(0.1501)	--	(0.1510)	--
Spouse Age 40-44	0.0930	0.0530*	0.0110	0.0350	0.0760**	0.1150	[0.0375]	0.1130	[0.0369]
	(0.0607)	(0.0300)	(0.0265)	(0.0250)	(0.0341)	(0.1296)	--	(0.1307)	--
Spouse Age 45-49	0.0380	0.0220	0.0040	0.0520	0.0930**	0.2380*	[0.0753]	0.2380*	[0.0752]
	(0.0574)	(0.0339)	(0.0253)	(0.0318)	(0.0371)	(0.1374)	--	(0.1374)	--
Spouse Age 50-54	0.0660	0.0540	0.0260	0.0420*	0.0400*	0.0690	[0.0226]	0.0660	[0.0217]
	(0.0852)	(0.0554)	(0.0359)	(0.0247)	(0.0243)	(0.1028)	--	(0.1052)	--
Spouse Non-White	0.0823**	0.0580	0.0100	0.0100	0.0100	0.0689	[0.0233]	0.0700	[0.0237]
	(0.0375)	(0.0450)	(0.0160)	(0.0361)	(0.0344)	(0.1130)	--	(0.1127)	--
Kids: 1	-0.0740***	-0.0670***	-0.0240	-0.0460**	-0.0430**	-0.0010	[-0.0002]	0.0040	[0.0014]
	(0.0240)	(0.0205)	(0.0155)	(0.0213)	(0.0216)	(0.0865)	--	(0.0884)	--
Kids: 2	-0.1210***	-0.1100***	-0.0390**	-0.0820***	-0.0780***	0.0210	[0.0071]	0.0280	[0.0094]
	(0.0268)	(0.0211)	(0.0152)	(0.0176)	(0.0173)	(0.0837)	--	(0.0861)	--
Kids: 3	-0.1900***	-0.1990***	-0.0510**	-0.1600***	-0.1540***	-0.0910	[-0.0309]	-0.0780	[-0.0265]
	(0.0457)	(0.0390)	(0.0208)	(0.0335)	(0.0337)	(0.1104)	--	(0.1122)	--
Kids: 4+	-0.1990***	-0.2070***	-0.0490**	-0.1750***	-0.1710***	-0.1720	[-0.0596]	-0.1650	[-0.0570]
	(0.0386)	(0.0301)	(0.0208)	(0.0305)	(0.0301)	(0.1234)	--	(0.1226)	--
Spouse Less HS	-0.1730***	-0.1490***	-0.1170***	-0.1170***	-0.1160***	-0.2310***	[-0.0798]	-0.1170***	[-0.0791]
	(0.0291)	(0.0252)	(0.0238)	(0.0238)	(0.0228)	(0.0777)	--	(0.0238)	--
Spouse Some College	0.0350	0.0410**	0.0470***	0.0470***	0.0450***	0.0330	[0.0108]	0.0470***	[0.0095]
	(0.0238)	(0.0202)	(0.0148)	(0.0148)	(0.0141)	(0.0620)	--	(0.0148)	--
Spouse College Graduate	0.0230	0.0520	0.0280	0.0280	0.0270	0.0500	[0.0165]	0.0280	[0.0158]
	(0.0506)	(0.0443)	(0.0333)	(0.0333)	(0.0321)	(0.1356)	--	(0.0333)	--
Suppressed Demographics	X	X	X	X	X	X		X	
Wage Splines	X	X	X	X	X	X		X	
Job Characteristics	X	X	X	X	X	X		X	
State Effects	X	X	X	X	X	X		X	
Year Effects	X	X	X	X	X	X		X	
Prior Spousal LS			X	X	X	X		X	
Observations	2,532	4,136	4,136	3,591	3,591	3,591		3,591	

Results correspond to estimating modifications of Equation (1). In specifications (1)-(5), the dependent variable is average monthly spousal labor participation across the duration of the unemployment spell of the primary earner or husband as relevant. In specifications (6)-(7), the dependent variable is an indicator for any spousal work over the course of the spell. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average spousal labor force participation. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001. Omitted categories include: spouse age 25-29, spouse white, no kids, and spouse HS. Suppressed demographics not reported include: primary earner age bins, primary earner race, and primary earner educational attainment bins.

[1] Specification (1) sample is constructed to proxy the baseline specification of Cullen and Gruber (2000). UI-eligible unemployment spells of prime-age males with sufficient earnings history to impute high-quarter wages are retained. Spells are drawn exclusively from the 1984-1988 and 1990-1992 panels. No restriction is placed on relative earnings of the spouses.

[2] Specification (2) and (3) extend the framework of specification (1) by incorporating the 1993, 1996, and 2001 panels. Specification (3) incorporates a binary control for pre-separation spousal labor supply.

[3] Specifications (4) and (5) do not restrict by gender, but rather restrict to UI-eligible separations of the household's empirically-identified primary earner. An additional gender control is incorporated into the demographics set. Specifications (6) and (7) consider probit specifications as an alternative to linear estimation. Marginal effects are reported in brackets alongside the relevant coefficients.

Table 4

Spousal Labor Supply - Hours Response

	OLS	2SLS	Tobit	IVTobit
Mean Dependent	21.5	21.5	21.5	21.5
Specification	(1)	(2)	(3)	(4)
UI WBA	-2.212**	-4.692**	-3.265**	-7.530**
	(1.027)	(2.162)	(1.494)	(3.366)
	--	--	[-2.013]	[-4.961]
Demographics	X	X	X	X
Wage Splines	X	X	X	X
Job Characteristics	X	X	X	X
Unemployment Rate	X	X	X	X
Predicted Wage	X	X	X	X
State Effects	X	X	X	X
Year Effects	X	X	X	X
Prior Spousal LS	X	X	X	X
Observations	3,591	3,591	3,591	3,591

Results correspond to estimating modifications of Equation (1). Dependent variable is average weekly spousal hours worked across the duration of the unemployment spell of the empirically-identified primary earner. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average weekly spousal hours worked. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Tobit marginal effects on observed average weekly spousal hours worker are reported in brackets alongside the relevant coefficients. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 5

Spousal Labor Supply - Earnings Response

	OLS	2SLS	Tobit	IVTobit	LAD	IVLAD
Mean Dependent	$190	$190	$190	$190	$190	$190
Specification	(1)	(2)	(3)	(4)	(5)	(6)
UI WBA	-15.42*	-29.26**	-26.53*	-58.26*	-7.57	-20.83**
	(8.55)	(14.61)	(15.124)	(30.03)	(6.59)	(9.40)
	--	--	[-14.33]	[-30.77]	--	--
Demographics	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X
Predicted Wage	X	X	X	X	X	X
State Effects	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X
Prior Spousal LS	X	X	X	X	X	X
Observations	3,591	3,591	3,591	3,591	3,591	3,591

Results correspond to estimating modifications of Equation (1). Dependent variable is average weekly spousal earnings across the duration of the unemployment spell of the empirically-identified primary earner. Earnings are expressed in real 1990 dollars. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average weekly spousal earnings. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Tobit marginal effects on observed average weekly spousal hours worker are reported in brackets alongside the relevant coefficients. LAD and IVLAD 'median regression' standard errors are bootstrapped, owing to the two-stage nature of the IVLAD estimator. Sampling units are defined as couples, rather than states, given severely limited states relative to couples. Thus bootstrapped standard errors will be robust to inter-spell correlation for a given couple, but not inter-spell correlation across couples within a state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 6

Purging Spousal UI Eligibility

	Employed				Hours				Earnings			
Sample Restriction	Non-Working Spouse		UI-Ineligible Spouse		Non-Working Spouse		UI-Ineligible Spouse		Non-Working Spouse		UI-Ineligible Spouse	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	0.250	0.250	0.168	0.168	9.9	9.9	6.1	6.1	$73	$73	$39	$39
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UI WBA	-0.0781*	-0.1377**	-0.0455*	-0.1124**	-2.269	-6.623**	-1.410	-4.701*	-20.34	-61.55**	-12.34	-32.95**
	(0.0443)	(0.0610)	(0.0260)	(0.0532)	(1.622)	(3.017)	(1.516)	(2.453)	(13.71)	(25.32)	(9.09)	(16.47)
Demographics	X	X	X	X	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X	X	X	X	X
Predicted Wage	X	X	X	X	X	X	X	X	X	X	X	X
State Effects	X	X	X	X	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X	X	X	X	X
Observations	1,743	1,743	1,208	1,208	1,743	1,743	1,208	1,208	1,743	1,743	1,208	1,208

Results correspond to estimating modifications of Equation (1). Dependent variable is average spousal labor supply (average monthly participation, weekly spousal hours worked, weekly spousal earnings) across the duration of the unemployment spell of the empirically-identified primary earner. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. All specifications are restricted to spells where the spouse is non-working in the wave prior to separation of the primary earner. 'UI-Ineligible' specifications impose further restrictions on the status of the spouse in the wave prior to separation to target UI-ineligible spouses at the point of separation. Specifically, a spell is excluded if: (a) the spouse is non-employed, reports layoff or searching for work, and UI-eligibility cannot be inferred, or (b) the spouse is unemployed and prior earnings appear to make the spouse UI-eligible. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average spousal labor supply. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 7

Summary Statistics by Net Liquid Wealth Quartile

		Net Liquid Wealth Quartile			
Sample	Pooled	1	2	3	4
Net Liquid Wealth Range	-	<($1,089)	($1,089)-$0	$1-$5,735	>$5,735
Spousal Labor Supply Across Spell					
Mean (Employed)	17.5	16.9	13.7	21.3	19.0
Mean (Weekly Hours)	5.3	6.7	4.4	7.1	3.3
Mean (Weekly Hours), Hours>0	27.1	31.4	26.3	28.5	21.1
Mean (Weekly Earnings)	$36	$38	$20	$43	$43
Mean (Weekly Earnings), Earnings>0	$156	$184	$106	$152	$182
Mean Hourly Wage	$6.70	$6.48	$6.21	$6.84	$7.46
Demographics					
Spouse Female	91.0	87.8	92.9	93.4	89.9
Age	38.7	37.8	37.9	38.6	40.8
Spouse Age	36.5	35.4	36.3	36.2	39.0
Non-White	6.5	2.9	11.5	7.4	3.6
Spouse Non-White	8.8	3.6	16.0	10.7	4.3
Number of Children	2.0	2.1	2.3	1.8	1.6
Education: Less than HS	27.9	28.1	45.5	22.1	12.9
Education: HS Graduate	33.6	37.4	36.5	41.8	19.4
Education: Some College	21.9	27.3	14.1	19.7	27.3
Education: 4+ Years of College	16.5	7.2	3.8	16.4	40.3
Spouse Education: Less than HS	25.9	27.3	40.4	23.0	10.8
Spouse Education: HS Graduate	41.7	43.2	42.3	48.4	33.8
Spouse Education: Some College	21.0	23.7	14.7	21.3	25.2
Spouse Education: 4+ Years of College	11.3	5.8	2.6	7.4	30.2
Annual Earnings	$22,681	$17,365	$14,333	$24,318	$35,931
Median Annual Earnings	$18,148	$15,113	$11,135	$23,102	$28,839
Predicted Mean Spousal Hourly Wage	$7.85	$7.81	$7.15	$7.55	$8.95
State Unemployment Rate	6.5	6.3	6.6	6.8	6.3
Completed Spell Durations (Weeks)					
Duration	21.6	19.4	24.9	21.0	20.7
Median Duration	17.0	16.0	17.0	16.0	17.0
UI Benefits					
Eligible Real WBA	$173	$164	$138	$188	$209
Eligible UI Replacement Rate	49.4	53.1	56.9	47.5	38.7
UI Take-Up Rate	55.6	56.8	48.7	54.9	62.6
Spousal Receipt	0.9	0.6	1.1	1.3	0.6
Assets					
Net Liquid Wealth	$11,676	-$6,827	-$228	$1,570	$52,409
Median Net Liquid Wealth	$0	-$4,520	$0	$1,082	$30,181
Liquid Wealth	$14,723	$740	$196	$2,743	$55,523
Median Liquid Wealth	$461	$70	$0	$1,444	$31,929
Unsecured Debt	$3,047	$7,567	$425	$1,172	$3,114
Median Unsecured Debt	$613	$4,895	$0	$97	$331
Home Equity	$28,909	$21,844	$9,409	$28,064	$58,602
Median Home Equity	$7,126	$7,636	$0	$11,594	$45,092
Mortgage Indicator	46.9	57.6	23.7	45.1	64.0
Renter Indicator	34.2	27.3	55.8	34.4	16.5
Number of Spells	659	165	184	145	165

Table entries are means unless otherwise noted. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. Replacement rate is individual weekly benefit amount divided by weekly pre-unemployment wage. Unemployment duration is defined as time elapsed from end of last job to start of next job and does not adjust for right-censoring. Asset and liability data are collected at a single point within the 1985, 1987, and 1990-1993 panels, at two points in the 1986 panel, and once annually in the 1996 and 2001 panel. Eligible unemployment spells without sufficient asset data are excluded, including spells for which asset data are observed following a job separation. This restriction excludes approximately 45% of the restricted sample of UI-ineligible, non-employed spouses prior to UI-eligible separation of the household primary earner. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debt. All monetary values are in real 1990 dollars.

Table 8

Heterogeneous Effects by Household NLW

	Household Net Liquid Wealth											
	Below Median						Above Median					
Measure of Spousal LS	Employed		Hours		Earnings		Employed		Hours		Earnings	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	0.152	0.152	5.5	5.5	$28	$28	0.201	0.201	5.1	5.1	$43	$43
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UI WBA	-0.0973	-0.3875**	-3.724	-10.043**	-18.30	-72.62*	-0.0188	-0.0699	-0.918	-1.724	-7.68	-19.02
	(0.083)	(0.1738)	(3.571)	(4.886)	(15.55)	(38.73)	(0.0615)	(0.2020)	(2.396)	(3.185)	(26.12)	(42.26)
Demographics	X	X	X	X	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X	X	X	X	X
Predicted Wage	X	X	X	X	X	X	X	X	X	X	X	X
State Effects	X	X	X	X	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X	X	X	X	X
Observations	349	349	349	349	349	349	310	310	310	310	310	310

Results correspond to estimating modifications of Equation (1). Dependent variable is average spousal labor supply (average monthly participation, weekly spousal hours worked, weekly spousal earnings) across the duration of the unemployment spell of the empirically-identified primary earner. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. Specifications are restricted to spells where the spouse is non-employed and UI-ineligible in the wave prior to the UI-eligible separation of the primary earner. A spell is excluded if: the spouse is non-employed, reports layoff or searching for work, and UI-eligibility cannot be inferred; or the spouse is unemployed and prior earnings appear to make the spouse UI-eligible. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Liquid wealth is defined as total household wealth minus home, business, and vehicle equity. Net liquid wealth is defined as liquid wealth minus unsecured debt. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average spousal labor supply. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.

Table 9

Heterogeneous Effects by Household Mortgage Status

	Household Mortgage Status at Point of Separation											
	Mortgage=1						Mortgage=0					
Measure of Spousal LS	Employed		Hours		Earnings		Employed		Hours		Earnings	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mean Dependent	0.182	0.182	4.8	4.8	$44	$44	0.169	0.169	5.7	5.7	$29	$29
Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UI WBA	-0.1245	-0.3506**	-6.084**	-15.260***	-54.83**	-127.17***	-0.0228	-0.0412	-1.005	-2.027	-16.36	-22.38
	(0.0964)	(0.1473)	(2.388)	(5.792)	(24.99)	(40.67)	(.0236)	(.0413)	(2.667)	(6.017)	(20.64)	(31.05)
Demographics	X	X	X	X	X	X	X	X	X	X	X	X
Wage Splines	X	X	X	X	X	X	X	X	X	X	X	X
Job Characteristics	X	X	X	X	X	X	X	X	X	X	X	X
Unemployment Rate	X	X	X	X	X	X	X	X	X	X	X	X
Predicted Wage	X	X	X	X	X	X	X	X	X	X	X	X
State Effects	X	X	X	X	X	X	X	X	X	X	X	X
Year Effects	X	X	X	X	X	X	X	X	X	X	X	X
Observations	309	309	309	309	309	309	350	350	350	350	350	350

Results correspond to estimating modifications of Equation (1). Dependent variable is average spousal labor supply (average monthly participation, weekly spousal hours worked, weekly spousal earnings) across the duration of the unemployment spell of the empirically-identified primary earner. Observations are person-spell level. Spells are restricted to couples continuously married throughout the respective panel. Specifications are restricted to spells where the spouse is non-employed and UI-ineligible in the wave prior to the UI-eligible separation of the primary earner. A spell is excluded if: the spouse is non-employed, reports layoff or searching for work, and UI-eligibility cannot be inferred; or the spouse is unemployed and prior earnings appear to make the spouse UI-eligible. Spells have been weighted by the reciprocal of the couple's number of spells, such that a couple's weights sum to 1. Mortgage proxy indicates household status at the point of separation. Individual UI weekly benefit amount is simulated per discussion in Section 4.2. The wba has been expressed in real 1990 dollars and scaled by 1/100, thus the coefficient corresponds to the effect of a $100 increase in wba on average spousal labor supply. Standard errors, reported in parentheses beneath the estimated coefficients, are adjusted to accommodate an arbitrary variance-covariance matrix within each state. Statistical significance is reported at the following p-values: *-0.10, **-0.05, ***-0.001.