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Employment separation and health insurance coverage

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Abstract

We study the interrelationship between employment separation and insurance coverage. We first document that employment separation is associated with large reductions in insurance coverage, even conditioning on underlying tastes for insurance. We then show that reducing 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 after separating from a job by 6.7%. These mandates also increase the number of individuals who separate 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. © 1997 Elsevier Science S.A.

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1. Introduction

In the U.S., most group health insurance is provided through the workplace. As a result, those 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 individuals who leave or lose their jobs often go uninsured. Indeed, among prime-aged (25–54 year-old) males during

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the 1983–1989 period, 89.3% of those who were employed had some form of private insurance coverage, while only 48.9% of those who separated from their jobs had private insurance.[1]

This low rate of insurance coverage among the job separators 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 those separating from employment does not prove that they face access problems. Individuals who leave or lose 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 and losers also tend to disproportionately separate from jobs that did not offer health insurance, so that their lower coverage rates may not result from separation 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 job separators therefore revolves around the answers to several heretofore unaddressed questions. 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 fully controlled for the underlying differences in tastes for insurance among those who do and do not lose their jobs.

Second, how much will lowering the cost of group health insurance for job separators increase their rate of insurance coverage? In addition to the factors mentioned above, separators may have a decreased demand for insurance due to the negative income shock that follows job leaving or losing. As a result, extensive subsidies may be required to significantly increase the level of insurance coverage among this group.

[1] Authors' tabulations based on SIPP data described below.

[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% 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% of the unemployed were both uninsured and had insurance on their previous job; however, another 10.3% 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, Podgursky and Swaim (1987) find that those who were displaced from their jobs and are still out of work several years later see reductions in their level of insurance coverage of 22–34%. And 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% upon job loss; several studies reviewed by Bazzoli (1986) also claim to find large declines in insurance coverage following job loss during this period.

Third, what will be the effects of reducing the cost of insurance for separators on non-employment behavior? Will lower costs of 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 reluctant to change jobs if doing so requires a subsequent period of unemployment and job search or of restricted health insurance coverage because of waiting requirements for coverage at a new job; they will also try to minimize the amount of time spent unemployed if they lose jobs with health insurance. Such behavior may be inefficient 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 reducing insurance costs for job separators.

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 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 separators 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 2, we describe our data source. In Section 3, we provide background on the insurance coverage of separators. In Section 4, we investigate the effect of continuation mandates on the insurance coverage of separators, and in Section 5 we model non-employment behavior and reemployment earnings as a function of continuation coverage availability. Section 6 concludes.

2. 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. In order to focus our analysis on a group that has a high attachment to the labor force, we restrict our analysis to 25–54 year-old males. This minimizes the amount of non-employment due to retirement, school, or child care decisions that would occur in a sample that also included women or individuals from a broader age range. We then use data on individuals only once we have observed at least one full wave of employment experience.[4] This restriction excludes unemployment spells that are in progress at the time the survey begins. By doing so, we are able to accurately measure both the duration of the non-employment spells that are included in our sample and the characteristics of pre-separation jobs, most importantly coverage by employer-provided health insurance. As a result, however, our findings apply only to the population of job leavers and losers, and not to those who are never in the labor force over the 2–3 year period covered by each SIPP panel, such as the disabled or those who have never worked. In the full SIPP sample of 25–54 year old men, 42% of the months of non-employment are among the persons who meet our sample criterion; the remaining 58% of months are among those who do not have one full wave of employment during the SIPP panel. Finally, we also exclude 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. For our models of transitions to and durations of non-employment, this 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. That paper and Gruber and Madrian (1995) pursue related analyses for older workers.

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

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 prime aged males is often disguised long-term unemployment (Clark and Summers, 1979).[5] 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.[6] In results not reported, we have redefined non-employment as being without a job *and* in the labor force (conventional unemployment), and as being not at work (including temporary layoffs in our definition); 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 (i.e. layoffs) and job leaving (i.e. 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, and we use the term ‘‘job leaving’’ to refer to both voluntary and involuntary separations. Overall, an average of 4.6% of our sample of men are defined as without a job in a given month, and 15.6% 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 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 (the ‘‘seam month’’) backwards through the preceding months. This problem may be particularly relevant for the timing of insurance coverage changes. In fact, 80% of insurance changes occur at the seam month, whereas 25% would be the expected figure given the essentially random timing of survey dates.

[5]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.

[6]About one-fifth of employees covered by employer-provided health insurance work in firms that continue to provide health insurance during a temporary lay-off (U.S. Bureau of Labor Statistics, 1990).

[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%.

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. We therefore correct the standard errors for this within-wave correlation in our insurance coverage analysis.[9] 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.

3. Background on the insurance coverage of the non-employed

3.1. Sampling and descriptive statistics

As noted above, because many spells of non-employment are short we analyze the effect of separation 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% sampling of every separator who experiences at least one month of non-employment, and a 1-in-3 random sampling of those individuals who do not separate. 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 have a separation during their participation in the SIPP.

Overall, 89.3% of employed males have some form of private insurance

[9] Our correction allows for a general form of correlation across the four observations that make up each wave. Our results are similar if we just use one monthly observation per wave, but they are somewhat less precise, as is to be expected since there is some information in the month-to-month transitions within waves.

[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 4 by using variation among the non-employed only, so that having a large sample of non-employed is critical to estimating precise effects.

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	–	–
Dentographic characteristics				
Age (years)	37.5	36.7	36.6	35.2
Married	77.6	63.6	74.6	65.7
Non-white	10.7	8.1	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 columns 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.

coverage; the vast majority of this coverage is provided by one's employer. In contrast, only 48.9% of those who have separated have private insurance coverage. Thus these raw data reveal a health insurance coverage gap of over 40 percentage points between the employed and job separators. Public insurance coverage rates are fairly low for this population; less than 1% among the employed, and only 6.7% among separators.

A comparison of other characteristics of the employed and job separators, 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 separate from their jobs 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 are unemployed is 24.3 percentage points lower than the probability of employer coverage for those who are employed.[11] Finally, those who separate 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 separation on insurance coverage.

3.2. 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 basic model is of the form:

$$PRIVINS_{it} = \alpha + \beta \cdot NOJOB_{it} + \boldsymbol{X}_i'\delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}. \qquad (1)$$

In this specification, *PRIVINS* is a dummy variable indicating private health insurance coverage in a particular month, either in one's own name or from some other source.[12] *NOJOB* is an indicator for being without a job in a given month. The vector of control variables, *X*, 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 (in some specifications) 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] In the first column, we include the demographic and job characteristics, but exclude any measure of lagged insurance coverage. We find that non-employment is associated with a statistically significant 33 percentage point drop in the rate of

[11] These findings are consistent with evidence in Marquis and Long (1991), who find that workers at firms that do not offer insurance appear to be quite similar in demographic characteristics to workers who turn down insurance when it is offered, suggesting that lack of insurance offering is a function of low insurance demand.

[12] Private health insurance is defined as any health insurance coverage other than Medicare or Medicaid (private insurance includes 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 for separators (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 4 for computational simplicity and ease in interpreting our coefficient estimates.

Table 2
The effect of job loss on private health insurance coverage

	OLS	OLS	Fixed effects
Without a job	−0.3347	−0.2913	−0.1994
	(0.0054)	(0.0055)	(0.0022)
Age	0.0042	0.0023	0.0098
	(0.0015)	(0.0014)	(0.0025)
Age^2/1000	−0.0047	−0.0027	−0.1227
	(0.0019)	(0.0017)	(0.0311)
	0.0185	0.0080	–
	(0.0043)	(0.0039)	–
Married	0.0777	0.0765	0.0205
	(0.0034)	(0.0030)	(0.0034)
Number of Children	−0.0060	−0.0075	0.0065
	(0.0011)	(0.0010)	(0.0013)
High school graduate	0.0651	0.0483	0.0012
	(0.0044)	(0.0039)	(0.0070)
Some College	0.0703	0.0534	−0.0038
	(0.0047)	(0.0042)	(0.0074)
College graduate	0.0665	0.0603	−0.0270
	(0.0051)	(0.0046)	(0.0084)
Log real wage	0.1415	0.0682	0.0127
(previous wave)	(0.0031)	(0.0027)	(0.0015)
Seam month	0.0018	0.0014	0.0006
	(0.0005)	(0.0005)	(0.0008)
Employer-provided insurance		0.3139	–
(previous wave)		(0.0045)	
Sample size	262,332	262,332	262,332

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.

insurance coverage. Thus, controlling for these characteristics somewhat reduces the raw 40 percentage point effect of non-employment on insurance coverage, but it remains quite sizeable.

In the next column, we include a direct measure of insurance demand: lagged insurance coverage. The effect of non-employment is reduced somewhat in this specification, falling to 29 percentage points. But this effect remains 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 leaving, 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]

The results of the fixed effects model are presented in the second column of Table 2.[16] The effect of non-employment is somewhat diminished; we now estimate that separation 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.

3.3. Non-employment durations

We next explore how these insurance coverage effects vary with non-employment durations among separators. The results of this analysis are depicted in Figs. 1 and 2. We estimate both models by both OLS (including lagged health insurance coverage) and fixed effects. In each model, we replace the dummy variable for job leaving 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

[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 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.

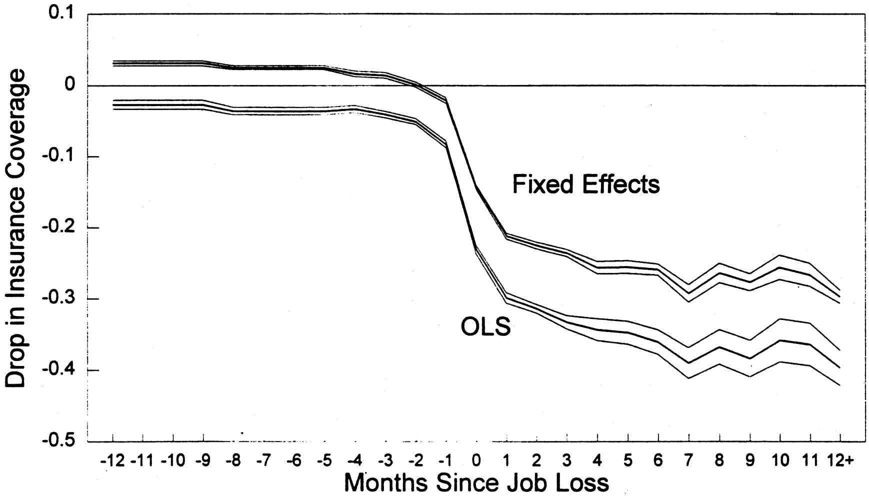

Fig. 1. The effect of job loss on the insurance coverage of the unemployed.

job loss, and 12 or more months after job loss. We also show the confidence intervals on our estimated effects, which are quite tight.

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

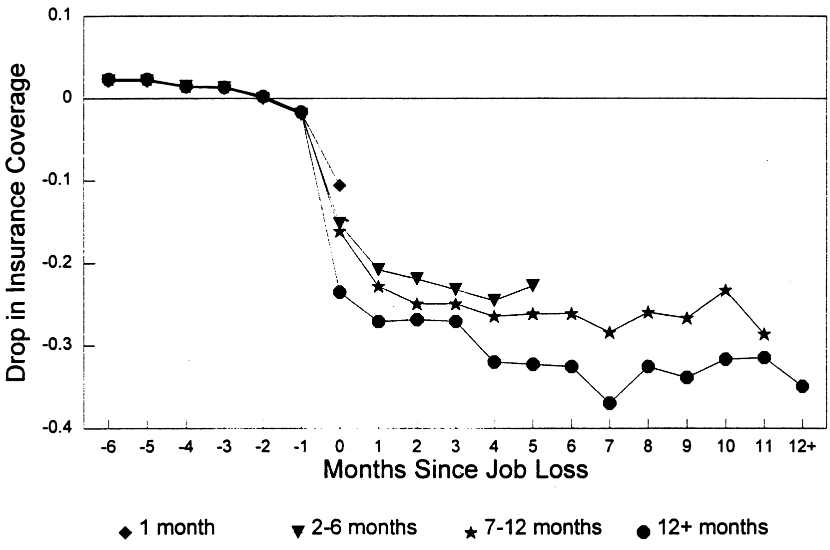

Fig. 2. The effect of job loss on insurance coverage by completed duration of unemployment.

apparent for *future* job leavers. This highlights the ex-ante differences between those who do and do not leave their jobs.

Once we include fixed effects, however, these negative lead terms are reduced, and only the indicator for the month before separation is significant (although still quite small).[17] 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 effect of separation in both cases; after two months, two-thirds of the long run effect has 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 realized.

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 Fig. 2 we graph the effect of job leaving on insurance coverage by the completed duration of non-employment spells.[18] The upper line in Fig. 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.[19]

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 Fig. 1 reflects both heterogeneity 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 within one month, 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 Fig. 1. These findings highlight the substantial heterogeneity underlying the average results in Fig. 1: job leaving has more severe implications for insurance coverage at all durations for those who ultimately have longer spells of non-employment.

At the same time, insurance coverage falls with the duration of non-employment

[17] This remaining negative lead term most likely reflects the effects of some mistiming in the data on insurance coverage and job leaving. This could be due to seam bias; but even if we restrict the analysis to seam months only, this drop before job leaving remains.

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

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

even within groups of the same completed duration, a sign of duration dependence.[20] 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.

3.4. 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 simple comparison of insurance coverage rates across those with and without a job overstates the decline by a factor of two, even after controlling for taste differences with individual fixed effects we still find that separating from a 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.

4. Continuation coverage and the insurance coverage of the non-employed

4.1. 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 3.[21] The length of coverage is often quite short,

[20]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 without a job more than one year, coverage rates decline throughout the year following job loss.

[21]Details on state laws come from Hewitt (1985) and Thompson Publishing Group (1992) and have been cross-checked against the actual state statutes. See Gruber and Madrian (1993), (1994) for more details. Most state laws stipulate that an employee must have been covered by the employer's insurance for 3–6 months before being eligible for continuation coverage; there is no such restriction for the federal mandate. This restriction should be fairly well approximated by our requirement that individuals have insurance coverage in the wave before separation in order to be in our ''mandate eligible'' sample.

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.

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 separation, we restrict our analysis to those states with laws that apply to both voluntary and involuntary separations.[22]

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% of the *average* employer cost of providing coverage. Although 102% 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

[22]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.

and the reduced potential for adverse selection with large employee groups. Gruber and Madrian (1994) calculate that continuation coverage costs approximately 40% 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 enrolment, and it may be medically underwritten so that particularly unhealthy individuals cannot obtain coverage at any price; non-group policies also typically cover fewer services and have higher copayments and deductibles than do group policies (Gruber and Madrian, 1994). Thus, while continuation coverage is not a subsidy from a government budgetary perspective, it is in effect a major reduction in the cost of insurance to the job leaver. The response to this mandate therefore provides some insight into the response to broader subsidization of the cost of insurance.

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, continuation coverage remains quite expensive. The average cost of one year of continuation coverage (approximately $3600 for family coverage in 1990) is roughly 26% of the contemporaneous family income of unemployed individuals in our SIPP sample, and 17% of family income in the period before job loss.[23]

4.2. Empirical framework

We now turn to estimating the effect of continuation mandates on the insurance coverage of job leavers during their non-employment spells. We begin by selecting the sample of individuals who separated from their jobs during our sample period, and we follow them as they remain non-employed. We then assign to each worker the number of months of continuation coverage available in the calendar month of their separation.[24] 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 (following Klerman,

[23] We have examined the effect of continuation mandates on job leaving and on insurance coverage among prime age males in Gruber and Madrian (1994). Our analysis here differs in several important ways. First, we are focusing here on transitions to non-employment only, whereas our previous analysis studied transitions of any sort (including job-job transitions); one might expect continuation mandates to have quite different effects on job-job transitions and transitions to non-employment. Second, our focus is broader here, as we consider effects on both total non-employment durations and the implications of continuation mandates for search efficiency.

[24] 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.

1991). 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 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 take the subsample of individuals who separate from jobs with health insurance and run a regression of the form:

$$PRIVINS_{it} = \alpha + \beta_1 \cdot MONTHSCOV_{st} + \boldsymbol{X}_i'\delta + \boldsymbol{\pi}_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}, \tag{2}$$

where $MONTHSCOV_{st}$ is the number of months of continuation coverage available in state s at time t. By including fixed state and year effects, the effect of continuation mandates is identified by the variation within states over time in the availability and duration of continuation benefits.[25]

In theory, continuation mandates only affect coverage by employer-provided insurance among separators. We use coverage by any private health insurance coverage as the dependent variable in our regression specification, 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 non-employment, rather than at four month intervals. Second, we are interested in how continuation mandates affect overall private insurance coverage among the jobless, including

[25]The key underlying regressor for this analysis is whether the individual has continuation coverage available at a point in time, not the potential total months available. This would be defined by a dummy variable which equals 1 if months of coverage available under the law is greater than or equal to the spell duration in the given month. However, this regressor is endogenous, since it is a function of unemployment durations which (as we discuss in Section 5) may respond to the presence of a continuation mandate. A natural strategy for dealing with this endogeneity, which we initially pursued, is to use this regression and to instrument by months of coverage available. In fact, the results from doing so are, not surprisingly, almost identical (at the mean) to the results from the reduced form regressions that we show below. We have therefore pursued the reduced form strategy for ease of interpretation.

any potential crowdout of non-employer sources of coverage. Finally, it is not clear whether SIPP respondents label their continuation coverage as employer-provided insurance or as other private (individually-purchased) coverage.

The first column of Table 4 reports the results from estimating Eq. (2). We report only the coefficients on interest in the table, although the regression includes all of the covariates shown in Table 2 (and noted in the footnote to that table). In this regressions, the coefficient on *Months of Coverage*, β_1, is positive but not statistically significant. It indicates that for every month of continuation coverage available to the formerly insured job leavers, insurance coverage among

Table 4
The effect of continuation coverage on private health insurance coverage following job loss

	Separators	Separators	Full sample	Separators
Effect of continuation coverage on insurance coverage after job loss				
Months of coverage	0.0044	0.0001	–	–
	(0.0029)	(0.0025)		
Employer-provided insurance	–	0.1059	–	–
(previous wave)		(0.0169)		
Months of coverage*	–	0.0020	–	–
Employer-provided insurance		(0.0012)		
Effect of job loss on insurance coverage by duration of unemployment spell				
Completed duration of 1 month or		–	−0.2292	–
less * Without a job			(0.0131)	
Completed duration of 2–12	–	–	−0.3670	–
months * Without a job			(0.0079)	
Completed duration of 13+	–	–	−0.4812	–
months * Without a job			(0.0190)	
Effect of continuation coverage on insurance coverage after job loss by duration of unemployment spell				
Completed duration of 1 month or	–	–	–	0.0002
less * Months of coverage *				(0.0027)
Employer HI				
Completed duration of 2–12	–	–	–	0.0006
months * Months of coverage *				(0.0014)
Employer HI				
Completed duration of 13+	–	–	–	0.0078
months * Months of coverage *				(0.0031)
Employer HI				
Sample size	14,510	25,169	201,095	25,169

Standard errors are in parentheses. The dependent variable is private health insurance coverage. In addition to the covariates included in Table 2, regressions also include month, year, industry, occupation, state and panel dummies. The sample in the first, second, and fourth columns is restricted to only those with non-employment spells; the first column is further restricted to those with employer-provided health insurance before unemployment; the third column includes the full sample of individuals. Regression in the third column is weighted to account for oversampling of unemployed.

the unemployed rises by 0.44 percentage points. Thus, a one year continuation of coverage mandate reduces the decline in insurance coverage among job leavers by about 5.3 percentage points, but the estimate is not very precise.

The ‘‘differences-in-differences’’ identification strategy of Eq. (2) 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.[26] 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 shock which leads to an increased incidence of job separations or to reduced insurance coverage among separators, 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 who separated from jobs without employer-provided health insurance. These are individuals who are subject to the same types of state–year specific shocks (or endogeneity), but who cannot take advantage of continuation coverage; any changes in their insurance status represents spurious state/year factors.

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

$$\begin{aligned} PRIVINS_{it} = \alpha + \beta_1 \cdot MONTHSCOV_{st} + \beta_2 \cdot EMPHI_i + \beta_3 \\ \cdot MONTHSCOV_{st}^{*} EMPHI_i \quad \boldsymbol{X}_i' \delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}, \end{aligned} \tag{2'}$$

where $EMPHI._i$ is an indicator for whether individual i had employer-provided insurance on his pre-separation job.[27] In this ‘‘differences-in-differences-in-differences’’ model, the interaction between *MONTHSCOV* and *EMPHI.* measures the

[26]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.

[27]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.

specific effect of continuation mandates on those who had insurance before separating. The *MONTHSCOV* main effect measures the residual effect of continuation mandates on those not insured. That is, the coefficient β_1 measures any 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.

The second column of Table 4 present the results from estimating Eq. (2′). In this case we find a smaller but more precisely estimated effect; the coefficient β_3 indicates that for every month of continuation coverage available, insurance coverage rises by 0.21 percentage points, so that a one year continuation of coverage mandate reduces the drop in insurance coverage among the non-employed by 2.5 percentage points. We can reject the null hypothesis of zero effect at the 10% level. Since this specification better controls other factors that may be correlated with the availability of continuation coverage, and since the estimates in column (2) are more precise, we rely on this ‘‘difference-in-difference-in-difference’’ model for the remainder of the analysis.

To scale these findings for insured job leavers, 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%.

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% 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, 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 loss 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 (marginally) 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% cheaper than individually purchased insurance, and we find that the net increase in insurance from one year of continuation coverage is only 4.2% 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 the low estimates of price elasticity of demand for insurance among small firms in Thorpe et al. (1992) and among individuals in Marquis and Long (1995). On the other hand, it is substantially below the estimate for small firms in Leibowitz and Chernew (1992) and 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 among job leavers.

4.3. Variation with completed non-employment durations

As we noted in Section 3, 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 the long-term non-employed. We can investigate this by extending the regression framework of Eq. (2′) to:

$$\begin{aligned} PRIVINS_{it} = \sum_{d=0}^{D} (\alpha^{d} \cdot COMPDUR_i^d + \beta_1^d \cdot COMPDUR_i^d * MONTHSCOV_{st} \\ + \beta_2^d \cdot COMPDUR_i^d * EMPHI_i + \beta_3^d \\ \cdot COMPDUR_i^d * MONTHSCOV_{st} * EMPHI_i) + \boldsymbol{X}_i' \delta + \pi_s' \cdot State \\ + \tau_t' \cdot Time + \varepsilon_{it}, \end{aligned} \tag{2''}$$

where *COMPDUR* is a series of dummies for completed spell durations of less than one month, 2–12 months, and more than 12 months.[28]

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 4 for the sample that has employer-provided health insurance in the last wave (or before job leaving). Our findings, which mimic those of Fig. 2, are that the effect of

[28] Relative to Fig. 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 two subgroups.

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 Eq. (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 separators 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 separators with non-employment durations of one year or less, continuation availability reduces the shortfall in insurance coverage by only 1–2%. But for those with durations of more than one year, the shortfall is reduced by 19%. The implication of this finding is that continuation mandates, despite small overall impacts, are effectively targeted to the population that experiences the greatest insurance loss after job leaving.

5. 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 job leavers. 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 inducing increased match efficiency or simply increased 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.

5.1. Continuation mandates and the probability of separating

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:[29]

[29] We use a probit model here rather than the linear probability model used in Tables 2 and 4 because the rates of transition to unemployment are low enough (around 5 percent) that the differences between a probit specification and a linear probability model are much more pronounced.

$$Pr[SEPARATE]_{it} = \Phi(\alpha + \beta_1 \cdot MONTHSCOV_{st} + \beta_2 \cdot EMPHI_i + \beta_3 \cdot MONTHSCOV_{st}{*}EMPHI_i + \boldsymbol{X}_i'\delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}) \quad (3)$$

where *Pr[SEPARATE]* is a dummy equal to one if an individual transits from employment to non-employment during the wave and *MONTHSCOV* is the months of continuation coverage 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; the marginal probabilities from the probit coefficients are presented in square brackets.[30] There is a highly significant effect of continuation mandates on the probability of separation 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% 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.

5.2. 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 the effects of continuation mandates on unemployment durations 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.

[30] The marginal effects in columns 1 and 2 of Table 5 are calculated as the average difference over the entire sample in the predicted transition probabilities (column 1) or predicted weeks unemployed (column 2) under the following scenarios: for dummy variables, 0 vs. 1; for age and family size, actual value vs. actual value plus one; for months of continuation coverage, 0 vs. 12 months; for real wages, actual value vs. actual value plus 10%.

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)	[−0.0369]	−0.8176 (0.0149)	[−1.5044]
Months of coverage	0.0010 (0.0036)	[0.0009]	0.0099 (0.0026)	[0.1722]
Employer-provided insurance* Months of coverage	0.0058 (0.0019)	[0.0055]	0.0119 (0.0013)	[0.2125]
Age	−0.0156 (0.0089)	[−0.0012]	−0.0422 (0.0059)	[−0.0609]
$Age^2/1000$	0.0001 (0.0001)	[0.0127]	0.0006 (0.0001)	[0.7653]
White	−0.0691 (0.0222)	[−0.0057]	−0.3301 (0.0129)	[−0.5462]
Married	−0.1513 (0.0187)	[−0.0128]	−0.4984 (0.0121)	[−0.8184]
Number of children	−0.0102 (0.0072)	[−0.0008]	−0.0722 (0.0051)	[−0.1028]
High school graduate	−0.0699 (0.0209)	[−0.0055]	−0.2715 (0.0128)	(−0.3925)
Some College	−0.0818 (0.0237)	[−0.0063]	−0.5159 (0.0151)	[−0.6779]
College graduate	−0.1274 (0.0286)	[−0.0096]	−0.7700 (0.0170)	[−0.9343]
Log real wage, (previous wave)	−0.1924 (0.0171)	[−0.0012]	−0.4467 (0.0123)	[−0.0615]
Sample size	113 173		29 081	

Standanl errors are in parentheses; marginal effects are in square brackets. 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 regression 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 also includes month, industry and occupation dummies.

We therefore take a more aggregate approach and look at the effect of continuation mandates on total non-employment during a one year period.[31] 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

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

are of the same form as Eq. (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.[32]

One problem with this regression is that our dependent variable, weeks of non-employment, is censored both at zero (~90% of our sample experiences no non-employment during a one-year period) and at 52. Furthermore, the data are discretely, not continuously, distributed between these endpoints. To deal with this data structure, we estimate a Poisson regression model.[33]

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 non-employed during the year. The estimates imply that a one year continuation mandate raises the average number of weeks spent unemployed by 15%. Because individuals in our sample spend only 1.5 weeks non-employed 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 non-employed (15%) and the transition rate from employment to non-employment (14%). 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 non-employed results from an increased number of separations and not from increased non-employment 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 non-employment durations, our findings suggest that continuation mandates have little effect on the duration of non-employment spells.

[32] 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 subsequent 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 don't believe that this is 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%, suggesting that the bias to our findings is small, since only a small share of this group will then experience another job separation (note also that only 5% 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.

[33] An alternative would be to estimate a two-limit Tobit model. However, doing so would amount to fitting a normal distribution from the 10 percent of individuals who are not censored at either 0 or 52 weeks of unemployment. We feel that this approach asks too much of the data at hand.

5.3. 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 low cost 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 inducing 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.[34]

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.[35] 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 separation, as well as earnings in the month of job separation, 1–3 months after job separation, 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.[36] To capture the net effect of both increased non-employment durations and (potentially) increased earnings upon reemployment, we do not

[34]There is some dispute about the magnitude of job-lock; see Holtz-Eakin (1994) or Penrod (1993) for dissenting views.

[35]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.

[36]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. We have estimated models which do not impose this restriction, or which restrict the sample at a shorter duration (we cannot consider a longer duration because the 1988 panel of the SIPP is relatively short). Either change weakens the effects estimated at 8–11 months after separation, which remains positive but is no longer significant.

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:

$$EARN_{i,\,t+k} = \alpha + \beta_1 \cdot MONTHSCOV_{st} + \beta_2 \cdot EMPHI_i + \beta_3 \cdot MONTHSCOV_{st}^{*}EMPHI_i + \beta_4 \cdot EPRESEP_i + \boldsymbol{X}_i'\delta + \pi_s' \cdot State + \tau_t' \cdot Time + \varepsilon_{it}. \quad (4)$$

In this model, the individual leaves his job in period t. We model future wages (*EARN* 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. These equations are estimated by OLS. Note that our conclusions are substantively the same whether we use earnings measured in logs or levels; the regressions we present use earnings in levels because we find these easier to interpret.

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-separation earnings (*EPRESEP*).[37] 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 effect on our estimates.

In order to scale the effects of continuation mandates on reemployment earnings, we must first measure the baseline earnings cost of separation. To do so, we first combine our subsample of separators with those individuals who do not separate. 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 and do not experience any non-employment, we then run a regression of earnings in each of these future periods on individual demographic characteristics and a separation dummy (as done in Eq. (1) for insurance coverage). The coefficient on the separation dummy yields the earnings

[37]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 Eq. (4).

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.[38] 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% below the level of those who did not separate.

Table 6
The effect of job loss on earnings

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

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 (sample size varies from 44 118 for current month regression to 44 849 for 12–15 months after). 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 eamings in the previous four months.

[38]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 become reemployed (which we define as having positive earnings). For this sample, there are smaller effects of separation, although earnings are still 18% lower 12–15 months after separation.[39] 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)).[40]

In Table 7, we report only the coefficient of interest (β_3) from our estimates of Eq. (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 separators, one year of continuation coverage is associated with a 6.1% increase in earnings over the 15 months after job leaving; this is over 17% 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% increase

[39] 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 in each of the finer subperiods. This same phenomenon occurs in Table 7: the total impact over the entire 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.

[40] 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% 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.

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	6.11%	118.83	6.92%	−1.341	−0.06%
	(53.47)	[17.3%]	(53.19)	[21.8%]	(13.24)	
Current month	2.328	1.63%	2.074	1.45	−0.530	−0.29%
	(6.317)	[2.95%]	(10.66)	[5.13]	(1.452)	
1–3 months after	0.182	0.04%	−9.945	−2.32	−2.234	−0.40%
	(17.15)	[0.08%]	(23.50)	[−7.90%]	(4.047)	
4–7 months after	−12.54	−2.19%	−13.93	−2.43%	1.706	0.23%
	(23.45)	[−7.04%]	(24.48)	[−14.1%]	(5.490)	
8–11 months after	46.20	8.07%	46.46	8.11%	1.265	O.17%
	(23.57)	[31.5%]	(24.70)	[54.3%]	(5.609)	
12–15 months after	56.35	9.84%	61.18	10.7%	0.158	0.02%
	(22.50)	[34.8%]	(22.51)	[58.6%]	(5.810)	

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 dividuals 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 (sample size ranges from 787 for the current month to 1489 for 12–15 months after). 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 IS months after the current month (43,638 individuals). 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.

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% 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 the earnings of those who are reemployed by almost 22% 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% 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. Indeed, if we normalize the overall effect (an earnings increase of 6.1%) by the increase in insurance coverage among separators due to the mandates (2.4 percentage points from a year of coverage), it implies an earnings increase of 254% for those who take-up! But this figure is too large for two reasons. First, as we discuss above, we are unable to measure takeup of continuation coverage; Table 4 documents instead the net change in insurance coverage among job separators. Some of these persons will have moved from much more expensive individual coverage to less expensive continuation coverage. This will affect their search behavior, since they will now be able to search longer at the same total insurance cost, but it will not be measured as a net increase in insurance coverage, so that the calculation above overstates the net wage gain among those who are affected by the mandates. Second, this large figure ignores the important heterogeneity in takeup behavior that was documented earlier. In Table 4, we showed that takeup was much higher among those with longer completed spell durations. And in Table 7, we find that the earnings effects are largest after one year, which is when these individuals are ending their spells (and presumably finding better jobs because of COBRA-subsidized search). Normalizing the earnings effect 12–15 months after separation (9.84%) by the effect of one year of continuation coverage on those with completed durations of more than a year (9.36 percentage points), we obtain an earnings increase per covered individual of 105%. This is still a quite large, but somewhat more reasonable, estimate.[41]

The very large magnitudes of these earnings effects raise the possibility that they are spurious, despite our detailed empirical framework. In order to assess the sensitivity of these findings, we have therefore 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 estimated 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

[41]It is striking that there is an effect on earnings 8–11 months after separation, when the only significant effects of continuation mandates appear after one year in Table 4. But if we reestimate the models in Table 4 allowing for a separate coefficient for 8–11 months, there is a sizeable, but imprecisely estimated, effect, which is roughly of the same size as the overall estimate (2.4 percentage points per year of continuation coverage); we cannot reject the null hypothesis that the impact on those non-employed 8–11 months is the same as for those non-employed more than one year.

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 types 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 Eq. (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 Table 7. We see that, in fact, there is no effect on this control group; the largest positive coefficient is only 0.23% 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.

6. Conclusions

While there is substantial concern in the public policy community about the low levels of insurance coverage among those leaving their jobs, 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 separation per se on the insurance coverage of job leavers, and the effects of lowering the cost of health insurance for the non-employed on the extent of insurance coverage among job leavers, on their non-employment behavior, and on their reemployment earnings.

To summarize, we have four findings of interest. First, job leaving precipitates a dramatic reduction in insurance coverage for the unemployed, even after conditioning on underlying tastes for insurance. 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 useful 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 upon job loss.

To the extent that continuation mandates decrease the cost of being non-employed by increasing access to health insurance, they may both encourage more people to separate and lengthen the duration of non-employment spells. We find that continuation mandates do indeed appear to increase the incidence of joblessness and the amount of time spent unemployed. Whether or not this is inefficient depends on whether this increased joblessness is associated with 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. This could result both from more productive (i.e. longer) job search among those that separate and from formerly ''job-locked'' individuals leaving their jobs in order to search for higher paying jobs. The very large magnitude of these results, however, suggests that further work is needed before one can conclude that insurance coverage availability increases job match efficiency.

These results have important policy implications. That individuals experience such a dramatic loss in health insurance coverage upon leaving their jobs, even after conditioning on factors that proxy for health insurance demand, suggests that there may be market imperfections in the provision of health insurance for job leavers. Thus, there may be a role for government in correcting these market deficiencies, either through outright provision of health insurance to those that are non-employed, or through some sort of government subsidy for the purchase of health insurance. In this regard, it is interesting to note that the health insurance coverage of both the employed and, to some extent the self-employed, receives favorable tax treatment, while insurance purchases of the non-employed, either through continuation mandates or in the private market, receive no favorable tax treatment unless they exceed 7.5% of income and an individual itemizes. At a minimum, therefore, levelling the playing field between insurance on and off the job would require an equalization of the tax treatment of insurance across these different modes of purchase.

While any such measures would likely increase the fraction of the non-employed with health insurance, they would have both direct budgetary costs and indirect costs through their distortions of behavior. In particular, they will induce more people to separate and may also lengthen non-employment durations. Either of these responses will increase the costs of a government program to make health insurance more accessible to job leavers; they may also increase the costs of other programs for the unemployed, such as expenditures on unemployment insurance. These costs, however, must be weighed against the benefits that accrue to individuals who can use their time spent non-employed to find more productive jobs with higher wages. Our estimates suggest that these gains could be quite sizeable. Further work to assess the magnitude of these budgetary costs and job match benefits should be a high priority.

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