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# CHRONIC ILLNESS AND HEALTH INSURANCE-RELATED JOB LOCK

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## Abstract

We examine job duration patterns for evidence of health insurance-related job lock among chronically ill workers or workers with a chronically ill family member. Using Cox proportional hazard models, we allow for more general insurance effects than in the existing literature to indicate the impact of health insurance and health status on workers' job durations. We use data for workers in Indiana predating the Health Insurance Portability and Accountability Act (HIPAA) to examine the potential impact of HIPAA on job mobility. Chronic illness reduced job mobility by about 40 percent among the workers in our sample who relied on their employers for coverage as compared to otherwise similar workers who did not rely on their employers for coverage. Our results identify previously under-appreciated job lock among chronically ill workers and workers with a chronically ill family member, clarify how one best researches job lock, and indicate the potential impact of policies aimed at alleviating job lock and promoting inter-employer worker mobility.

## Introduction

Because over 64 percent of non-elderly insured Americans have health insurance through an employer (Fronstin 1998) the connection between employment and health insurance coverage can have significant effects on job mobility. The propensity of workers to stay in a job to retain insurance coverage, which has been termed job lock, is a significant problem if it inhibits efficient use of talent by employers and limits the life choices of employees. Because they face high medical bills and may value retaining coverage that could be lost by changing jobs, workers who are themselves chronically ill or who have a chronically ill family member are the most susceptible to job lock. We examine job mobility patterns among workers for whom job lock is most likely to exist using a more general multivariate statistical approach than the existing literature and find evidence of job lock among both men and women. Other things the same, workers who relied on their employers for coverage in our data were about 40 percent less likely to quit their jobs.

## Background

Underlying job lock are medical insurers' underwriting practices that may limit workers' health insurance coverage or increase workers' premiums at a new employer. In the past, underwriting practices in employer-sponsored health plans excluded coverage for workers or their family members with costly illnesses through preexisting conditions exclusions (Kinney et al. 1997; Kinney and Steinmetz 1994; Light 1992; Cotton 1991; Beauregard 1991). The extent

of possible job lock was revealed in a 1991 CBS/*New York Times* poll where 30 percent of respondents said they remained at their jobs for fear of losing health care benefits (Echolm 1991).

Implemented in June 1997, Public Law 104-191 or the Health Insurance Portability Accountability Act (HIPAA) and some complementary state-level policies seek to mitigate some problematic underwriting practices. HIPAA limits insurers' ability to impose permanent pre-existing condition exclusions on persons who already have insurance and move to a new group. HIPAA also guarantees a worker and family members who are covered by an employer-provided insurance plan access to either group or individual insurance if the worker changes jobs (Fuchs and Smith 1996).

Although supporters of HIPAA intended it to guarantee access to coverage by persons who have had group coverage the law does not prevent all situations that might create job lock. HIPAA does not prohibit a reduction in coverage for a specific illness by lowering maximum lifetime payouts for the illness, and HIPAA does not limit premiums insurers can charge. If the alternative coverage available to workers through either a group or individual policy includes coverage limitations or high premiums, workers facing the costs of a chronic illness might remain locked into their current jobs. In a survey by the Employee Benefit Research Institute, done in 1998 after HIPAA was enacted, 27 percent of Americans still said that they or a family member had experienced insurance-related job lock (Silverstein 1998). Despite HIPAA, workers facing a chronic illness remain at risk of being job locked.

Important for our research and interpreting our empirical results are recent insurance reforms in Indiana. IC 27-8-15, enacted in 1992, initially established rate bands to control premiums. In 1995 the law was amended to include prohibitions on discriminating against certain small employer groups or particular group members, restrictions on pre-existing condition

exclusions and limitations, and conversion opportunities for individual employees who terminate employment. To comply with HIPAA the Indiana amended its small group reforms in 1996 to include HIPAA's so-called guaranteed issue provision and to expand the definition of a small group from 3 to 50 employees to 2 to 50 employees. Indiana's pre-existing condition exclusion cap for small groups is nine months (15 months for late enrollees), which is tighter than HIPAA's 12 months (18 months for late enrollees). According to the Health Care Financing Administration (HCFA), which oversees the enforcement of HIPAA in states, Indiana is in compliance with HIPAA (USGAO 2000).

## Previous Research

Econometric evidence to date on job lock comes from three large economy-wide micro data sets, the National Medical Expenditure Survey (NMES), the Survey of Income and Program Participation (SIPP), and the Panel Study of Income Dynamics (PSID). The first researchers using the NMES found that insurance-related job lock reduced job mobility rates for married men by 25 to 30 percent (Madrian 1994; Cooper and Monheit 1993; Monheit and Cooper 1994). More recent research with the NMES using improved statistical procedures concluded that job lock is small and statistically insignificant (Kapur 1998). There is some evidence of job lock among women but not among men in the SIPP and little evidence of job lock in the PSID (Holtz-Eakin 1994; Penrod 1993; Buchmuller and Valletta 1996). Taken as a group, existing studies create an unclear picture of job lock as a labor market phenomenon. Our research responds to the clarion call for additional empirical research on the sensitivity of job lock estimates to sample composition and estimation strategy (Currie and Madrian 1999, p. 3400).

Because of the potentially high value of insurance benefits, workers who have high medical expenses from chronic illnesses are the most susceptible to insurance-related job lock.

Previous studies have not isolated workers with chronic illness problems. For example, there is little evidence of a connection between illness and job mobility in research using the NMES because the survey has relatively few seriously ill persons from whom to make inferences (Cooper and Monheit 1993; Monheit and Cooper 1994). The closest past research using the NMES has come to isolating workers with medically costly situations has been to study men with pregnant wives, where incapacity was temporary, which provides little evidence that a costly chronic illness affects job mobility (Madrian 1994). Researchers who found some evidence of job lock among women using the SIPP did not include a measure of health status in their models (Buchmuller and Valletta 1996). The econometric literature questioning the importance of job lock overall is silent on two important related policy questions that we seek to answer here. First, how serious is job lock among the subset of persons most likely to be concerned with their ability to maintain health insurance after a job change, workers with costly chronic conditions afflicting them or their family members? Second, is the resulting estimate of job lock sensitive to how the researcher conceptualizes differences in job duration within a regression model?

## Data and Methods

Our research uses data intended to distinguish our results from previous studies. The details of how we assembled our data appear in the Appendix. By way of summary, we study persons facing a chronic or prolonged medical condition themselves or in an immediate family member in Indiana during 1994. The advantage of studying job mobility in a single state is that every worker had the same laws regulating continued coverage after a job change. The chronic illness requirement for being in our data means that the workers we study have both economically meaningful costs of health care to consider when changing jobs and benefits from policies that may mitigate insurance-related job lock. By focusing on the potentially most severely job-locked workers at a time before HIPAA, we can infer the maximum impact of

policies to mandate coverage continuation in the sense that the amount of job lock we estimate is the amount that public policy could eliminate.

Data from respondents' and spouses' job histories during 1984 to 1994 allow reasonable sized parallel data sets for men versus women so that we can uncover any subtle gender differences in job lock and related public policy. The unit of observation is an employment episode so that a worker can be involved in more than one observation and can have episodes both with and without a chronic illness and both with and without work-related health insurance coverage. The duration of each employment episode is measured in months. For each employment episode the focal independent variables for the purpose of metering job lock are whether the worker or a family member had a chronic illness in effect during the employment episode and whether the worker relied on his or her employer for health insurance. Other factors that empirical researchers believe are important conditioners of employment duration also appear in our regressions—pay, race, gender, marital status, education, industry, and occupation, which can vary too across multiple episodes for a worker. Table 1 presents the descriptive statistics for our data, which highlight the background differences between persons who rely on employment-related health insurance and persons who do not.

We reiterate that our sample is not representative of the general population because one of our points of emphasis is on whether job lock may be mostly a phenomenon of persons confronted with serious illness. Because relatively few seriously ill persons appear in large surveys of the general population such as the PSID, the SIPP, and the NMES, past research has had not detected problems specific to persons touched by a serious illness (Monheit 1994). We deliberately selected samples of persons affected by chronic illness to make more detailed inferences about persons most likely to suffer from job lock and in turn to benefit from HIPAA. Our non-random sampling does not contaminate the parameter estimates of interest because the

non-randomness is not related to the outcome being studied, job lock, but rather the non-randomness is related to an independent variable, chronic illness. Non-random sampling based on the outcome being studied is what causes so-called sample selection bias. Non-random sampling based on an independent variable, as we do here, is commonly used to improve efficiency of parameter estimates in cases where a characteristic is relatively rare in nature, such as over-sampling African-Americans to ensure statistically reliable estimates of racial differences in outcomes.

## Statistical Procedure

To test for insurance-related job lock, we examine the effect of health insurance on the duration that a worker remained at a job using a Cox proportional hazard rate model (Lancaster 1990). The hazard rate $\lambda(t)$ is the likelihood that a worker quits at time $t$ given that the worker has remained at the job until $t$. In the Cox model the hazard rate is $\lambda(t|\mathbf{X}, \gamma) = \exp(\mathbf{X}\ \gamma)\ \lambda_0(t)$, where $\lambda_0(t)$ is an unknown baseline hazard, and the elements of $\mathbf{X}$ are explanatory variables. An advantage of the Cox model is that over can recover the effects of the explanatory variables on the hazard rate, the elements of $\gamma$, without specifying a functional form for the baseline hazard (Marubini and Valsecchi 1995).[1]

### Testing for Job Lock

If employer-provided insurance results in job lock, then workers who rely on their employer for coverage should have a lower hazard rate (likelihood of quitting). Comparing the raw hazard rates of workers who did versus did not rely on their employer for coverage would not provide an adequate indication of job lock. Workers at firms not offering insurance may have lower quality jobs, increasing the likelihood they will quit so that one would over-estimate the amount of job lock by comparing simple group average hazard rates. Workers who do not rely on

their employer for insurance because they have other sources of coverage, such as through a spouse's employer, may also have other financial resources, such as high spousal income, which make quitting a job a less risky economic event, so again one would over-estimate the amount of job lock by comparing simple group average hazard rates. Even comparing hazard rates conditioned for observed worker characteristics can be uninformative if the researcher misses interaction effects between conditioning factors and health insurance status that might affect employment duration.

After incorporating through **X** the effects of relevant personal and job characteristics we compare the difference in hazard rates (*H*) pre versus post chronic illness diagnosis for workers who relied on their employer for coverage with the difference in hazard rates pre versus post diagnosis for workers who did not rely on their employer for health insurance coverage. The so-called difference-in differences we estimate eliminates the effects of time invariant unmeasured intervening factors (Holz-Eakin 1994; Madrian 1994; Penrod 1993). Algebraically, the amount of job lock is $[H(Pre,Rely) - H(Post,Rely)] - [H(Pre,Not\ Rely) - H(Post,Not\ Rely)]$.

## Conceptualizing Job Lock Empirically

In any observational study, behavioral outcomes determine membership in the implicit treatment and control groups. A researcher must be careful not to assume similarities across the two groups of persons that are not present in reality. The mode specification in past research on job lock has forced all the coefficients of the job separation regression function to be the same across insurance groups except for the effect of illness (see Currie and Madrian 1999 and references therein).

Workers with employer-provided health insurance may be less likely to quit because they have better jobs for reasons other than health insurance availability, which may also not be captured fully in the available set of regression control variables, **X**, so that the estimated

coefficients of all the regressors differ across insurance groups. Because there is no compelling ex ante reason to force identical hazard functions on the two insurance groups, we have avoided a potentially serious specification error that might have occurred had we pooled the data across insurance availability groups and used only an interaction term between reliance on employer-provided insurance and illness to infer job lock. To produce results with greater generality than characterize the current empirical literature on job lock we estimate separate hazard functions for persons who do versus do not rely on their employer for health insurance

$$\lambda(t|\mathbf{X})_{\text{Relies = yes}} = \lambda_0(t)\exp(\alpha_0 + \alpha_{\text{Dignose}} \cdot \textit{Diagnose} + \mathbf{X}\,\alpha) \text{ and} \quad (1)$$

$$\lambda(t|\mathbf{X})_{\text{Relies = no}} = \lambda_0(t)\exp(\beta_0 + \beta_{\text{Dignose}} \cdot \textit{Diagnose} + \mathbf{X}\,\beta). \quad (2)$$

The two indicator variables, *Relies* and *Diagnose*, track insurance and health status during each employment episode.[2] For healthy workers, *Relies* = yes means that the family member who will develop a chronic illness relies on the workers' employer for coverage. For workers who are chronically ill, *Relies* = yes means the workers rely on their employer for coverage. *Diagnose* = 1 indicates that the chronic illness had been diagnosed either during or prior to the employment episode.

The ratio of the post-diagnosis hazard rate to the pre-diagnosis hazard rate for workers who rely on their employer for coverage is $HR_{(\text{Relies = Yes})} = \exp(\alpha_{\text{Dignose}})$. The ratio of the post-diagnosis hazard rate to the pre-diagnosis hazard rate for workers who do not rely on their employer for coverage is $HR_{(\text{Relies = No})} = \exp(\beta_{\text{Dignose}})$. The percentage reduction in the hazard rate $HR_{(\text{Relies = Yes})}$ relative to the hazard rate $HR_{(\text{Relies = No})}$ measures insurance-related job lock then as $[(HR_{(\text{Relies = Yes})}/HR_{(\text{Relies = No})}) - 1] \times 100 = [(\exp(\alpha_{\text{Diagnose}})/\exp(\beta_{\text{Diagnose}})) - 1] \times 100$.

## Results

There are well-known gender differences in pay, fringe benefit, and job duration that also appear in our data, which are summarized in Table 1. The men we study are twice as likely as the

women to rely on their own employer for health insurance (39 percent versus 20 percent). In comparison 33 percent of non-elderly Americans have insurance coverage through their own employer (Custer and Ketsche 2000). Because gender differences in labor supply and job quality will carry over into the decision to leave a job voluntarily we estimated separate hazard functions by gender. Our core results appear in Tables 2 and 3. The hazard regressions have substantially different coefficients for men versus women and are consistent with the empirical literature on worker quits (Mincer and Jovanovic 1981; Bartel and Borjas 1981; Topel 1986, 1991; Shaw 1987; Light and Ureta 1992].[3] In all four regressions in Tables 2 and 3 the estimated partial effects of costly medical conditions for the worker or a family member are negative and statistically significant ($p < 0.01$). As we will soon show, our evidence of job lock among both men and women is not only statistically significant but it is also economically significant.

## Basic Results on Job Lock

For men who relied on their employer for coverage, a chronic illness or a chronically ill family member reduced men's likelihood of voluntarily leaving a job by about 82 percent. For men who did not rely on their employer for coverage, a chronic illness or a chronically ill family member reduced their likelihood of leaving a job by about 70 percent. The difference between the effects of illness on men according to whether or not health insurance was obtained on the job is statistically significant ($p < 0.0001$) (Kanji 1993, p. 29). Put differently, job lock reduced the quit propensity of men confronting a chronic illness by about 41 percent.[4]

Among women who relied on their employer for coverage, a chronic illness or a chronically ill family member reduced their likelihood of voluntarily leaving a job by about 83 percent. Among women workers who did not rely on their employer for coverage, a chronic illness or a chronically ill family member reduced their likelihood of leaving a job by about 73 percent. The difference between the effects of chronic illness on women according to whether or

not health insurance was obtained on the job is statistically significant ($p < 0.0001$). Job lock reduced the quit propensity of women confronting a chronic illness by about 39 percent.[5] Job lock is similar for the men and women in our data.[6]

## The Importance of Data Versus Model Specification

Our results shed new light on how a researcher best goes about examining the empirical importance of job lock. We have noted that others have not found much evidence of job lock. The typical worker does not have sufficiently severe medical conditions to alter his or her job mobility based on the availability of employer-provided health insurance. That the average worker is not job locked misses any mobility loss among workers for whom illness is a severe economic problem. In addition, we have argued that it may be important to begin with a maximally informative empirical model permitting different effects of all independent variables, not just insurance availability, on the decision by a worker to quit a job. How relatively important are the data versus the model specification to our finding of 40 percent job lock?

To infer the comparative importance in locating job lock of the generality of the hazard function specification versus the data that are for workers with a chronic illness or a chronically ill family member, we estimate a more restrictive functional form with our data. The models in Tables 2 and 3 allowed the estimated coefficients of all variables in the models to differ across the insurance availability groups. In Table 4 we restrict the hazard functions for men and women such that the only difference permitted ex ante in the estimated hazard function across insurance availability is in the coefficient of illness. In Table 4 the coefficient of (*DiagRely*), the interaction term between illness (*Diagnose*) and employment-related health insurance (*Rely*), estimates the amount of job lock. In the more restrictive model the amount of job lock is insignificant for both men and women. Had we used a restricted model that typifies the existing literature in which the

estimated coefficients of variables in the models were restricted to be the same across insurance groups, we too would have concluded that job lock is unimportant.

The results in Tables 2 through 4 demonstrate that it is the more general specification and not limiting the sample to persons with chronic illness problems that reveals the job lock we find. An implication is that future research may benefit from a statistical model that is rich enough to permit heterogeneity across insurance settings in the effects of all independent variables on job mobility, not just differences in the effect of illness.

## Gender Differences in the Origins of Job Lock

It is informative to policymakers if we investigate further the origins of job lock. In Tables 5 and 6 we disentangle the *Diagnose* variable into whether the medical problem is a chronic illness for the worker (*Diag_Slf*) or the worker's family member (*Diag_Fam*). For men, job lock stems mostly from the man's own chronic illness. For women, job lock is largely the result of a family member with a chronic illness. Among men with a chronic illness, relying on employer-provided health insurance for coverage lowered the likelihood of quitting by 64 percent while among men with a chronically ill family member, relying on employer-provided health insurance lowered the likelihood of quitting by about half as much, 32 percent. Among women with a chronic illness, relying on employer-provided health insurance lowered the likelihood of quitting by 21 percent while among women with a chronically ill family member, relying on employer-provided health insurance lowered the likelihood of quitting by about three times as much, 65 percent. The policy implication of the results in Tables 5 and 6 is that dependents' health insurance is relatively important to women's job mobility so that policies to promote continuation of coverage will have the largest impact on women workers if it protects dependent coverage.

## Discussion

Can HIPAA, which became effective during June 1997, have much impact on worker mobility? HIPAA guarantees that workers who had group coverage through their previous employer within the preceding 62 days will have access to group coverage at their new firms or to individual coverage if group coverage is not offered. Our data precede its implementation, so our estimates speak to the maximum potential impact of HIPAA. Although past research suggests that job lock is apparently not a problem for the average worker, in our data we find that job lock is substantial among workers with a chronic illness to consider. Among the workers we study, who are themselves chronically ill or who have a chronically ill family member, a perfectly operating HIPAA could increase job mobility by up to 40 percent.

There are a number of reasons why HIPAA and state-level policies seem likely to fall short of alleviating job lock fully. HIPAA limits pre-existing condition exclusions for workers with health insurance who switch employers. The act does not regulate other characteristics that may be found in health insurance policies available to workers who switch employers that may influence workers' out-of-pocket medical care expenses, such as annual expenditure limits or co-insurance rates. We have found that even if pre-existing conditions exclusions were removed from policies some persons who are chronically ill are still more likely to face the other characteristics of coverage that could increase their out-of-pocket payments (Stroupe, Kinney, and Kniesner 2000).

The implementation of HIPAA has also not been straightforward. In both 1997 and 1998 the U.S. General Accounting Office reported problems with enforcement of HIPAA regarding availability of affordable health insurance for eligible persons leaving group plans and seeking individual coverage (USGAO 1997, 1998a; Pear 1997). In a hearing on the GAO's 1998 report, federal administrators complained of constraints on resources for executing the relatively limited

federal enforcement powers in HIPAA against employer-sponsored plans regulated under the Employment Retirement Income Security Act (ERISA) of 1973 (Public Law No. 93-406) and state-regulated commercial group and individual plans (U.S. Senate Committee on Labor and Human Resources 1998). The Senate hearings and subsequent GAO investigation noted problems with HIPAA enforcement of sales of group coverage for smaller employers (USGAO 1998b). Moreover, a GAO report in 2000 indicated that HCFA, which has responsibility for overseeing enforcement of HIPAA, is proceeding slowly in using its enforcement authority in states that do not conform to HIPAA and related laws. HCFA has assumed limited regulatory activities in California, Missouri, and Rhode Island, three states that voluntarily identified themselves as nonconforming. HCFA has also identified approximately 20 states that appear to be deviating from one or more federal standards. However, HCFA has no timetable for completing reviews of the 20 suspect states to determine whether they are aligned or not with all federal standards. Until the reviews of the states are completed HCFA is fulfilling its statutory mandate primarily by reacting to consumer complaints (USGAO 2000).

Perhaps most relevant here, neither HIPAA nor most states' reforms target the cost of insurance to the consumer, and prohibitively expensive insurance leaves the amount of job lock unchanged. Individual insurance is always more expensive to the consumer than group insurance and recent evidence suggests that persons eligible for coverage in the individual market under HIPAA can face premiums that are 140–200 percent of the standard rates (USGAO 1998a). Without assurance of continued relatively low cost group coverage a worker confronting a chronic illness is unlikely to switch jobs. The insurance reforms of the past decade, which do not seem to have been concerned with spreading risks more broadly to lower premiums and out-of-pocket cost for higher-risk persons, have little over lap with the particular aspects of health

insurance of most interest to workers with a chronic illness or a chronically ill family member needed to underpin policy effective in reducing job lock.

## Appendix: Data Design

The data we use in our hazard rate regressions come from three separate data collection efforts in the state of Indiana during 1994. We will refer to the three samples that we combine for our ultimate data set as the South Bend sample, the cancer sample, and the parents' sample.

### The South Bend Sample

What we call the South Bend sample involves a stratified random sample of households in St. Joseph County, IN (the greater South Bend MSA), for a separate study of hypertension by the Indiana State Department of Health (Murphy 1993). Two methods generated the South Bend sample: a county-wide, random-digit dialing of households and a house-to-house survey in 13 census tracts with high proportions of minorities and low-income people. We asked each non-elderly resident (aged 19 to 64) in the Department of Health study to participate in our study. The Department of Health forwarded the names, addresses, and telephone numbers to project investigators at Indiana University. Of the 1,225 random contacts, 329 were too old (65+) to participate in our study. Of the remaining 896 persons, 508 agreed to participate in our study (Kinney et al. 1997).

Questionnaires sent to persons in the South Bend sample asked for information about their health status. Based on their questionnaires respondents were divided into two groups depending on whether or not they suffered from a serious illness. The illnesses consisted of serious conditions affecting the major organs: heart disease, diseases of the nervous system, lung diseases, diseases of the glandular system, kidney diseases, liver diseases, skeletal diseases, muscle diseases, diseases of the immune system, drug or alcohol addiction, serious mental illness, and cancer (Kinney et al. 1997). In all, there were 138 seriously ill respondents in the

South Bend sample who completed the interviews; 108 respondents had data available on their date of diagnosis, which we needed for our research.

### The Cancer Sample

Our data also contain two groups of cancer patients: women with breast cancer and men with testicular cancer. The breast cancer group comes from the tumor registries of the seven hospitals that treat over 90 percent of the women with breast cancer in Marion County, IN (the greater Indianapolis MSA). Women in the cancer sample resided in Marion County, had been diagnosed or treated in the study hospitals, were diagnosed or treated for breast cancer between January 1987 and December 1990, and were between 19 and 64 years old at the time of the survey. To protect confidentiality of patients the tumor registries contacted the patients and forwarded names of willing participants to the project investigators. Tumor registries contacted 821 patients whom the hospitals recorded as having breast cancer during the relevant time interval. Of the 208 women who agreed to participate in the study, 34 had died, moved, were too ill, or were otherwise unreachable for a phone interview, so that there are 174 women with breast cancer in the cancer sample (Kinney et al. 1997).

The sample of men with testicular cancer included male residents of Indiana aged 18 to 64 who were diagnosed between 1987 and 1990. The sample comes from the tumor registry of Indiana University Medical Center (IUMC). Because oncologists at IUMC developed the prevailing treatment of testis cancer, it treats nearly all testis cancer patients in Indiana (Einhorn and Donohue 1977). Of the 195 patients contacted, 41 men with testicular cancer completed the interviews.

### The Parents' Sample

The parents' sample includes persons with a child suffering from a serious or chronic illness who was treated at the Riley Hospital for Children at the Indiana University School of

Medicine in Indianapolis, IN. Riley Hospital is the only children's hospital in Indiana and is the state's major pediatric tertiary care center. The diseases of the sample children are those for which Riley Hospital treats more than 80 percent of afflicted minor residents of Indiana. The diagnoses include childhood cancers, inborn errors of metabolism, rheumatological diseases, and chronic renal failure. To protect confidentiality, Riley Hospital first contacted families and asked them to participate. Riley Hospital sent out letters to 1,943 parents of children that presumably met the sampling criteria. The hospital released contact information on 436 consenting participants, of which 330 persons had a child meeting the age and disease criteria and were available for the interviews.

### Summary

Respondents and their spouses in all three samples were called for a phone interview about their health insurance benefits and job histories since 1984. The interviewers mailed follow-up questionnaires to obtain maximum feasible accuracy and to enable respondents and spouses to check relevant records. Overall, our study had 653 respondents. Because we explore insurance-related job lock, we examined only the subset of 605 respondents who were employed at some time during 1984 to 1994, which included 95 respondents from the South Bend sample, 195 from the cancer sample (154 breast cancer and 41 testicular cancer patients), and 315 from the parents' sample. There were 426 employed spouses: 40 from the South Bend sample, 135 from the cancer sample (109 spouses of breast cancer patients and 26 spouses of testicular cancer patients), and 251 from the parents' sample.

## Endnotes

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1. Workers with employment spells in progress at the time of the survey in 1994 are so-called right censored because we do not know their ultimate completion date and are accounted for in the estimation procedure (Marubini and Valsecchi 1995). We treat as also right censored the employment spells where the worker reported an involuntary termination of employment (Allison 1984).

2. Because our unit of observation is the employment spell we make two adjustments to account for multiple employment spells per worker. We include as a regressor the number of previously completed employment episodes. We also adjust the standard errors in (1) and (2) upward by $(N/n)1/2$, where $N$ is the total number of employment spells and $n$ is the number of workers in each sample. The logic behind adjusting the standard errors upward is that if employment spell lengths are similar for the typical worker with multiple spells then the multiple spells are uninformative and a worker with multiple spells should not be treated as contributing more information than a worker with a single employment spell. The adjustment is conservative and will most likely overstate the true standard errors (Allison 1984, pp. 54–55).

3. For example, higher wages indicate better jobs, ceteris paribus. In our models higher wages significantly decrease the likelihood of quitting ($p < 0.01$).

4. $HR$(Relies = Yes)/$HR$(Relies = No) = 0.177/0.301 = 0.59.

5. *HR*(Relies = Yes)/*HR*(Relies = No) = 0.167/0.273 = 0.61.

6. Because our survey did not gather information on the age of all workers hazard regression we present do not control for the person's age, which is often an independent variable in quit rate studies. It is unclear how including an age variable might change our job lock estimates. In regressions for the subsets of workers for whom age was recorded including age reduced slightly the job lock estimate for men and increased it somewhat for women.

**Table 1. Characteristics of All Workers**

| | Men | | Women | |
|---|---|---|---|---|
| | **Relies = Yes** | **Relies = No** | **Relies = Yes** | **Relies = No** |
| | **Mean (Standard Deviation)** | | | |
| **Variable** | | | | |
| Duration (in months) | 123 (102) | 75 (90) | 116 (94) | 58 (66) |
| Real Wage* ($ per week) | 614 (449) | 442 (422) | 351 (252) | 229 (204) |
| Hours (per week) | 48 (8) | 45 (11) | 42 (9) | 34 (13) |
| Prev. completed employment episodes | 0.8 (1.1) | 1.0 (1.3) | 0.9 (1.2) | 0.9 (1.2) |
| | **Percent** | | | |
| Diagnose | 86 | 71 | 86 | 71 |
| Worker Diagnosed (Diag_slf) | 22 | 14 | 54 | 25 |
| Family Member Diagnosed (Diag_fam) | 64 | 57 | 32 | 46 |
| Nonwhite | 7 | 7 | 8 | 6 |
| Married | 88 | 81 | 62 | 78 |
| Self-Employed | 6 | 17 | 4 | 9 |
| **Education**† | | | | |
| 12 years or less | 23 | 20 | 35 | 34 |
| Some college or Technical degree | 44 | 64 | 27 | 38 |
| Bachelors degree | 13 | 8 | 19 | 18 |
| Some post graduate or graduate degree | 20 | 8 | 19 | 10 |
| **Industry** | | | | |
| Manufacturing | 33 | 17 | 13 | 8 |
| Construction, Agriculture | 6 | 19 | – | – |
| Transportation, Communication, Utilities‡ | 8 | 6 | 6 | 3 |
| Trade | 14 | 21 | 17 | 27 |
| Service, Finance | 34 | 34 | 56 | 59 |
| Public Administration | 5 | 3 | 8 | 3 |
| **Occupation** | | | | |
| Professional | 55 | 34 | 42 | 26 |
| Clerical, Service | 20 | 27 | 50 | 68 |
| Blue Collar¶ | 25 | 39 | 8 | 6 |
| **No. Observations**: | 333 | 512 | 182 | 734 |

* The base year for the real wage is 1983.
† Education is the highest level an individual had received at the time of the survey.
‡ Women in construction and agriculture are included in this category due to the small number of observations.
¶ Blue collar includes agriculture, construction, and manufacturing occupations.
Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

**Table 2. Estimated Hazard Functions for Men**

| Variable | Relies = Yes | | Relies = No | |
|---|---|---|---|---|
| | **Coefficient α (S.E.)** | **Percentage Change in Hazard Rate¶** | **Coefficient β (S.E.)** | **Percentage Change in Hazard Rate¶** |
| Diagnose | –1.73 (0.27)* | –82 | –1.20 (0.19)* | –70 |
| | | | | |
| Log Real Wage | –0.90 (0.23)* | –59 | –0.42 (0.15)* | –34 |
| Hours | 0.01 (0.02) | 1 | –0.02 (0.01)** | –2 |
| Nonwhite | –0.17 (0.45) | –16 | –0.75 (0.34)** | –53 |
| Married | –0.38 (0.31) | –32 | –0.53 (0.20)* | –41 |
| Prev. completed employment episodes | 0.34 (0.09)* | 41 | 0.27 (0.07)* | 31 |
| Self-Employed | –0.76 (0.57) | –53 | –0.38 (0.29) | –32 |
| | | | | |
| **Education** | | | | |
| 12 years or less† | – | | – | |
| Some college or Technical degree | –0.11 (0.30) | –10 | 0.10 (0.21) | 11 |
| Bachelors degree | –0.24 (0.47) | –21 | 0.13 (0.37) | 14 |
| Some post graduate or Graduate degree | 0.16 (0.43) | 17 | 0.42 (0.39) | 52 |
| | | | | |
| **Industry** | | | | |
| Manufacturing† | – | | – | |
| Construction, Agriculture | 1.13 (0.45)** | 210 | 0.36 (0.27) | 43 |
| Trans, Communication, Utilities | 0.41 (0.50) | 51 | 0.11 (0.39) | 12 |
| Trade | 0.89 (0.38)** | 144 | 0.34 (0.28) | 41 |
| Service, Finance | 1.06 (0.34)* | 189 | 0.00 (0.27) | 0 |
| Public Administration | 0.29 (0.72) | 34 | –0.52 (0.52) | –41 |
| | | | | |
| **Occupation** | | | | |
| Professional† | – | | – | |
| Clerical, Service | 0.13 (0.32) | 14 | 0.24 (0.25) | 27 |
| Blue Collar | 0.43 (0.36) | 54 | 0.20 (0.26) | 22 |
| No. Observations**:** | 333 | | 512 | |

* $p < 0.01$; ** $p < 0.05$; *** $p < 0.1$

† Omitted category. (Education is the level the individual had received at the time of the survey)

¶ Calculated as $[\exp(\alpha \text{ or } \beta) - 1] \times 100$.

Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

**Table 3. Estimated Hazard Functions for Women**

| Variable | Relies = Yes | | Relies = No | |
|---|---|---|---|---|
| | Coefficient $\alpha$ (S.E.) | Percentage Change in Hazard Rate¶ | Coefficient $\beta$ (S.E.) | Percentage Change in Hazard Rate¶ |
| Diagnose | –1.79 (0.38)* | –83 | –1.30 (0.14)* | –73 |
| Log Real Wage | –1.27 (0.38)* | –72 | –0.55 (0.13)* | –42 |
| Hours | 0.05 (0.02)* | 5 | 0.00 (0.01) | 0 |
| Nonwhite | 0.73 (0.48) | 108 | –0.37 (0.30) | –31 |
| Married | –0.21 (0.31) | –19 | –0.45 (0.15)* | –36 |
| Prev. completed employment episodes | 0.09 (0.14) | 9 | 0.20 (0.05)* | 22 |
| Self-Employed | –1.93 (1.10)*** | –86 | –0.73 (0.29)** | –52 |
| **Education** | | | | |
| 12 years or less† | – | | – | |
| Some college or Technical degree | 0.32 (0.42) | 38 | –0.03 (0.15) | –3 |
| Bachelors degree | 1.03 (0.44)** | 180 | –0.09 (0.21) | –9 |
| Some post graduate or Graduate degree | 0.44 (0.61) | 55 | –0.22 (0.27) | –20 |
| **Industry** | | | | |
| Manufacturing† | – | | – | |
| Construction, Agriculture, Trans., Communication, Utilities | –0.43 (0.98) | –35 | –0.24 (0.47) | –21 |
| Trade | –0.16 (0.60) | –15 | –0.11 (0.30) | –10 |
| Service, Finance | –0.15 (0.61) | –14 | –0.10 (0.28) | –10 |
| Public Administration | 0.23 (0.74) | 26 | –0.33 (0.51) | –28 |
| **Occupation** | | | | |
| Professional† | – | | – | |
| Clerical, Service | 0.41 (0.46) | 51 | 0.19 (0.19) | 21 |
| Blue Collar | –0.77 (0.82) | –54 | –0.09 (0.41) | –9 |
| No. Observations**:** | 182 | | 734 | |

* $p < 0.01$; ** $p < 0.05$; *** $p < 0.1$

† Omitted category. (Education is the level the individual had received at the time of the survey)

¶ Calculated as $[\exp(\alpha \text{ or } \beta) - 1] \times 100$.

Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

**Table 4. Estimated Hazard Functions: Illness-Insurance Interaction Term Specification**

| Variable | All Men: Coefficient γ (S.E.) | All Men: Percentage Change in Hazard Rate¶ | All Women: Coefficient γ (S.E.) | All Women: Percentage Change in Hazard Rate¶ |
|---|---|---|---|---|
| Rely | –0.18 (0.27) | –17 | –0.70 (0.32)** | –50 |
| Diagnose | –1.27 (0.19)* | –72 | –1.31 (0.14)* | –73 |
| DiagRely | –0.26 (0.32) | –23 | –0.33 (0.40) | –28 |
| | | | | |
| Log Real Wage | –0.54 (0.13)* | –42 | –0.62 (0.12)* | –46 |
| Hours | –0.01 (0.01)*** | –1 | 0.01 (0.01) | 1 |
| Nonwhite | –0.60 (0.29)** | –45 | –0.21 (0.27) | –19 |
| Married | –0.48 (0.18)* | –38 | –0.44 (0.14)* | –36 |
| Prev. completed employment episodes | 0.31 (0.06)* | 36 | 0.19 (0.05)* | 21 |
| Self-Employed | –0.48 (0.28)*** | –38 | –0.82 (0.29)* | –56 |
| | | | | |
| **Education** | | | | |
| 12 years or less† | – | | – | |
| Some college or technical degree | 0.08 (0.18) | 8 | 0.02 (0.15) | 2 |
| Bachelors degree | 0.06 (0.31) | 6 | 0.06 (0.19) | 6 |
| Some post graduate or graduate degree | 0.48 (0.30) | 62 | –0.11 (0.25) | –10 |
| | | | | |
| **Industry** | | | | |
| Manufacturing† | – | | – | |
| Construction, Agriculture | 0.59 (0.25)** | 80 | – | |
| Trans. Communication, Utilities‡ | 0.25 (0.33) | 28 | –0.24 (0.44) | –21 |
| Trade | 0.62 (0.24)** | 86 | –0.07 (0.28) | –7 |
| Service, Finance | 0.36 (0.22) | 43 | –0.08 (0.27) | –8 |
| Public Administration | –0.23 (0.45) | 21 | 0.02 (0.41) | 2 |
| | | | | |
| **Occupation** | | | | |
| Professional† | – | | – | |
| Clerical, Service | 0.23 (0.21) | 26 | 0.23 (0.18) | 26 |
| Blue Collar | 0.29 (0.22) | 34 | –0.12 (0.38) | –11 |
| No. Observations: | 845 | | 916 | |

***** $p < 0.01$; ** $p < 0.05$; *** $p < 0.1$

† Omitted category. (Education is the level the individual had received at the time of the survey)

‡ Women in construction and agriculture are included along with this category due to the small number of observations.

¶ Calculated as $[\exp(\gamma) - 1] \times 100$.

Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

**Table 5. Estimated Hazard Functions for Men: Source of Illness Specification**

| Variable | Relies = Yes: Coefficient α (S.E.) | Relies = Yes: Percentage Change in Hazard Rate¶ | Relies = No: Coefficient β (S.E.) | Relies = No: Percentage Change in Hazard Rate¶ |
|---|---|---|---|---|
| Worker Diagnosed (diag_slf) | –1.41 (0.34)* | –76 | –0.39 (0.27) | –32 |
| Family Member Diagnosed (diag_fam) | –1.88 (0.29)* | –85 | –1.50 (0.21)* | –78 |
| | | | | |
| Log Real Wage | –0.84 (0.23)* | –57 | –0.38 (0.15)** | –32 |
| Hours | 0.01 (0.02) | 1 | –0.02 (0.01)** | –2 |
| Nonwhite | –0.20 (0.45) | –18 | –0.75 (0.35)** | –53 |
| Married | –0.31 (0.31) | –27 | –0.34 (0.20)*** | –29 |
| Prev. completed employment episodes | 0.30 (0.09)* | 34 | 0.26 (0.07)* | 30 |
| Self-Employed | –0.60 (0.57) | –45 | –0.41 (0.29) | –34 |
| | | | | |
| **Education** | | | | |
| 12 years or less† | – | | – | |
| Some college or technical degree | –0.07 (0.31) | –7 | 0.34 (0.22) | 41 |
| Bachelors degree | –0.25 (0.47) | –22 | 0.18 (0.37) | 20 |
| Some post graduate or graduate degree | 0.14 (0.43) | 15 | 0.54 (0.40) | 72 |
| | | | | |
| **Industry** | | | | |
| Manufacturing† | – | | – | |
| Construction, Agriculture | 1.21 (0.46)* | 235 | 0.30 (0.27) | 35 |
| Trans., Communication, Utilities | 0.33 (0.51) | 39 | 0.00 (0.39) | 0 |
| Trade | 0.87 (0.38)** | 139 | 0.24 (0.28) | 27 |
| Service, Finance | 1.04 (0.33)* | 183 | –0.08 (0.27) | –8 |
| Public Administration | 0.31 (0.71) | 36 | –0.68 (0.52) | –49 |
| | | | | |
| **Occupation** | | | | |
| Professional† | – | | – | |
| Clerical, Service | 0.07 (0.33) | 7 | 0.19 (0.26) | 21 |
| Blue Collar | 0.36 (0.36) | 43 | 0.10 (0.26) | 11 |
| No. Observations: | 333 | | 512 | |

* $p < 0.01$; ** $p < 0.05$; *** $p < 0.1$

† Omitted category. (Education is the level the individual had received at the time of the survey)

¶ Calculated as $[\exp(\alpha \text{ or } \beta) - 1] \times 100$.

Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

**Table 6. Estimated Hazard Functions for Women: Source of Illness Specification**

| Variable | Relies = Yes: Coefficient α (S.E.) | Relies = Yes: Percentage Change in Hazard Rate¶ | Relies = No: Coefficient β (S.E.) | Relies = No: Percentage Change in Hazard Rate¶ |
|---|---|---|---|---|
| Worker Diagnosed (diag_slf) | –1.55 (0.40)* | –79 | –1.32 (0.18)* | –73 |
| Family Member Diagnosed (diag_fam) | –2.35 (0.53)* | –91 | –1.29 (0.15)* | –73 |
| | | | | |
| Log Real Wage | –1.26 (0.38)* | –72 | –0.55 (0.13)* | –42 |
| Hours | 0.06 (0.02)* | 6 | 0.00 (0.01) | 0 |
| Nonwhite | 0.69 (0.48) | 99 | –0.37 (0.30) | –31 |
| Married | 0.04 (0.35) | 4 | –0.45 (0.15)* | –36 |
| Prev. completed employment episodes | 0.08 (0.14) | 8 | 0.20 (0.05)* | 22 |
| Self-Employed | –1.96 (1.11)*** | –86 | –0.73 (0.29)** | –52 |
| | | | | |
| **Education** | | | | |
| 12 years or less† | – | | – | |
| Some college or technical degree | 0.31 (0.42) | 36 | –0.03 (0.15) | –3 |
| Bachelors degree | 1.20 (0.45)* | 232 | –0.10 (0.21) | –10 |
| Some post graduate or graduate degree | 0.53 (0.61) | 70 | –0.21 (0.27) | –19 |
| | | | | |
| **Industry** | | | | |
| Manufacturing† | – | | – | |
| Const., Agriculture, Transportation, Communication, Utilities | –0.59 (1.00) | –45 | –0.24 (0.47) | –21 |
| Trade | –0.23 (0.61) | –21 | –0.10 (0.30) | –10 |
| Service, Finance | –0.10 (0.63) | –10 | –0.10 (0.28) | –10 |
| Public Administration | 0.02 (0.76) | 2 | –0.33 (0.52) | –28 |
| | | | | |
| **Occupation** | | | | |
| Professional† | – | | – | |
| Clerical, Service | 0.43 (0.46) | 54 | 0.19 (0.19) | 21 |
| Blue Collar | –0.82 (0.84) | –56 | –0.09 (0.41) | –9 |
| No. Observations: | 182 | | 734 | |

* $p < 0.01$; ** $p < 0.05$; *** $p < 0.1$

† Omitted category. (Education is the level the individual had received at the time of the survey)

¶ Calculated as $[\exp(\alpha \text{ or } \beta) - 1] \times 100$.

Source: Robert Wood Johnson Barriers to Insurance Survey, 1994.

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