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The Effect of Parental Medicaid Expansions on Job Mobility

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Abstract

In this paper, we examine the effect of parental Medicaid expansions on job mobility. If expanded Medicaid eligibility makes it easier for a person to have health coverage between jobs, we expect it to reduce “job lock” that occurs for workers with employer-provided health insurance. Expanded eligibility could also decrease mobility among those in jobs without health insurance, since they experience less pressure to move to an insured job (“job push”). We find strong evidence that expanded eligibility reduces job lock among unmarried women but not men or married women, and only weak evidence of reduced job push among men.

Keywords

health insurance; job lock; job mobility; Medicaid

All around America, families are grappling with health-care concerns. They wonder if they’ll have insurance at a price they can afford. ... Some feel trapped in jobs they don’t like out of fear of losing their health insurance.

– Karl Rove, Wall Street Journal, Sept. 18, 2007

An estimated 18,000 Americans die every year because they can’t afford or can’t qualify for health insurance. ... Not to mention all the people who are stuck in jobs they hate because they don’t dare lose their current insurance.

– Barbara Ehrenreich, The Huffington Post, Sept. 20, 2007

I. Introduction

A major feature of the health insurance market in the U.S. is that the large majority of privately-held insurance is offered through the workplace. In 2005, 88 percent of privately-insured individuals obtained their insurance coverage through the workplace, either from their own or their spouses’ employment (U.S Census Bureau, 2007). The incentive for employers to provide health insurance is two-fold: first, the workplace provides a pooling mechanism that employers can use to reduce adverse selection and lower administrative expenses; and second, tax laws explicitly encourage the provision of health insurance in the workplace by allowing firms and workers to exclude the value of the insurance from workers’ incomes, for both income and payroll purposes. However, this process of linking employment and employer-provided health insurance (EPI) may create labor market distortions if workers choose to stay in less-preferred jobs for fear of losing insurance coverage; this type of reduction in worker mobility is called “job lock.” More formally, job lock occurs if individuals do not change jobs when new

JEL classification numbers: I1, I3, J6

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employment opportunities with higher match-specific productivities arise because wages do not perfectly offset the differences in the valuation of health insurance across jobs (Gruber and Madrian, 1994). Thus job lock artificially lowers job mobility. At the same time, EPHI may also artificially increase job mobility if workers without EPHI leave otherwise well-matched jobs in search of (perhaps less suitable) jobs with health coverage. This less-studied phenomenon has been coined “job push” (Anderson, 1997). In this paper we will focus on the possible effects of public health insurance changes on job lock and job push, generating separate estimates by gender and focusing on low-income workers.

Some researchers have found evidence of job lock (e.g., Madrian, 1994; Rashad and Sarpong, 2006), while others find no such evidence (e.g., Kapur, 1998; Berger, *et al.*, 2004). However, even if all researchers agreed that job lock exists, job lock may not affect individuals uniformly along the income distribution. Table 1 shows that the proportion of jobs with EPHI for workers ages 20–54 increases (roughly monotonically) with income. Since job lock affects only workers in jobs with EPHI, it follows that the magnitude of job lock potentially decreases with income. High-income individuals may be less affected by job lock since they are more likely to obtain EPHI across a job change (though high-income individuals in jobs without EPHI may be the most affected by job push, for the same reason). Consequently, job lock may be more severe for low-income individuals, for whom the provision of EPHI is less likely.¹ Compounding this issue is the fact that even when EPHI may be available to a worker, those who are low-income tend to be less able to afford the coverage, and coverage may be of lower quality than that offered to higher-income workers.² If job lock is largely a low-income problem, it may be difficult to detect in any study that samples from the entire income distribution, which may be one reason why some previous research has failed to find evidence of job lock.

The Personal Responsibility and Work Opportunities and Reconciliation Act of 1996 (PRWORA) removed the eligibility link between public health insurance (Medicaid) and cash welfare (Aid to Families with Dependent Children, or AFDC).³ This reform provided states with the opportunity to expand Medicaid eligibility to working families with incomes beyond previous AFDC-related limits. In particular, many states expanded Medicaid eligibility to previously-ineligible adults (Aizer and Grogger, 2003).

In this paper, we examine whether these Medicaid expansions alleviate job lock. In theory, expanding Medicaid eligibility to previously-ineligible parents should reduce job lock by increasing their ability to switch jobs without the risk of being uninsured. The parental Medicaid expansions are particularly useful to study since they target the poor and near-poor, the populations most likely affected by job lock. Since public assistance tends to have its strongest impact on single-parent female-headed families, we estimate separate effects for men and women, with further disaggregation into married and unmarried women.⁴ We also consider whether Medicaid expansions may mitigate job push by making it easier for workers to stay in jobs without EPHI.

II. Public policy and job lock

Within the last two decades, there has been considerable legislative attention focused on reducing job lock. For example, the Consolidated Omnibus Budget Reconciliation Act of 1985

¹Additionally, job lock may be more severe for low-income individuals since they are less able to pay for health insurance from a non-employer source.

²We thank an anonymous referee for pointing this out.

³Analogous to job lock, the previous link between Medicaid and AFDC most likely caused “welfare lock,” since individuals may have been unwilling to leave welfare if they lost Medicaid as well.

⁴The population of unmarried fathers who live with their children is too small to disaggregate married and unmarried fathers in the analysis.

(COBRA) and the Health Insurance Portability and Accountability Act of 1996 (HIPAA) were both designed to increase the continuity or portability of health insurance across job switches. However, despite these laws, job lock may still persist. While COBRA requires employers who sponsor group health insurance to allow employees to purchase insurance even after leaving a job, the employee must pay the full cost of coverage, which is substantially higher than what an individual paid as an employee (Gruber and Madrian, 1994). Thus, even if an individual has access to group health insurance, the required premiums may be prohibitively high to impart any real benefit.⁵ In much the same way, while HIPAA limits the use of pre-existing condition exclusions, it does not eliminate all pre-existing condition exclusions, nor does it require group health plans to offer specific employee benefits. In short, while COBRA and HIPAA may serve to increase health insurance portability and reduce job lock, one would not expect these laws to eliminate all job lock, particularly for low-income workers.

It is possible that workers whose job lock was not mitigated by COBRA or HIPAA may experience some relief through increased availability of public health insurance. In the last 10 years, states have utilized increased flexibility in the design of their Medicaid programs. Many states have chosen to make Medicaid more easily available to working parents by raising earnings limits, with the hope that this will prevent a “welfare trap” in which non-working parents have little incentive to work due to the potential loss of many public benefits at once. Table 2 lists the Medicaid earnings thresholds for working parents by state for the years 1996 through 2003, sorted by their 1996 thresholds.⁶ Two important patterns are evident in this table. First, there is very wide variation in earnings limits across states, even before major expansions, far beyond what might be expected due to differences in local wages or cost of living. Second, many states have taken the opportunity to expand Medicaid earnings limits for working parents since 1996, but there is also variation here: 11 states have made no changes, 15 states have changed their thresholds only once, and the remaining states have increased their thresholds multiple times during this period (in many cases tying the threshold to some fraction of the federal poverty guidelines so that it increases a bit each year). We will examine the effects of these thresholds—and changes in them—on job lock and job push among low income workers.

III. Problems estimating job lock

A number of researchers have estimated the magnitude of job lock, and the effects of public policies on job lock, over the last 15 years. While we will mention only a few here to emphasize the place of our analysis in the wider literature, more comprehensive reviews of the literature can be found in Gruber and Madrian (2002) and Rashad and Sarpong (2006).

One of the empirical difficulties of identifying job lock is the endogeneity of labor supply and health insurance decisions. Most previous research utilizes a difference-in-differences strategy to estimate the extent of job lock within a “treatment effects” framework (e.g., Madrian, 1994). These papers obtain identification by assuming the exogeneity of different “demand-shifters” for EPHI that can define “treatment” and “control” groups, such as having other sources of insurance (e.g., a sample of married men with wives who do or do not have health insurance that will cover their husbands) and differences in expected health expenditures (e.g., a sample of married men whose wives are or are not pregnant, or a sample of people with and without chronic health conditions in their families). In studies with this structure, the authors consider the interaction term between own EPHI and the demand-shifter as the proper test for job lock, and calculate the magnitude of job lock as the relative reduction in the voluntary

⁵However, even though the continuation coverage may cost more than what a person paid as an employee, it will most likely be substantially less than what a person would have to pay in the individual market (Gruber and Madrian, 1994).

⁶The sources of these data—including surveys by non-profits and government records—are described in detail in Hamersma (2007). Two states are not included, for reasons explained in the table notes.

turnover rate (or the increase in average job tenure) between the treatment and control groups (e.g., Berger, *et al.*, 2004; Rashad and Sarpong, 2006). Madrian (1994) estimates that job lock reduces the rate of voluntary turnover for men by about 25 percent (from 16 percent to 12 percent per year), though more recent work by Berger, *et al.* (2004) does not find evidence of job lock. However, in any case, if the demand-shifters of spousal insurance availability, pregnancy, or family health conditions are endogenous, this approach does not identify the extent of job lock.

A second potential endogeneity problem is the almost certain positive correlation between EPHI and other positive job characteristics that affect job turnover. Specifically, one might expect that a job that offers EPHI may have other positive job characteristics that make it a “better” job than one that does not offer EPHI. Therefore, Gilleskie and Lutz (2002) propose an estimation strategy that considers both whether an individual received an offer for EPHI, and whether the individual decided to take up the offer. By comparing individuals with EPHI to individuals without EPHI (but whose employers offered EPHI), they are able to compare individuals who are in “similar” jobs, since both jobs offer EPHI, and therefore are assumed to have other similar positive job characteristics. By including the “EPHI offered” variable, Gilleskie and Lutz obtain an estimate of job lock that, they argue, is unbiased through correlation with positive job characteristics. They find no evidence of job lock among married men, and a modest amount of job lock (10–15 percent) among unmarried men.

Gruber and Madrian (1994) avoid these endogeneity problems by focusing on identifying changes in job lock rather than measuring the extent of job lock directly. They use an exogenous source of variation: “continuation of coverage” mandates (the COBRA legislation), which allow individuals to temporarily purchase health insurance from their previous employers after leaving their jobs. They find that these mandates, which decrease the likelihood of being uninsured across job switches, increase the job mobility of working men: one year of continuation benefits increases job mobility by approximately 10 percent. Given the estimates of the total magnitude of job lock in Madrian (1994) and Gilleskie and Lutz (2002), a substantial share of job lock is apparently eliminated. They conclude that job lock arises from short-run concerns over portability and not long-run problems.

Sanz-de-Galdeano (2006) also examines the potential mitigating effects of government policy on job lock. Her study uses the 1996 panel of the Survey of Income and Program Participation to examine the effects of HIPAA, which (among other things) improved prospects for job mobility among people with pre-existing conditions. Many states passed similar provisions prior to federal passage of HIPAA, so she is able to examine HIPAA’s potential effects across states with different initial policies. The study finds no evidence that HIPAA reduced job lock. In contrast, Bansak and Raphael (2005), also using data from the Survey of Income and Program Participation, find that the introduction of the State Children’s Health Insurance Program leads to a six percentage point increase in the likelihood of a job separation among workers without insured spouses, with no comparable increase among workers with insured spouses.

If job lock is largely a short-run portability problem, as suggested by Gruber and Madrian (1994), then parental Medicaid expansions may alleviate job lock in a similar manner to continuation of coverage mandates, by reducing an individual’s likelihood of being uninsured across job switches. Previous studies (e.g., Aizer and Grogger, 2003; Busch and Duchovny, 2003; Dubay and Kenney, 2003) all find positive enrollment effects from the parental Medicaid expansions—both for parents and children. These results indicate that the expansions had real effects on the target individuals; therefore, the expansions may have affected job mobility as well.

IV. Data and empirical strategy

The data for this analysis come from the 1996 and 2001 panels of the Survey of Income and Program Participation (SIPP). The SIPP is a nationally-representative panel survey designed to study, among other things, program eligibility and participation. The SIPP oversamples the low-income population, which makes the SIPP particularly appropriate for this study. Each panel is divided into four-month periods, called “waves.”⁷ In each wave, respondents are asked a series of questions regarding each of the previous four months, including information for up to two jobs per wave. Using this information, we are able to determine if an individual remained with or separated from their main job in a given wave.⁸ We then use only one observation per person per wave (using their covariates from the first month of the wave) so that each job separation for a given person is counted only once. We also observe start- and end-dates for each job; thus, we are able to determine tenure for every job.

Table 3 provides descriptive statistics for our sample. We limit our sample to employed men and women aged 20–54, who were not self-employed or receiving disability payments.⁹ The sample excludes those without children in their family, since many states’ Medicaid expansions apply only to parents (not to adults without dependents).¹⁰ We further restrict our sample to workers whose incomes were between 50 and 200 percent of the FPG when we first observe them in the data, since these individuals are most likely to be affected by the expansions. Finally, we exclude individuals from a few states: Hawaii, which has mandated health insurance for employees and Minnesota and Oregon, which have state-run programs that make the role of Medicaid less clear. The SIPP also clusters some states together for confidentiality purposes, so we are unable to include these smaller states (Maine, North Dakota, South Dakota, Vermont, and Wyoming). The final sample consists of 16,838 male observations (on 3,836 individuals) and 20,781 female observations (on 5,582 individuals).

We analyze job mobility patterns separately for men and women in this study. Past work on job lock has often focused on men because of their more predictable labor force patterns. However, we might expect Medicaid to be particularly relevant to the decisions of single-parent households, many of which are female-headed. Since other factors determining job mobility may also differ by gender, we choose the flexible approach of performing all of our estimation separately for men and women. We introduce additional flexibility by dividing the sample of women into married and unmarried, since married women are more likely to have spousal insurance availability and thus less likely to be job locked.¹¹ This broad set of estimates will help us better understand whether there are differences between men’s and women’s responsiveness to factors expected to influence job mobility.

⁷The 1996 panel consists of 12 waves (covering four years), while the 2001 panel consists of nine waves (covering three years).

⁸Having two job records available is important, since in many cases they are sequential and we can observe the job change. However, we consider “turnover” to occur only if the “main job” in a wave ends. For most of the sample, this is defined as the job in progress when the wave begins. For a few workers that do not have a job on the first day of the wave, we identify the “main job” as the first one to begin during that wave. For those who begin the waves with two simultaneous jobs, we identify the “main job” as the one that started earlier (as long as that job is reported to have positive earnings in the current wave).

⁹We exclude the self-employed due to the typical lack of availability of group employer coverage for these workers. We exclude those receiving disability payments (Supplemental Security Income, or SSI) if the benefits are on their own behalf, since this may reflect limited work and earnings options; families with other SSI recipients in the family are kept in the data. We exclude 55–65-year-olds since their turnover behavior may be confounded by the retirement decision (Gruber and Madrian, 1995). Gruber and Madrian (1995) find a significant effect on the retirement hazard from continuation of coverage mandates. Therefore, it is possible that Medicaid expansions—which act similarly as continuation of coverage mandates by reducing uninsurance across job switches—may also affect the retirement decision in a similar manner.

¹⁰We require both a positive number of “children in the family” and a designation of “parent” for a person to be in our sample.

¹¹Unfortunately, the SIPP does not indicate whether a spouse has an available offer of insurance; we control for whether spouses are working full-time in order to proxy for the offer of insurance.

We use the SIPP data to estimate a model of voluntary job turnover. The voluntary turnover rate for the sample of men is 3.3 percent per wave (four-month period), compared with the overall (both voluntary and involuntary) turnover rate of 5.6 percent.¹² While these rates may seem fairly low, one should keep in mind that men with children have more stable employment than other demographic groups within the labor force.¹³ The voluntary turnover rate for the sample of married women is 4.0 percent per wave, with overall turnover of 6.0 percent. Unmarried women have the highest turnover rates, with a voluntary rate of 4.5 percent and an overall rate of 7.4 percent.

We choose to assess changes in job lock by modeling the probability of voluntary turnover without distinguishing between destinations (for example, one person may move to a different job and another may leave the labor force). The reason we include all types of transitions is because we expect that changing the cost of quitting (via expanding Medicaid eligibility) is likely to affect the cost-benefit analysis of a person's "marginal opportunity," whatever that next-best option might be. Identifying the marginal opportunity on an individual basis to determine whether someone suffered from job lock is not possible—for instance, even a person who quit her job in order to move to another one may have suffered from job lock but took the new job because it was substantially better than the previous one (whereas if she had not been job-locked she may have taken a more marginal opportunity earlier). In a sense, we argue that changing the Medicaid threshold may change the reservation threshold for a person quitting to take their next-best option. While we cannot observe this reservation threshold (but only realizations that results in moves), we can estimate changes in job lock in the aggregate, by looking at patterns in the voluntary turnover rate in the population we think may be affected by the change in Medicaid policy.

We examine the effect of the parental Medicaid expansions on job mobility using the following probit regression framework:

$$\Pr(\text{quit}_{ijt}) = \Phi(\beta_1 M_{ijt} + \beta_2 X_{ijt} + \beta_3 \text{labormkt}_{jt} + \beta_4 \text{state}_j + \beta_5 \text{time}_t),$$

where i indexes individuals, j indexes states, and t indexes time. The dependent variable, quit_{ijt} , equals one if individual i living in state j voluntarily left a job at time t , and zero otherwise. X_{ijt} is a vector of individual demographics (including race, marital status, age and age squared, education, number of children ages 6–18 and ages 0–5, an indicator for having a disabled family member, and an indicator for having a spouse who works full-time) and job characteristics (hourly wage, tenure, union status, occupation, and industry). The variable labormkt_{jt} includes state-by-month employment growth and unemployment rate, state_j is a set of state fixed effects, time_t is a set of month and year fixed effects, and $\Phi(\bullet)$ denotes the standard normal cumulative distribution function.¹⁴ To calculate hourly wages for those who do not report being paid by the hour, we divide total salary earnings over the entire wave by usual hours per week multiplied by the number of weeks in the wave the individual was employed.

¹⁵ Note that the demographic and job characteristics are the values observed at the start of the

¹²“Voluntary” turnover is defined as leaving a job for any of the following reasons: retirement, childcare problems, family or personal obligations, school or training, job was temporary and ended, quit to take another job, or unsatisfactory work arrangements. “Involuntary” turnover is defined as: on layoff, own illness or injury, discharged or fired, employer bankrupt or sold business, slack work or business conditions, or quit for some other reason.

¹³Men in general (not restricted to fathers) have monthly turnover rates of about three to four percent during this time period, but aggregation to a four-month period is not straightforward since we do not expect turnover to involve completely distinct workers every month.

¹⁴We also try an alternative specification with a full set of year-by-month fixed effects. Results are similar, but in the smaller samples many observations need to be dropped due to small cell sizes. To maintain sample sizes, we use the more parsimonious specification for our main reported results.

wave, and the regression models the probability that an individual voluntarily leaves a job in a particular wave.¹⁶

The variable of greatest interest is the Medicaid variable M_{ijt} , which we define as the monthly Medicaid threshold for the individual's state in that month. This threshold is assigned by family size; we were able to assemble reliable thresholds for families with up to 6 people, who account for over 98% of our overall sample.¹⁷ Unlike previous work on parental Medicaid expansions that code them as binary "expansion" indicators (e.g., Aizer and Grogger, 2003), this approach has the advantage of exploiting variation in the size of the expansions and handling multiple expansions easily. Rather than estimating the effect of a "Medicaid expansion" in general, our approach is designed to answer the more precise question, "How does job mobility change in response to an \$X change in the Medicaid threshold?"

The expansion of Medicaid—both within and across states—allows us to disentangle the effects of Medicaid thresholds from more general effects of living in a particular state at a particular time. We include state fixed effects to account for any time-invariant differences across states. The time fixed effects control for national annual time trends as well as seasonal effects (by month).¹⁸ Under the assumption that the expansions were exogenous, the inclusion of these fixed effects identifies the effect of Medicaid thresholds on job turnover propensity. We examine this assumption of exogeneity in supplemental tests following the estimation.

V. Results

A. Basic results

Table 4 summarizes the basic regression results when we assess the effects of the Medicaid expansions on the job mobility of workers with EPHI (who are by definition the only workers who can experience job lock). Higher wages and longer job tenure have negative effects on voluntary job turnover, as would be expected, and unions also have a precisely-measured negative effect on turnover for men. A college education is associated with much higher turnover for men: a college graduate's predicted voluntary turnover rate is 1.6 percentage points higher than a high school dropout (the omitted category), and this is in a sample with a baseline voluntary turnover rate of only about two percent or less. This may reflect a more-educated worker's expectation that alternative jobs also carry EPHI. Alternatively, some more-educated workers may appear in our low-income sample due to a temporary low-earnings job, such that simple mean reversion would suggest more mobility among this group. The educational results for women vary according to marital status; there is little evidence of an educational effect for married women, but unmarried women with "some college" or "college or more" have estimated effects similar in magnitude to those found for male college graduates (though only the "more college" estimate is statistically significant at the five percent level). State employment growth has a statistically significant coefficient for men but not women, with a

¹⁵Gruber and Madrian (1994) use this same procedure to calculate wages for workers who report not being paid an hourly wage. To explicitly account for this in the estimation, we allow these calculated wages to have a separate slope and intercept from directly reported wages. We also drop observations whose wage is outside a reasonable range (below \$4.75 per hour—the minimum wage in 1996—or above \$100 per hour), under the assumption that this represents an inaccurate measurement of hourly pay. This eliminates about five percent of our low-income sample.

¹⁶We ignore concerns such as the endogeneity of educational attainment. Most previous research considers these as second-order issues, and therefore we do not examine them.

¹⁷Almost all state-months could be calculated exactly based on either the FPG or based on state formulas and payments standards (for those states still using something akin to the AFDC formula utilized prior to 1996). A few state-months were interpolated or approximated based on available information. Details on the assembly of these data, including references to the multiple sources used, are available from the authors upon request.

¹⁸This is operationalized as a set of year indicators and a set of month indicators. Results using year-by-month indicators are quite similar, but the regressions are more demanding of the data and many indicators must be dropped to obtain identification.

magnitude much larger than the women's estimates, suggesting more responsiveness of male labor supply to state labor demand conditions.

The Medicaid threshold does not appear to be correlated with voluntary job turnover among men or among married women with EPHI, but it has strong explanatory power for unmarried women. While the estimates are small and statistically insignificant for men and married women, the estimates indicate that unmarried women's turnover rates increase by 0.11 percentage points per \$100 change in the threshold, which is about a 4 percent increase in turnover relative to the baseline. The average size of a monthly Medicaid threshold change (in months where a change is made) is \$136, so an average change in the threshold would generate a $(0.11 * 1.36) = 0.15$ percentage point change in the employment rate—more than 5 percent increase relative to the baseline. This result suggests that the Medicaid expansions have not had a large role in alleviating job lock for men or married women, but have been important for increasing the job mobility of unmarried women. Given higher rates of Medicaid use among single mothers, this result is quite sensible.

Beyond affecting job lock, the Medicaid expansions may have helped alleviate job push among those without EPHI. We would expect this group to generally have a high mobility rate if they are looking for a job with EPHI, and this is reflected in a higher baseline quit rate of about six percent. The Medicaid expansions may lower this rate by allowing people to stay at their current employers and receive Medicaid benefits (where in the past they may have had to quit in order to reduce their income enough to qualify or to find an alternative job with EPHI even if the quality of the new job match were lower).

Table 5 reports estimates of the effects of the Medicaid expansions on quit rates of those who began their current wave of work without EPHI. The roles of the covariates are fairly similar to the EPHI sample, although union membership now lacks statistical significance and log wages are only statistically significant for men and not women. These differences compared to the EPHI sample provide support for our decision to estimate the equations separately rather than using a simple EPHI indicator (as sometimes done in previous work). None of the estimated effects of the Medicaid expansions are statistically significant at the five percent level. These insignificant estimates are consistent with estimates using uninsured workers in Gruber and Madrian (1994) (although they do not discuss the notion of job push explicitly). However, our coefficient for the sample of men is marginally statistically significant and of the expected sign. The estimate suggests that men may more likely to stay in a job in the presence of higher Medicaid thresholds (with about a three percent decrease in turnover per \$100 increase in the threshold). We test the robustness of this finding later in the paper.

It is possible that our Medicaid thresholds are insufficient for describing the changes in public health insurance options available to families, particularly because the State Children's Health Insurance Program (SCHIP) came into existence in 1997 and experienced a great deal of expansion over our sample period. Therefore, we also generate versions of Tables 4 and 5 that included the SCHIP income threshold that would apply to the youngest child in each family, by family size and month. The results (available upon request) indicate no statistically significant role for SCHIP thresholds in alleviating job lock independently of parental Medicaid, and the two thresholds are only jointly statistically significant for unmarried women, reflecting similar results to Table 4. In the alternate version of Table 5, the SCHIP and Medicaid thresholds are never jointly statistically significant at even the ten percent level in any specification. Hence it appears that our results using the parental Medicaid threshold alone are not substantially altered by controlling for SCHIP.

Our estimates of the effects of the Medicaid expansions suggest that unmarried mothers have experienced a meaningful reduction in job lock as Medicaid thresholds have increased. There

is no evidence of reduced job lock for men or married women, and only suggestive evidence of reduced job push (for men only).¹⁹ There could be reasons for this other than, of course, that the expansions truly had such limited effects. First, because we are focusing on a relatively small subset of the population (i.e., near-poor individuals), the sample size is not as large as previous studies, making it difficult to generate precise estimates of the effect.²⁰ Second, we are relying on relatively little variation in outcomes (turnover) to detect the effect of the expansions. For both of these reasons, it may be difficult to detect an expansion effect, even if one exists; that is, the econometric test may have low power. Therefore we believe it is appropriate to provide supplementary evidence to establish whether our findings are robust and rest on secure assumptions.

B. Falsification tests

The validity of the results rests upon the identification assumption that the state-level expansions *exogenously* increased the value of a job quit, by reducing the cost of being uninsured while switching jobs. If the assumption is true, then the state fixed effects allow us to identify the effect of the expansions. However, if the expansions are themselves responses to differences in state-level mobility differences (i.e., endogenous), then the state fixed effects are not sufficient for identification, and we have less confidence in our estimates of the relationship (or lack thereof) between Medicaid thresholds and job mobility.

We first provide two falsification tests to check whether our identification strategy has any obvious flaws. In our first falsification test, we estimate the same models we have already estimated, but restrict the sample to high-income workers (which we define as over 400 percent of the federal poverty guidelines). These workers are less likely to experience much job lock; almost all available jobs at their skill level provide insurance, or they are insured by their spouse's job. Moreover, this group is unlikely to claim Medicaid benefits (they are typically ineligible, unless there is an extenuating circumstance involving very high medical costs). For these reasons, we should expect no effect of Medicaid expansions on voluntary turnover among this population.²¹ Some workers may experience job push if they are among the few in this income range without EPHI, since they can often successfully get EPHI at a different job. However, the Medicaid thresholds are still unlikely to affect them due to ineligibility for benefits. In our second falsification test, we estimate our models again using "involuntary" separations (layoffs and firings) as the dependent variable. Since these separations are primarily decided by the firm, which does not directly benefit from changes in the Medicaid threshold, we would be surprised if they were affected by the Medicaid expansions.

The results for these two falsification tests are reported in Table 6. Panel A contains the results for the high-income sample. We find no statistically significant (at the five percent level) changes in voluntary job turnover in response to the Medicaid expansions. However, among men with EPHI, the coefficient on the Medicaid threshold is positive and statistically significant

¹⁹We also generated estimates that do not use time dummy variables in the specification. There are several reasons that time dummy variables are not always appropriate: they may cause overfitting of the model, they are not well-justified theoretically because they do not measure anything specific, and the dummies for late dates in the sample may begin to absorb some ongoing policy effects if many states have already changed their policies. If we allow the state-month labor market variables to carry the weight of the key labor market changes over time rather than including the time dummies, we find smaller estimates of job lock reduction among single women (0.0006, with a t-statistic of 1.02) as well as smaller estimates of job push reduction among men (−0.0009, with a t-statistic of 1.20). These changes indicate to us that time dummy variables provides additional controls beyond basic labor market variables.

²⁰For example, since Gruber and Madrian (1994) consider the entire population, their SIPP sample contains 155,151 observations on 29,841 individuals.

²¹Some other natural choices for a falsification sample are unfortunately not ideal in this context. Using people whose incomes are just above the eligible range would cause us to rely too strongly on a variable measured with error, hence our choice of a high-income sample instead. Those in the lowest income range who are not included in the sample (below 50% FPG) may also appear to be a plausibly "unaffected" group; however, some states began with such low Medicaid thresholds (below 50% FPG) that a subset of this group may still have been affected by the expansions.

at the ten percent level. This could suggest a reduction in job lock for this group, but the magnitude of the coefficient indicates that this estimated effect—even if it were more precisely estimated—is extremely small. Panel B reports our estimates of the effects of the expansions on involuntary job turnover. If the expansions increased job mobility by reducing job lock, then the expansions should only affect voluntary turnover, but not involuntary turnover (e.g., being fired).²² These six regressions do not provide any evidence of a Medicaid expansion effect on involuntary job separations. There does not appear to be a systematic selection or specification problem, based on these falsification tests.

C. Robustness checks

To examine the robustness of our findings, we also estimate three variations of our model that make adjustments in some key definitions. First, we replicate the results for the original sample with an alternative definition of an individual's "main" job as the job with more hours worked (instead of the job that began earlier). This change in definition, of course, only affects workers with multiple jobs in a given wave. Our results using this definition to re-examine the specifications in Table 4 indicate similar insignificant estimated effects of Medicaid on job lock for men and married women and smaller but positive, statistically significant effects for unmarried women (0.0008, with a t-statistic of 2.59). The sample and specifications analogous to Table 5 are similar to the original table, with a slightly smaller estimated coefficient for men (−0.0012 compared to −0.0017) but a slightly larger t-statistic (2.00 compared to 1.91). We thus conclude from this robustness check that there continues to be evidence for job lock reduction among unmarried women and some evidence of job push reduction among men.²³

Second, we estimate our model using two alternative low-income samples: first, all employed people whose *average* income over *all waves* was between 50 and 200 percent of the federal poverty guidelines, and second, all employed person-waves with income in this range. The first alternative sample is distinct from our original sample because inclusion is based on average income rather than starting income, which may be a better measure of long-term disadvantage. The second sample is distinct from both of the others because it retains individual person-waves based on income rather than persons. This sample thus focuses on people in need during a point in time. In this case, some individuals may be in the sample for only one or two observations, while the other samples established a set of people that were kept in the sample, and for whom we then maintained all available waves of data. The cost of focusing on individual person-waves is that there is more concern about endogenous selection of the sample, since income could itself be affected by Medicaid policy changes during the panel. However, Hamersma (2007) finds little evidence that earnings respond to changes in the Medicaid thresholds.

Using these two alternative samples yields very similar results for Tables 4 and 5.²⁴ In the job lock analysis, the statistically significant coefficient on the Medicaid threshold for unmarried women stays within close range of the original estimates, the estimates for men and women remain statistically insignificant (with coefficients averaging −0.00035 for men and 0.0006 for married women). All of the Medicaid threshold coefficients in the job push analysis are statistically insignificant.

In a final effort to examine the robustness of our findings, we examine results that use variations in the definition of the key Medicaid policy variable.²⁵ While we believe the Medicaid

²²In theory, there are reasons why employers and workers may prefer a designation of "fired" or "quit," for example for the purposes of unemployment insurance. However, we ignore these potential instances of false survey reporting.

²³Tables are available from the authors upon request.

²⁴Tables are available from the authors upon request.

²⁵All the results discussed in this section are available in tabular format upon request.

threshold itself is the most precise measure of the policy, there are some advantages to trying additional options. First, we estimate the models using a Medicaid expansion indicator instead of the threshold, defining M_{ijt} as an indicator variable that equals one if an individual lived in a state that expanded Medicaid in or after the current month, and zero otherwise. For states with multiple expansions during this time period, we identify the largest expansion and define the indicator according to that expansion. This approach potentially answers the question, “How does job mobility change in response to a Medicaid expansion?” without addressing the size of the expansion. This definition of the policy change is comparable to that used by Aizer and Grogger (2003) in their study of insurance coverage. This measure of the policy yields a statistically significant estimated reduction in job lock for unmarried women only, and no evidence of job push, supporting our initial results.

Second, we seek to avoid the possible endogeneity of family size by using a measure of the Medicaid thresholds by state-month that is not conditioned on family size. We use the threshold for a family of 3 (though the choice of a family size is inconsequential) and use this variable in place of the Medicaid threshold. The results again are consistent with our original specification: a statistically significant reduction in job lock for unmarried women, and no evidence of job push.

Finally, we experiment with using family-based measures of eligibility in place of the thresholds. We try one specification using “percent of family members eligible for Medicaid or SCHIP” and one using “percent of children eligible for Medicaid or SCHIP.” One should note that we are assigning eligibility by comparing family income to the eligibility threshold, which is likely to cause some measurement error given our limited ability to re-create each state’s full eligibility determination system (which may include, for instance, special treatment of child care expenses, or differing rules about transitional coverage when income exceeds the eligibility threshold).²⁶ When doing these analyses, we find no evidence that an increase in a family’s “percent eligible” for Medicaid/SCHIP reduces job lock for any group, but there is some limited evidence of a reduction in job push for women. There is some concern with interpreting these estimates, as the percent eligible in the family can change either because of a change in the Medicaid threshold or an increase in a child’s age (as there are three separate age categories in SCHIP with potentially different thresholds). In addition, since eligibility is ultimately endogenous (individuals can choose to earn an income above or below an eligibility threshold) we believe it is more appropriate to use the threshold by state as the measure of Medicaid policy.

In summary, our robustness checks generally support our main results that the Medicaid expansions reduced job lock among unmarried women, but not among married women or men. We also continue to find very little evidence that these expansions alleviate job push, though if there was any effect it appears to be most likely among men. The broad consistency across estimates in this section supports the validity of our main estimates, and these estimates are basically robust to an alternative definition of the worker’s “main job,” alternative mechanisms for choosing the low-income sample, and alternative measures of the Medicaid policy variable.

VI. Conclusion

With the passage of PRWORA, states gained flexibility to expand Medicaid to previously-ineligible adults. In addition to increasing enrollments, Medicaid expansions could have the spillover effect of increasing job mobility among working near-poor individuals by reducing

²⁶Unfortunately, obtaining information on these additional eligibility issues at the state-month level for the 8 years covered by our analysis is not feasible. To the extent that different states tend to have distinct approaches to policy that are consistent over time, these differences will be accounted for in the state fixed effects.

job lock. Furthermore, job lock may be more severe for low-income individuals, for whom the provision of EPHI is less likely. In this paper, we investigate this hypothesis and find strong evidence that the parental Medicaid expansions were successful in alleviating job lock among low-income prime-age unmarried women, but not among men or married women. We find little evidence that these expansions caused individuals to be more willing to stay at an uninsured job; that is, we find little to no consistent effect on job push.

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Table 1
EPHI coverage by income (% Federal Poverty Guidelines)

%FPG	Men		Married Women		Unmarried Women	
	mean	N	mean	N	mean	N
1–100	0.384	2,768	0.298	1,266	0.283	3,714
100–200	0.656	13,296	0.447	6,550	0.651	8,456
200–300	0.797	18,072	0.491	11,764	0.837	5,747
300–400	0.829	16,609	0.525	12,515	0.899	3,050
400–500	0.826	12,075	0.538	9,597	0.927	1,472
500+	0.829	19,195	0.555	16,904	0.918	1,250
Total	0.778	182,015	0.516	58,586	0.702	23,689

Notes: Authors' calculations from the 1996 and 2001 panels of the SIPP. The sample contains working parents aged 20–54 (with further restrictions, explained in detail in the section of the text describing the data). Each observation represents a person-wave, where a wave is a four-month period. EPHI = Employer-provided health insurance from own employer at start of wave. FPG = Federal poverty guidelines.

Table 2

Medicaid Income Thresholds by State, 1996–2003

state	1996	1997	1998	1999	2000	2001	2002	2003
AL	254	254	254	254	254	254	254	254
LA	280	280	280	280	280	280	280	280
AR	294	638	638	638	638	638	638	638
WV	343	343	343	343	343	343	343	343
IN	378	378	378	378	378	378	378	378
VA	381	381	381	381	381	381	391	391
MO	382	382	382	1157	1179	1219	1219	979
FL	393	806	806	806	806	806	806	806
TX	395	395	395	395	395	395	395	395
OK	397	427	427	591	591	591	591	591
ID	407	407	407	407	407	407	407	407
OH	431	431	974	974	1179	1219	1252	1272
NV	438	438	438	438	438	438	438	438
NE	455	455	669	669	669	694	734	734
MS	458	458	458	458	458	458	458	458
MD	463	471	485	523	523	523	523	523
NM	479	479	703	703	703	703	903	903
PA	493	806	806	806	806	806	806	806
DC	505	505	505	2314	2358	2438	2504	2544
CO	511	511	511	511	511	511	511	511
GA	514	514	514	514	514	514	514	514
KS	519	762	762	762	762	762	762	762
ND	521	521	521	960	988	871	891	930
NJ	533	533	533	533	1568	1621	1665	533
MI	549	549	549	549	549	774	774	774
SD	597	597	796	796	796	796	796	796
WI	607	607	607	1736	1769	1829	1878	1908
SC	614	630	658	668	668	668	709	725
KY	616	616	616	616	616	616	616	616

state	1996	1997	1998	1999	2000	2001	2002	2003
MT	631	836	836	836	836	858	858	858
NC	634	634	634	634	750	750	750	750
WA	636	636	1092	1092	1092	1092	1092	1092
NH	640	688	688	688	750	1200	1250	1250
AZ	641	641	641	641	641	1309	1342	1362
UT	658	658	658	673	673	673	673	673
NY	667	667	1139	1139	1179	1221	1665	1908
WY	680	790	790	790	790	790	790	790
TN	767	767	863	889	930	990	1009	1030
CA	820	844	1748	1826	1826	1872	1958	2022
CT	962	1082	1111	1138	1157	1859	1919	1968
MA	968	1478	1514	1539	1568	1621	1665	1691
VT	971	965	965	979	979	989	1001	1002
IA	1065	1065	1065	1065	1065	1065	1065	1065
ME	1172	1201	1228	1247	1269	1919	1968	1998
AK	1118	1736	1767	1788	1827	1884	1929	1953
IL	1131	1131	1131	1131	1131	1131	1188	1235
DE	1172	1201	1228	1247	1269	1309	1342	1362
HI	1244	1278	1308	1331	1356	1403	1440	1463
RI	2092	2145	2195	2230	2271	2345	2345	1998

Note: Table reports the “free coverage” threshold prevailing for the majority of the year for a family of 3 (sorted by 1996 threshold). In cases of a tie, the higher threshold is reported. States with no change are shaded. We drop OR and MN due to their state health insurance programs that complicate the role of Medicaid. See Hamersma (2007) for more details on data sources and construction. The full set of monthly thresholds for families of 2–6 is available upon request.

Table 3

Descriptive Statistics (weighted)

	Men			Married Women			Unmarried Women		
	Mean	S.D.		Mean	S.D.		Mean	S.D.	
Voluntary separation	0.033	0.179		0.040	0.196		0.045	0.207	
Any separation	0.056	0.230		0.060	0.238		0.074	0.262	
EPHI	0.625	0.484		0.441	0.497		0.588	0.492	
Log hourly wage	2.337	0.394		2.133	0.374		2.131	0.327	
Union	0.116	0.321		0.067	0.250		0.092	0.289	
Tenure (in years)	6.467	6.315		4.581	4.884		4.356	4.692	
Black	0.109	0.311		0.156	0.362		0.371	0.483	
Other Race (nonwh.)	0.058	0.234		0.065	0.246		0.038	0.191	
Married	0.877	0.329		1	0		0	0	
# Children 6–18	1.372	1.094		1.559	1.055		1.294	0.996	
# Children under 6	0.789	0.826		0.588	0.774		0.497	0.680	
SSI receipt in family (indicator)	0.032	0.176		0.038	0.191		0.034	0.181	
Full-time Working									
Spouse (indicator)	0.234	0.423		0.724	0.447		0	0	
Age (in years)	35.92	7.630		35.63	7.342		34.22	7.905	
Less than HS	0.290	0.454		0.202	0.401		0.158	0.365	
High school	0.392	0.488		0.416	0.493		0.413	0.492	
Some college	0.235	0.424		0.289	0.453		0.382	0.486	
College or more	0.083	0.276		0.093	0.291		0.047	0.211	
Employment Change									
(%, 4-month rate)	0.089	0.210		0.089	0.197		0.080	0.232	
Unemployment Rate	5.195	1.066		5.085	1.053		5.068	1.055	
Medicaid threshold	994.89	636.46		971.06	615.39		808.62	498.57	
Sample Size	16,838 person-waves (3,836 individuals)			8,325 person-waves (2,157 individuals)			12,456 person-waves (3,425 individuals)		

Notes: The data are from the 1996 and 2001 panels of the SIPP. The sample is working parents aged 20–54 with incomes between 50 and 200 percent of the federal poverty guidelines (upon our first observation of them) and families of size 2, 3, 4, 5, or 6. EPHI = Employer-provided health insurance from own employer.

Table 4
Effect of Parental Medicaid Expansions on Voluntary Job Turnover (EPHI sample)

	(1) Men	(2) Married Women	(3) Unmarried Women
Medicaid threshold (in \$100s)	-0.0001 (-0.219)	0.0001 (0.188)	0.0011[*] (2.143)
Log hourly wage	-0.0119 ^{**} (-3.205)	-0.0121 [*] (-2.485)	-0.0135 [*] (-2.426)
Union	-0.0054 [*] (-2.381)	-0.0003 (-0.0711)	-0.0034 (-0.868)
Tenure (in years)	-0.0013 ^{**} (-4.603)	-0.0014 ^{**} (-2.876)	-0.0020 ^{**} (-4.502)
Married	-0.0034 (-1.187)	N/A	N/A
Number of children 6–18	-0.0008 (-0.779)	0.0019 (1.503)	-0.0017 (-0.946)
Number of children under 6	-0.0019 (-1.520)	-0.0007 (-0.475)	0.0003 (0.141)
SSI receipt in family	0.0014 (0.307)	-0.0018 (-0.443)	0.018 (1.949)
Full-time working spouse	0.0005 (0.238)	-0.0005 (-0.216)	N/A
Age (in years)	0.0001 (0.146)	-0.0013 (-0.861)	0.0001 (0.0393)
Age ² /100	-0.0006 (-0.428)	0.0011 (0.501)	-0.0002 (-0.113)
High school	-0.0013 (-0.521)	0.0030 (0.782)	0.0082 (1.677)
Some college	0.0053 (1.756)	0.0028 (0.631)	0.0167 ^{**} (3.024)
College or more	0.0162 [*] (2.440)	0.0083 (1.163)	0.0179 (1.702)
% change in state employment	-0.0074 [*] (-2.444)	-0.0024 (-0.830)	-0.0017 (-0.376)
State unemployment rate (%)	-0.0011 (-0.679)	0.0007 (0.320)	-0.0021 (-0.886)
Sample size	10,287	2,650	6,861
Baseline quit rate	0.0202	0.0236	0.0266

Notes: This table reports the marginal-effect estimates from a probit regression for whether an individual voluntarily quit a job in a survey wave. Data are from the 1996 and 2001 SIPP. The sample is all person-waves with employed parents with starting incomes between 50 and 200 percent of the federal poverty guidelines and family sizes of 2, 3, 4, 5, or 6. Sample includes only those with employer-provided health insurance (EPHI) at the beginning of the wave. Clustered standard errors account for the intra-personal correlation in the error term across time and are reflected in the t-statistics (in parentheses). Alternative clustering by state or state-year does not change statistical significance of the Medicaid coefficients. Regressions include state, month, and year fixed effects, 10 industry indicators, and 8 occupation indicators. In addition, the regressions include race indicators (black, other) that were not statistically significant, and calculated hourly wages (estimated from non-hourly earnings reports) have their own slopes and intercepts (not reported). Explicitly excluded states are listed in the text. In addition, due to insufficient cell sizes the estimates for men also omit DC, IA, and NM; estimates for unmarried women omit DC, ID, LA, and OK; estimates for married women (by far the smallest of the three samples) omit AL, CT, DE, IA, ID, MD, NE, NH, NJ, NM, NV, OK, RI, and VA (as well as the 2003 year indicator). Results are qualitatively similar if all three columns are estimated using the same set of states (i.e. the largest common subset for which all three can be estimated).

* significant at five percent;

** significant at one percent;

Table 5

Effect of Parental Medicaid Expansions on Voluntary Job Turnover (non-EPHI sample)

	(1) Men	(2) Married Women	(3) Unmarried Women
Medicaid threshold (in \$100s)	-0.0017 (-1.914)	0.0002 (0.145)	-0.0009 (-0.680)
Log hourly wage	-0.0338 ** (-2.911)	-0.0245 (-1.886)	-0.0108 (-0.612)
Union	0.0081 (0.516)	-0.0186 (-1.222)	-0.0195 (-1.155)
Tenure (in years)	-0.0048 ** (-4.119)	-0.0064 ** (-4.641)	-0.0071 ** (-3.339)
Married	0.0055 (0.749)	N/A	N/A
Number of children 6–18	-0.0021 (-0.652)	-0.0058 (-1.314)	-0.0020 (-0.438)
Number of children under 6	-0.0001 (-0.0274)	-0.0040 (-0.763)	0.0001 (0.00918)
SSI receipt in family	-0.0148 (-1.135)	-0.0025 (-0.152)	0.0314 * (2.001)
Full-time working spouse	0.0052 (0.830)	-0.0095 (-1.274)	N/A
Age (in years)	-0.0043 (-1.589)	0.0052 (1.250)	0.0042 (1.009)
Age ² /100	0.0040 (1.058)	-0.0083 (-1.418)	-0.0077 (-1.256)
High school	0.0052 (0.814)	-0.0011 (-0.121)	0.0006 (0.0658)
Some college	0.0340 ** (3.667)	-0.0034 (-0.339)	0.0175 (1.638)
College or more	0.0186 (1.297)	0.0006 (0.0399)	0.0045 (0.188)
% change in state employment	-0.0099 (-1.012)	-0.0119 (-0.640)	-0.0038 (-0.333)
State unemployment rate (%)	-0.0069 (-1.369)	-0.0133 * (-2.166)	-0.0086 (-1.233)
Sample size	6,271	4,301	5,167
Baseline quit rate	0.0565	0.0572	0.0733

Notes: See notes for Table 4. The only differences are that this sample uses workers who do *not* have their own EPHI at the beginning of the wave (they may have no insurance or other insurance) and there are fewer states omitted due to sample size (with DC, DE, NH, and RI for men, CT, DC, and NH for married women, and CT for unmarried women). Alternative clustering by state (instead of individual) does not change the statistical significance of the Medicaid coefficient, though clustering by state-year slightly increases the statistical significance of the Medicaid coefficient for men ($t = -2.31$).

* significant at five percent;

** significant at one percent.

Table 6
Falsification Tests

	(1) Men	(2) Married Women	(3) Unmarried Women
Panel A: Effects of Medicaid Expansions on High-Income Workers			
<i>EPHI Sample</i>			
Medicaid threshold (in \$100s)	0.00035 (1.781)	−0.00010 (−0.493)	−0.00013 (−0.313)
Sample size	25,134	13,151	1,053 [†]
Baseline turnover rate	0.0139	0.0136	0.0292
<i>Non-EPHI Sample</i>			
Medicaid threshold (in \$100s)	0.00001 (0.00743)	0.00006 (0.155)	Too few observations
Sample size	5,000	11,400	
Baseline turnover rate	0.0372	0.0276	
Panel B: Effects of Medicaid Expansions on Involuntary Job Separations			
<i>EPHI Sample</i>			
Medicaid threshold (in \$100s)	0.00022 (0.733)	−0.00014 (−1.203)	−0.00037 (−0.960)
Sample size	9,942	2,013	6,874
Baseline turnover rate	0.0145	0.0148	0.0163
<i>Non-EPHI Sample</i>			
Medicaid threshold (in \$100s)	−0.00014 (−0.202)	0.00055 (0.697)	−0.00084 (−0.731)
Sample size	6,202	3,739	5,105
Baseline turnover rate	0.0379	0.0335	0.0504

Notes: See notes for Table 4 for the sample structure. Sample sizes reflect the fact that there was no variation in outcomes in some states, months, or subsamples, causing some observations/variables to be dropped in some specifications. Detailed tables are available upon request. The top panel of the table uses high-income observations (above 400 percent of the Federal Poverty Guidelines); the bottom uses involuntary separations as the dependent variable.

* significant at five percent;

** significant at one percent.

[†] Note that this sample includes only about half of the states due to small cell size issues with this subsample of high-income single women with EPHI.