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NON-EMPLOYMENT AND HEALTH INSURANCE COVERAGE

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ABSTRACT

Low rates of health insurance coverage among the non-employed have motivated consideration of policies to subsidize the purchase of insurance for those who are without a job. But there is little evidence on the extent to which coverage differentials between the employed and the non-employed reflect the effects of job loss or merely different underlying tastes for insurance. If the latter, subsidies may not be successful in increasing the rate of insurance coverage among the non-employed. Furthermore, subsidies which lower the costs of non-employment may increase both the incidence and duration of joblessness.

We provide new evidence on these issues by analyzing longitudinal data on 25-54 year-old men over the 1983-1989 period. We have four findings of interest. First, even after modelling differences in underlying tastes for insurance, the likelihood of insurance coverage drops by roughly 20 percentage points following job separation. Second, limited subsidization of the cost of insurance through state laws mandating continued access to employer-provided health insurance for the non-employed increases the likelihood of having insurance while without a job by 6.7 percent. Third, these mandates also increase the number of individuals with spells of non-employment and the total amount of time spent jobless. Finally, at least some of this increased non-employment appears to be spent in productive job search as the availability of continuation coverage is related to significant wage gains among those who separate from their jobs.

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In the U.S., most group health insurance is provided through the workplace. As a result, individuals not attached to jobs generally do not have access to private group insurance markets. Since insurance is both more expensive and less generous in individual insurance markets, the result is that the non-employed often go uninsured. Indeed, among prime-aged (25-54 year-old) males during the 1983-1989 period, 85 percent of those who were employed had some form of private insurance coverage, while only 38 percent of those not employed had private insurance.[1]

This low rate of insurance coverage among the jobless has motivated considerable public policy debate over interventions in insurance markets to increase access for the non-employed. But the simple fact that coverage rates are lower among the non-employed does not prove that they face access problems. Individuals who leave their jobs differ along a number of dimensions from those who do not; for example they tend to be younger and have smaller families, and thus may have a lower demand for insurance. Job leavers also tend to disproportionately separate from jobs that did not offer health insurance, so that their lower coverage rates may not result from job leaving per se.

Furthermore, any government intervention to protect the unemployed may come at the cost of distorting employment decisions. When working, individuals pay for the cost of health insurance, either explicitly through employee premiums or implicitly through lower wages. If individuals do not bear the full cost of health insurance when not working, unemployment effectively becomes a subsidized activity.

The optimal government response to low rates of insurance coverage among the non-employed therefore revolves around the answers to several heretofore unaddressed questions.

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[1] Authors' tabulations based on SIPP data described below. These rates of insurance coverage by employment status are similar to those obtained from other data sets, such as the March Current Population Surveys.

First, how large an impact does job separation have on private insurance coverage? The conclusions of previous research on this question are contradictory.[2] Moreover, none of the previous research on this particular question has modelled in detail the underlying differences in tastes for insurance among those who do and do not lose their jobs.

Second, how effective are subsidies for group health insurance in terms of increasing insurance coverage among the non-employed? In addition to the factors mentioned above, the non-employed may have a decreased demand for insurance due to the negative income shock that follows job leaving. As a result, extensive subsidies may be required to significantly increase the level of insurance coverage among this group.

Third, what will be the effects of insurance subsidies on non-employment behavior? Will subsidizing health insurance off-the-job encourage more job leaving? And will it lengthen unemployment durations among those who leave their jobs?

Finally, to the extent that non-employment increases in response to the availability of subsidized coverage, does this allow for more productive job search, or does it merely subsidize increased leisure? This is related to the problem of "job-lock" discussed in Madrian (1994) and Gruber and Madrian (1994). If there are failures in the non-group health insurance market, workers will be reticent to change jobs if doing so requires a subsequent period of unemployment and job search; they will also try to minimize the amount of time spent unemployed if they lose

[2] Monheit *et al.* (1984) note that most of the uninsured unemployed did not have insurance on their previous job. They estimate that in 1977, only 8 percent of the unemployed lost insurance due to unemployment. This finding is echoed by Klerman and Rahman (1992), who calculate that over the 1983-1987 period, only 5.1 percent of the unemployed were both uninsured and had insurance on their previous job; however, another 10.3 percent of the uninsured in their sample did not have a job during their window of observation, so that some of this group may have been previously employed in jobs with health insurance as well. In contrast, specific area studies during the early 1980s for Detroit (Berki *et al.* (1984)) and Maryland (Gold *et al.* (1984)) conclude that the rate of insurance coverage falls by 33-40 percent upon job loss. Several studies reviewed by Bazzoli (1986) also claim to find large declines in insurance coverage following job loss during this period.

jobs with health insurance. Such behavior may be innefficient if job mobility and job search result in higher productivity job matches. In this case, increased non-employment may be a positive, rather than a negative, consequence of insurance subsidization.

This paper addresses each of these questions using data from the 1984-1988 panels of the Survey of Income and Program Participation (SIPP), a large nationally representative survey data set which follows individuals for a period of two to three years. We focus on prime age (25-54 year-old) men over the 1983-1989 period. Using a sample of workers who are highly attached to the labor force allows us to control for underlying tastes for insurance by using information on insurance status before job leaving.

We begin by documenting the effects of job separation on insurance coverage, conditioning on differences between those who do and do not leave their jobs. We then provide evidence on the effects of making group health insurance more readily available to the non-employed on their insurance coverage, their employment behavior, and their reemployment earnings. We do so by exploiting a plausibly exogenous source of variation in the menu of choices facing job separators: government mandated continuation coverage. Under the Consolidated Omnibus Reconciliation Act of 1985 (COBRA), job leavers are entitled to continue purchasing group coverage from their former employers for up to 18 months after a separation at the average group insurance premium paid by the employer. COBRA built upon a set of earlier state statutes, and the resulting variation in the availability of continuation coverage across states and over time allows us to identify the effects of these mandates. In addition, because continuation mandates affect only those workers who had insurance on their pre-separation jobs, we can use the experience of those workers who did not have prior health insurance coverage as a control for omitted state/year factors that may be correlated with the passage of a continuation mandate.

The paper proceeds as follows. In Section I, we describe our data source. In Section II, we provide background on the insurance coverage of the unemployed. In Section III, we investigate the effect of continuation mandates on the insurance coverage of the non-employed, and in Section IV we model non-employment behavior and reemployment earnings as a function of continuation coverage availability. Section V concludes.

I. Data

Our data source is the 1984-1988 panels of the Survey of Income and Program Participation (SIPP). The SIPP is a nationally representative survey which collects information from a large sample of households every four months (waves) over a period of two to three years.[3] The reference period from the interviews that we use spans the period from June 1983 to the end of 1989. At each interview, households are asked questions about both the entire previous four month period and each month in that period. Data are collected on the demographic and economic characteristics of each household member and of the household as a whole.

We have a number of sample selection criteria. Most importantly, we use data on individuals only once we have observed at least one full wave of employment experience.[4] This restriction excludes from our dataset unemployment spells that are in progress at the time the

[3] The SIPP began in the 1984 Panel by surveying individuals for 9 waves or 36 months. Over the succeeding panels, three waves of interviews were eliminated so that by the 1988 panel individuals were followed for only six waves or 24 months.

[4] This need not be the first wave of the survey; rather, we follow individuals until they have one wave of employment, and then use data from that wave onwards.

survey begins. By doing this, we are able to accurately measure both the duration of the non-employment spells that are included and the characteristics of pre-separation jobs, most importantly coverage by employer-provided health insurance. At the same time, however, this left-censoring may result in disproportionately omitting the long-term unemployed, thereby skewing the composition of our sample so that it is no longer representative of the underlying population.

Because of this restriction, we focus our analysis on 25-54 year-old males. For this demographic group, our selection rule only excludes 10.5 percent of the sample; for women or for older or younger men, the share excluded would be at least three times as large. For prime aged men, therefore, our selection rule does not do great violence to the representativeness of our conclusions.[5]

We also lose a number of observations that are missing information on wages or health insurance coverage; these are critical control variables, as the analysis below will reveal. We also exclude the self-employed from the analysis.

We define non-employment as being without a job at any time during a given month. According to this definition, the non-employed include those who are out of the labor force, but exclude those who are not working but with a job (many of whom are on temporary layoff). We include those out of the labor force because labor force nonparticipation among this sample of

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[5] For our models of transitions to and durations of non-employment, left-censoring could cause a potential sample selection bias: for example, after a continuation mandate has been in place for some period of time, those who remain employed have revealed themselves to be less sensitive to continuation mandates in terms of their employment decisions. Gruber and Madrian (1993) show that this bias is small for their analysis of the effect of continuation mandates on retirement.

prime aged males is often disguised long term unemployment (Clark and Summers, 1979).[6] We exclude those not at work but attached to a job because these individuals may have quite different access to group insurance markets than those without jobs (for example, firms may have provisions for individuals to maintain insurance coverage during a temporary layoff). In results not reported, we have redefined non-employment as being without a job *and* in the labor force, and as being not at work (with or without a job); for both of these alternative definitions of nonemployment, the results are quite similar to those reported below. We are unable, however, to distinguish between job losing (ie. layoffs) and job leaving (ie. quits) because the SIPP does not contain very useful information on the causes of job separation.[7] Thus, we focus on all job separations for this analysis. Overall, an average of 4.6 percent of our sample of men are defined as without a job in a given month, and 15.6 percent experience some non-employment during the SIPP panel.[8]

An important issue that must be addressed in using the SIPP data is "seam bias." Although individuals are asked questions about the preceding four months in each wavely interview, it is unclear how much unique information is contained in these monthly responses since some individuals have a tendency to propagate their status at the point of the interview backwards through the preceding months. This problem may be particularly relevant for the

[6] In addition, it will be difficult to discriminate job searchers from those out of the labor force in implementing policies to extend insurance coverage to the unemployed. Thus, our results will have more predictive power for the effects of policy if we do not make this distinction either.

[7] A question about the reason for job separations was added in the 1986 survey; however, the question is asked of only about one-half of job leavers. Furthermore, the question would be largely useless in our analysis of continuation mandates since by 1987 this benefit was available to all workers.

[8] This point in time non-employment rate is much lower than the non-employment rate for the full population since we have excluded men who do not have one full wave of work. For the full SIPP sample of 25-54 year old men, the non-employment rate is 11.3 percent.

timing of insurance coverage changes. For example, Klerman and Rahman (1991) examine those individuals who lose their jobs during a wave and who report having employer-provided insurance in the previous wave. Of this group, 25 percent do not report employer-provided insurance in the month before job loss. Some individuals in this sample may have truly lost their employer-provided insurance in the month before losing their job, or may have switched jobs since the last interview. But most of this anomaly is likely due to seam bias.[9]

In our analysis of the effect of non-employment on health insurance coverage, we use monthly observations on individuals. We do this because many spells of unemployment are less than four months (the time between waves) in duration. To the extent that there is seam bias, however, using monthly insurance observations will overstate the sample size and understate our standard errors. The maximum increase in our standard errors (if there were complete seam bias) would be a factor of two; given the tight confidence intervals for most of the insurance estimates below, such an increase would not significantly affect our inferences. For our other analyses (non-employment and wages), we will use a frame of observation of one wave or longer, so that seam bias is not a concern.

II. Background on the Insurance Coverage of the Non-Employed

Sampling and Descriptive Statistics

As noted above, because many spells of non-employment are short we analyze the effect of non-employment on health insurance coverage using monthly data on individuals. However, using the full sample of person/months for this analysis was computationally unwieldy, so we have instead used a subset of our full sample. Our subsample consists of a 100 percent sampling

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

of every person who experiences at least one month of non-employment, and a 1-in-3 random sampling of those individuals who do not experience non-employment. We then weight each person in this latter group by three when computing descriptive statistics and regression results in order to produce results which are representative of the full sample. This allows us to both create a manageable file of person/month observations and to retain the maximum amount of information on that small share of our sample that becomes non-employed.[10]

Descriptive statistics for our sample are presented in Table 1. The statistics in the first two columns are calculated from the full sample of person/months for the subset of individuals described above and are presented separately by employment status in any given month. The last two columns present statistics for the first month that individuals are included in the sample stratified on the basis of whether or not these individuals experience any non-employment during their participation in the SIPP.

Overall, 89.3 percent of employed males have some form of private insurance coverage; the vast majority of this coverage is provided by one's employer. In contrast, only 48.9 percent of the non-employed have private insurance coverage.[11] Thus these raw data reveal a health insurance coverage gap of over 40 percentage points between the employed and the non-employed. Public insurance coverage rates are fairly low for this population; less than 1 percent among the employed, and only 6.7 percent among the non-employed.

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[10] Exploratory analysis suggests that the conclusions from this weighted sampling procedure are very similar to the conclusions from using a completely random subsample of the data. The advantage of our approach is that we will identify the effects of continuation mandates on insurance coverage in Section III by using variation among the non-employed only, so that having a large sample of non-employed is critical to estimating precise effects.

[11] Note that these coverage rates are somewhat different from those mentioned in the opening paragraph because we have excluded left-censored unemployment spells from the sample in Table 1.

A comparison of other characteristics of the employed and the unemployed, however, suggest that some of the discrepancy in the private health insurance coverage rates of these two groups may arise from differences in tastes for insurance rather than from job leaving per se. As columns three and four show, even in the first month of the sample (when all persons are employed), the insurance coverage rate among those who ultimately experience some unemployment is much lower: this group is initially 22.9 percentage points less likely to have any private insurance and is 23.8 percentage points less likely to have employer-provided insurance. This point is reinforced in the first panel by noting that the likelihood of employer-provided insurance before job loss for those who lose their jobs is 24.3 percentage points lower than the probability of employer coverage for those who are employed. Finally, those who become non-employed differ along a number of dimensions which would indicate a lower demand for insurance: they are younger, less likely to be married, have smaller families, are less educated, and earn less. Thus, it is obviously important to control for underlying tastes for insurance in assessing the effects of job leaving on insurance coverage.

Basic Regression Results

In order to control for heterogeneity across those who do and do not leave their jobs, we estimate multivariate regression models which control for a number of characteristics of individuals. Our models are of the form:

$$\begin{bmatrix} \textit{Private} \\ \textit{Insurance} \end{bmatrix}_{it} = \alpha + \beta \cdot \begin{bmatrix} \textit{Without} \\ \textit{Job} \end{bmatrix}_{it} + \mathbf{X}_i' \delta + \pi_s' \cdot \textit{State} + \tau_t' \cdot \textit{Time} + \varepsilon_{it}, \tag{1}$$

where *Private Insurance* is a dummy variable indicating private health insurance coverage,[12] *Without Job* is an indicator for being non-employed, **X** is a vector of individual and job characteristics, *State* is a vector of state dummy variables, and *Time* is a vector of month, year, and panel dummy variables.

The dependent variable measures coverage by any type of private health insurance in a particular month, either in one's own name or from some other source. Our key independent variable is a dummy for being without a job in a given month. The vector of control variables includes a number of individual characteristics: age and its square; race; marital status; number of children; education; and whether a particular month is a "seam" month (the month in which a wave ends). We also include the wage, industry, occupation, and (most importantly) coverage by employer-provided insurance coverage from the previous wave for the employed; for the non-employed, these variables refer to the wave before job loss.[13] Finally, we include a full set of state, year, month, and panel dummies.

The regressions, which are presented in Table 2, are run as linear probability models weighted to account for our oversampling of those who do not leave their jobs.[14] For the full sample, we find that non-employment is associated with a statistically significant 29 percentage

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[12] Private health insurance is defined as any health insurance coverage other than Medicare or Medicaid (including continuation coverage, which is described in more detail below).

[13] That is, for those without a job, these variables are fixed at their pre-job leaving values for the entire non-employment spell: for the first wave of non-employment, these variables refer to the previous wave; for the second wave, they refer to two waves prior; and so on. The result is that the timing is somewhat different for the employed (for whom the variables refer to the previous wave) and the non-employed (for whom they refer to the wave before job leaving).

[14] We have estimated probit models as well, with similar results. We use linear probability models here and in Section III for computational simplicity and ease in interpreting our coefficient estimates.

point drop in the rate of insurance coverage. This effect is substantial: by comparison, it is of the same order of magnitude as the positive effect of having had employer-provided insurance in the previous wave. Thus, even after controlling for the underlying taste for insurance coverage through demographic and job characteristics, including whether workers were covered by insurance prior to job loss, the estimated effect of being without a job remains roughly three-quarters as large as in the raw data.

The control variables have their expected effects. Insurance coverage is more likely for whites, those with more education and higher wages, and those who had employer-provided health insurance in the previous wave. Being married also increases the likelihood of insurance coverage, an effect which likely reflects both an increased demand for insurance and the availability of private health insurance coverage through one's spouse.

Even controlling for lagged employer-provided coverage, however, may not be sufficient to control for heterogeneity in insurance demand among job leavers and others. In order to model this heterogeneity in more detail, we also estimate a fixed effects regression by including an individual constant term for each person in our sample. In this model, the effect of job leaving is identified by comparing the change in insurance coverage for those leaving their job to the change for those not leaving their job. In this way, we fully capture any underlying demand for insurance which is person-specific and time-invariant, allowing us to more precisely separate the effect of job leaving per se.[15]

[15] Of course, we will not capture any time-varying demand for insurance which happens to be correlated with the event of job leaving. For example, individuals may leave their jobs with insurance at the point in time when their demand for insurance drops (ie., because children leave the household). Omitting these taste changes would lead us to overstate the effect of job leaving per se. We have attempted to surmount this problem by instrumenting our job leaving indicator with a variable which is independent of the individual's taste for insurance: the unemployment rate in his industry and state of residence in the month of the observation (tabulated from Current Population Survey data). This instrumental variables strategy yielded coefficient estimates which were similar to those reported in Table 2, but with large standard errors which made it difficult

The results of the fixed effects model are presented in the second column of Table 2.[16] The effect of job leaving is somewhat diminished; we now estimate that non-employment lowers the probability of being insured by 20 percentage points. Nevertheless, even after controlling for underlying tastes for insurance in this relatively comprehensive manner, the effect of job leaving remains one-half as large as in the raw data.

Non-Employment Durations

We next explore how these insurance coverage effects vary with non-employment durations; the results are depicted in Figures 1 and 2, and the regressions coefficients are presented in the Appendix Table. In the first column, we present OLS models where we have replaced the dummy variable for job loss in Table 2 with a set of indicator variables for 9-12 months before job loss; 5-8 months before job loss; 4 months before job loss through the month before job loss, the month of job loss, the month after through the 11th month after job loss, and 12 or more months after job loss. In the second column, we present the analogous fixed effects regression in order to control explicitly for underlying tastes for insurance.

In addition to the sizeable decline in insurance coverage following job loss, in the OLS regression model there are large negative effects on insurance coverage apparent for ***future*** job leavers. This highlights the ex-ante differences between those who do and do not lose their jobs.

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to draw precise inferences.

[16] Note that we exclude the control for lagged employer-provided insurance in this specification. This is because the introduction of fixed effects automatically induces serial correlation in the error term, so that lagged values of the dependent variable (or strong correlates like employer-provided insurance) are biased. Note also that the number of observations is slightly larger for our fixed effects analysis; this is because a small number of our observations are missing data on educational attainment, which is constant over the panel and is therefore excluded from the fixed effects regressions.

Once we include fixed effects, however, these negative lead terms are reduced, and only the indicator for the month before job loss is significant (although still quite small). For both models, there is a sharp drop in private insurance coverage in the month after the separation. This drop is on the order of one-half of the long run effects of separation in both cases; after two months, two-thirds of the long run effects have been realized. Thus, the decline in insurance coverage following job loss occurs fairly quickly, and by 10 months after job loss the long run effects appear to have been mostly played out.

One problem with interpreting these results is separating heterogeneity from duration dependence. It may be that, even conditional on covariates, those prone to have longer spells of unemployment are less likely to be insured while unemployed. In this case, part of the fall in the probability of being non-insured as non-employment durations increase will reflect this underlying heterogeneity. In order to separate the effects of heterogeneity from duration dependence, in Figure 2 (and the last four columns of the Appendix Table) we graph the effect of job loss on insurance coverage by the completed duration of non-employment spells.[17] The upper line in Figure 2 shows the drop in insurance coverage for the first month of non-employment for those who are only out of work for one month. The succeeding lines show the drop in insurance coverage for those who are non-employed for 2-6 months, 7-12 months, and 13 months or more.[18]

By comparing the coefficients at a given point in time across these different groups, it becomes apparent that the decline in coverage over time in Figure 1 reflects both heterogeneity

[17] Approximately one-third of the spells in our data are right-censored so that we do not know completed duration. Figure 2 includes these spells at their censored durations; the results are similar if we exclude them.

[18] In each case, of course, those who remain employed are in the regression as controls.

and duration dependence. At any given point in time, the drop in insurance coverage is much larger for those who ultimately experience longer spells of non-employment. For example, the drop in insurance coverage in the first month of non-employment is 11 percentage points for those whose spells end in one month or less, 15-16 percentage points for those whose spells last 2-6 or 7-12 months, and 24 percentage points for those whose spells last more than one year. Furthermore, within each group the time pattern of insurance loss is much flatter than in Figure 1. These findings highlight the substantial heterogeneity underlying the average results in Figure 1: job loss has more severe implications for insurance coverage at all durations for those who ultimately have longer spells of non-employment.

At the same time, within each group there is a decline in coverage over time, even during the period when all members of the group are non-employed.[19] That is, insurance coverage falls with the duration of non-employment even within groups of the same completed duration, a sign of duration dependence. This duration dependence may arise from time limited employment-based insurance plans which expire after several months of non-employment. Alternatively, it may result from a pure income effect: as individuals are out of work longer, they may be less able to afford insurance.

Summary

These results suggest that, for the population that we are studying, job leaving is associated with a dramatic reduction in the likelihood of insurance coverage. While a simple comparison of insurance coverage rates across those with and without a job overstates the

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[19] That is, for the group that has completed durations of at least 6 months, coverage rates decline with the duration of unemployment from month 1 to month 6; similarly, for those out more than one year, coverage rates decline throughout the year following job loss.

decline, even after controlling for taste differences with individual fixed effects we still find that leaving one's job decreases the likelihood of being covered by private insurance by 20 percentage points. This effect increases with the duration of unemployment, although most of the impact occurs in the first month after job loss. The continued decline in insurance coverage with time since job loss reflects both heterogeneity and duration dependence, with the effects of job leaving being most severe at all durations for those who have longer completed spells.

III. Continuation Coverage and the Insurance Coverage of the Non-Employed

Background on Continuation Mandates

The existing public policy response to insurance coverage shortfalls for the non-employed has been mandated continuation of coverage benefits. These mandates require that employers sponsoring group health insurance plans offer terminating employees and their families the right to continue their health insurance coverage through the employer's plan for a specified period of time. The first such law was implemented by Minnesota in 1974. More than 20 states passed similar laws over the next decade before the federal government, in 1986, mandated such coverage at the national level under COBRA. The various state statutes are summarized in Table 4.[20] The length of coverage is often quite short, from 3-6 months, although 10 states mandated coverage of nine months or more before COBRA was in place. Most of the laws generally apply to all separations (except those due to an employee's gross misconduct), although some apply only to involuntary terminations. Because we do not have data on the nature of the

[20] Details on state laws come from Hewitt (1985) and Thompson Publishing (1992) and have been cross-checked against the actual state statutes. See Gruber and Madrian (1993, 1994) for more details.

separation, we restrict our analysis to those states with laws that apply to both voluntary and involuntary separations.[21]

Both the state and federal laws stipulate that the employee must pay the full cost of coverage. At the federal level, this is defined specifically as 102 percent of the *average* employer cost of providing coverage. Although 102 percent of the average employer cost is typically much more than individuals pay as active employees, it is substantially less than the cost of buying equivalent coverage in the individual insurance market, due to the economies of scale in administering group insurance and the reduced potential for adverse selection with large employee groups. Gruber and Madrian (1994) calculate that continuation coverage costs approximately 40 percent less than a comparable individual policy for a family headed by a prime age male worker. Furthermore, individual coverage generally excludes pre-existing medical conditions for some period of time after enrollment, and it may be medically underwritten so that particularly unhealthy individuals cannot obtain coverage at any price. Gruber and Madrian (1994) also show that group policies typically cover many more services and have lower copayments and deductibles than do non-group policies.

Despite the attractiveness of continuation coverage relative to individual insurance, there are two reasons why continuation mandates may not be successful in significantly increasing the insurance coverage of the unemployed. First, as noted in Table 2, a substantial fraction of those separating from jobs did not have employer-provided health insurance to begin with. Second,

[21] These states, which are noted in Table 3, are excluded only in the periods in which a mandate applying only to involuntary separations was in place. After the federal law (or in some cases state law changes) which extended coverage to voluntary terminations as well, these states are included. In addition, we exclude several small states because they cannot be uniquely identified in the SIPP. These states are Hawaii, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, Wyoming, New Mexico and Mississippi. West Virginia is also excluded because we have been unable to definitively date the implementation of their state mandate.

continuation coverage remains quite expensive. The average cost of one year of continuation coverage (approximately $3600 for family coverage in 1990) is roughly 26 percent of the contemporaneous family income of unemployed individuals in our SIPP sample, and 17 percent of family income in the period before job loss.

Empirical Framework

We now turn to estimating the effect of continuation mandates on the insurance coverage of non-employed individuals. We begin by selecting the sample of individuals who separated from their jobs during our sample period. We then assign to each worker the number of months of continuation coverage available in the calendar month of their separation.[22] Federal COBRA legislation actually mandated that firms offer continuation benefits at the start of the next plan year after July, 1986, so that COBRA was phased in over a one year period. However, under the assumption that most plan years begin in January, we model this transition by assigning the full 18 months of federal coverage beginning in January 1987. Individuals are then given the maximum number of months of continuation coverage available under either their state or the federal law in place at the time. Note that continuation coverage is only available to those that had employer-provided insurance before the separation, since they are the population which has insurance to continue after job loss.

As Table 3 documents, continuation mandates varied substantially across states at the start of our sample period. A number of states also passed or revised their mandates during the time period covered by our SIPP data, and the Federal COBRA legislation, which was implemented in

[22] We hold this availability constant during the non-employment spell since it is the continuation regime in place at the time of separation which determines the availability of this benefit.

mid-1986, made such coverage uniform at 18 months in all states. As a result of these law changes, there is substantial variation within states over time in continuation availability. The goal of our empirical strategy, for both analyzing insurance coverage and non-employment behavior, is to identify the effect of continuation mandates on behavior by exploiting this variation.

One way to do so would be to run a regression of the form:

$$\begin{bmatrix} Private \\ Insurance \end{bmatrix}_{it} = \alpha + \beta_1 \cdot \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} + X_i' \delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}, \tag{2}$$

where *Months of Coverage*$_{st}$ is the number of months of continuation coverage available in state s at time t. By including fixed state and year effects, we identify the effect of continuation mandates by the variation within states over time in the availability and duration of continuation benefits.

This "differences-in-differences" identification strategy would be sufficient if there were no other changes within states over time that were correlated with the availability of continuation coverage. But it is difficult to control for the many other factors that are changing at the same time as continuation availability.[23] Furthermore, there is the possibility that continuation mandates are an endogenous response to the economic conditions in the state; that is, if there is a

[23] One potentially important omitted variable, particularly for the analysis of non-employment behavior below, is the generosity of the state unemployment insurance (UI) system. Since more generous UI benefits have been shown to lengthen unemployment durations (e.g. Meyer, 1990) and increase the incidence of unemployment (e.g. Topel, 1983), changes in UI generosity which are correlated with changes in continuation availability could bias our estimates of non-employment behavior. We have investigated this possibility by collecting data on the maximum UI benefit available in each state and year over this time period, and by modelling the availability of continuation coverage as a function of this maximum benefit (since changes in the maximum are the primary means by which states increase the generosity of UI). Conditioning on a full set of state and time dummies, there is an insignificant relationship between months of continuation coverage and the UI maximum benefit, suggesting little scope for omitted variable bias from not simultaneously modelling the UI system.

shock which leads to increased non-employment or reduced insurance coverage among the non-employed, state legislatures may respond by mandating continuation coverage.

We can control for this possibility, however, by using a *within-state control group* of the non-employed. These are individuals who are subject to the same types of state-year specific shocks (or endogeneity) but who do not have continuation mandates available, so that any changes in their insurance status represent spurious state/year factors. One natural such group is those who did *not* have employer-provided coverage before their separation. This group does not have access to continuation mandates, since they have no insurance to continue. As a result, they can serve as a control group for capturing any omitted factors specific to the non-employed in a given state and year.

In order to employ these individuals as a control group, we estimate an extended version of equation (2):

$$\begin{aligned}\begin{bmatrix}\textit{Private}\\ \textit{Insurance}\end{bmatrix}_{it} = \alpha + \beta_1 \cdot \begin{bmatrix}\textit{Months of}\\ \textit{Coverage}\end{bmatrix}_{st} + \beta_2 \cdot \begin{bmatrix}\textit{Employer}\\ \textit{Hlth. Ins.}\end{bmatrix}_{i} + \beta_3 \cdot \begin{bmatrix}\textit{Months of}\\ \textit{Coverage}\end{bmatrix}_{st} * \begin{bmatrix}\textit{Employer}\\ \textit{Hlth. Ins.}\end{bmatrix}_{i} \\ + X_i' \delta + \pi_s' \cdot \textit{State} + \tau_t' \cdot \textit{Time} + \varepsilon_{it} ,\end{aligned} \tag{2'}$$

where *Employer Hlth. Ins.*$_i$ is an indicator for whether individual *i* had employer-provided insurance on his pre-separation job.[24] In this model, the interaction between *Months of Coverage* and *Employer Hlth. Ins.* measures the specific effect of continuation mandates on those who had insurance before separating. The *Months of Coverage* main effect measures the residual effect of continuation mandates on those not insured. That is, the coefficient β_1 measures any

[24] Individuals are only asked about employer-provided insurance once per wave, so it is difficult to assess whether this insurance is from before a separation for a worker who finds reemployment in the same wave. We therefore measure employer-provided insurance in the wave before separation. This will induce some measurement error into our indicator for pre-separation insurance, but given the relatively low 3.9 percentage point transition rate over a one wave period, this error should be minimal.

overall state/year factors correlated with the passage of continuation mandates, and β_3 measures the causal impact of the mandates on private insurance coverage.

In theory, continuation mandates only affect coverage by employer-provided insurance among the non-employed. We use coverage by any private health insurance coverage as the dependent variable in our regressions, however, for three reasons. First, private health insurance coverage is measured monthly in the SIPP, whereas employer-provided health insurance is measured only wavely. Using a monthly measure rather than a wavely measure allows us to ascertain what happens to health insurance coverage in the first few months of unemployment, rather than at four month intervals. Second, we are interested in how continuation mandates affect overall private insurance coverage among the jobless. Finally, it is not clear whether SIPP respondents label their continuation coverage as employer-provided insurance or as other private (individually-purchased) coverage.

Continuation Mandates and Insurance Coverage for the Non-Employed

The effect of continuation mandates on insurance coverage is reported in the top panel of Table 4; we report only the coefficients of interest in the table, although the regressions include all of the covariates shown in Table 2 (and noted in the footnote to that table). The first column shows the coefficients from estimating equation (2'). We find that the interaction coefficient β_3 is statistically significant, and indicates that for every month of continuation coverage available, insurance coverage among the non-employed rises by 0.21 percentage points. Thus, a one year continuation of coverage mandate reduces the drop in insurance coverage among the non-employed by 2.5 percentage points.

To scale this finding, it is necessary to estimate the effect of job leaving on insurance coverage, for those that left jobs with insurance only. Estimating equations such as (1), but restricting the sample to those with insurance on their previous job, we find that job leaving is associated with a 37 percentage point reduction in the likelihood of having private insurance. This implies that having one year of continuation coverage reduces the likelihood of losing private insurance among the non-employed by only 6.7 percent. The coefficient on the continuation coverage main effect, β_1, is insignificant, suggesting that there are no important omitted factors which are correlated with both state/year variation in continuation availability and with insurance coverage.

It would be interesting to use the above estimates to compute some sort of continuation coverage take-up rate. Using data from a company administering continuation coverage for other employers, Flynn (1992) calculates that about 17 percent of all job separators (both those that do and do not experience unemployment) take-up continuation benefits. Our estimate of the net increase in private health insurance coverage is substantially lower than her figure. This likely results from many individuals replacing other types of coverage (for example, coverage from a spouse's plan or health insurance purchased in the private market) with continuation coverage. It may also be due to lower take-up rates among those transiting from employment to non-employment than among those transiting directly to other jobs (perhaps due to lower transitory incomes), or because the long-term unemployed who had initially taken up continuation benefits eventually drop their coverage or exhaust their benefits. Finally, it may result from delays in take-up among the non-employed in our sample. Individuals have 60 days following job less to retroactively elect continuation coverage; consequently, even those that eventually take-up continuation benefits may appear uninsured for the first two months of their spells.

Thus, while continuation coverage availability results in a significant increase in insurance coverage, continuation mandates do not appear to substantially reduce the extent of uninsurance among the those who are jobless. Considering the fact that continuation mandates lower the price of insurance substantially for the non-employed, our estimates imply a fairly small price elasticity of demand for insurance. As noted earlier, continuation coverage is roughly 40 percent cheaper than individually purchased insurance, and we find that the net increase in insurance from one year of continuation coverage is only 4.2 percent of the baseline rate of insurance coverage for the non-employed who left jobs with insurance. This implies a price elasticity of demand on the order of only -0.1. This estimate is similar to estimates of price elasticity of demand for insurance among small firms in Thorpe et al. (1992), but it is substantially below the estimate for the self-employed in Gruber and Poterba (1994). The low elasticity in our case is likely due to the contemporaneous income effect from leaving one's job, as well as low overall insurance demand for job leavers.

Variation with Completed Non-Employment Durations

As we noted in Section II, the effect of job leaving on insurance coverage is much larger for those who ultimately have longer non-employment spells, even at the start of their spells. One question of interest is therefore whether continuation mandates have their largest impacts on those who are in greatest need of subsidies, the long-term non-employed. We can investigate this by extending the regression framework of equation (2′) to:

$$\begin{bmatrix} Private \\ Insurance \end{bmatrix}_{it} = \sum_{d=0}^{D} \Bigg\{ \alpha^d \cdot \begin{bmatrix} Completed \\ Duration \end{bmatrix}_i^d + \beta_1^d \cdot \begin{bmatrix} Completed \\ Duration \end{bmatrix}_i^d * \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} + \beta_2^d \cdot \begin{bmatrix} Completed \\ Duration \end{bmatrix}_i^d * \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_i$$

$$+ \beta_3^d \cdot \begin{bmatrix} Completed \\ Duration \end{bmatrix}_i^d * \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} * \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_i \Bigg\} + \mathbf{X}_i' \delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it} , \qquad (2'')$$

where *Completed Duration*$_i^d$ is a series of dummies for completed spell durations of less than one month, 2-12 months, and more than 12 months.[25]

In order to have a baseline insurance change with which to compare these estimates, we respecify our basic regressions to estimate the impact of job leaving on insurance coverage separately by these categories of completed duration. The results from so doing are presented in the second column of Table 3 for the sample that has employer-provided health insurance in the last wave (or before job leaving). Our findings, which mimic those of Figure 2, are that the effect of non-employment on insurance coverage is much larger for those with longer completed durations; those whose durations are more than one year are almost twice as likely to lose insurance as those whose durations are one month or less.

The results from estimating equation (2'') are presented in the final column of Table 4. We find that continuation mandates have a much larger impact on the insurance coverage of those with longer completed durations. In fact, the effect of continuation mandates is insignificant for those with completed durations of one year or less. For those with completed durations of more than one year, however, the effects are quite large, indicating that one year of continuation availability raises the likelihood of insurance coverage by 9.4 percentage points. That is, for those with durations of less a year or less, continuation availability reduces the shortfall in

[25] Relative to Figure 2, we have collapsed the categories for 2-6 months and 7-12 months, since it was difficult to distinguish differential effects of continuation mandates within these different groups.

insurance coverage by only 1 to 2 percent. But for those with durations of more than one year, the shortfall is reduced by 19 percent.

Thus, continuation mandates have their greatest impact on those with longer spells, both in absolute terms and relative to the baseline decrease in insurance coverage for this group. The implication is that continuation mandates, despite small overall impacts, are effectively targeted to the population of greatest need.

IV. Continuation Mandates and Non-Employment Behavior

The results above suggest that, despite potential barriers to their effectiveness, continuation mandates have important benefits for the insurance coverage of the non-employed. At the same time, by completing the missing market for group insurance for those with employer-provided insurance pre-separation, continuation mandates may make non-employment more attractive. The welfare implications of any resulting non-employment are unclear, and depend on whether continuation mandates are subsidizing increased match efficiency or simply subsidizing leisure. In this section, we first estimate whether continuation mandates affect the extent of non-employment; we then turn to measuring the welfare implications of this effect.

Continuation Mandates and the Probability of Becoming Non-Employed

We begin by examining the effect of continuation mandates on the wave-to-wave transition from employment to non-employment. To do this, we estimate a probit model of the following form:

where *Become Unemployed* is a dummy equal to one if an individual transits from employment to non-employment during the wave and *Months of Coverage* is the months of continuation coverage

$$Pr\begin{bmatrix} Become \\ Unemployed \end{bmatrix}_{it} = \Phi\Big(\alpha + \beta_1 \cdot \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} + \beta_2 \cdot \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_{i} + \beta_3 \cdot \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} * \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_{i}$$

$$+ \mathbf{X}_i' \delta + \pi_r' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}\Big)\ , \qquad (3)$$

available to an individual at the start of the wave. Once again, this model is identified by changes in continuation of coverage mandates across states and over time, where those without employer-provided insurance in the previous wave are used as a control to capture omitted state/year effects.

The results of this regression analysis are presented in Table 5. There is a highly significant effect of continuation mandates on the probability of becoming non-employed among those with employer-provided health insurance (the coefficient on the interaction in the third row of Table 5). The estimate implies that having one year of continuation coverage raises the odds of a transition during a given wave from 3.7 percentage points to 4.2 percentage points, a 14 percent increase. This is similar in magnitude to the effect of continuation mandates on any transition, including job-to-job transitions, estimated by Gruber and Madrian (1994), suggesting that most of the effects of continuation mandates are to subsidize moves to non-employment. It is about half of the magnitude of "job-lock" estimated by Madrian (1994). Thus, subsidizing non-employment through continuation mandates does raise the likelihood that prime-age males leave their jobs.

Continuation Mandates and Total Non-Employment

Another margin along which continuation mandates may have effects is the duration of non-employment spells. In theory, we could investigate continuation mandate effects on duration directly, using hazard models of the type employed by Meyer (1990). In practice, however, the above findings would make it difficult to interpret the results, since the sample of individuals becoming unemployed is itself affected by the availability of continuation coverage. For example, if those becoming non-employed due to continuation mandates have disproportionately short spells, this would bias downwards the estimated effect of the mandates on unemployment durations.

We therefore take a more aggregate approach and look at the effect of continuation mandates on total non-employment during a one year period.[26] That is, we take a sample of men in their first wave of work, and measure their weeks of non-employment over the subsequent three waves (12 months). The change in non-employment will be a function of both increased transition rates and increased durations; while we cannot disentangle these factors due to the selection bias discussed above, we can measure the net effect of the two. The regressions here are of the same form as equation (3), but the dependent variable is annual weeks of non-employment. Our key regressor is the months of continuation coverage available at the beginning of this 12-month period.[27]

One problem with this regression is that our dependent variable, weeks of non-employment, is censored both at zero (90 percent of our sample have no weeks of non-employment during a one-year period) and at 52. Furthermore, the data are discretely, not

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[26] This parallels the approach of Levine (1993).

[27] Note that in this context, those without employer-provided insurance in the previous wave are a less valid control group since during the year they may switch into jobs that have insurance, and then have their non-employment decisions influenced by continuation mandates. Given the relatively low transition rates in our sample, however, and the fact that individuals tend to move to jobs with the same insurance status as their previous job, we believe this is not a significant problem. An upper bound estimate of the bias from using those without insurance at the start of the year as a control is the fraction of this group that has moved to jobs with insurance by the end of the year. This fraction is 31 percent, suggesting that the bias to our findings is small (note also that only 5 percent of those with insurance at the start of the year are in jobs without insurance at the end of the year). In any case, to the extent that the control group is impacted by continuation mandates, we will understate the true effect of the mandates on those with insurance only.

continuously, distributed between these endpoints. To deal with this data structure, we estimate a Poisson regression model.

The results of this analysis are presented in the second column of Table 5. There is a sizeable and significant effect of months of continuation coverage on weeks spent unemployed during the year. The estimates imply that a one year continuation mandate raises the average number of weeks spent unemployed by 15 percent. Because individuals in our sample spend only 1.5 weeks unemployed on average, this large percentage effect translates into a small absolute effect of only 0.23 weeks per year.

Note that a one year continuation mandate leads to approximately the same relative increase in both total weeks spent unemployed (15 percent) and the transition rate from employment to non-employment (14 percent). If the individuals induced to separate by continuation availability have the average non-employment duration of all individuals leaving their jobs (no selection bias), these results suggest that all of the increase in time spent unemployed results from an increased number of unemployed individuals and not from increased unemployment durations among those who are out of work. That is, unless the individuals induced to leave their jobs by continuation mandates have an underlying propensity for shorter unemployment durations, our findings suggest that continuation mandates have little effect on the duration of non-employment spells.

Reemployment Earnings

This continuation coverage-induced non-employment need not be viewed as a cost associated with making health insurance more readily available for the unemployed. If individuals were "locked" into lower productivity positions before such mandates were in place, then the mandates could reduce this pre-existing distortion. Similarly, if the individuals who were displaced from jobs with insurance were taking the first available job with insurance coverage rather than searching for the most productive match, allowing them to purchase subsidized coverage while jobless could increase match quality as well. In either case, this government intervention may result in efficiency gains. On the other hand, if average job matches are no better, it suggests that these policies are simply subsidizing unproductive non-employment. While a number of studies have suggested that "job-lock" is quantitatively important (ie. Madrian, 1994; Gruber and Madrian, 1994; Monheit and Cooper, 1994), there has been no effort to measure empirically its impact on match quality.

Our goal in this section is therefore to assess whether the quality of job matches is higher for those separating when continuation coverage is available than for those separating when it is not available. While it is difficult to quantify the quality of job matches, we use a rough proxy: the reemployment wages of individuals who become unemployed.[28] That is, for those who separate from their jobs, we consider how their earnings post-separation compare to their earnings pre-separation. To answer this question, we reorganize our data so that we have one observation per separation. We then measure total earnings in the 15 months following the job loss, as well as earnings in the month of job loss, 1-3 months after job loss, 4-7 months after, 8-11 months after, and 12-15 months after. The analysis is restricted to individuals who have data through 15 months after the point of separation.[29] To capture the net effect of both increased

[28] This approach follows the literature on the job match consequences of increasing the generosity of unemployment insurance benefits; see Ehrenberg and Oaxaca (1976) or Meyer (1989). This literature has produced a broad range of estimates (most recently, in Meyer (1989), zero), making it difficult to compare our findings to the reemployment wage effects of unemployment insurance.

[29] This restriction allows us to have a balanced panel of workers at each point in time, so that differential effects at different durations reflect true duration dependence and not heterogeneity.

non-employment durations and (potentially) increased earnings upon reemployment, we do not condition on individuals becoming reemployed, although we also present results with the sample restricted to those who do become reemployed.

We run regressions of the form:

$$Earnings_{i,t+k} = \alpha + \beta_1 \cdot \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} + \beta_2 \cdot \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_i + \beta_3 \cdot \begin{bmatrix} Months\ of \\ Coverage \end{bmatrix}_{st} * \begin{bmatrix} Employer \\ Hlth.\ Ins. \end{bmatrix}_i$$

$$+ \beta_4 \cdot \begin{bmatrix} Pre\text{-}Separation \\ Earnings \end{bmatrix}_i + \mathbf{X}_i' \delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}. \qquad (4)$$

In this model, the individual leaves his job in period t. We model future wages (in period *k*) as a function of the usual set of covariates: continuation coverage available at the point of separation, lagged employer-provided insurance, and the interaction of the two, along with the other individual/job controls and state and year effects.

One important potential problem with this regression framework is selection bias. We have already demonstrated that the transition into non-employment appears to be affected by the availability of continuation coverage. As discussed above in the context of non-employment durations, if the individuals who are induced to leave their jobs are disproportionately high or low wage, it will lead us to misstate the effect of continuation mandates on reemployment wages. Unlike the case of durations, however, we have a natural control for this selection bias: pre-unemployment earnings.[30] We therefore include in the regression model earnings in the four months before job leaving. We have experimented with including higher order terms in lagged wages, with little effects on our estimates.

[30] The use of pre-separation earnings to control for heterogeneity in post-separation outcomes parallels the strategy pursued by Ruhm (1991). Furthermore, if the cost of continuation mandates is passed onto workers' wages, then the presence of a mandate would affect reemployment wages through this incidence channel even if there were no search effects. But any such incidence effect will be captured in our lagged wage control in (4).

In order to scale the effects of continuation mandates on reemployment earnings, we must measure the baseline earnings cost of separation. To do so, we first match to our sample of separators data for individuals who experience no unemployment. For these non-separators, we calculate earnings over the next 15 months, as well as earnings in the current month, in the next 1-3 months, the next 4-7 months, the next 8-11 months, and the next 12-15 months. With this dataset on earnings of those who both do an do not experience any unemployment, we then run a regression of earnings in each of these future periods on individual demographic characteristics and a separation dummy (as done in equation (1) for insurance coverage). The coefficient on the separation dummy yields the earnings effects of separation per se, relative to the wage growth over time for those who remain employed.

The results of running this baseline earnings loss regression are presented in Table 6. As expected, we find that there is a sizeable impact of separation on future earnings. Over the full 15 month period, earnings fall by almost $7300. In the second column of this panel, we express our estimates as a percentage of the pre-separation baseline earnings of those insured workers that eventually do separate.[31] This $7300 reduction is slightly more than one-third of the annualized earnings over the four months before separation.

The time pattern of earnings effects is displayed in the remainder of the column. In the first month after separation, earnings fall by $946, which is more than one-half of the average monthly earnings over the four months before separation. The effect of separation on earnings is about the same (in percentage terms) for the next three months, and then declines sharply. However, even 12-15 months after separation, average earnings are almost 30 percent below the level of those who have not separated.

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[31] Baseline earnings are $6873 in the four months before separation for this group.

Part of this earnings decline arises from a reduced likelihood of employment, while part arises from lower earnings conditional on reemployment. In the last two columns of Table 6, we restrict our sample to those who are reemployed (which we define as having positive earnings). For this sample, there are smaller effects of separation, although earnings are still 18 percent lower 12-15 months after separation.[32] These findings are similar to, although somewhat smaller than, those of other detailed studies of the effects of separation on reemployment earnings (such as Ruhm (1991) and Jacobson *et al.* (1993)).[33]

In the Table 7, we report only the coefficient of interest (β_3) from our estimates of equation (4). In the first two columns we examine the full set of separators. Over the 15 month period following job leaving, there is a significant increment to earnings associated with the presence of a continuation mandate. The top number in the second and fourth columns is the effect on earnings of one year of continuation coverage as a percent of pre-separation earnings (comparable to Table 6), and the figure in square brackets is this effect as a percent of the earnings loss following job separation (as a percent of the coefficients in Table 6). For all

[32] Note that in the second column, the total impact over the 1-15 month period is substantially higher than the sum of the coefficients over each of the individual periods ($5468). This is because in the first row we restrict the sample only to those with earnings at some point over the 15 month period, while in the other rows we restrict it to those with earnings at each of the finer time periods. This same phenomenon occurs in Table 7: the total impact over the 1-15 month period is substantially higher than the sum of the coefficients over each of the individual periods. The percentage effect of continuation coverage over 1-15 months is quite similar, however, when calculated either as the regression coefficient from the first row of Table 7 divided by the regression coefficient from the first row of Table 6, or as the sum of the regression coefficients from the other rows in Table 7 divided by the sum of the regression coefficients from the other rows in Table 6.

[33] Ruhm (1991) finds almost precisely the same reemployment earnings impact one year after separation as we do in Table 6. Jacobson *et al.* (1993) find that there is roughly a 24 percent earnings decline one year after separation (this is a weighted average of the estimates for their mass layoff sample and their non-mass layoff sample). This is slightly larger than our estimate for reemployment earnings 12-15 months after separation. Their estimate should be somewhat higher than ours, however, since they are specifically examining high tenure workers (who experience larger wage declines from separation), since a larger fraction of their sample is laid off (rather than voluntary leavers), and since their estimates are from a recessionary time period with worse reemployment prospects.

separators, one year of continuation coverage is associated with a 6.1 percent increase in earnings over the 15 months after job leaving; this is over 17 percent of the baseline earnings reduction resulting from separation. Thus, continuation coverage availability significantly increases earnings prospects over the 15 months following separation.

In the remaining rows of the table we once again display the time pattern of these effects. Continuation mandates have insignificant effects on earnings through 4-7 months after separation. By 8-11 months and 12-15 months after, however, there is a positive and significant coefficient. These coefficients indicate fairly sizeable effects of continuation mandates on reemployment earnings. For all separators, one year of continuation coverage is associated with an 8.1 percent increase in earnings 8-11 months after separation; this is almost one-third of the baseline earnings reduction resulting from separation. The effect rises somewhat over the next four months, so that one year of continuation coverage reduces the separation-induced earnings decline by 35 percent 12-15 months out.

Conditioning on reemployment in columns (3) and (4), the effects of continuation mandates are even stronger, and are also significant from 8 months onwards. Over the entire 15 month period following job leaving, we find that having one year of continuation coverage raises raises the earnings of those who are reemployed by almost 22 percent relative to those that do not have continuation coverage available. This effect is very sizeable 12-15 months after separation, indicating an increase in reemployment earnings at that point of almost 60 percent for those with one year of continuation coverage.

These effects are strikingly large, particularly since we are measuring the impact of continuation mandates on the average separation, not just on those who take-up coverage. In order to assess the sensitivity of these findings, we have pursued two additional specification checks. First, we reestimated our models using robust regression techniques which first exclude influential outlying observations, and then iterate towards a solution by downweighting those observations with larger residuals (Berk, 1990). This procedure reduced our estimates somewhat, particularly at 8-11 months (where the coefficient becomes only marginally significant), but the basic strong pattern of effects remained, and the estimate at 12-15 months was almost identical (for this reason, we do not report these results in our tables).

Second, in the last two columns of Table 7, we consider whether, even in the rich specification outlined above, there are omitted variables that are driving these results. Such variables would have to be correlated with both the presence of a continuation mandate and with the relative growth rates of wages of separators who do and do not have health insurance on their previous job. We have an additional control group, however, which can capture the influence of these type of omitted variables: those who do not separate. That is, we can take the sample of individuals who did not separate, and run regressions such as equation (4). If there is some omitted state/year/insurance status variable correlated with wage growth, then it will be captured by the coefficient β_3 among the employed.

The results of running regression (4) for non-separators only are presented in the last two columns of Table 7. We see that, in fact, there is no effect on this control group; the largest positive coefficient is only 0.23 percent of average wages for insured non-separators. This provides evidence that our large reemployment earnings estimates are not simply driven by some omitted variable correlated with insurance status within states and years. These results suggest that the increased non-employment that follows from the availability of continuation coverage is being spent at least partially in productive job search that results in more efficient job matches.

V. Conclusions

While there is substantial concern in the public policy community about the low levels of insurance coverage among the non-employed, there is little understanding of either the magnitude of this problem or the implications of policy interventions to address it. In this paper, we consider both the effects of job leaving on the insurance coverage of the unemployed, and the effects of subsidized health insurance for the unemployed on the extent of insurance coverage among the jobless, on non-employment behavior, and on reemployment earnings.

To summarize, we have four findings of interest. First, job leaving precipitates a dramatic reduction in insurance coverage, even after conditioning on underlying tastes for insurance coverage. The shortfall in insurance coverage rises with non-employment durations and is most severe at all durations for those who experience the longest eventual completed spells of unemployment. Second, despite their cost and the fact that they are available only to those with insurance on the previous job, continuation mandates offer a mechanism for increasing the insurance coverage of the non-employed. Their net effect on insurance coverage is fairly small overall, but it is strongest for those with the longest non-employment durations, precisely the group who see the greatest reduction in their insurance coverage following job leaving.

At the same time, continuation mandates appear to increase the incidence of joblessness and the amount of time spent unemployed. Whether this is inefficient or not depends on whether continuation benefits subsidize productive job search. There is evidence that this is the case, as we find that the earnings of separators who have continuation benefits available are much higher 8 months or more after separation than the earnings of individuals who do not have access to continuation benefits.

These findings have two important policy implications. First, the fact that non-employment imposes the greatest costs (in terms of insurance coverage loss) on those with the longest spells raises doubts about the efficacy of time-limited continuation coverage. This is particularly true given that continuation mandates have their greatest impact on the long-term non-employed. Second, our findings for reemployment wages suggest that there may be large efficiency costs from individuals being "locked" into lower productivity job matches. Further work to assess the magnitude of these efficiency costs should be a high priority.

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TABLE 1. Descriptive Statistics

	All Months		First Month	
	Employed	Not Employed	No Job Loss	Job Loss
Health Insurance Coverage				
Private health insurance	89.3	48.9	87.5	69.4
Private health insurance in own name	83.9	32.4	81.9	59.0
Public health insurance	0.57	6.74	0.63	2.84
Employer-provided health insurance	81.9	--	79.6	55.8
Employer provided health insurance before job loss	--	57.6	--	--
Demographic Characteristics				
Age (years)	37.5	36.7	36.6	35.2
Married	77.6	63.6	74.6	65.7
Non-white	10.7	18.1	11.1	14.6
Number of children	1.06	0.88	0.99	0.94
Education: less than high school	14.1	26.5	14.7	22.9
Education: high school graduate	32.9	37.1	33.4	37.4
Education: some college	24.9	22.1	24.2	23.4
Education: 4+ years of college	28.1	14.3	27.7	16.3
Real hourly earnings (1987 dollars)	\$10.32	\$8.24	\$10.22	\$8.71
Sample size	238,332	27,736	9,558	5,284

Authors' tabulations for 25-54 year-old men from the 1984-1988 panels of the Survey of Income and Program Participation. Statistics in the first two colums are calculated for the full sample of person/months described in the text. Statistics from the last two columns are calculated for only the first month that an individual is in the sample. All means are weighted to reflect oversampling of the unemployed.

Table 2. The Effect of Job Loss on Private Health Insurance Coverage

	OLS	Fixed Effects
Without a job	-0.2913 (0.0054)	-0.1996 (0.0022)
Age	0.0023 (0.0007)	0.0113 (0.0024)
$Age^2/1000$	-0.0027 (0.0008)	-0.1443 (0.0308)
White	0.0080 (0.0017)	--
Married	0.0765 (0.0014)	0.0227 (0.0033)
Number of Children	-0.0075 (0.0005)	--
High school graduate	0.0483 (0.0016)	--
Some College	0.0534 (0.0018)	--
College graduate	0.0603 (0.0021)	--
Log real wage (previous wave)	0.0682 (0.0012)	0.0128 (0.0015)
Seam month	0.0014 (0.0012)	0.0006 (0.0008)
Employer-provided insurance (previous wave)	0.3139 (0.0015)	--
Sample size	262,332	262,808

Standard errors are in parentheses. The dependent variable is private health insurance coverage. All regressions include month, year, industry and occupation dummies. Regressions without fixed effects also include state and panel dummies. Regressions are weighted to account for oversampling of the unemployed.

TABLE 3. Continuation of Coverage Laws

State	Date	Months	Voluntary	State	Date	Months	Voluntary
Arkansas	7/20/79	4	Y	New Mexico	7/1/83	6	Y
California	1/1/85	3	Y	North Carolina	1/1/82	3	Y
Colorado	7/1/86	3	Y	North Dakota	7/1/83	10	Y
Connecticut	10/1/75	10	Y	New York	1/1/86	6	Y
	1/1/87	20	Y	Ohio	7/1/84	6	N
Georgia	7/1/86	3	Y	Oklahoma	1/1/76	1	Y
Illinois	1/1/84	6	Y	Oregon	1/1/82	6	Y
	8/23/85	9	Y	Rhode Island	9/1/77	10	N
Iowa	6/1/84	6	N	South Carolina	1/1/79	2	Y
	7/1/87	9	Y		1/1/90	6	Y
Kansas	1/1/78	6	Y	South Dakota	7/1/84	3	Y
Kentucky	7/15/80	9	Y	South Dakota	3/3/88	18	Y
Maryland	7/1/86	18	N	Tennessee	1/1/81	3	Y
Massachusetts	1/1/77	10	N	Texas	1/1/81	6	Y
Minnesota	8/1/74	6	Y	United States	7/1/86	18	Y
	3/19/83	12	Y	Utah	7/1/86	2	Y
	6/1/87	18	Y	Vermont	5/14/86	6	Y
Missouri	9/28/85	9	Y	Virginia	4/17/86	3	Y
Nebraska	1/1/78	6	N	Wisconsin	5/14/80	18	Y
New Hampshire	8/22/81	10	Y				

Sources: Hewitt (1985), Thompson Publishing Group (1992), and state statutes. Only state statutes that took effect before COBRA was fully implemented are included in the table.

Table 4. The Effect of Continuation Coverage on Private Health Insurance Coverage Following Job Loss

	Separators	Full Sample	Separators
Effect of Continuation Coverage on Insurance Coverage After Job Loss			
Months of coverage	0.0001 (0.0015)	--	--
Employer-provided insurance (previous wave)	0.1059 (0.0096)	--	--
Months of coverage* Employer-provided insurance	0.0020 (0.0007)	--	--
Effect of Job Loss on Insurance Coverage by Duration of Unemployment Spell			
Completed duration of 1 month or less* Without a job	--	-0.2293 (0.0070)	--
Completed duration of 2-12 months* Without a job	--	-0.3772 (0.0027)	--
Completed duration of 13+ months* Without a job	--	-0.4893 (0.0058)	--
Effect of Continuation Coverage on Insurance Coverage After Job Loss by Duration of Unemployment Spell			
Completed duration of 1 month or less* Months of coverage*Employer HI	--	--	0.0002 (0.0022)
Completed duration of 2-12 months* Months of coverage*Employer HI	--	--	0.0006 (0.0008)
Completed duration of 13+ months* Months of coverage*Employer HI	--	--	0.0078 (0.0017)
Sample size	25,169	240,579	25,169

Standard errors are in parentheses. The dependent variable is private health insurance coverage. Regressions include month, year, industry, occupation, state and panel dummies. The sample in the first and third column is restricted to only those with non-employment spells; the second column includes the full sample of individuals. Regression in the second column is weighted to account for oversampling of unemployed.

Table 5. The Effect of Continuation Coverage on Transitions to Unemployment and Weeks of Unemployment

	Dependent Variable	
	Transition to Unemployment	Annual Weeks of Unemployment
Employer-provided insurance (previous wave)	-0.3849 (0.0249)	-0.8176 (0.0149)
Months of coverage	0.0010 (0.0036)	0.0099 (0.0026)
Employer-provided insurance* Months of coverage	0.0058 (0.0019)	0.0119 (0.0013)
Age	-0.0156 (0.0089)	-0.0422 (0.0059)
$Age^2/1000$	0.0001 (0.0001)	0.0006 (0.0001)
White	-0.0691 (0.0222)	-0.3301 (0.0129)
Married	-0.1513 (0.0187)	-0.4984 (0.0121)
Number of childran	-0.0102 (0.0072)	-0.0722 (0.0051)
High school graduate	-0.0699 (0.0209)	-0.2715 (0.0128)
Some College	-0.0818 (0.0237)	-0.5159 (0.0151)
College graduate	-0.1274 (0.0286)	-0.7700 (0.0170)
Log real wage (previous wave)	-0.1924 (0.0171)	-0.4467 (0.0123)
Sample size	113,173	29,081

Standard errors are in parentheses. The first column gives coefficient estimates from a probit regression for the transition from employment to unemployment; the unit of observation is a person/wave. The second column gives coefficient estimates from a Poisson regressions with annual weeks of unemployment as the dependent variable; the unit of observation is a person/year. All specifications include year, panel and state dummies. Column 1 also includes month, industry and occupation dummies.

Table 6. The Effect of Job Loss on Earnings

Coefficient on being without a job	Full Sample of Separators and Non-Separators		Separators with Positive Earnings and Non-Separators	
	Coefficient	Percentage Effect	Coefficient	Percentage Effect
Dependent Variable--Earnings in:				
1-15 months after	-7295.39 (187.31)	-35.38%	-6540.68 (193.58)	-31.72%
Current month	-946.20 (20.61)	-55.07%	-485.92 (28.23)	-28.28%
1-3 months after	-2653.80 (57.37)	-51.48%	-1511.57 (73.32)	-29.32%
4-7 months after	-2139.78 (77.76)	-31.13%	-1185.60 (85.03)	-17.24%
8-11 months after	-1758.93 (79.31)	-25.59%	-1026.35 (84.86)	-14.93%
12-15 months after	-1936.47 (81.83)	-28.17%	-1260.0 (87.25)	-18.33%

Standard errors are in parentheses. The dependent variable is total earnings in the period stated at the left of the table. The sample in the first two columns is the full sample of individuals who are observed through the next 15 months (45,429 observations). The sample in the third and fourth columns is further restricted to those individuals who have positive earnings. The second and fourth columns present the estimated effects as a percentage of average pre-separation earnings. All regressions include the set of control variables listed in Table 2, replacing the wage rate with earnings in the previous four months.

Table 7. The Effect of Continuation Coverage on Earnings Following Job Loss

	Separators				Non-Separators	
	All		Reemployed Only			
	Coefficient	Percentage Effect	Coefficient	Percentage Effect	Coefficient	Percentage Effect
Dependent Variable--Earnings in:						
1-15 months after	105.06 (53.47)	6.11% [17.3%]	118.83 (53.19)	6.92% [21.8%]	-1.341 (13.24)	-0.06%
Current month	2.328 (6.317)	1.63% [2.95%]	2.074 (10.66)	1.45% [5.13%]	-0.530 (1.452)	-0.29%
1-3 months after	0.182 (17.15)	0.04% [0.08%]	-9.945 (23.50)	-2.32% [-7.90%]	-2.234 (4.047)	-0.40%
4-7 months after	-12.54 (23.45)	-2.19% [-7.04%]	-13.93 (24.48)	-2.43% [-14.1%]	1.706 (5.490)	0.23%
8-11 months after	46.20 (23.57)	8.07% [31.5%]	46.46 (24.70)	8.11% [54.3%]	1.265 (5.609)	0.17%
12-15 months after	56.35 (22.50)	9.84% [34.8%]	61.18 (22.51)	10.7% [58.6%]	0.158 (5.810)	0.02%

Standard errors are in parentheses. Each cell presents the coefficient on the interaction of employer-provided insurance and months of coverage in the wave before job loss (separators) or the previous wave (non-separators) from regressions outlined in equation (4) of the text. The dependent variable is total earnings in the period stated at the left of the table. The sample in the first two columns is the full sample of individuals who separate from their jobs are observed through 15 months post-separation (1,791 individuals). The sample in the third and fourth columns is further restricted to those individuals who have positive earnings in the relevant post-separation period. The sample in the last two columns is the full sample of individuals who do not experience a spell of non-employment and have earnings through 15 months after the current month. The second, fourth and sixth columns present the estimated effects as a percentage of average pre-separation earnings, and as a percentage of the earnings loss associated with job separation (square brackets). All regressions include the set of control variables listed in Table 2, replacing the wage rate with earnings in the previous four months.

APPENDIX TABLE 1. The Effect of Job Loss on Insurance Coverage by Duration of Unemployment

	Full Sample		Completed Duration of Unemployment Spell			
	OLS	Fixed Effect	≤ 1 Month	1-6 Months	7-12 Months	12+ Months
9-12 months before	-0.0265 (0.0046)	0.0312 (0.0035)	0.0237 (0.0034)	0.0264 (0.0035)	0.0275 (0.0034)	0.0258 (0.0034)
5-8 months before	-0.0356 (0.0037)	0.0251 (0.0029)	0.0213 (0.0028)	0.0217 (0.0029)	0.0238 (0.0028)	0.0234 (0.0028)
4 months before	-0.0328 (0.0057)	0.0134 (0.0039)	0.0135 (0.0038)	0.0146 (0.0038)	0.0151 (0.0038)	0.0146 (0.0038)
3 months before	-0.0405 (0.0055)	0.0142 (0.0038)	0.0123 (0.0037)	0.0128 (0.0038)	0.0145 (0.0037)	0.0136 (0.0037)
2 months before	-0.0502 (0.0053)	0.0014 (0.0036)	0.0018 (0.0035)	-0.0000 (0.0037)	0.0034 (0.0035)	0.0024 (0.0035)
1 months before	-0.0822 (0.0055)	-0.0205 (0.0039)	-0.0170 (0.0037)	-0.0191 (0.0038)	-0.0162 (0.0038)	-0.0164 (0.0037)
Month of job loss	-0.2307 (0.0040)	-0.1430 (0.0032)	-0.1058 (0.0051)	-0.1523 (0.0039)	-0.1612 (0.0090)	-0.2350 (0.0158)
1 month after	-0.2987 (0.0059)	-0.2118 (0.0043)	--	-0.2080 (0.0047)	-0.2278 (0.0101)	-0.2707 (0.0174)
2 months after	-0.3138 (0.0065)	-0.2250 (0.0047)	--	-0.2181 (0.0054)	-0.2492 (0.0097)	-0.2685 (0.0169)
3 months after	-0.3327 (0.0071)	-0.2352 (0.0052)	--	-0.2320 (0.0066)	-0.2644 (0.0112)	-0.2706 (0.0154)

4 months after	-0.3431 (0.0123)	-0.2562 (0.0085)	--	-0.2453 (0.0157)	-0.2615 (0.0108)	-0.3203 (0.0193)
5 months after	-0.3476 (0.0129)	-0.2554 (0.0089)	--	-0.2273 (0.0253)	-0.2840 (0.0149)	-0.3229 (0.0174)
6 months after	-0.3607 (0.0114)	-0.2591 (0.0081)	--	--	-0.2594 (0.0190)	-0.3255 (0.0155)
7 months after	-0.3902 (0.0177)	-0.2925 (0.0121)	--	--	-0.2665 (0.0183)	-0.3670 (0.0195)
8 months after	-0.3674 (0.0198)	-0.2638 (0.0135)	--	--	-0.2325 (0.0328)	-0.3257 (0.0186)
9 months after	-0.3836 (0.0170)	-0.2766 (0.0119)	--	--	-0.2863 (0.0338)	-0.3390 (0.0168)
10 months after	-0.3582 (0.0251)	-0.2557 (0.0170)	--	--		-0.3168 (0.0197)
11 months after	-0.3639 (0.0237)	-0.2664 (0.0161)	--	--		-0.3149 (0.0174)
12+ months after	-0.3967 (0.0106)	-0.2969 (0.0091)	--	--		-0.3495 (0.0111)

Standard errors are in parentheses. The dependent variable is private health insurance coverage. The regressions in the first two columns include the full sample of person/months. The regressions in the last four columns include all person/months in which individuals are with a job and the subset of person/months when individuals are without a job with the completed duration given in the column heading. All regressions include month, year, industry and occupation dummies. The regression in the first column also includes state and panel dummies, as well as the other control variables listed in Table 2. Regressions are weighted to account for oversampling of the unemployed.

FIGURE 1. The Effect of Job Loss on the Insurance Coverage of the Unemployed

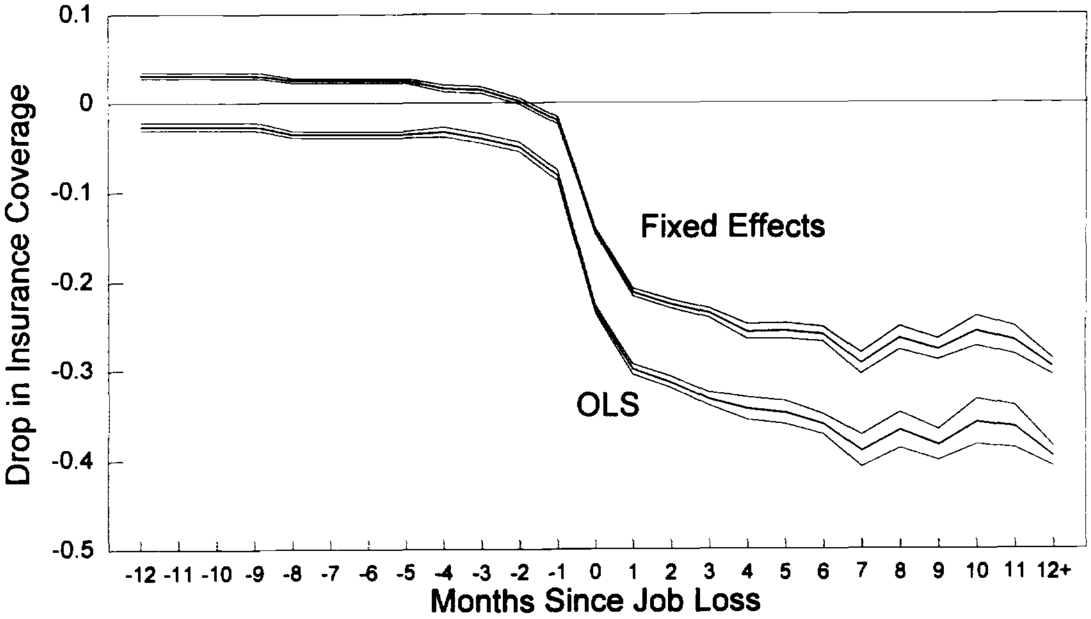

FIGURE 2. The Effect of Job Loss on Insurance Coverage by Completed Duration of Unemployment