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THE IMPACT OF HEALTH ON JOB MOBILITY: A MEASURE OF JOB LOCK

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The author analyzes data from the National Medical Expenditure Survey of 1987 to measure the importance of "job lock"—the reduction in job mobility due to the non-portability of employer-provided health insurance. Refining the approach commonly used by other researchers investigating the same question, the author finds insignificant estimates of job lock; moreover, the confidence intervals of these estimates exclude large levels of job lock. A replication of an influential previous study that used the same data source shows large and significant job lock, as did that study, but when methodological problems are corrected and improved data are used to construct the job lock variables, job lock is found to be small and statistically insignificant.

The majority of Americans under the age of 65 are covered by employer-provided health insurance. A result of the link between employment and health insurance is that insurance is not portable across jobs. The reduction in job mobility due to the non-portability of employer-provided health insurance is called "job lock." A source of job lock that has attracted the attention of legislators is "pre-existing" health condition exclusions. Since it is common for insurance policies to exclude

pre-existing health conditions, a worker with a personal or family health problem would find it difficult to get an alternative health insurance plan with complete coverage if he quit his job. Workers may experience job lock even if they are not subject to pre-existing condition exclusions on their prospective jobs. A job change may entail lack of insurance during unemployed job search or a waiting period for coverage on a new job. Workers may also have idiosyncratic preferences for their current health plan; for example, they may want to remain enrolled in a particular health maintenance organization, and therefore may be locked into their jobs.

The 1985 Consolidated Omnibus Budget Reconciliation Act (COBRA) aimed to

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The data and computer programs for this study, and an appendix of additional results, are available on request to the author.

reduce temporary portability problems by requiring employers to provide employees with the option to continue coverage for up to 18 months after the termination of employment. In 1996, health insurance portability legislation was passed with overwhelming support in both the House and the Senate. The core provision of both the House and the Senate versions of the bill limits the length of time for which pre-existing health condition clauses can restrict health care coverage, and hence reduces job lock due to pre-existing health conditions. To evaluate the effect of such portability legislation on job mobility, it is important to estimate the magnitude of job lock.

The economics literature has not reached a consensus on the severity of job lock. Studies using the National Medical Expenditure Survey of 1987 (NMES) have found that job lock is large and significant (Madrian 1994; Cooper and Monheit 1993). Other evidence from the Panel Study of Income Dynamics (Holtz-Eakin 1994) and the Survey of Income and Program Participation (Penrod 1994; Buchmueller and Valletta 1996) have indicated that job lock is insignificant for married men. However, these estimates are imprecise; hence, they are unable to reject the possibility of large levels of job lock as suggested by the studies using the NMES.

This conflicting evidence on job lock provided by papers using different data sources and empirical methodologies is puzzling for both researchers and policy makers. In this study I use the NMES, which has yielded precise estimates of job lock, to answer the question, "Is the conflicting evidence on job lock for married men a result of differences in the data or in the methodology?"

Workers who are sick or have sick family members should be more prone to job lock than other workers, since they are likely to face pre-existing condition exclusions if they change jobs. In addition, they are more likely to find burdensome such factors as waiting periods for coverage on a new job and lack of insurance during job search. I use previously unexploited, de-

tailed data on medical conditions, health utilization, and medical expenses to proxy for sickness. I refine the difference-in-difference (D-D) methodology used in the literature by using these proxies for sickness in conjunction with comparable control and experimental groups. I then reconcile my findings with those in Madrian (1994), an influential study of NMES data that found job lock to be severe.

Literature Review

Several recent papers study job lock. To identify potentially job-locked groups in her sample of married men, Madrian (1994) used data from the NMES to estimate a reduced form probit model with variables such as pregnancy of spouse, holding of alternative health insurance policies (policies other than employer-provided ones), and family size. She then used a quasi-experimental D-D estimator to compare the mobility of the potentially job-locked groups to the mobility of groups that did not suffer from job lock. She estimated that job lock reduced voluntary turnover by 4 percentage points, from 16% to 12%.

Cooper and Monheit (1993), also using the NMES, estimated a wage offer equation and an insurance offer equation for individuals who changed jobs. They used the estimates to construct a predicted wage and insurance offer for all individuals, and hence a predicted wage gain/loss and insurance gain/loss for the entire sample. They estimated a job change probit equation and found that the possibility of losing insurance had a significant negative impact on the probability of changing jobs, a finding they interpreted as evidence of job lock.

Holtz-Eakin (1994) used data from the Panel Study of Income Dynamics (PSID) to estimate several job lock D-D equations. His empirical work for married individuals suggested that access to a spouse's employer-provided health insurance policy had similar effects on the job mobility of individuals who had employer-provided health insurance through their own employment and those who did not. In addition, the interac-

tion of poor health with holding both own and spousal health insurance had an insignificant effect on the job mobility of married individuals. Holtz-Eakin concluded that married individuals do not experience job lock.

Buchmueller and Valletta (1996) used the Survey of Income and Program Participation (SIPP) to replicate the D-D estimator developed by Madrian (1994) and Holtz-Eakin (1994).¹ They refined this estimator by including controls for tenure and pensions, and by estimating a model of joint mobility of dual-earner couples. For married men, both with and without the refined estimator, they found that the mobility effect of access to a spouse's plan did not significantly differ between holders and non-holders of employer-provided health insurance plans.

Penrod (1994), also using SIPP data, estimated several D-D estimators using self-reported health measures and an aggregated health index as proxies for the demand for health insurance. In general, he found insignificant job lock. While Penrod's estimates do not suffer from non-comparability between the control group and the experimental group, his estimates are imprecise, and he was unable to reject large levels of job lock.

In related work, Gruber and Madrian (1994) found that continuation of coverage mandates, aimed at increasing the portability of health insurance, increased the mobility of individuals with health insurance. However, this result was significant only at the 10% level. Holtz-Eakin, Penrod, and Rosen (1996) found no evidence that lack of health insurance portability deterred moves into self-employment.

This brief survey of the literature shows that there is no consensus on the severity of job lock. In addition, some potential prob-

lems exist with each of these studies. First, the direct effect of holding employer-provided health insurance on the probability of changing jobs is not a good measure of job lock, since health insurance is a part of compensation and also is correlated with other forms of compensation. The fact that a worker with a good compensation package, possibly due to high match-specific productivity, is less likely than other workers to change jobs is not evidence of job lock, since even if insurance were portable, a worker with a good match and high compensation would be less likely to quit. Cooper and Monheit's (1993) significant job lock estimate using the NMES suffers from this problem.

Second, if job lock is identified by the effect of family sickness on job mobility, it is useful to have good exogenous health measures. Holtz-Eakin (1994) used self-reports of bad health of the male head of the household to measure job lock for married men. He did not use family health to identify job lock. Madrian (1994) used variables such as family size to proxy for expected medical expenses. Family size is only weakly correlated (the correlation coefficient is 0.1) with the number of chronic health conditions in the family (author's calculations). Penrod (1994) had limited data on medical utilization and child health, and no data on medical conditions and expenditures. If the correlation between sickness and the proxy for sickness is weak, it is probable that the estimate of job lock is capturing some effect other than job lock.

Third, as noted by Meyer (1995), the results from estimation using a quasi-experimental D-D technique may be biased if the control group and experimental group are not comparable. Differences in the mean of the dependent variable indicate a lack of comparability. In most of the empirical work using D-D estimators to measure job lock (Madrian 1994; Holtz-Eakin 1994; Buchmueller and Valletta 1996; Gruber and Madrian 1994), the control group consists of persons without employer-provided health insurance and the experimental group consists of persons with employer-provided health insurance. The dif-

¹A problem with the SIPP data is the inability to distinguish voluntary from involuntary job turnover. Since job lock applies only to voluntary turnover, the inclusion of involuntary job changes in the data could bias the estimate of job lock.

Table 1. Means in National Medical Expenditure Survey Data.

<i>Sample</i>	<i>Holders of Employer Health Insurance</i>	<i>Non-Holders of Employer Health Insurance</i>	<i>Employer Health Ins. and Spouse Insurance</i>	<i>Employer Health Ins., No Spouse Health Insurance</i>	<i>Full Sample</i>
Age	38.71	36.03	38.36	38.85	38.10
Education	13.26	11.84	13.36	13.23	12.94
Black	0.15	0.16	0.22	0.11	0.15
Family Size	3.52	3.55	3.21	3.67	3.53
Wage	12.38	8.81	11.65	12.71	11.56
Union	0.25	0.19	0.22	0.26	0.24
Experience	19.06	17.50	18.63	19.23	18.70
Tenure	9.37	4.83	9.01	9.54	8.33
Job Change in 1987	0.09	0.29	0.11	0.08	0.14
Health Insurance on Job	1.00	0.00	1.00	1.00	0.77
Spouse Health Insurance	0.30	0.39	1.00	0.00	0.32
Medical Conditions	1.03	0.77	0.92	1.08	0.97
Nights in Hospital	1.30	0.89	1.42	1.26	1.21
Emergency Room Visits	0.65	0.77	0.58	0.68	0.68
Medical Equipment	0.22	0.18	0.18	0.24	0.21
Prescribed Medicines	9.85	7.94	8.49	10.47	9.42
Medical Visits	12.01	9.82	10.54	12.68	11.51
Medical Expenses	2,884.70	2,208.43	3,204.77	2,757.79	2,730.69
N (number of observations)	2,255	665	672	1,577	2,920

Note: All health variables are defined over the entire family.

ference in voluntary mobility rates between these groups is substantial—29% for those without employer-provided health insurance and 9% for those with it (see columns 1 and 2 of Table 1). These two groups seem to be different along other dimensions as well. Those holding employer-provided health insurance are older and more educated, and they have higher wages (Table 1). These data show that earlier studies estimating job lock using holders and non-holders of employer-provided health insurance have used control and experimental groups that are very different. Hence, there is a concern that the estimates of job lock in the literature are not consistent.

In this paper, I develop a consistent estimate of job lock using the NMES. I contribute to the literature in three main ways. First, I create good measures of family sickness to identify job lock. Second, I use more comparable control and experimental groups for the D-D estimator of job lock. Third, I reconcile my results of little to no

job lock with Madrian's influential study, which finds large and significant job lock.

Data: 1987 National Medical Expenditure Survey

The data for this study are from the 1987 National Medical Expenditure Survey (NMES) conducted by the Agency for Health Care Policy and Research. The survey has detailed information, from approximately 14,000 households, on health insurance, medical conditions, medical care utilization, and medical care expenditure in 1987. These data were collected from the households in a series of four interviews over the year. The medical data were cross-checked by interviews with medical providers. Information regarding demographic characteristics, family relationships, income, and employment were also gathered.

For this study, the data set is restricted to married men who are employed at the time of the first interview in 1987 and are be-

Figure 1. Probability of Moving
by Sickness and Group Status.

Probability of Moving	Control Group	Experimental Group
Not Sick (Sick = 0)	α_{11}	α_{12}
Sick (Sick = 1)	α_{21}	α_{22}

tween the ages of 20 and 55. Married men are most likely to hold the family health insurance policy (Schur and Taylor 1991), and therefore to be job-locked. Hence, I focus primarily on this group, while briefly noting the results for other groups. Individuals who are self-employed, full-time students, in the military, or residing abroad are excluded from the sample. Individuals who are laid off during the year are also excluded, since the focus is on voluntary mobility. Descriptive statistics for the sample of 2,920 observations are in Table 1.

Estimating Job Lock Using Family Sickness Measures

The presence of job lock should be apparent if we carefully compare the mobility of a relatively job-locked experimental group with the mobility of a control group that is not job-locked. There is considerable flexibility in the choice of both groups and in the variable measuring the cause of job lock. Job lock should be greater for individuals who are sick or have sick family members, since they are likely to face pre-existing condition exclusions on a new health insurance policy. Another reason individuals in that group may be less likely than average to change jobs is that they would find restrictions such as waiting periods for coverage on their new jobs to be expensive. Hence, I use family sickness to identify job lock. The identification of job lock should be based on good proxies for family sickness and a job-locked experimental group and non-job-locked control group that are comparable.

The comparability of the control group and experimental group is important for three main reasons. First, the D-D estima-

tor relies on the similarity of the control and experimental groups in order to separate the effect of interest from other exogenous influences. The key assumption, which is likely to hold only if the groups are comparable, is that the effect of these exogenous influences is the same on the control and experimental groups. Second, the more different the control and experimental groups are, the higher is the likelihood of bias from hidden interactions. For example, an estimate of job lock that uses the uninsured as a control group for the insured may be biased if individuals with sick families who hold employer-provided health insurance (and hence good jobs) are less likely to leave their jobs because of benefits such as sick leave. Third, since the true model equation is unknown, there is a risk of misspecifying the form of the estimating equation. For example, if the true equation is in levels, a transformation to a non-linear (probit) form will result in a misspecification bias. This bias may be exacerbated if the control group and the experimental group are dissimilar in the mean or the distribution of the dependent variable (Meyer 1995).

I provide a simple calculation of the job lock D-D estimator to illustrate the importance of comparability. Consider the 2x2 matrix shown in Figure 1. In this matrix, the probability of moving is α_{11} for the individual in the control group who is not sick, α_{21} for one who is sick, and so on. For simplicity, I assume a linear probability model:

$$(1) \quad \begin{aligned} \text{Pr(ChangeJob)} = & \beta_0 + \beta_1 \cdot \\ & \text{Experimental Group} + \beta_2 \cdot \text{Sick} + \beta_3 \cdot \\ & \text{Sick} * \text{Experimental Group} + \varepsilon \end{aligned}$$

The parameter vector β is calculated by applying the formula for the ordinary least square estimator to the four data points provided in the 2x2 matrix:

$$\begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_3 \end{bmatrix} = \begin{bmatrix} \alpha_{11} \\ \alpha_{12} - \alpha_{11} \\ \alpha_{21} - \alpha_{11} \\ (\alpha_{22} - \alpha_{12}) - (\alpha_{21} - \alpha_{11}) \end{bmatrix}$$

The effect of sickness on the potentially

job-locked experimental group is a combination of the "direct" effect of sickness on mobility (for example, sickness may reduce job search and, hence, the probability of changing jobs) and job lock. The estimate of job lock, β_3 , is calculated by subtracting the effect of sickness on mobility for the non-job-locked control group from the effect of sickness on mobility for the job-locked experimental group. Hence, to calculate the job lock component, the direct effect of sickness on the experimental group is assumed to equal the total effect of sickness on the control group. This assumption is most likely to hold if the two groups have similar demographic characteristics, job characteristics, and mobility behavior. In most of the earlier work estimating job lock, the insured form the experimental group and the uninsured form the control group. As discussed above, the uninsured differ from the insured in terms of base levels of mobility, demographic characteristics, and job characteristics; hence it is unlikely that the direct effect of sickness on the insured group is equal to the total effect of sickness on the uninsured group. The use of these groups to estimate job lock may yield inconsistent estimates.

To estimate job lock, it is essential to find two groups that are comparable, one of which can be hypothesized to be more severely affected by job lock than the other. For this reason, I turn to spousal health insurance. Individuals who have access to spousal health insurance (that is, individuals with a spouse who holds employer-provided health insurance) in addition to their own employment-related health insurance are possibly already covered by their spouse's policy or may succeed in getting on a spouse's policy with loose rules, even if they suffer from some pre-existing conditions (Cotton 1991). Data from the May 1988 Current Population Survey show that 80% of women who hold employer-provided health insurance cover others in their families. Madrian (1994) found that the availability of alternative health insurance (primarily spousal health insurance) in addition to own employer-provided health

insurance reduced job lock.² Hence, individuals who only have employer-provided health insurance should be more likely to be job-locked than those who have their own employer-provided health insurance and spousal health insurance in addition.³

While individuals with no spousal insurance are relatively more job-locked than individuals with spousal insurance, it cannot be asserted that those with spousal insurance are not job-locked. Hence, this measure of job lock provides an estimate of the incremental effect of spousal insurance on job mobility. The advantage of using these two groups for the analysis is that they are more comparable in age, education, wage, and mobility than the insured and the uninsured (see columns 3 and 4 of Table 1).

Family Sickness Measures

To estimate job lock using family sickness, I construct three sickness measures. The first, medical conditions, is the number of chronic medical conditions in the family (see Manning et al. 1982 for a similar measure). Based on the National Center for Health Statistics' (1993) list of chronic conditions, I define a total of 59 medical conditions as chronic.⁴ Examples of these conditions are asthma, cancer, cardiac con-

²Spousal health insurance mitigates job lock only if the husband is considering a move within the same geographical area. Since, however, annual geographical mobility rates from the Current Population Survey show that 65% of moves are within-county, it seems likely that access to spousal health insurance will, in general, reduce job lock.

³After completing the estimation in this paper, I became aware of Penrod's (1994) empirical strategy for estimating job lock that exploits this fact as well. His estimates, using the SIPP, do not suffer from non-comparability of the control and experimental groups; hence, they serve as a useful comparison for the estimates in this paper.

⁴I exclude the following NCHS chronic conditions from my measure: (1) a condition that is not experienced by anyone in my sample, and (2) refractive optical errors such as myopia, since they are not covered by standard health insurance policies.

ditions, cerebrovascular disease, diabetes, hypertension, and personality disorders.

A pre-existing condition is generally defined as any medical problem that has been treated or diagnosed within the past six months to two years.⁵ The medical conditions that I use to construct the sickness measure are chronic; hence, they are extremely likely to be pre-existing health conditions. While I do not observe pre-existing condition exclusions on job offers, the medical conditions measure should be a good proxy for pre-existing condition exclusions. Furthermore, an individual with a high value of the medical condition measure is likely to have a high valuation of his current health insurance plan due to factors other than pre-existing condition exclusions, such as a preference for a physician or a waiting period for coverage on the new job. Hence, the medical condition measure should be useful for identifying job lock.

The second measure is a medical conditions index. Job lock is greater if the medical condition that is covered under the current health insurance policy, but may not be covered under a new policy or during unemployment, is expensive. The variable medical conditions implicitly assigns an equal weight to every condition. I develop an alternative measure by weighting each medical condition by its relative potential expense. To do so, I regress the logarithm of family medical expenses on a list of chronic medical conditions and other controls. I use the regression estimates to calculate the logarithm of medical expenses predicted from chronic medical conditions. This measure, which is described further in the appendix, is the medical conditions index.

The third health measure is a health utilization index. Medical conditions and the medical condition index are based on

dummy variables for the presence of medical conditions. Hence, they are possibly less precise and informative measures of family sickness than a health utilization-based variable. Therefore, measures of family utilization of health resources over 1987 are constructed for emergency room visits, nights spent in hospital, visits to medical practitioners, purchases of medical equipment, and prescribed medicines. The construction of the health utilization index, which is described in the appendix, is similar to that of the medical condition index. The health utilization index is strongly correlated with medical conditions and the medical conditions index (correlation coefficients of 0.5).

There may be a concern that the health utilization index and, to a lesser extent, the medical condition-based measures depend on health insurance status. For example, individuals who hold health insurance may be more likely than others to discover that they have a medical condition and utilize health resources. Since I estimate job lock on a sample of insured men, this is not a problem. It could be, however, that the sickness measures are endogenous to policy quality. This is unlikely to be a problem, because there does not seem to be a significant relationship between policy quality and medical conditions (Cameron and Trivedi 1991). Note that job lock estimates based on utilization may be biased toward finding job lock if individuals with no spousal health insurance who change jobs are less likely to utilize medical resources after their job change because of pre-existing condition exclusions on their new policies and the lack of alternative coverage. In the appendix, I discuss several specification checks for these measures.

Estimation

Empirically, the following probit equation is estimated on the sample of men who hold employer-provided health insurance:

$$(2) \quad \text{Pr}(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{NoSpouseHealthIns} + \beta_2 \cdot \text{Sick} + \beta_3 \cdot \text{Sick} * \text{NoSpouseHealthIns} + Z'\gamma)$$

⁵Pre-existing condition exclusions are relatively common: 57% of plans in establishments with more than 100 employees (Bureau of Labor Statistics 1994) and 64% of plans in firms with fewer than 50 employees have such exclusions (Morrisey, Jensen, and Morlock 1994).

The dependent variable equals one if the worker voluntarily changes jobs or moves to non-employment during the survey year. If job lock exists, β_3 should be negative and significant, since men with sick families who do not have access to spousal health insurance are less likely to change jobs. Three probit equations are reported, each with a different measure of sickness.⁶ The vector Z contains the following control variables: the logarithm of the wage on the job held in the beginning of 1987, schooling, experience, square of experience, tenure, union status, black, family size, the logarithm of family income, four region dummies, four occupation dummies, and four industry dummies. The industry and occupation dummies are proxies for the turnover rate the individual faces regardless of health. The variable *experience* is defined as potential experience (age – education – 6) minus the number of years spent out of the labor force after the age of 21.

A possible problem with this estimation strategy is that unlike the experimental group (those with no spousal health insurance), the control group (those with spousal health insurance) consists solely of dual-earner families. This non-comparability between the experimental and control groups could lead to inconsistent estimates. For example, if the wife does not hold a job, she is better able to take care of a sick family member, freeing the male job holder to search on the job. To circumvent this problem, I re-estimate the above equation after restricting the sample to men whose spouses are employed at the time of the first interview.

In Panel A of Table 2, I present the results for the estimation in which the sample is restricted to those who hold employer-provided health insurance. The coefficients on wage, experience, union

status, tenure, and no spousal health insurance are negative.⁷ The direct effect of sickness is statistically insignificant and varies in sign over the three measures. The estimate of job lock, the coefficient on *NoSpouseHealthIns * Sick*, should be negative and significant if individuals without access to spousal health insurance are less likely to change jobs (more likely to be job-locked) than the rest of the sample. However, the results show no support for the existence of job lock. All the interaction coefficients are insignificant and have the wrong sign.⁸ Focusing on the medical conditions index, the marginal effect of an increase in the interaction term from the median to the 75th percentile, reported in the last row, is 0.9 percentage points, which is the wrong sign for job lock. Furthermore, this estimate is quite precise, since the 90% confidence interval shows that job lock cannot account for more than a 0.8 percentage point fall in job mobility.⁹ The

⁷No spousal health insurance could be a proxy for family income, wealth, and the presence of alternative income sources; therefore, the effect of no spousal health insurance on quits should not be interpreted as a measure of job lock.

⁸I perform four specification checks. First, since tenure may be endogenous in a quit equation, the equations are re-estimated excluding tenure. Second, the health of the male head of the family can have a large impact on his job mobility. Hence, the probit equations are re-estimated using family sickness measures that are computed excluding the health status of the male head. Third, since the distributions of the sickness variables are skewed, I re-specify (1) the medical conditions variable as a dummy variable and (2) the medical conditions index and the health utilization index in levels and as polynomials. Fourth, to reduce the possible endogeneity of medical conditions with respect to policy quality and job change in 1987, I re-specify the medical condition-based measures to include only those conditions that are discovered before 1987. These specification checks produce no qualitative change in the results: the coefficients on job lock remain statistically insignificant and continue to carry the wrong sign.

⁹The confidence interval on an alternative measure of the marginal effect of job lock, calculated as an increase in the interaction term from the median to the 90th percentile, shows that job lock cannot account for more than a 2 percentage point reduction in job mobility.

⁶Since the family sickness indices used as explanatory variables in the probit equation are estimated variables, the standard errors of the probit equation are biased (Pagan 1984). Hence, I use a bootstrap procedure to estimate the standard errors for these equations.

Table 2. Estimation of Job Lock Using Family Health Measures as the Job Lock Variables.

Explanatory Variable	Panel A: Insurance Holders			Panel B: Full Sample		
	Medical Conditions	Medical Conditions Index	Health Utilization Index	Medical Conditions	Medical Conditions Index	Health Utilization Index
Wage (log)	-0.107 (0.080)	-0.108 (0.080)	-0.108 (0.080)	-0.175*** (0.067)	-0.174*** (0.067)	-0.176*** (0.067)
NoSpouseHealthIns	-0.164 (0.108)	-0.191 (0.106)	-0.163 (0.112)			
Sick ^a	-0.047 (0.063)	-0.133 (0.144)	0.005 (0.086)	0.035 (0.050)	0.120 (0.125)	-0.027 (0.058)
Sick*NoSpouseHealthIns^a	0.031 (0.073)	0.139 (0.159)	0.027 (0.087)			
Health Insurance				-0.445*** (0.087)	-0.441*** (0.085)	-0.533*** (0.090)
Sick*HealthIns^a				-0.054 (0.059)	-0.134 (0.143)	0.051 (0.068)
Schooling	0.011 (0.018)	0.011 (0.018)	0.010 (0.018)	0.003 (0.014)	0.003 (0.014)	0.003 (0.014)
Experience	-0.056*** (0.019)	-0.057*** (0.019)	-0.056*** (0.019)	-0.052*** (0.015)	-0.053*** (0.015)	-0.053*** (0.015)
(Experience) ²	0.001*** (0.0004)	0.002*** (0.0005)	0.001*** (0.0005)	0.001*** (0.0003)	0.001*** (0.0004)	0.001*** (0.0003)
Tenure	-0.043*** (0.007)	-0.043*** (0.007)	-0.043*** (0.007)	-0.040*** (0.006)	-0.040*** (0.006)	-0.040*** (0.006)
Black	0.080 (0.113)	0.081 (0.113)	0.089 (0.112)	0.104 (0.090)	0.107 (0.090)	0.107 (0.090)
Union	-0.134 (0.109)	-0.132 (0.109)	-0.136 (0.109)	-0.181** (0.092)	-0.183** (0.093)	-0.178** (0.092)
Marginal Effect of Job Lock on Mobility ^b	0.004 (0.009)	0.009 (0.010)	0.002 (0.006)	-0.008 (0.009)	-0.011 (0.011)	0.005 (0.006)

Notes: The coefficients on industry, occupation, region, family income, family size, and other controls are not reported here. The number of observations is 2,249 in Panel A and 2,920 in Panel B.

^aThe measure of sickness used in each specification is named in the column heading.

^bThis is calculated as the effect of increasing the interaction term from the median of the distribution to the 75th percentile on the probability of changing jobs.

Coefficient is significant at the 5% level; *at the 1% level.

mean job mobility is 9%. The additional estimation that further enhances comparability by restricting the sample to men with working spouses provides similar results.¹⁰

In Panel B, I provide estimates of job

lock using the insured as the experimental group and the uninsured as the control group. Despite the problems noted with this estimation strategy, these results facilitate comparison to results in the literature that use this estimator. Note that the coefficient on no spousal health insurance in panel A is small and insignificant compared to the coefficient on health insurance in panel B, verifying that the experimental and control groups in panel A are more comparable in terms of base mobility rates. The coefficients on wage and union

¹⁰While I focus on married men, I find that job lock estimates for married women and single individuals are also insignificant and mostly small in magnitude, suggesting that job lock is insignificantly different from zero for all groups. These estimates are available on request to the author.

fall somewhat when the sample is restricted to insurance holders. This may be attributed to wage and union status being more positively correlated with other components of current compensation for the uninsured. The estimate of job lock in panel B, the coefficient on $\text{HealthIns} \cdot \text{Sick}$, should be negative and significant if insured individuals with sick families are most likely to be job-locked. However, it is insignificant in all three cases.

Estimating Job Lock Using Family Size

In her influential study, Madrian (1994) used the same data set that I use in this paper and found that job lock reduced quit probability by 4 percentage points, from 16% to 12%, equivalent to a 25% reduction in job mobility. The next two sections reconcile the difference between the results reported in the Madrian paper and in this paper.

Madrian used three job lock measures: holding of alternative health insurance policies, pregnancy of spouse, and family size. She measured job lock by estimating probit equations, similar to those described above (in the section "Estimating Job Lock Using Family Sickness Measures"), with those holding employer-provided health insurance as the experimental group and those without as the control group. She obtained large and significant estimates of job lock using all three measures. The interaction of holding employer-provided health insurance and having alternative coverage is a questionable measure of job lock, since it could be inconsistent if those with health insurance are not comparable to those without health insurance. Hence, I re-estimate job lock using only the family size and pregnancy of spouse measures used by Madrian. For these two estimators, I can create comparable control and experimental groups by using the estimation strategy developed above.

In this section, I focus on the estimation using family size as a job lock variable. Larger families have a higher probability of having a medical condition and higher routine medical costs. Hence, men with large

families who hold employer-provided health insurance are most likely to be job-locked. Madrian's large and significant estimate of job lock using family size is the coefficient on the interaction of a variable measuring health insurance status and family size. I show that this estimate suffers from an omitted variable bias since the direct effect of the health insurance variable in the interaction term is excluded from the estimating equation. Once I include a control for this variable in the estimation, the estimate of job lock becomes insignificant. Furthermore, I show that using comparable control and experimental groups results in a small and insignificant estimate of job lock.

I begin by replicating Madrian's family size job lock probit equation,¹¹

$$(3) \quad \Pr(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{HealthIns} + \beta_2 \cdot \text{FamilySize} + \beta_3 \cdot \text{FamilySize} \cdot \text{HealthIns}_c + Z'\gamma),$$

where HealthIns_c , defined in the same way as above (under "Estimating Job Lock Using Family Sickness Measures"), is a dummy variable that indicates whether the individual holds employer-provided health insurance. For the family size estimation, Madrian introduced a new health insurance dummy variable that indicated whether the individual held an employer-provided health insurance policy covering others in the individual's family.¹² For convenience, I label this new variable HealthIns_c . The

¹¹I thank Brigitte Madrian for making her data and programs available.

¹²Madrian assumed that an individual's policy covers others if the individual's spouse does not hold health insurance and at least one person other than the individual is covered by an employer-provided policy. Hence, if HealthIns_c equals 1, HealthIns must equal 1 as well. Madrian also estimated job lock using a more "liberal" measure of HealthIns_c in which the husband is assumed to cover others even if the wife holds an employer plan. This estimation gives weaker job lock results. Madrian found larger significant estimates of job lock when the sample was restricted to men whose wives did not hold health insurance. As with the estimation using the full sample, this estimate of job lock suffers from an omitted variable bias and the non-comparability of the control and experimental groups.

following exogenous variables are in the vector Z : black, education, experience, union status, log hourly wage, and months elapsed between the first NMES interview and the last NMES interview in 1987. The parameter estimates obtained by replicating Madrian's results are reported in column (1) of Table 3.

Holding employer-provided health insurance has a large negative effect on mobility. The coefficient on the wage measure is negative and significant.¹³ The job lock measure, which is the interaction of holding a policy that covers others and family size, is negative and significant at the 5% level. The marginal effect of job lock, calculated as the effect of an increase in the interaction term from the median to the 75th percentile on the quit probability, is 1.8 percentage points. This differs from the calculation used by Madrian, which involves comparing the job mobility of individuals with 1 child to the mobility of individuals with 5 children.

In the estimating equation described above, Madrian did not include a control for the direct effect of HealthIns_c , the health insurance variable that is interacted with family size. Since HealthIns_c is omitted from the estimating equation, the coefficient on the interaction term is likely to be biased toward finding job lock if HealthIns_c deters mobility after conditioning on HealthIns . For example, it is likely that HealthIns_c is correlated with unobserved job quality, since the individual with a good, stable job obtains coverage for the family (that is, he has $\text{HealthIns}_c = 1$). This leads to a negative coefficient on HealthIns_c in a quit equation. Madrian's measure would

incorrectly attribute this effect to job lock.

To correct this possible bias, I include HealthIns_c as an explanatory variable and estimate

$$(4) \quad \text{Pr}(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{HealthIns} + \beta_2 \cdot \text{FamilySize} + \beta_3 \cdot \text{HealthIns}_c + \beta_4 \cdot \text{FamilySize} * \text{HealthIns}_c + Z'\gamma).$$

The coefficient on the interaction term provides a corrected measure of job lock. The results from this estimation, reported in column (2), show that once HealthIns_c is included as a control, the estimate of job lock becomes smaller and insignificant. The marginal effect of job lock now drops to 1 percentage point.¹⁴

As explained above, the group without employer-provided health insurance is not an appropriate control group for those holding employer-provided health insurance. Hence, to create comparable experimental and control groups, the sample is restricted to those holding employer-provided health insurance. Since those holding only employer-provided health insurance are more job-locked than those holding both employer-provided health insurance and spousal health insurance, the interaction of no spousal health insurance and family size provides an estimate of job lock. The following probit equation is estimated on the sample of insured men:

$$(5) \quad \text{Pr}(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{NoSpouseHealthIns} + \beta_2 \cdot \text{FamilySize} + \beta_3 \cdot \text{FamilySize} * \text{NoSpouseHealthIns} + Z'\gamma)$$

A negative and significant coefficient on the interaction term would indicate the presence of job lock.

¹³Madrian used the wage and union status from the job held at the end of 1987 in her estimation. It is inappropriate to use these measures, especially if an individual changes jobs during 1987. Hence, these are replaced by wage and union status at the beginning of 1987. These data became available after Madrian completed her study. When the specification in column (1) is re-estimated with Madrian's measure of wage and union, the coefficient (standard error) on the interaction term is -0.053 (0.023). The coefficient on wage is -0.085 (0.065).

¹⁴Two additional D-D estimators of job lock obtained by (1) the interaction of HealthIns_c and family size, where the only health insurance control is HealthIns_c , and (2) the interaction of HealthIns and family size, where the only health insurance control is HealthIns , yield estimates of job lock that are larger in absolute value than those reported in column (2), but remain insignificant.

Table 3. Estimation of Job Lock Using Family Size as the Job Lock Variable.

<i>Explanatory Variable</i>	<i>(1) Replication: Full Sample</i>	<i>(2) Control for Health Ins.: Full Sample</i>	<i>(3) Use Spouse Health Ins: Insurance Holders</i>	<i>(4) Restrict to Dual-Worker Families</i>
Wage (log)	-0.157** (0.064)	-0.157** (0.064)	-0.094 (0.078)	-0.169 (0.094)
Family Size	-0.017 (0.030)	-0.027 (0.035)	-0.045 (0.068)	-0.028 (0.069)
Schooling	-0.011 (0.015)	-0.011 (0.015)	-0.003 (0.019)	-0.011 (0.023)
Experience	-0.017*** (0.004)	-0.017*** (0.004)	-0.017*** (0.005)	-0.019*** (0.006)
Black	-0.014 (0.090)	-0.015 (0.090)	-0.010 (0.116)	0.047 (0.135)
Union	-0.381*** (0.089)	-0.381*** (0.089)	-0.391*** (0.109)	-0.464*** (0.143)
Between Interviews	0.002*** (0.001)	0.002** (0.001)	0.002 (0.001)	0.0002 (0.002)
HealthIns	-0.429*** (0.088)	-0.422*** (0.089)		
HealthIns _{Coverer}		-0.114 (0.203)		
HealthIns _c *Family Size	-0.059** (0.023)	-0.030 (0.056)		
NoSpouseHealthIns			-0.058 (0.261)	-0.168 (0.307)
NoSpouseHealthIns*FamilySize			-0.029 (0.079)	-0.003 (0.092)
Marginal Effect of Job Lock [†]	-0.018 (0.007)	-0.010 (0.017)	-0.004 (0.010)	-0.0008 (0.024)
No. Observations	2,942	2,942	2,231	1,439

Note: In addition, an intercept, four occupation dummies, and four industry dummies were included in the estimation.

[†]This is calculated as the effect of increasing the interaction term from the median of the distribution to the 75th percentile on the probability of changing jobs.

Statistically significant at the .05 level; *at the .01 level.

The results, in column (3) of Table 3, show that the estimate of job lock, the coefficient on the interaction term, is negative and insignificant. Comparing the mobility of those with only employer-provided health insurance to the mobility of those who have employer-provided health insurance and spousal health insurance improves the comparability of the control and experimental groups. However, now the control group consists solely of dual-earner couples, whereas the experimental group does not. To further improve comparabil-

ity, I re-estimate the specification in column (3) after the sample is restricted to men with employed spouses. The results from this modification, reported in column (4) of Table 3, predict that job lock accounts for a mere 0.08 percentage point reduction in mobility, where the average probability of job change is 9%.

On the whole, it is clear that Madrian's estimate of large and significant job lock using family size results from excluding the direct effect of the health insurance variable in the interaction term. Once this is

corrected, the estimate of job lock becomes insignificant. Furthermore, the use of comparable control and experimental groups results in a very small estimate of job lock.

Estimating Job Lock Using Pregnancy of Spouse

A common provision in health insurance policies is the stipulation that pregnancy-related expenses are covered only if conception occurs while the policy is in effect. Thus, a man holding employer-provided health insurance may be job-locked for the period of his wife's pregnancy, since the costs of the pregnancy may not be covered by a new policy. Hence, the fraction of time during the survey year that the wife is pregnant measures the time the man is potentially job-locked. This "time pregnant" measure captures job lock since insured men whose wives are pregnant for a small fraction of the survey year should be more likely to change jobs during the year than insured men whose wives are pregnant for most of the year.

Madrian's measure of time pregnant only includes pregnancies that result in an addition to the family unit during 1987. Thus, pregnancies that were ongoing at the end of 1987 and resulted in a birth in 1988 are not counted. It is likely that there is a relationship between timing of pregnancy and job mobility of the husband. Hence, by excluding all pregnancies that result in births in 1988, Madrian's measure is likely to lead to biased estimates. Note that since pregnancies are not excluded randomly, the measurement error in Madrian's pregnancy variable is not classical. *Ex ante*, the direction of bias of the estimates using Madrian's variable is unclear.

A good measure of time pregnant incorporates all the time during the survey year that the wife is pregnant, regardless of when the birth occurred. The NMES has released data that can be used to construct such a variable.¹⁵ I improve on Madrian's

estimation by using these data to construct a "corrected" time pregnant variable that includes all the time in the survey year that an individual is potentially job-locked. Furthermore, I use comparable control and experimental groups to estimate job lock. I find that after these improvements are made, the estimate of job lock has the wrong sign and is insignificant.

First, I replicate Madrian's finding of job lock by estimating the following probit equation using Madrian's measure of time pregnant:¹⁶

$$(6) \quad \text{Pr}(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{HealthIns} + \beta_2 \cdot \text{TimePregnant} + \beta_3 \cdot \text{TimePregnant} * \text{HealthIns} + Z'\gamma)$$

A negative and significant β_3 implies significant job lock. The results from replicating Madrian's estimates are reported in column (1) of Table 4.¹⁷ The coefficient on the interaction term, which captures job lock, is negative and significant at the 5% level. The marginal effect of job lock from this estimation is 5.1 percentage points. The average job change probability is 14%.

I re-estimate the specification reported in column (1) after replacing Madrian's time pregnant measure with the corrected time pregnant measure, which measures the total fraction of the year that the man's

ing in abortions from the corrected time pregnant measure, since a pregnancy is unlikely to be a deterrent to job mobility if an abortion is planned. In this sample, my measure agrees with Madrian's on the status of 95% of the cases. However, my measure adds 75% more pregnancies to the data.

¹⁶Madrian also used the birth of a child during 1987 as a pregnancy measure. This estimation gives a smaller estimate of job lock.

¹⁷As in the estimation using family size, Madrian used the wage and union status from the job held at the end of 1987 in her estimation. I replace these with wage and union status at the beginning of 1987. When the specification in column (1) is re-estimated with Madrian's measure of wage and union, the coefficient (standard error) on the interaction term is -0.908 (0.464). The coefficient on wage is -0.089 (0.064).

¹⁵The NMES asks individuals about the current status, the duration, and the nature of the termination of their pregnancy. I exclude pregnancies end-

Table 4. Estimation of Job Lock Using Pregnancy of Spouse as the Job Lock Variable.

<i>Explanatory Variable</i>	<i>(1) Replication: Full Sample</i>	<i>(2) Corrected Time Pregnant Measure</i>	<i>(3) Use Spouse Health Ins.: Insurance Holders</i>	<i>(4) Restrict to Dual-Worker Families</i>
Wage (log)	-0.153** (0.063)	-0.157** (0.063)	-0.094 (0.078)	-0.135 (0.087)
Time Pregnant	0.776** (0.382)	0.285 (0.276)	0.0001 (0.350)	-0.031 (0.352)
Schooling	-0.009 (0.014)	-0.009 (0.014)	-0.002 (0.019)	-0.001 (0.021)
Experience	-0.015*** (0.004)	-0.014 (0.004)	-0.013*** (0.005)	-0.017*** (0.006)
Black	0.013 (0.089)	0.013 (0.089)	-0.020 (0.115)	0.011 (0.123)
Union	-0.394*** (0.089)	-0.387*** (0.089)	-0.388*** (0.109)	-0.435*** (0.126)
Betw. Interviews	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
HealthIns	-0.521*** (0.074)	-0.548*** (0.075)		
HealthIns*TimePregnant	-1.015** (0.477)	-0.139 (0.333)		
NoSpouseHealthIns			-0.200** (0.090)	-0.164** (0.097)
NoSpouseHealthIns*TimePregnant			0.245 (0.416)	0.291 (0.452)
Marginal Effect of Job Lock [†]	-0.051 (0.017)	-0.009 (0.020)	0.012 (0.021)	0.010 (0.015)
No. Observations	2,946	2,946	2,231	1,698

Note: In addition, an intercept, four occupation dummies, and four industry dummies were included in the estimation.

[†]This is calculated as the effect of increasing the interaction term from the median of the distribution to the 95th percentile (a change to the 75th percentile was equal to 0) on the probability of changing jobs.

Statistically significant at the .05 level; *at the .01 level.

spouse is pregnant. The results are reported in column (2) of Table 4. With the corrected measure of pregnancy, the interaction term falls in absolute value and becomes insignificant. The marginal effect of job lock falls from 5.1 percentage points in column (1) to 0.9 percentage points in column (2).¹⁸

Next, I construct more comparable control and experimental groups by comparing the mobility of men who have only

employer-provided health insurance with the mobility of those who have employer-provided health insurance and spousal health insurance. Men whose spouses hold employer-provided health insurance should be less likely to be job-locked than those whose spouses do not hold health insurance, since the spouse's policy should, al-

¹⁸One possible explanation for this result is that insurance non-holders may be more likely than insurance holders to move to better jobs after the birth of

a child. Since my measure of time pregnant adds pregnancies that do not result in births during 1987, they are associated with a lower probability of job change. The addition of these pregnancies reduces the large positive significant effect of time pregnant on the turnover probability of the uninsured, leading to a small and insignificant D-D estimator.

most certainly, cover her own pregnancy. I estimate the following equation on the sample of insured men:

$$(7) \quad \text{Pr}(\text{ChangeJob}) = f(\beta_0 + \beta_1 \cdot \text{NoSpouseHealthIns} + \beta_2 \cdot \text{TimePregnant} + \beta_3 \cdot \text{TimePregnant} * \text{NoSpouseHealthIns} + Z'\gamma)$$

A negative and significant β_3 would imply significant job lock.¹⁹ The results for this estimation, reported in column (3), show that the coefficient on the interaction term is positive, which is the wrong sign for job lock. Note that this result holds when Madrian's time pregnant measure is used instead of my corrected time pregnant measure.

To further enhance the comparability of the experimental and control groups, I re-estimate the above equation after restricting the sample to men whose spouses are employed at some point in the year. This ensures that both the control and experimental groups consist of dual-earner couples. The coefficient on the interaction term that results from this sample restriction (column 4) remains positive. The 90% confidence interval on this estimate excludes the level of job lock reported in column (1).

¹⁹There are four possible problems with this estimator. First, if the spouse's policy does not cover post-natal care, the man may remain job-locked. From the NMES data, I find that over 90% of policies provide immediate coverage for the new-born, suggesting that since, in general, policies seem to offer some form of post-natal care, spousal health insurance will tend to reduce job lock. Second, pregnant women with spousal health insurance may be likely to quit during their pregnancy and lose coverage. Hence, their spouses would be as job-locked as the rest of the sample, biasing the estimate of job lock. When the specifications in columns (3) and (4) are re-estimated excluding pregnant women who quit, the results are unaffected. Third, pregnant women with good jobs and health insurance may be more reluctant to quit than those without health insurance, thus constraining their spouses with respect to geographical job moves. However, I find that pregnant women in good jobs (measured by a high wage or health insurance) are no less likely to quit their jobs. Fourth, if a dummy variable for whether or not pregnant is included in this estimation, the estimates of job lock continue to have the wrong sign.

To summarize, the large and significant estimate of job lock using pregnancy of the spouse as the job lock variable disappears when better data are used to construct the pregnancy variable. Furthermore, the use of comparable control and experimental groups results in job lock estimates that are the wrong sign and that exclude Madrian's estimate from their confidence intervals.

Conclusion

The concern that non-mandatory employer-provided health insurance distorts job mobility has driven many academic studies and public policy discussions. In this paper, I measure the importance of job lock. Estimating a precise measure of job lock is important in order to evaluate the potential impact of legislation that aims to increase the portability of employer-provided health insurance. I refine the difference-in-difference technique used in the literature by using good proxies for family sickness in conjunction with more comparable control and experimental groups. I find that the estimates of job lock obtained using this revised technique have the wrong sign and are insignificant. Furthermore, the estimates are precise enough to exclude large levels of job lock from their confidence intervals.

While these estimates are consistent with those from studies using the SIPP and the PSID, they are far smaller than estimates from studies using the NMES. To explore the reasons for that disparity, I re-analyze Madrian's (1994) finding of significant job lock using family size and pregnancy of spouse as job lock measures in the NMES. After correcting methodological problems and using better data to construct the job lock variables, I find that job lock is insignificantly different from zero.

The empirical work in this paper thus shows that once a consistent estimation technique is used, job lock is small and statistically insignificant. This finding suggests that legislation aimed at enhancing the portability of employer-provided health insurance is unlikely to have a large impact on job mobility.

APPENDIX

To construct the medical conditions index, I estimate the following OLS equation on the sample of individuals who hold employer-provided health insurance:

$$\log(\text{Family Medical Expenses} + 5) = \alpha_0 + \alpha_1 \text{MedicalConditions} + \alpha_2 X + \varepsilon$$

The constant \$5 minimizes the skewness of residuals (Duan et al. 1983). Family medical expenses are defined as the sum of expenses over the family from the following categories: ambulatory physician and non-physician (such as chiropractor) care, ambulatory hospital outpatient care, emergency room care, inpatient hospital and physician care, home health care, prescribed medicines and medical equipment (excluding eye-glasses and contact lenses). The vector Medical Conditions includes 25 chronic medical conditions. While 59 distinct medical conditions are coded, I aggregate several of these conditions, because the frequencies for some conditions are low. The vector X contains a set of control variables that refer to the male head (log wage, schooling, tenure, age, age squared, black, union, four occupation dummies, and four industry dummies) and a set of family-level variables (family size, log family income, region, and SMSA controls). I construct the logarithm of family medical expenses predicted from medical conditions by calculating $\hat{\alpha}_1 \text{MedicalConditions}$ for the full sample. This measure is the medical conditions index.

To construct the health utilization index, I follow the same procedure as with the medical conditions index; however, I replace the vector Medical Conditions with Health Utilization. Health Utilization is a vector of the following five types of family health utilization: number of emergency room visits, nights spent in hospital, number of visits to physicians and other medical practitioners in hospital out-patient facilities or provider offices, number of purchases or rentals of medical equipment (for example, orthopedic items, wheelchairs, and diabetic equipment such as syringes), and number of purchased or otherwise obtained outpatient medicines prescribed by a physician (including refills). I exclude utilization due to routine medical examinations, immunizations, or minor accidents and injuries, since these events are unlikely to cause job lock.

I carry out several specification checks on the indices to ensure that the estimates of job lock are not sensitive to the form of the medical expense prediction equation. First, the dependent variable, family medical expenses, in the prediction equation is specified in both logarithms and levels. I report the specification using logarithms because the distribution of log medical expenses is less skewed than the distribution of expenses in levels. I also use alternative definitions of family medical expenses that exclude prescribed medicine expenses or ambulatory non-physician expenses (expenses unrelated to hospital care) from the family medical expenses measure, since insurance policies often do not cover these expenses.

Second, I estimate the medical expenses equation at the individual level, rather than the family level. I aggregate the predicted individual medical expenses over the family to calculate the family level index.

Third, a possible problem with the construction of these indices is that health insurance status may be endogenous. If there is a relationship between sickness and health insurance status, estimating the OLS medical expenses equation on the sample of the insured may provide biased estimates. However, individuals who are sick do not seem to be more likely to have either employer-provided health insurance or better insurance policies (Buchmueller 1995; Cameron and Trivedi 1991). This suggests that the estimates of job lock in the paper using the sickness indices are consistent. As a specification check, I estimate the indices using a switching regression model with a first stage health insurance status equation and a second stage family medical expenses equation with a selection correction term. Note that when the sample is restricted to the insured, the first stage equation is the probability of having access to spousal health insurance. For the estimation with the full sample, the first stage equation is the probability of having health insurance. A drawback to this approach is that there are no exclusion restrictions in the first stage equation; hence the identification of the medical expenses equation rests solely on functional form.

I re-estimate the difference-in-difference job lock equations using the sickness indices, incorporating each of these specification checks. The conclusion that there is little to no job lock is robust with respect to all of these checks.

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