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Does Government Health Insurance Reduce Job Lock and Job Push?

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Abstract

I study job lock and job push, the twin phenomena believed to be caused by employment-contingent health insurance (ECHI). Using variation in Medicaid eligibility among household members of male workers as a proxy for shifts in workers dependence on employment for health insurance, I estimate large job lock and job push effects. For married workers, Medicaid eligibility for one household member results in an increase in the likelihood of a voluntary job exit over a four-month period by approximately 34%. For job push, the transition rate into jobs with ECHI among all workers falls on average by 26%.

Keywords: Job Lock; Job Push; Medicaid; Job Mobility
JEL categories: H41; I13; J6

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1 Introduction

The American health insurance market shares a close association with the country's labor market, a link made evident by the fact that in 2010, 78% of full-time, year-round workers bought health insurance through an employer (Brault and Blumenthal, 2011, Table 1). The main reason for this link is that employment-contingent health insurance (ECHI) is generally provided at much lower cost than insurance coverage available through alternative sources like the individual market. This lower cost is a result of factors such as tax incentives for both the employer and the employee, reduced administrative costs for groups, and reduced incidence of adverse selection (Gruber, 2000). The benefits of employer provision of insurance come at a cost, though, as access to this lower cost insurance is usually limited to firm employees and their immediate families.

Such restrictions on access could have important implications on the frequency that workers enter and exit jobs. For workers with ECHI, non-portability of coverage implies costs that must be incurred by the worker when leaving an employer. These costs could be relatively small, such as losing access to certain physicians under a new insurance plan, or large, as when a health condition delays or prevents the worker from obtaining coverage. To avoid these costs, an employee may choose to stay in a job he or she would otherwise leave, a phenomenon referred to as "job lock". On the other hand, when a person does not have ECHI, whether in a job or not, the higher cost of obtaining insurance through other sources will provide an incentive to find a job that provides access to ECHI. When such a person chooses to take a job that he or she would otherwise not in order to gain access to health insurance, it is called "job push" (Cooper and Monheit, 1993; Anderson, 1997; Hamersma and Kim, 2009). In either case, job lock or job push, the underlying mechanism is the same: the relationship between employment and insurance increases the value of jobs offering access to health insurance relative to other employment states.

Understanding the role ECHI plays in the operation of the labor market has taken a renewed importance in recent years with the debate over, and eventual passage of, the Patient Protection and Affordable Care Act (ACA) in 2010. Before the law was adopted in 2009, supporters of the law argued that the reduction of job lock was one mechanism through which the law would improve the economy (e.g., Council of Economic Advisors, 2009; Gruber, 2009). However, there has been little consensus in the literature regarding the prevalence of job mobility effects of ECHI, and studies have estimated a large range of effect sizes.[1] Moreover, if a job lock effect

[1] Large estimates can be found in Cooper and Monheit (1993); Madrian (1994); Gruber and Madrian (1994); Buchmueller and Valletta (1996); Adams (2004); Bansak and Raphael (2008); Dave et al. (2015). Moderate or those only affecting certain sub-populations can be found in Gilleskie and Lutz (2002); Hamersma and Kim (2009); Boyle and Lahey (2010); Garthwaite et al. (2014); Heim and Lurie (2015). Small or non-existent estimates are obtained by Holtz-Eakin (1994); Kapur (1998); Berger et al. (2004); Sanz-De-Galdeano (2006); Bradley et al. (2007, 2013); Baicker et al. (2014); Depew (2015)

indeed exists in the job market, so far the evidence suggests that the ACA has been ineffective in affecting its prevalence (Heim et al., 2014; Bailey and Chorniy, 2016).

The goal of this paper is to contribute to this debate regarding the existence of job lock and job push in the American labor market, and the ability of government provided health insurance to mitigate it (if it exists). Using individual-level panel data from the Survey of Income and Program Participation (SIPP), I estimate job mobility effects for men that are identified via variation in eligibility of household members for Medicaid, the state and federal government provided health insurance program. Medicaid provides a source of portable insurance coverage, so its availability to household members of a worker represents a reduction in the cost of leaving an employer for those who hold ECHI. We would, therefore, expect workers to be more likely to leave their jobs when they themselves, or their household members, are eligible for Medicaid coverage. Conversely, for individuals without ECHI, Medicaid eligibility for household members makes their current state of employment more attractive relative to job options that offer ECHI, so one would expect these people to be less likely to move into such jobs. Medicaid eligibility, rather than outright Medicaid enrollment, is used since eligible individuals always have the option to enroll in the program after becoming sick without exclusions. Thus, eligibility is the same as actual enrollment, as far as behavior that is affected by insurance coverage is concerned (Cutler and Gruber, 1996; Gruber and Yelowitz, 1999).

This approach of relying on Medicaid to study job lock is possible because of a large expansion of the Medicaid program during the period I study, the late 1980s and early 1990s. This expansion took place among children and pregnant women, the result of an effort by the government to reduce the number of kids without health insurance. As Table 1 shows, from 1986 to 1993 the share of children in the USA under 21 who were eligible for Medicaid expanded from 21% to 38%. For women, pregnancy eligibility rose from 11% to 25%. The implication of this was that many working class people became eligible, something which had previously been restricted primarily to individuals who did not work, thereby making Medicaid relevant for studying labor market issues like job lock.

The use of Medicaid as a basis for identification requires care, however, since individual-level behavior plays a critical role in eligibility, and because measurement of eligibility in the first place is difficult. My empirical approach, therefore, employs three main strategies to compensate for these issues, with the first being a difference-in-differences (DD) regression framework for estimation. In this strategy, which is in the same spirit as Madrian (1994), individuals who have insurance through alternative sources that are not their employers, such as through spouse employers, are used in control groups. For these individuals, Medicaid eligibility represents a redundant source of health insurance, rendering it a placebo. This is intended to address the possibility that Medicaid eligibility is correlated with unobserved factors that influence the propensity to move between jobs, but are unrelated to worker concerns over health insurance. For example, people who are prone to move frequently between jobs may never develop industry- or firm-specific human capital, and therefore be more likely to be eligible for Medicaid. This approach, however, relies on these unobserved propensities being similar across individuals with or without alternative sources of insurance (i.e. across the treatment and control groups). This may not be true given that health insurance source is chosen by the individual. Therefore, in my second strategy, I augment the DD model with individual-level fixed effects to help address any differences in the distribution of unobserved job transition propensities between the treatment and control groups. Additionally, they also strengthen my model's ability to address the endogeneity of Medicaid eligibility in finer way than the DD approach alone would. In my third primary strategy, I address the possibilities that individual-level Medicaid eligibility may be correlated with unobserved, time-varying factors, and that it may be measured with error – a particular concern due to the use of fixed effects. To do so, I replace Medicaid eligibility measured at the individual-level with a state-level measure of the likelihood of being eligible during the same period for people in the same state, education, and age categories (which are all characteristics that are relatively better measured).

Using this empirical framework, I find evidence of both job lock and job push for men that are substantial in size when considered in proportion to population rates of mobility. For job lock, I estimate that Medicaid eligibility for one additional household member of a married male worker increases his probability of leaving his job voluntarily over a four-month period by approximately 34%, as compared to if the household member were dependent on the worker's ECHI for coverage. For job push, I estimate that eligibility versus no coverage at all for a household member results in a 26% lower likelihood of transitioning into a job with ECHI over four months (based on a sample of married and unmarried workers).

Given these results, this paper has two primary contributions to the field. The first is that, despite a sizable and contradictory previous literature, this is one of a very few papers that has made any attempt at all to separately identify job lock and job push effects (Cooper and Monheit, 1993; Anderson, 1997; Hamersma and Kim, 2009). This step is important, though, because the two effects have opposite directions – job lock slows down job mobility, job push increases it – meaning that if an analysis does not carefully look for both, they could end up offsetting each other. The second primary contribution of this paper improves on the methodology necessary for separating job lock from job push, and differentiates it from the previous papers studying both phenomena. The key here is to account for all of the household members of a worker when studying his or her job mobility, since health insurance decisions are made at the household level, not the individual level (Cutler and Gruber, 1996). This point is intuitively clear; for example, a man might have ECHI through his employer, suggesting he could be job locked, but if his child does not have coverage through his employer as well, he might actually seek to leave his job to obtain coverage for his child. This suggests he is affected by job push, rather than job lock. Previous papers have only accounted for the worker's coverage alone when trying to parse job lock and job push, but examples like this, and my results which are driven by household members besides the worker, show that approach is not sufficient.

Besides papers that attempted to separately study both job lock and job push, the most similar papers to this one are Bansak and Raphael (2008) and Dave et al. (2015). Bansak and Raphael (2008) study mobility of male workers using SIPP data and a similar DD approach where identification is based on the 1990s and 2000s' expansion of the State Children's Health Insurance Program (which built on the earlier Medicaid expansions I study).[2] Dave et al. (2015) study the same Medicaid expansion as my analysis does, but with a very different focus and data set, investigating job mobility of pregnant women and the resulting effects on private insurance coverage using the Current Population Survey. Thus, their paper is very much complementary to my analysis of men's mobility using the SIPP.

2 Medicaid during the late 1980s and early 1990s

Medicaid is a joint federal and state program that provides health care insurance coverage for poor children and their parents, poor pregnant women, and poor elderly and disabled.[3] Each state administers its own Medicaid program and sets its own eligibility rules that are subject to federal requirements. It covers a broad range of medical services with little or no cost to the beneficiary, making its coverage valuable to the covered individual.

Before 1984, Medicaid was only available to non-elderly, non-disabled individuals if they participated in the federal cash welfare program, Aid to Families with Dependent Children (AFDC).[4] During this time period, AFDC provided income to very poor families with a primary goal of supporting children lacking parental support. Eligibility rules for AFDC were quite restrictive, though, admitting only some family composition types in addition to requiring low incomes. For example, childless adults and, in many states, two-parent families with children were ineligible. As a result, eligible families were nearly entirely headed by single women who did not work. In 1984, 90% of AFDC families were headed by single mothers, fewer than 5% of participating mothers worked, and 88% of participating families had no income at all outside

[2] In addition to not accounting for job push, Bansak and Raphael (2008) holds another important flaw: it measures job mobility using employer ID numbers that incorrectly track employers across survey waves. These IDs falsely indicate employer changes in more than 20% of all jobs (Abowd and Stinson, 2013, online appendix).

[3] The discussion in this section relies heavily on a number of sources, each of which contributed important facts, concepts, and background information throughout. To avoid distracting the reader with numerous citations, I acknowledge these works here except to note a source when specific statistics are mentioned: Committee on Ways and Means, U.S. House of Representatives (1987); Congressional Research Service (1988, 1993); Currie and Gruber (1996a,b); Cutler and Gruber (1996); Gruber (2003); Gruber and Yelowitz (1999); Yelowitz (1995).

[4] The elderly and disabled were eligible for Medicaid if they were eligible for the federal cash welfare program, Supplemental Security Income.

of AFDC (Committee on Ways and Means, U.S. House of Representatives, 1987, p. 432).

Medicaid's link to AFDC combined with the restrictiveness of AFDC eligibility resulted in a large number of low-income families being excluded from Medicaid. This feature of the Medicaid program drew attention from Congress, which feared that children were going without health care since their families could not afford their own insurance coverage. Beginning in 1984 and continuing into the 1990's, Congress addressed the issue by passing a series of federal level laws that gradually reduced the link between AFDC and Medicaid, resulting in the expansion of Medicaid eligibility to historically excluded segments of the population. In particular, families with much higher income levels than before became eligible, including many headed by adults in the working ranks.

Given the government's goal of increasing insurance coverage for children, the expansions were designed to either directly make kids Medicaid eligible, or to indirectly cover them by providing coverage of pregnancy care for women (that is, only pre-natal care and birth related expenses were covered for women, not general health care when not pregnant). Men, therefore, were generally not eligible for Medicaid themselves.[5] However, men did still receive indirect benefits from the expansion since their children or spouses could be covered (or any female adult family members, whether spouses or not).

The implementation of the expansion took place via a number of different eligibility rule changes. Eligibility age limits for children were increased, family income thresholds were raised, and family composition restrictions were eased. These changes were effected in a varied fashion, with federal law at times imposing nationwide requirements, and at others giving states the option to expand eligibility requirements. These optional expansions were then, in some cases, later made requirements. As a result, Medicaid eligibility expanded over time and did so at different rates across states.

3 Identifying job mobility effects

Before proceeding with the discussion of identifying job mobility effects, it would be useful to note that I estimate models that explicitly account for insurance status of multiple members of a given household, even though it is only one member of the household whose job mobility is modeled in any particular regression. To avoid ambiguity, I refer to the individual whose mobility is being modeled (that is, the individual to whom the outcome variables refer) as "the

[5]Men could be directly covered via eligibility for AFDC, which primarily covered women but did cover some men such as the case of a poor, single father. Eligibility of AFDC did expand during this period, but its magnitude was very small compared to the expansion of eligibility for women beyond AFDC (Cutler and Gruber, 1996). Men could also be eligible for Medicaid as a recipient of Supplemental Security Income (SSI), a federal welfare program for poor elderly, blind, and disabled. However, those individuals are excluded from my analysis.

worker", and use "household members" to refer to individuals living with the worker at the time of his inclusion in my analysis. I allude to individuals or people in the household when referencing the combination of the worker and household members without making distinctions between them. Additionally, it should be emphasized here that the term "ECHI" always refers to health insurance coverage that is provided by the worker's employer, and not by that of any household member. Finally, note that since I only analyze job mobility of male workers in this paper, the workers are always men, but their household members could be male or female.

3.1 Job lock

The dependent variable of interest in my job lock analysis, Y_{it}, equals one if worker i reported a voluntary exit, either a quit or a retirement, from his job during period t, and zero otherwise. The goal of the analysis is to estimate λ, the effect of the link between health insurance and employment on Y_{it}:

$$\begin{aligned}\lambda \equiv & E[Y_{it}|i \text{ \underline{not} dependent on employment for health insurance during } t] \\ & - E[Y_{it}|i \text{ dependent on employment for health insurance during } t]. \end{aligned} \tag{3.1}$$

A positive value for λ indicates that voluntary job exits are more frequent when workers are not dependent on their jobs for health insurance. That is, a positive estimate of λ would be evidence consistent with job lock.

To estimate λ, I start by restricting my SIPP sample to include only male workers from households in which all members are covered by an employer-provided health insurance plan. This step is necessary to be able to differentiate job lock from job push; workers in households where everyone is covered by insurance from an employer have no incentive to seek alternative employment in order to gain employer-provided coverage (which would be job push, not job lock). Without this restriction, the sign of the theoretical effect of ECHI is ambiguous. Consider the example of a worker with a spouse, whose job provides coverage for the worker himself only. I might assume that the worker is dependent on his job for insurance and hence less likely to leave his job. However, this may not be the case – the worker may seek another job that provides coverage for both himself and his spouse, and so he would be more likely to leave his job. So by conditioning on ECHI coverage for the entire household, I omit individuals who are more likely to be influenced by job push rather than job lock.

Having so restricted the data, I rely on Medicaid *eligibility* to provide variation in worker ECHI dependence. Medicaid is a source of health insurance that is not contingent on employment and, due to the expansions of the program during the time period studied, common among the working population. Eligibility, rather than explicit enrollment, is used because it implies coverage due to the fact that eligible individuals always have the option to enroll even after becoming sick (or pregnant, in the case of women). Cutler and Gruber (1996, p. 392) called this aspect of Medicaid eligibility "conditional coverage". I therefore base identification on the comparison of workers with Medicaid eligible household members to those with ineligible household members. While Medicaid was not a direct replacement for most employer provided plans, for the eligible its availability did reduce the potential costs associated with choosing to leave a job, since the workers' household members would not be completely without health insurance. For those with household members who were not eligible, leaving a job still implied that access to health insurance would be limited. Thus, if job mobility is truly affected by ECHI, we would expect to observe different mobility rates between those with household members who were eligible and not eligible for Medicaid.

To account for the fact that individuals with and without Medicaid eligibility differ in important observed and unobserved ways, I implement an expanded version of the difference-in-differences (DD) framework applied by previous authors (e.g., Madrian, 1994; Holtz-Eakin, 1994; Buchmueller and Valletta, 1996; Bansak and Raphael, 2008). My model of voluntary job terminations takes the following form:

$$\begin{aligned} Y_{i(t+1)} = \beta_1 \left(\sum_{j=1}^{J_{it}} E_{itj} \right) + \beta_2 \left(\sum_{j=1}^{J_{it}} M_{itj} \right) + \beta_3 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} \right) \\ + \beta_4 \left(\sum_{j=1}^{J_{it}} E_{itj} M_{itj} \right) + \beta_5 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} M_{itj} \right) + X_{it}' \gamma + u_{it}. \end{aligned} \tag{3.2}$$

In equation 3.2, i and t identify the worker and time period, respectively, while j indexes the J_{it} individuals in i's household during period t. The worker himself is included in the j index, so everyone in i's household plays a role determining his job mobility in this model. The insurance coverage variables, E_{itj}, N_{itj}, and M_{itj}, each identify coverage during period t for the j^{th} person in worker i's household by taking a value of one, and the lack of coverage by taking a value of zero. E_{itj} indicates ECHI coverage through worker i's employer during period t. N_{itj} indicates insurance from a source that is not worker i's employer and not Medicaid. I refer to this type of coverage as "non-ECHI", since this insurance is not contingent on the worker's employer. That said, this type of insurance can come from a household member's employer – just not worker i's. M_{itj} identifies individuals who are Medicaid eligible (including those explicitly enrolled in Medicaid). Finally, X_{it} is a vector of other controls and u_{it} is the unobserved error term.

It is worth highlighting that the key factor affecting whether a particular insurance plan is indicated by E_{itj} or N_{itj} is whether it comes from worker i's employer or not. For example, consider a household consisting of a husband and wife. Suppose both work and each have insurance coverage through their own employers that cover themselves only, not their spouses, and that neither spouse has any other source of health insurance. For my analysis, the husband is the worker with index $j = 1$ and his wife has $j = 2$. Since he only has insurance through his employer, his status variables are then $E_{it1} = 1$, $N_{it1} = 0$, and $M_{it1} = 0$. His wife's coverage is indicated as $E_{it2} = 0$, $N_{it2} = 1$, and $M_{it2} = 0$, since her only insurance is not through *his* employer, even though it is through hers. Other combinations are possible, but all values are assigned in this fashion.

To understand the rationale behind equation 3.2, consider a simpler version of the model, which only involves the worker himself:[6]

$$Y_{i(t+1)} = \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} + X_{it}' \gamma + u_{it}. \tag{3.3}$$

Here the j index is dropped since only the worker's insurance status is considered. This model could be interpreted as a standard triple-differences specification in which N_{it} and $N_{it}M_{it}$ are excluded due to the restricted sample,[7] but I argue that it is more appropriate to think of it as a dual difference-in-differences specification, a point which is illustrated in Table 2. The "treatment" group is composed of individuals who depended on their employers for insurance access. That is, those with ECHI but without non-ECHI. For comparison, this specification offers two potential "control" groups. The first contains individuals with both ECHI and non-ECHI, while the second contains those without ECHI but with non-ECHI. In this framework, Medicaid eligibility represents a treatment for individuals in the treatment group since it was their only insurance besides ECHI. For both of the control groups, however, the members already had insurance not related to their own employment, rendering Medicaid eligibility redundant – and hence, a placebo – as far as job mobility is concerned.

As Table 2 shows, the availability of two control groups allows for the identification of the job lock parameter, λ, via two separate DD rationales, given by

$$\begin{aligned} \lambda_1 \equiv\ & E[Y_{i(t+1)}|E_{it} = 1, N_{it} = 0, M_{it} = 1] - E[Y_{i(t+1)}|E_{it} = 1, N_{it} = 0, M_{it} = 0] \\ & - \big(E[Y_{i(t+1)}|E_{it} = 1, N_{it} = 1, M_{it} = 1] - E[Y_{i(t+1)}|E_{it} = 1, N_{it} = 1, M_{it} = 0]\big) = -\beta_5, \end{aligned} \tag{3.4}$$

[6]For the purpose of explaining the rationale of the model, I leave aside the fact that men were typically not eligible for Medicaid.

[7]In a standard triple-differences regression, three indicator variables divide the sample into eight sub-groups. In my sample, though, there are only six sub-groups because of the restriction that the workers all lived in households where everyone had insurance through an employer. This means that there are no individuals for whom E_{itj} and N_{itj} both equal zero, and hence, that $E_{itj} = 0$ implies $N_{itj} = 1$ and, conversely, that $N_{itj} = 0$ implies $E_{itj} = 1$. As a result, variables E_{itj}, N_{itj}, and $E_{itj}N_{itj}$ are collinear, as $E_{itj} + N_{itj} - E_{itj}N_{itj} = 1$. I therefore drop N_{itj} and $N_{itj}M_{itj}$ ($\sum_j N_{itj}$ and $\sum_j N_{itj}M_{itj}$ from the ultimate version of the model), since keeping E_{itj} and $E_{itj}M_{itj}$ makes interpretation of the results more straightforward.

and

$$
\begin{aligned}
\lambda_2 \equiv\ & E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=1, N_{it}=0, M_{it}=0] \\
& - \big(E[Y_{i(t+1)}|E_{it}=0, N_{it}=1, M_{it}=1] - E[Y_{i(t+1)}|E_{it}=0, N_{it}=1, M_{it}=0]\big) = \beta_4. \qquad (3.5)
\end{aligned}
$$

Here the first lines of both equations are the same, showing that both methods are based on the same core comparison: the difference between the averages for the Medicaid eligible and ineligible individuals in the treatment group. The second lines of the two expressions are both intended to remove job mobility effects of Medicaid eligibility that are not related to the demand for ECHI, but do so using two different control groups. The first equation uses the "preferred control" group (those with ECHI) and the second equation uses the "alternative control" group (those without ECHI). Estimates for job lock using both identification methods can be simultaneously obtained from a single regression using equation 3.3, where $\hat{\lambda}_1 = -\hat{\beta}_5$ and $\hat{\lambda}_2 = \hat{\beta}_4$. Thus a positive coefficient estimate for β_4 and a negative estimate for β_5 are evidence consistent with job lock in this model.

From an experimental design perspective, $\hat{\lambda}_1$ is favored over $\hat{\lambda}_2$ because it is based on the preferred control group in which individuals all have ECHI, as they do in the treatment group. Workers in the alternative control group, on which $\hat{\lambda}_2$ is based, do not have ECHI – implying an important difference from the treatment group members. From a practitioner's viewpoint, however, the ideal outcome would be for the estimators to produce similar estimates, especially since sample sizes for the alternative control group are much larger than the preferred control group. Thus, even though $\hat{\lambda}_1$ is expected to be more reliable, the estimates produced by $\hat{\lambda}_2$ are still valuable for comparison with the $\hat{\lambda}_1$ estimates.

In appendix A, I carefully discuss the intuition and assumptions that link this simple version of the model (equation 3.3) with the full, empirical model (equation 3.2). In short, the full model is as if a separate dual DD for the worker and each of the household members were included in the regression, and that corresponding term coefficients are assumed constant across household members. That is, that every household member's insurance coverage affects the outcome variable in the same way.

Appendix A also shows that the interpretation of the parameters in the full model (equation 3.2) are analogous to the interpretation for the simple model (equation 3.3). That is, β_4 and $-\beta_5$ represent the incremental change in job mobility from a person in the worker's household becoming less dependent on the worker's ECHI for coverage. An important distinction that should not be overlooked, though, is that since adult men were not generally able to be Medicaid eligible, estimates of β_4 and $-\beta_5$ are identified based on workers' responses to changes in the Medicaid coverage of their household members. So job lock estimates obtained in my analysis represent the incremental change in job mobility from a person – *besides the worker himself* –

in the worker's household becoming less dependent on the worker's ECHI for coverage.

3.2 Job push

The empirical strategy I rely on to detect job push is similar conceptually to the one I use for job lock, but requires several important changes. The first difference is that the outcome variable of interest is Z_{it} in this analysis, an indicator for an individual moving into a job with ECHI, regardless of whether it was from another job or from non-employment. If a worker moves to an ECHI job (after voluntarily leaving his previous job, if the transition is between jobs), then Z_{it} equals one, otherwise it is zero. Given this outcome, I can define the job push parameter of interest,

$$\begin{aligned}\pi \equiv & E[Z_{it}|i \text{ seeks employer provided health insurance during } t] \\ & -E[Z_{it}|i \text{ does not seek employer provided health insurance during } t]. \end{aligned} \tag{3.6}$$

A positive value of π means that individuals seeking employer health insurance are more likely to transition from their current states into jobs offering ECHI. Thus, positive estimates of π constitute evidence of job push.

The requirement that the individual moves to a job with ECHI has not been used by most authors studying job push. Anderson (1997) and Hamersma and Kim (2009) studied job exits without specifying anything about where the workers went, while Cooper and Monheit (1993) studied job changes for those *predicted* to gain health insurance if they switched jobs. Only Dave et al. (2013) and Garthwaite et al. (2014), besides this current study, include analyses of moves into jobs with ECHI (though in these studies the moves are implicit, since they do not observe individuals over time). The requirement that the new job has ECHI, though, is an important step to avoid bias since movement into jobs without ECHI is inconsistent with insurance seeking behavior. Appendix B shows formally that bias is introduced by the use of a dependent variable that combines both moves into jobs with ECHI and moves into those without it, but this point can be understood intuitively when one recognizes that jobs with ECHI and those without are *substitutes* when one seeks ECHI insurance. A worker seeking ECHI is more likely to move to a job with ECHI than one without, and vice versa, and thus job push has effects with opposite directions on these two types of job moves. Treating both types of moves as the same, then, would result in effects potentially canceling each other out.[8]

[8] This reasoning suggests that treating both types of job moves as the same introduces negative bias. All of the other papers studying Medicaid influences on job push are affected by this to some degree. It provides a possible explanation why Hamersma and Kim (2009) find relatively weak evidence of job push, and suggests that the results of Dave et al. (2013) might understate the true effect. Baicker et al. (2014) find no employment effect of Medicaid, but it is hard to evaluate the impact this issue would have on their analysis since they do not make any attempt to separately identify job lock versus job push. Interestingly, Garthwaite et al. (2014)

Like my use of the jobs with ECHI outcome, my inclusion of the population of people moving from non-employment into employment in my analyses is unusual for the job push literature, though Dave et al. (2013) and Garthwaite et al. (2014) are again exceptions that study this group implicitly. This population, though, should not be ignored since the mechanism behind job push is that jobs with ECHI are more attractive than similar (or even better) jobs without such insurance. This is true regardless of the individual's state before making the transition into a job. This point has been noted by previous authors with regards to job lock, as well (e.g., Madrian, 1994; Buchmueller and Valletta, 1996; Hamersma and Kim, 2009), and the inclusion of the non-employed in studying job push is simply the analog to the job lock practice of including voluntary job quits for any reason.

Another change in comparison to my job lock analysis is the manner in which I restrict the SIPP data to obtain my estimation sample. As discussed in Section 3.1, for some individuals there is theoretical ambiguity as to whether one should expect to observe job lock or job push. In an effort to address this, I omit individuals that could potentially be affected by job lock, leaving job push as the only potential effect. To be included in my job push sample, a worker must not have held ECHI, *or*, if the worker *did* have ECHI from his own employer, then everyone (including the worker) in his household must have been covered by employer provided insurance originating from one or more employers of household members (not the worker's employer). Selecting the sample in this way limits the chance that job push and job lock would be conflated since individuals without ECHI obviously could not be affected by job lock, and if everybody in the family already had an alternative source of insurance that was high quality and affordable (as would be expected since it was employer provided), then there is no reason to think the workers needed to stay in their jobs out of insurance concerns.[9]

In the final change versus my job lock analysis, I redefine the ECHI and Medicaid status variables to help maximize the symmetry between my job lock and job push analyses: let $\tilde{E}_{itj} \equiv 1 - E_{itj}$ and $\tilde{M}_{itj} \equiv 1 - M_{itj}$. These variables indicate when the individuals *do not* have ECHI or Medicaid eligibility, respectively, and are used in the job push analogue of my job lock

perform analyses both ways, employment without restriction and with ECHI requirement, and find *smaller* estimates when restricting to jobs with ECHI. They base their work on the Current Population Survey, though, so are unable to precisely measure ECHI coverage and Medicaid eligibility. Thus, this unexpected result may be due to other confounding effects related defining the relevant job push sample.

[9]My strategy of using different data restrictions to differentiate job lock and job push is similar to that of Hamersma and Kim (2009). Their samples were both more restrictive than mine since they split their samples strictly on ECHI status. This is not a problem for their job push analysis, though their job lock regressions may have been confounded by the presence of some individuals influenced by job push rather than job lock. As shown by the example of the worker and spouse I gave in section 3.1, requiring all workers to have ECHI is not sufficient to remove all possible influence of job push.

model:

$$Z_{i(t+1)} = \delta_1 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \right) + \delta_2 \left(\sum_{j=1}^{J_{it}} \tilde{M}_{itj} \right) + \delta_3 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \right)$$
$$+ \delta_4 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \tilde{M}_{itj} \right) + \delta_5 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \tilde{M}_{itj} \right) + X_{it}' \theta + \varepsilon_{it}. \tag{3.7}$$

As in the job lock case, this model produces a set of two DD estimators: the preferred estimator, $\hat{\pi}_1 = -\hat{\delta}_5$, and the alternative estimator, $\hat{\pi}_2 = \hat{\delta}_4$. The justification for interpreting this model as a dual DD follows reasoning analogous to the job lock case, which is discussed in more detail in Appendix C. Both estimators are based on a treatment group composed of individuals with neither ECHI nor non-ECHI. Lack of Medicaid eligibility represents the treatment since those without it are expected to seek jobs with health insurance access more frequently than those who are eligible. The control group for the preferred estimator ($\hat{\pi}_1$) is comprised of individuals without ECHI, like the treatment group, but who do have non-ECHI. The alternative estimator ($\hat{\pi}_2$) is based on a control group comprised of individuals who differ from the treatment group on two dimensions, having both ECHI and non-ECHI coverage. For both of these control groups, Medicaid coverage is redundant because the individuals have non-ECHI, so eligibility would not be expected to cause demand for ECHI to vary.

These estimators represent the incremental change in the likelihood of a worker moving to a job with ECHI due to a decrease in demand for ECHI coverage (via the worker) for one of the worker's household members. Hence, like the job lock case, identification is primarily based on Medicaid eligibility for the workers' household members – *not the workers themselves.* Unlike the lob lock case, however, sample size concerns favor the preferred estimator here, since membership in the alternative control group requires both ECHI and non-ECHI coverage, and is, hence, more restrictive.

3.3 Estimation

I depart from the bulk of the previous literature in basing estimation on the linear probability model. Previous authors have used the probit model almost exclusively. This change affords me the ability to consistently estimate models with individual-level fixed effects and state-by-year dummies. I view the individual-level fixed effects to be of particular importance since they account for individuals' unobserved propensities to transition between employment states and jobs. These could cause inconsistency if, for example, firm and industry specific skills and experience influence peoples' wages and abilities to get good jobs. Frequent transition behavior would therefore be correlated with lower earnings and hence Medicaid eligibility. To some extent, this issue is already addressed by the DD models described above since they are designed to address unobserved factors correlated with eligibility. The fixed effects, though, help address differences in the distribution of these unobserved propensities across the treatment and control groups.[10]

In addition to the fixed effects, all models also include the following dummy variable controls: education, marital status, five-year age group, earnings decile, household income decile, firm type, number of household members, and month-year. Indicators for having a child less than two years old, between two and five, and between six and 17 are also included, and these child dummies are fully interacted with each other.[11] Descriptive statistics for all model dependent variables and controls can be found in Table 4. All sample descriptive statistics and regression estimates are weighted using panel weights from the SIPP longitudinal files.[12]

Two-way, cluster-robust standard errors are reported for all models (Cameron et al., 2011). The use of two-way clustering is intended to address, at least in part, the problem of individuals who move between states over the course of a panel. For example, consider the case of a man who moves from California to South Carolina. Since he could be influenced by state-specific factors in both states that are not accounted for explicitly, his error term could be correlated with those of other Californians and South Carolinians both. This implies that clustering by one state only, say California, is deficient, since no correlation with residents of South Carolina is assumed. To implement two-way clustering to address this issue, I create two variables where one equals California and the other South Carolina for each of the man's observations in the panel. More generally, for any worker moving between two states, one variable identifies the first state of residence and the other identifies the second. Workers who do not move have both variables equal to the same state where he or she lives. Both state variables are then used

[10]Individual-level fixed effects are unusual in the job lock literature usually because either panel data is not available or the identifying variation does not occur over time. In this case, both are available due to my use of the SIPP and the expansion of Medicaid. Other examples of job lock studies (both of which study more narrowly focused populations) using individual-level fixed effects can be found in Heim and Lurie (2010, 2015). One concern related to the Medicaid expansion, however, is that it could have been correlated with state-specific time-trends in job mobility. The inclusion of state-by-year dummy variables in some versions of my models are, therefore, intended to address this issue.

[11]All models are estimated via the Within Estimator, and all estimates, including standard errors, are implemented using Stata module "xtivreg2" (Baum et al., 2010; Schaffer, 2010) via Stata/SE 12.1 for Windows (StataCorp, 2011).

[12]Since my analysis combines five SIPP panels (described in Section 4 below) which cover nearly eight full years, I adjust the panel weights to account for growth in the American population over this period. I adjust all panel weights such that for any particular wave, the sum of all panel weights in the pooled sample (before I restrict the data) is equal to the country population used as the base for the 1990 panel weights. So if A is the sum of the 1990 panel weights for any wave (all waves have the same sum), and B_p is the sum of the weights for a given wave of panel p, then I multiply all the longitudinal weights for panel p by $(A/B_p) \times 0.2$. This adjustment factor for each panel is: 0.20789744 for 86, 0.20607208 for 87, 0.20414484 for 88, 0.2 for 90, and 0.19590615 for 91. The effect of this adjustment is to increase the weight for the earlier panels since, in comparison to individuals drawn in the 1990 panel, respondents in the earlier panels were more likely to be chosen to be panel members (because the population was smaller during the time of those panels). The unadjusted panel weights do not reflect this difference.

as cluster-group variables in calculating the two-way-clustered standard errors. Appendix D presents additional details about this approach.

4 Data

I perform my analysis using the 1986, 1987, 1988, 1990, and 1991 panels of the SIPP (U.S. Dept. of Commerce, Bureau of the Census, 2006, 2008), an individual-level, longitudinal survey of the USA's civilian, non-institutional population.[13] Individuals were interviewed every four months over the life of the panels, with each four-month-long subdivision known as a panel "wave". The interviews are backwards looking, covering the four months immediately preceding the interview month, so survey observations are available for each month of the wave. However, the survey was not designed to track changes in an individual's source of health insurance over the course of a wave, so I only used one observation per respondent, per wave: the fourth month record, which immediately preceded an individual's interview month. This has the additional advantages of helping to minimize error in the respondent's recollection and avoid the so-called "seam bias" (Gruber and Madrian, 1997; Ham and Shore-Sheppard, 2005; Callegaro, 2008). After combining all five panels, my data includes observations for every month from January 1986 through August of 1993.

I enforced a number of restrictions on the data to obtain my final analysis samples. Table 3 lists the impact of these restrictions on my sample sizes in detail. Sample members were required to be between the ages of 21 to 55 and have a valid interview for all waves. Since Medicaid eligibility is dependent on the individual's state of residence, I only included individuals who lived in one of the SIPP's uniquely identifiable states for all waves. I also excluded individuals who moved between states more than once, since those moving frequently were probably different in important ways from other sample members (they comprise only a very small portion of the sample). Respondents who reported receiving SSI were dropped since they likely had a disability that drastically alters labor force attachment.

Individuals with jobs were only included in my samples if they reported holding one job only at the end of a given wave *and* at the beginning of the next wave.[14] The reason for requiring the individual to have only one job is that the survey did not observe which employer provided the respondent's ECHI explicitly. If a worker reported working two jobs simultaneously, it is not clear which provided the insurance coverage. Considering only workers with one job eliminates this ambiguity. Moreover, since job state transitions are observed between waves (i.e., between interviews in waves t and $t+1$), if there is only one job held at the end of wave t, then it is

[13] The 1989 panel was not included since it only lasted three waves, as compared to the other panels which each lasted at least six.

[14] The SIPP collected information on up to two jobs (plus self-employment, if any).

clear out of which job and health insurance state the transition in wave $t + 1$ is coming.[15] The second requirement of one job at the beginning of the next wave $(t + 1)$ serves as a check of the worker's survey responses. Since the end of a wave and beginning of the next occur at the same point in time, it shows that the respondent is providing employment status consistently across two separate interviews.

The job lock and job push samples are differentiated as already described above in Sections 3.1 and 3.2. Two additional restrictions were also made for estimation reasons: I excluded potential sample members who would only be observed in the sample one time (so that person fixed effects could be estimated) and dropped respondents with zero panel weights.

4.1 Imputation of Medicaid eligibility

One of the prerequisites to implementing the analyses described in Sections 3.1 and 3.2 is that Medicaid *eligibility* must be observed for sample members. While the SIPP questionnaire does ask respondents about Medicaid enrollment, it does not explicitly observe eligibility for those not actually enrolled. For my analysis, therefore, I impute Medicaid eligibility on the basis of observable data and detailed, state-level eligibility rules (Currie and Gruber, 1996a,b; Cutler and Gruber, 1996; Gruber and Yelowitz, 1999; Gruber and Simon, 2008). I rely on the programs developed and used by Gruber and Yelowitz (1999) to impute eligibility for my study since their analysis used the SIPP during the same time period I use.[16] In my data, I code individuals as Medicaid eligible if they are imputed as eligible by the Gruber and Yelowitz program or are reported as actually enrolled in Medicaid by the SIPP survey.[17]

I impute eligibility for children in the households of the workers up to and inclusive of age 20, the highest possible age a person could be eligible as a child.[18] For women in the households of the workers, I follow Gruber and Yelowitz (1999) and I impute pregnancy eligibility for the child bearing ages of 15 through 44. For women 45 and older, this implies an assumption that there is no value for Medicaid pregnancy eligibility for women 45 and older. To the extent that women above 44 do not expect to have pregnancy expenses, this is a reasonable assumption.

[15]In theory, tracking transitions and which jobs they come from should not be very difficult, as the SIPP tracks employer ID numbers which could be used to identify these transitions. However, the SIPP has incorrectly tracked these employer IDs over its entire history of existence, so they should be viewed as completely unreliable for research purposes. This is partially why my analysis relies on survey questions about employment state transitions, not employer IDs. See Stinson (2003) and the online appendix of Abowd and Stinson (2013) for more details.

[16]The eligibility imputation programs developed by Gruber and Yelowitz were obtained from Professor Yelowitz.

[17]This includes coding men as Medicaid eligible if they are reported as receiving Medicaid benefits in the SIPP.

[18]Eligibility for older children came through the "Ribicoff Children" program (Congressional Research Service, 1988). Gruber and Yelowitz (1999) only imputed child eligibility through age 18 for their analysis. This was the only change I made in using their imputation programs.

For women from ages 15 through 20, I code the variable M_{it} equal to one if they are eligible either as a child or for pregnancy coverage.

5 Results

Tables 5 through 7 contain my main regression results, which are presented separately for job lock and job push. It should be noted that all estimates are multiplied by 100 and all models included individual-level fixed effects and year-month dummies. Each table contains estimates based on three samples: the full sample; one restricted to individuals who were married each time they were observed in the data; and one restricted to married people *and* excluding observations of individuals with household income or job earnings in the top decile. The marriage restriction has been common in the job lock literature given that dual insurance coverage is most likely among married couples. My models also involve children, providing additional reason to examine the results for married people. The most important reason for the top income exclusion is that such individuals are unlikely to be Medicaid eligible. Additionally, since incomes are top coded in the SIPP, there is potentially a very large range of incomes in the top decile.

5.1 Job lock

Table 5 presents my estimates of equation 3.2 for my job lock sample, where the dependent variable is voluntary job exits. Columns (1) and (2) show the results for the full sample, with the difference between the two being that column (2) includes state-by-year interaction dummies in the model. For column (1), the job lock estimate given by the preferred estimator, $\hat{\lambda}_1$ (the negative of the β_5 coefficient), is 0.49, while the alternative estimator, $\hat{\lambda}_1$ (β_4 coefficient), is similar at 0.46. Given the statistical model that generated them, the interpretation of these estimates is that an additional household member eligible for Medicaid results in an increase in the likelihood of the worker leaving his job in the next four months by just under half a probability point (that is, the change in probability times 100). However, neither estimate is statistically significant at conventional significance levels. Despite that, the estimates are large when compared to the voluntary exit rate of 1.7% for men in households where all members are dependent on his ECHI for coverage, as they both would imply an increase of more than 27%. Estimates from the model including state-by-year dummies in column (2) produce estimates that are numerically similar and also not statistically significant.

Turning next to estimates on the sample restricted to married workers, found in columns (3) and (4), we see that $\hat{\lambda}_1$ produces estimates that are slightly larger than for the full sample, while $\hat{\lambda}_2$ produces estimates that are more than twice the magnitude of the full sample. Only one estimate, from $\hat{\lambda}_1$ and the model without state-by-year interactions, is statistically significant (at the 10% significance level), with a point estimate of 0.57 – implying an exit rate increase of approximately 34%.

When the sample is further restricted to exclude those waves when the worker had incomes in the top decile, as in columns (5) and (6), the estimates produced by $\hat{\lambda}_1$ are much larger at 1.2 and 1.1 probability points, respectively. Both of these estimates are statistically significant at the 1% level, with the 1.2 estimate in column (5) implying an exit rate increase of almost 71% and the 1.1 figure in column (6) implying a nearly 65% increase. The estimates produced by the alternative estimator, $\hat{\lambda}_2$, are very similar to those produced by the sample that includes the top incomes: their magnitudes are large, both suggesting job mobility increases of more than 45%, but neither is estimated precisely enough to be statistically significant at conventional levels.

There are two points worth noticing regarding the table as a whole. First, the various estimates are relatively similar across estimators (preferred and alternative), samples, and models, and all generally tell the same story, even though they are not estimated with enough precision for most of them to be statistically significant. Secondly, the largest point estimates come from the samples that exclude workers with top incomes, a result that is consistent with the argument made by Hamersma and Kim (2009), suggesting those workers should not be affected by job lock.

Table 6 presents a supplementary set of regressions on a placebo outcome, involuntary terminations (which was previously used by Hamersma and Kim, 2009). Here, the dependent variable is equal to one if the worker reports being laid-off or discharged over the next four months, and zero otherwise. Since a worker does not choose to be laid-off or fired, we do not expect a job lock effect on involuntary terminations, so if a large estimate is obtained, it is suggestive of bias (that would presumably also effect the main regressions). Examining the estimates in Table 6 from these placebo regressions, however, there is not any apparent source of bias that could also affect the main results in Table 5. I find all estimates are comparatively small in magnitude and are not statistically significant at conventional levels, even though standard errors are similar (or smaller) to those found in the voluntary exit regressions.

5.2 Job push

Results for my job push analysis can be found in Table 7. Here the econometric model is given by equation 3.7 and the dependent variable is moves to jobs with ECHI. Because of the redefinition of the ECHI and Medicaid eligibility variables, the signs of the coefficients that are consistent with job push effects are the same as they were in the job lock regressions: negative δ_5 and positive δ_4. For the full job push sample and the model without state-by-year dummies, column (1), the preferred estimator, $\hat{\pi}_1$ (the negative of the δ_5 coefficient), produces a job push estimate of 0.47, which is statistically significant at the one percent level. This means that an additional household member eligible for Medicaid appears to decrease the worker's likelihood of moving to a job with ECHI by a little less than half a percentage point. In my job push sample, for households where all members are without health insurance, men transition into jobs with ECHI over four month periods at an overall rate of 1.8%. Based on this benchmark, the estimate of 0.47 implies that eligibility decreases job push by 26%. The second job push estimator, $\hat{\pi}_2$ (the δ_4 coefficient), produces a much larger estimate of 1.2 percentage points – a job push decrease of almost 67%. Column (2) shows that the addition of state-by-year dummies to the econometric model makes little difference in the estimates.

Turning to the sample of married workers, column (3) shows that $\hat{\pi}_1$ produces an estimate of 0.29, which is significant at the ten percent level. This estimate implies a job push decrease of 16%. The alternative estimator, $\hat{\pi}_2$, estimates job push at 0.97 (significant at one percent), a 54% decrease. Adding state-by-year dummies again makes little difference on the estimates: 0.30 (significant at ten percent, a 17% decrease) from $\hat{\pi}_1$ and 1.0 (significant at one percent, a 56% decrease) from $\hat{\pi}_2$. It is noticeable that the estimates from $\hat{\pi}_1$ in columns (3) and (4) are smaller than those in columns (1) and (2), though if there were no change in the standard errors, the estimates in columns (3) and (4) would be significant at the five percent level.

Dropping the top income individuals from the sample, as in columns (5) and (6), results in estimates that are nearly identical to those with the high earners. The main difference is a slight loss in precision from the loss of observations. Because of this, the estimates produced by $\hat{\pi}_1$ are not statistically significant, despite being same as the corresponding estimates in columns (3) and (4).[19]

5.3 Estimates using a state-level measure of eligibility

Despite that my regression models include several features intended to minimize bias due to endogeneity, there still might be concern about my estimates due to the fact that Medicaid eligibility can be chosen by an individual by working fewer hours or choosing a job that pays less. Perhaps more importantly, there is also concern that imputed Medicaid eligibility might be measured with error, something for which I have not yet made any attempt to compensate. Since imputed eligibility relies on the measurement of individual characteristics via the SIPP, if those characteristics are mis-measured in the survey, the resulting imputation would also contain

[19] I note here that I do not present any placebo regressions for my job push analysis as there are no appropriate outcomes available. A valid placebo dependent variable would have to be something similar to transitions into jobs with ECHI, but where it is clear that eligibility for Medicaid would have no influence. In the job lock case, I relied on the fact that involuntary terminations are the employer's choice, not the employee. An analogous outcome in the job push case would require the observation of situations where an individual did not choose to transition into a job – *but did anyway*. Obviously such situations do not exist.

error. Moreover, this mis-measurement could be compounded due to the use of individual-level fixed effects.

To address both of these issues, I calculate a state-level measure of the generosity of Medicaid eligibility rules, and use it to replace individual-level eligibility in my regressions. Here state-level generosity is measured by the probability of eligibility in a given state conditional on individual-level characteristics that are more convincingly exogenous.[20] In my econometric models, I indicate the probability of eligibility as I_{itj} (since it was used as an instrumental variable in most of the previous literature). The value of I_{itj} varies on the state of residence, time (quarter), age, and education level (for kids, education level is based on household adults). Before describing the calculation of I_{itj} more carefully, the basic idea is to start with a static, national sample – static to eliminate the possibility of time trends in population characteristics, national to remove state-specific characteristics – and calculate the eligibility of the whole population *as if they all lived in a given state*, regardless of where the people actually lived. The probability of eligibility, then, is taken as the fraction of eligible individuals in this sample within the state, time, age, and education level categories.

The advantage of this strategy is that this state-level measure of eligibility no longer depends on characteristics that are often changed by the individual (state and education can vary in concept but seldom do for working age adults), and so it reduces the threat of endogeneity. Additionally, since the state-level calculation is based on the imputed eligibility for many individuals, the impact of mis-measurement for individuals plays a lesser role. The problem is transformed from one of measuring eligibility for particular individuals to one of measuring the policies of a state – which a fundamentally easier task. Thus, we would expect the errors-in-variables problem to be reduced by this approach. The major caveat, though, is that this approach will not address any mis-measurement introduced by errors in the Medicaid imputation program itself.[21]

The calculation of I_{itj} starts with the entire first wave sample from the 1990 panel of the SIPP, which has the largest sample size of any wave in my data. For each state and each quarter in my panel, I impute eligibility for *almost* all individuals in the sample as if they were residents of the state in the given quarter. I write almost because no individuals were used in the calculation of the eligibility probability for the state in which they were actual residents during the first wave of the 1990 panel. So the measure of state-level eligibility is, in a sense, a type of "leave-one-out estimator". This is intended to reduce the influence of state-specific

[20] This is essentially the strategy relied upon by Currie and Gruber (1996a), Currie and Gruber (1996b), Cutler and Gruber (1996), Gruber and Yelowitz (1999), Ham and Shore-Sheppard (2005), and Gruber and Simon (2008), who were all working in other research areas; Dave et al. (2013), who studied crowd-out and, to a lesser-extent, job push; and has similarities to the approach of Hamersma and Kim (2009) in the job lock literature.

[21] Currie and Gruber (1996a), Currie and Gruber (1996b), and Cutler and Gruber (1996) have more extensive discussion about the exogeneity and measurement error characteristics of I_{itj}.

population characteristics on the calculation of I_{itj} (and follows the method used by Ham and Shore-Sheppard, 2005). I adjust incomes used in the imputation process for inflation using the Consumer Price Index (so dollar amounts reflect prices prevailing during the given quarter, not 1990), but all the other characteristics of the individuals in the first-wave of the 1990 panel are fixed as they were at that time.

Once this eligibility imputation process is complete, I compute the weighted average of imputed eligibility (using the SIPP final person weights) within the state, quarter, age, and education level categories. For children, there is a separate age category for each year from age zero through 20. The education level is taken as the highest education level of adults 18 years or older in the household (less than high school, high school graduate, some college, or four years of college or more). If there are no adults in the household, then the education level of the child him- or herself is used.[22] For women aged 15 or older, the education level of the woman herself is used, and age groups are one for each year for those 22 or younger, and then two-year groups for those 23 and older. The full interaction of these age and education levels along with the state and quarter variables creates the average eligibility categories – with one important exception: education level is not used in creating the categories for women 22 or younger. The reason for this is that many people are still in the process of determining their final education level at that age. So instead of interacting age with education level, I used a different age category for each year. After 22, the age categories are two-year groups to ensure enough individuals in each age-by-education category. Finally, since that women age 15 through 20 could be eligible for Medicaid either as a child or for pregnancy coverage, for women in this age range I use the greater of the two probabilities as their state-level measures of eligibility.

Given this calculation of I_{itj}, it replaces M_{itj} in the models used to estimate job lock and job push above. For job lock, the model becomes

$$\begin{aligned} Y_{i(t+1)} = \beta_1 \left(\sum_{j=1}^{J_{it}} E_{itj} \right) + \beta_2 \left(\sum_{j=1}^{J_{it}} I_{itj} \right) + \beta_3 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} \right) \\ + \beta_4 \left(\sum_{j=1}^{J_{it}} E_{itj} I_{itj} \right) + \beta_5 \left(\sum_{j=1}^{J_{it}} E_{itj} N_{itj} I_{itj} \right) + X'_{it}\gamma + u_{it}. \end{aligned} \tag{5.1}$$

[22]Note that in this discussion I am dealing with nearly the entire SIPP sample – not the sample used in my analysis. My analysis is performed on households with at least one male adult, but my calculation of state-wide eligibility includes individuals all types of households, such as those with without any male adults, or even any adults at all, in some cases. This is necessary to calculate eligibility rates for children and women who might be household members of the workers I study.

For job push, after defining $\tilde{I}_{itj} = 1 - I_{itj}$ and replacing $\tilde{M}_{itj}$, the model becomes

$$\begin{aligned} Z_{i(t+1)} = \delta_1 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \right) + \delta_2 \left(\sum_{j=1}^{J_{it}} \tilde{I}_{itj} \right) + \delta_3 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \right) \\ + \delta_4 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} \tilde{I}_{itj} \right) + \delta_5 \left(\sum_{j=1}^{J_{it}} \tilde{E}_{itj} N_{itj} \tilde{I}_{itj} \right) + X_{it}' \theta + \varepsilon_{it}. \end{aligned} \tag{5.2}$$

These models are then estimated on the same samples as the individual-level eligibility models.

In interpreting the results of the state-level measure models above, the focus should be on whether they tell the same story as the main regressions in terms of the signs and magnitudes of the point estimates. If endogeneity and measurement error are truly problems that cause faulty conclusions, then the coefficient estimates should be drastically different when using the state-level measure that is thought to suffer less from these problems. Therefore, the state-level results are intended to be viewed as robustness-checks on the individual-level results, rather than primary analyses of their own.

Table 8 reports estimates for equations 5.1 and 5.2. The top panel presents the job lock figures, which are substantially consistent with the individual-level results. In nearly all cases, the estimates have signs that are predicted by the job lock theory. The exceptions are the figures from the alternative estimator, $\hat{\pi}_1$, on the married, excluding top incomes sample (which, it is worth noting, have significantly larger standard errors than estimates from the other samples). Somewhat surprisingly considering the fact that the state-level data has less variation, the state-level results share the same pattern of statistical significance across estimates as the individual-level estimates. Turning to the bottom panel of Table 8, which presents estimates using the job push sample, there is again a high degree of consistency between these results and the individual-level ones. In this case, all estimates have the sign predicted by the job push theory, and this analysis again shares the same pattern of statistical significance across estimates as the individual-level analysis had. Taken as a whole, then, this analysis based on state-level eligibility is inconsistent with the argument that the main, individual-level estimates are severely affected by endogeneity or measurement error biases.

6 Conclusion

The results of my analysis suggest that job lock and job push had extensive presences in the USA during the late 1980s and early 1990s, and that the Medicaid expansion resulted in large reductions in the magnitudes of their effects. For job lock, the largest effects of the expansions are estimated on my sample of married workers without incomes in the top decile. For these individuals, I find increases in job exit rates in the range of 65% to 71% over four month periods for each household member who became Medicaid eligible. Estimates from other samples are similar in that they have magnitudes that are relatively large, though usually not statistically significant at conventional levels. Similarly, for job push, I also find evidence of large effects. For the full sample, the preferred estimator suggests Medicaid eligibility for one household member decreases transitions into jobs with ECHI by about 26% to 28%. Once again, similar results are obtained across the other samples as well, though these job push estimates tend to be more precisely estimated than the job lock ones, resulting in most being statistically significant. Moreover, for both job lock and job push, when individual-level Medicaid eligibility is replaced in my econometric models with a state-level measure to address endogeneity and measurement error, very similar results are obtained.

It should be noted that my estimates, though they are already relatively large, possibly understate true job mobility effects of the expansion because there were undoubtedly some number of workers that were not aware of their Medicaid eligibility, and hence would not have behaved as if they were insured. My estimates, therefore, represent an average of effects between those who felt no change in their reliance on their jobs for health insurance – presumably a zero effect – and those who did. The effect for the true population of interest – those who were aware of their eligibility – would thus apparently be larger than the effects I estimate.

Another point that should be made here is that the effects measured in this analysis originate from Medicaid eligibility of household members of workers – not from coverage for workers themselves. This underscores the importance household members play in workers' job mobility decisions, and suggests that their role in job market decision merits additional research.

Finally, in considering the implications of my results for the job market, it should not be overlooked that although my estimates are large in a relative sense, they are small in an absolute sense. For example, in the case of job lock, the benchmark job exit rate I use is 1.7%. The largest job lock estimate in my analysis implied one additional household member becoming Medicaid eligible would cause this rate to rise to 2.9%, implying that the worker would *not* exit his job more than 97% of the time over a four month period. This means that, were government insurance to be provided to a large portion of the labor market, we should not expect to observe transformative effects over the short run. The question as to whether small short run effects could have cumulative impact over the long run is left unanswered by my analysis, though, due to the short run nature of my data. That said, a sensible argument could be made that a long run impact would not be surprising: small, short run effects accrue over the course of a career and result in missed opportunities to develop skills and accumulate human capital, resulting in diminished earnings potential over the long run. The empirical validity of this argument is another open question worthy of future research.

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Table 1: Nationwide Medicaid eligibility over time

	Fraction of population	
Year	Children (under 21)	Women (21 or over)
1986	0.210	0.105
1987	0.220	0.106
1988	0.211	0.113
1989	0.222	0.139
1990	0.241	0.185
1991	0.295	0.217
1992	0.321	0.240
1993	0.379	0.248

All estimates are calculated for the month of March (which tends to have large sample sizes due to panel overlap) of the given year and are weighted using month-level, individual, cross-sectional weights.

Table 2: Identification of *job lock* parameters

Example Model: $Y_{i(t+1)} = \beta_1 E_{it} + \beta_2 M_{it} + \beta_3 E_{it} N_{it} + \beta_4 E_{it} M_{it} + \beta_5 E_{it} N_{it} M_{it} + u_{it}$

	ECHI (E_{it}) and Non-ECHI (N_{it}) Insurance Status		
Medicaid Eligible?	Treatment Group $E_{it} = 1, N_{it} = 0$	Preferred Control Group $E_{it} = 1, N_{it} = 1$	Alternative Control Group $E_{it} = 0, N_{it} = 1$
$M_{it} = 1$	$E[Y_{i(t+1)} \vert E_{it} = 1, N_{it} = 0, M_{it} = 1]$ $= \beta_1 + \beta_2 + \beta_4$	$E[Y_{i(t+1)} \vert E_{it} = 1, N_{it} = 1, M_{it} = 1]$ $= \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5$	$E[Y_{i(t+1)} \vert E_{it} = 0, N_{it} = 1, M_{it} = 1]$ $= \beta_2$
$M_{it} = 0$	$E[Y_{i(t+1)} \vert E_{it} = 1, N_{it} = 0, M_{it} = 0]$ $= \beta_1$	$E[Y_{i(t+1)} \vert E_{it} = 1, N_{it} = 1, M_{it} = 0]$ $= \beta_1 + \beta_3$	$E[Y_{i(t+1)} \vert E_{it} = 0, N_{it} = 1, M_{it} = 0]$ $= 0$
Column Difference	$= \beta_2 + \beta_4$	$= \beta_2 + \beta_4 + \beta_5$	$= \beta_2$

Job lock parameter 1: $\boldsymbol{\lambda_1} \equiv \beta_2 + \beta_4 - \beta_2 - \beta_4 - \beta_5 = \boldsymbol{-\beta_5}$

Job lock parameter 2: $\boldsymbol{\lambda_2} \equiv \beta_2 + \beta_4 - \beta_2 = \boldsymbol{\beta_4}$

Table 3: Restrictions to the SIPP sample of men (1986 - 1988, 1990, & 1991 panels)

Sample restrictions	Person - wave records	Unique individuals
All month four records, excluding the last interview of each panel*	554,742	104,768
Age 21 to 55 during all waves	259,573	49,541
Valid interview for all panel waves	188,594	30,249
Uniquely identifiable state all waves	179,913	28,851
At most one move between states	178,972	28,700
Not an SSI recipient	176,664	28,334
Job lock sample		
All household members have employer provided coverage	114,890	22,667
One job at end of wave and start of next	96,565	19,811
Observed in job lock sample for at least two waves	94,821	18,067
Positive panel weight	94,809	18,064
Job push sample		
Sample member does not have ECHI *or* the entire household has employer coverage from a household member besides the sample member	71,645	16,291
No job *or* one job at end of wave and start of next	67,399	16,022
Observed in job push sample for at least two waves	64,690	13,313
Positive panel weight	64,664	13,309

*Also excludes ten records which did not have an assigned gender in the SIPP longitudinal files.

Table 4: Sample averages and standard deviations
Figures are presented as percentages (multiplied by 100), except where noted by *

Job lock sample			Job push sample		
Variable	Average	Std.Dev.	Variable	Average	Std.Dev.
Quit/retired ($Y_{i(t+1)}$)	2.160	14.537	Moved to ECHI ($Z_{i(t+1)}$)	1.713	12.977
Laid-off/fired	1.208	10.923			
ECHI	91.922	27.250	ECHI	12.930	33.554
Non-ECHI	18.374	38.728	Non-ECHI	60.320	48.924
Medicaid eligible	0.091	3.022	Medicaid eligible	3.425	18.188
$\sum_j E_{itj}$*	2.558	1.655	$\sum_j \tilde{E}_{itj}$*	3.036	1.815
$\sum_j M_{itj}$*	0.177	0.605	$\sum_j \tilde{M}_{itj}$*	2.802	1.461
$\sum_j E_{itj}N_{itj}$*	0.352	0.959	$\sum_j \tilde{E}_{itj}N_{itj}$*	1.809	1.758
$\sum_j E_{itj}M_{itj}$*	0.142	0.563	$\sum_j \tilde{E}_{itj}\tilde{M}_{itj}$*	2.456	1.617
$\sum_j E_{itj}N_{itj}M_{itj}$*	0.010	0.115	$\sum_j \tilde{E}_{itj}N_{itj}\tilde{M}_{itj}$*	1.640	1.647
$\sum_j I_{itj}$*	0.420	0.482	$\sum_j \tilde{I}_{itj}$*	2.891	1.263
$\sum_j E_{itj}I_{itj}$*	0.325	0.461	$\sum_j \tilde{E}_{itj}\tilde{I}_{itj}$*	2.577	1.490
$\sum_j E_{itj}N_{itj}I_{itj}$*	0.046	0.172	$\sum_j \tilde{E}_{itj}N_{itj}\tilde{I}_{itj}$*	1.586	1.533
Age*	36.810	8.412	Age*	35.713	9.150
Earnings decile*	7.507	2.207	Earnings decile*	5.588	2.727
Household income decile*	6.915	2.265	Household income decile*	5.501	2.951
Married	76.149	42.617	Married	65.295	47.603
Previously married	8.650	28.111	Previously married	9.317	29.067
Never married	15.201	35.903	Never married	25.388	43.523
Less than HS	9.565	29.411	Less than HS	20.088	40.066
High School	35.963	47.989	High School	36.227	48.066
Some college	22.830	41.974	Some college	22.728	41.908
College	31.643	46.508	College	20.957	40.701
Private job	82.426	38.060	Private job	83.666	36.968
Public job	17.574	38.060	Public job	16.334	36.968
Has child under 2	11.754	32.206	Has child under 2	11.814	32.278
Has child 2 to 5	21.857	41.328	Has child 2 to 5	20.501	40.371
Has child 6 to 17	41.237	49.226	Has child 6 to 17	38.592	48.682
Count of HH members*	2.174	1.404	Count of HH members*	2.390	1.558
Observations	94,809		Observations	64,664	

All estimates are weighted using panel weights adjusted as described in footnote 12. All control variables were implemented in regressions as category dummies, but in order to save space here, firm type and marital status categories have been condensed, and age, earning and income deciles, and counts of household members are represented as numbers. Complete child age interactions used in the regressions are also not presented for the same reason.

Table 5: Job lock results: dependent variable = voluntary job exits
(All estimates multiplied by 100)

	Full Sample		Married Only			
					Excludes Top Incomes	
Coefficient – Variable	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_1 - \sum_j E_{itj}$	-0.244*	-0.267**	-0.159	-0.155	-0.153	-0.184
	(0.13)	(0.13)	(0.14)	(0.14)	(0.18)	(0.18)
	[0.07]	[0.04]	[0.25]	[0.25]	[0.41]	[0.31]
$\beta_2 - \sum_j M_{itj}$	-0.546	-0.509	-1.051*	-1.000*	-1.015	-0.899
	(0.41)	(0.43)	(0.58)	(0.61)	(0.66)	(0.66)
	[0.19]	[0.24]	[0.07]	[0.10]	[0.12]	[0.17]
$\beta_3 - \sum_j E_{itj}N_{itj}$	0.056	0.052	0.052	0.031	0.137	0.134
	(0.11)	(0.11)	(0.12)	(0.13)	(0.16)	(0.18)
	[0.62]	[0.65]	[0.67]	[0.81]	[0.40]	[0.45]
$\beta_4 - \sum_j E_{itj}M_{itj}$ ($\hat{\lambda}_2$ estimator)	0.463	0.425	0.908	0.882	0.878	0.777
	(0.44)	(0.46)	(0.57)	(0.60)	(0.65)	(0.66)
	[0.29]	[0.35]	[0.11]	[0.14]	[0.18]	[0.24]
$\beta_5 - \sum_j E_{itj}N_{itj}M_{itj}$ ($-\hat{\lambda}_1$ estimator)	-0.485	-0.458	-0.566*	-0.492	-1.212***	-1.089***
	(0.31)	(0.34)	(0.30)	(0.35)	(0.38)	(0.38)
	[0.12]	[0.17]	[0.06]	[0.16]	[0.00]	[0.00]
Person-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year dummies	No	Yes	No	Yes	No	Yes
Observations	94,809	94,809	70,252	70,252	49,359	49,359

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 3.3 for details. Samples for Columns (3) through (6) include only those who were married every time they were observed. Columns (5) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 6: Job lock results: dependent variable = involuntary job exits (placebo outcome) (All estimates multiplied by 100)

			Married Only			
	Full Sample				Excludes Top Incomes	
Coefficient − Variable	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_1 - \sum_j E_{itj}$	0.386***	0.388***	0.329**	0.328**	0.393**	0.378**
	(0.14)	(0.14)	(0.17)	(0.17)	(0.19)	(0.19)
	[0.01]	[0.01]	[0.05]	[0.05]	[0.04]	[0.05]
$\beta_2 - \sum_j M_{itj}$	-0.195	-0.183	-0.428	-0.404	-0.523	-0.489
	(0.36)	(0.37)	(0.40)	(0.40)	(0.37)	(0.38)
	[0.59]	[0.62]	[0.28]	[0.31]	[0.15]	[0.20]
$\beta_3 - \sum_j E_{itj} N_{itj}$	-0.076	-0.087	-0.058	-0.064	-0.084	-0.090
	(0.06)	(0.06)	(0.06)	(0.06)	(0.09)	(0.08)
	[0.18]	[0.12]	[0.34]	[0.28]	[0.32]	[0.27]
$\beta_4 - \sum_j E_{itj} M_{itj}$ ($\hat{\lambda}_2$ estimator)	-0.093	-0.098	0.231	0.214	0.332	0.312
	(0.36)	(0.37)	(0.37)	(0.37)	(0.36)	(0.36)
	[0.80]	[0.79]	[0.53]	[0.56]	[0.35]	[0.39]
$\beta_5 - \sum_j E_{itj} N_{itj} M_{itj}$ ($-\hat{\lambda}_1$ estimator)	-0.167	-0.168	-0.195	-0.230	-0.337	-0.384
	(0.34)	(0.34)	(0.32)	(0.32)	(0.41)	(0.40)
	[0.62]	[0.62]	[0.55]	[0.48]	[0.41]	[0.34]
Person-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year dummies	No	Yes	No	Yes	No	Yes
Observations	94,809	94,809	70,252	70,252	49,359	49,359

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 3.3 for details. Samples for Columns (3) through (6) include only those who were married every time they were observed. Columns (5) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 7: Job push results: dependent variable = moves to ECHI jobs
(All estimates multiplied by 100)

	Full Sample		Married Only		Excludes Top Incomes	
Coefficient – Variable	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_1 - \sum_j \tilde{E}_{itj}$	0.313	0.299	0.327	0.295	0.408	0.401
	(0.24)	(0.24)	(0.35)	(0.33)	(0.39)	(0.37)
	[0.19]	[0.22]	[0.35]	[0.37]	[0.30]	[0.28]
$\delta_2 - \sum_j \tilde{M}_{itj}$	-0.980***	-1.016***	-0.705***	-0.753***	-0.693**	-0.747**
	(0.29)	(0.30)	(0.26)	(0.25)	(0.32)	(0.32)
	[0.00]	[0.00]	[0.01]	[0.00]	[0.03]	[0.02]
$\delta_3 - \sum_j \tilde{E}_{itj} N_{itj}$	-0.156	-0.135	-0.265	-0.261	-0.252	-0.247
	(0.16)	(0.15)	(0.21)	(0.21)	(0.22)	(0.22)
	[0.32]	[0.37]	[0.21]	[0.21]	[0.26]	[0.26]
$\delta_4 - \sum_j \tilde{E}_{itj} \tilde{M}_{itj}$ ($\hat{\pi}_2$ estimator)	1.165***	1.219***	0.973***	1.022***	0.951***	0.993***
	(0.27)	(0.29)	(0.28)	(0.27)	(0.32)	(0.33)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
$\delta_5 - \sum_j \tilde{E}_{itj} N_{itj} \tilde{M}_{itj}$ ($-\hat{\pi}_1$ estimator)	-0.467***	-0.506***	-0.294*	-0.303*	-0.294	-0.303
	(0.14)	(0.14)	(0.18)	(0.18)	(0.20)	(0.20)
	[0.00]	[0.00]	[0.10]	[0.09]	[0.13]	[0.13]
Person-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year dummies	No	Yes	No	Yes	No	Yes
Observations	64,664	64,664	42,347	42,347	35,424	35,424

Estimates are weighted using panel weights adjusted as described in footnote 12. Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Both cluster variables indicate state of residence, and can be different for movers. See Section 3.3 for details. Samples for Columns (3) through (6) include only those who were married every time they were observed. Columns (5) and (6) also exclude any observations for individuals with either job earnings or household income in the top decile. Additional, unlisted control variables include dummies for: child age (indicators for a child under two, two through five, and six through 17, and these are fully interacted); education (less than high school, high school diploma, some college, and four years of college or more); marital status (married – spouse present, married – spouse absent, widowed, divorced, separated, never married); five-year-age group, meaning 21-24, 25-29, 30-34, and so on; job earnings and household earnings deciles; firm type (not working, private, non-profit, federal, state , or local government, armed forces, or unpaid family business or farm); and number of household members, with five or more treated as one group.

Table 8: Estimates using a state-level measure of eligibility (estimates multiplied by 100)

JOB LOCK: *dependent variable = voluntary job exits*

			Married Only			
	Full Sample				Excludes Top Incomes	
Coefficient – Variable	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_4 - \sum_j E_{itj} I_{itj}$ ($\hat{\lambda}_2$ estimator)	0.922	0.680	0.653	0.562	-0.220	-0.151
	(0.67)	(0.64)	(0.67)	(0.63)	(0.85)	(0.84)
	[0.17]	[0.29]	[0.33]	[0.38]	[0.80]	[0.86]
$\beta_5 - \sum_j E_{itj} N_{itj} I_{itj}$ ($-\hat{\lambda}_1$ estimator)	-0.361	-0.294	-1.082*	-0.910	-1.544**	-1.371*
	(0.59)	(0.60)	(0.62)	(0.65)	(0.75)	(0.83)
	[0.54]	[0.63]	[0.08]	[0.16]	[0.04]	[0.10]
Person-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year dummies	No	Yes	No	Yes	No	Yes
Observations	94,809	94,809	70,252	70,252	49,359	49,359

JOB PUSH: *dependent variable = moves to ECHI jobs*

			Married Only			
	Full Sample				Excludes Top Incomes	
Coefficient – Variable	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_4 - \sum_j \tilde{E}_{itj} \tilde{I}_{itj}$ ($\hat{\pi}_2$ estimator)	1.704***	1.880***	1.650***	1.663***	1.710***	1.645**
	(0.49)	(0.55)	(0.55)	(0.62)	(0.66)	(0.77)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.03]
$\delta_5 - \sum_j \tilde{E}_{itj} N_{itj} \tilde{I}_{itj}$ ($-\hat{\pi}_1$ estimator)	-0.993***	-1.060***	-0.770**	-0.834**	-0.711	-0.780
	(0.36)	(0.36)	(0.38)	(0.41)	(0.45)	(0.47)
	[0.01]	[0.00]	[0.04]	[0.04]	[0.11]	[0.10]
Person-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year dummies	No	Yes	No	Yes	No	Yes
Observations	64,664	64,664	42,347	42,347	35,424	35,424

Two-way-clustered standard errors in parentheses, p-values in brackets. Statistical significance for two-sided t-tests indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. All models are the same as in Tables 5 and 7, except the Medicaid eligibility indicator variable has been replaced with a state-level measure of eligibility, I, as described in Section 5.3. See notes for Tables 5 or 7 for model and standard error details.

Appendix A

To show that link between the simple version of my job lock model (equation 3.3) and the full, empirical model (equation 3.2), I start with the following, hypothetical regression:

$$\begin{aligned} Y_{i(t+1)} &= \beta_{11}E_{it1} + \beta_{21}M_{it1} + \beta_{31}E_{it1}N_{it1} + \beta_{41}E_{it1}M_{it1} + \beta_{51}E_{it1}N_{it1}M_{it1} \\ &+ \beta_{12}E_{it2} + \beta_{22}M_{it2} + \beta_{32}E_{it2}N_{it2} + \beta_{42}E_{it2}M_{it2} + \beta_{52}E_{it2}N_{it2}M_{it2} \\ &\dots \\ &+ \beta_{1J_{it}}E_{itJ_{it}} + \beta_{2J_{it}}M_{itJ_{it}} + \beta_{3J_{it}}E_{itJ_{it}}N_{itJ_{it}} + \beta_{4J_{it}}E_{itJ_{it}}M_{itJ_{it}} + \beta_{5J_{it}}E_{itJ_{it}}N_{itJ_{it}}M_{itJ_{it}} \\ &+ X'_{it}\gamma + u_{it}. \end{aligned} \tag{A.1}$$

In this model, everyone in the household has his or her own dual DD model (each on a separate line above) that produces two individualized estimates of his or her effect on the worker's job leaving rate. That is, two estimates of λ would be generated for each person in the household, and each person's estimates could be different from all others. This specification is flexible, but implementation would be difficult, as would be interpretation, since it would depend on how household members are indexed across households. To address this, I make the assumption that each of the J_{it} people in the household carry the same weight in determining the worker's job leaving behavior. This assumption implies, for example, that a father does not care more about his wife losing insurance coverage than his child, but wants both to have it and acts equally in response to both of their insurance statuses. To implement this assumption, I restrict all the coefficients on corresponding insurance status variables to be equal across individuals in the household: $\beta_{hj} = \beta_h$ for all j and $h = 1, 2, \dots 5$. Given this assumption, equation A.1 collapses to the model I use in my analysis, equation 3.2.

The interpretation of the coefficients for the full model can be shown by adapting the argument used to derive the interpretation of the simple model parameters. Start by adapting the definition of λ_1 for the full econometric model:

$$\begin{aligned} \lambda_1 \equiv\ & E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 0, M_{itj} = 1] - E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 0, M_{itj} = 0] \\ & - \left(E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 1, M_{itj} = 1] - E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 1, M_{itj} = 0]\right). \end{aligned} \tag{A.2}$$

Note that each of these expectations also conditions on the other control variables, X, and the insurance status variables for the other household members besides j, but I suppress them here to simplify the notation. Next, define five variables, e, m, en, em, and enm, such that if $E_{itj} = 1$, $N_{itj} = 0$, and $M_{itj} = 0$, then $\sum_j E_{itj} = e$, $\sum_j M_{itj} = m$, $\sum_j E_{itj}N_{itj} = en$, $\sum_j E_{itj}M_{itj} = em$, and $\sum_j E_{itj}N_{itj}M_{itj} = enm$.

Using the above defined variables, it is possible to write the conditional expectations in equation A.2 as follows:

$$E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 0, M_{itj} = 1] = \beta_1 e + \beta_2(m+1) + \beta_3 en + \beta_4(em+1) + \beta_5 enm + X'_{it}\gamma$$

$$E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 0, M_{itj} = 0] = \beta_1 e + \beta_2 m + \beta_3 en + \beta_4 em + \beta_5 enm + X'_{it}\gamma$$

$$E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 1, M_{itj} = 1] = \beta_1 e + \beta_2(m+1) + \beta_3(en+1) + \beta_4(em+1) + \beta_5(enm+1) + X'_{it}\gamma$$

$$E[Y_{i(t+1)}|E_{itj} = 1, N_{itj} = 1, M_{itj} = 0] = \beta_1 e + \beta_2 m + \beta_3(en+1) + \beta_4 em + \beta_5 enm + X'_{it}\gamma.$$

These conditional expectations can then be substituted into equation A.2 to show that $\lambda_1 = -\beta_5$.

The same approach can be used to show that $\lambda_2 = \beta_4$ in the full model, and that the corresponding relationships hold for the job push model, as well.

Appendix B

To show that including transitions into jobs without ECHI in the outcome variable for a job push analysis leads to bias, I begin by defining two additional variables, W and V, to supplement the already defined Z (dropping subscripts that are unnecessary for this discussion). $W = 1$ if the worker moves into a job without ECHI, and $W = 0$ otherwise, and $V \equiv Z + W$ – that is, $V = 1$ indicates movement into any job, and $V = 0$ otherwise. A job push analysis using V as the outcome of interest would identify

$$E[V|seeking\ ECHI] - E[V|not\ seeking\ ECHI].$$

Substitution for V allows this to be decomposed into a job push effect plus a bias term

$$\begin{aligned} E[V|seeking\ ECHI] - E[V|not\ seeking\ ECHI] = \\ E[Z|seeking\ ECHI] - E[Z|not\ seeking\ ECHI] + \\ E[W|seeking\ ECHI] - E[W|not\ seeking\ ECHI] = \\ \pi + \underline{E[W|seeking\ ECHI] - E[W|not\ seeking\ ECHI]}. \end{aligned}$$

If there is a true job push effect, then we would not expect the underlined bias term to equal zero. We would expect the likelihood of moving into a job without ECHI for someone seeking ECHI to be less than that likelihood if that person were not seeking ECHI. Hence, we would expect the underlined term to be negative.

Appendix C

As I did in discussing the interpretation of the job lock model, I consider a simple version of the full empirical model for my job push analysis, hoping that this will make the rationale more transparent. This simple model is given by

$$Z_{i(t+1)} = \delta_1 \tilde{E}_{it} + \delta_2 \tilde{M}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{M}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{M}_{it} + X'_{it}\theta + \varepsilon_{it}, \tag{C.1}$$

where the j index has been omitted since here I focus on the worker only. This model can be used to identify two separate job push parameters in a manner that is analogous to the method used for the job lock case:

$$\begin{aligned} \pi_1 \equiv\ & E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1] - E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0] \\ & - \Big(E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 1] - E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 0]\Big) = -\delta_5, \end{aligned} \tag{C.2}$$

and

$$\begin{aligned} \pi_2 \equiv\ & E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1] - E[Z_{i(t+1)}|\tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0] \\ & - \Big(E[Z_{i(t+1)}|\tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 1] - E[Z_{i(t+1)}|\tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 0]\Big) = \delta_4. \end{aligned} \tag{C.3}$$

The equivalence of these job push parameters to the δ_4 and $-\delta_5$ parameters of the model is detailed in Appendix Table C.1, thereby justifying $\hat{\pi}_1 = -\hat{\delta}_5$ and $\hat{\pi}_2 = \hat{\delta}_4$ as DD job push estimators.

Moving from this simple model to the full, empirical model in equation 3.7 is easily shown via analogous reasoning as used for the same move from simple to full model in the job lock case (see Appendix A).

Appendix Table C.1: Identification of *job push* parameters*

Example Model: $Z_{i(t+1)} = \delta_1 \tilde{E}_{it} + \delta_2 \tilde{M}_{it} + \delta_3 \tilde{E}_{it} N_{it} + \delta_4 \tilde{E}_{it} \tilde{M}_{it} + \delta_5 \tilde{E}_{it} N_{it} \tilde{M}_{it} + \varepsilon_{it}$

	ECHI ($\tilde{E}_{it}$) and Non-ECHI (N_{it}) Insurance Status		
Medicaid	Treatment Group	Preferred Control Group	Alternative Control Group
Eligible?	$\tilde{E}_{it} = 1, N_{it} = 0$	$\tilde{E}_{it} = 1, N_{it} = 1$	$\tilde{E}_{it} = 0, N_{it} = 1$
$\tilde{M}_{it} = 1$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 1]$ $= \delta_1 + \delta_2 + \delta_4$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 1]$ $= \delta_1 + \delta_2 + \delta_3 + \delta_4 + \delta_5$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 1]$ $= \delta_2$
$\tilde{M}_{it} = 0$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 0, \tilde{M}_{it} = 0]$ $= \delta_1$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 1, N_{it} = 1, \tilde{M}_{it} = 0]$ $= \delta_1 + \delta_3$	$E[Z_{i(t+1)} \mid \tilde{E}_{it} = 0, N_{it} = 1, \tilde{M}_{it} = 0]$ $= 0$
Column Difference	$= \delta_2 + \delta_4$	$= \delta_2 + \delta_4 + \delta_5$	$= \delta_2$

Job push parameter 1: $\boldsymbol{\pi_1} \equiv \delta_2 + \delta_4 - \delta_2 - \delta_4 - \delta_5 \boldsymbol{= -\delta_5}$

Job push parameter 2: $\boldsymbol{\pi_2} \equiv \delta_2 + \delta_4 - \delta_2 \boldsymbol{= \delta_4}$

*Note that here $\tilde{E}_{it} = 1$ implies *no* ECHI and $\tilde{M}_{it} = 1$ implies *not* Medicaid eligible.

Appendix D

Tables D.1 and D.2 present hypothetical data illustrating creation of the cluster-group variables for implementation of two-way clustering in my analyses. The use of these variables is described in Section 3.3 of the text.

Table D.3 summarizes the types of observations that will be allowed to be correlated under this method. For a given pair of individuals, they are either both stayers, both movers, or there is one stayer and one mover. Table D.3 divides the observations into these categories and lists them by whether the individuals have any values in common for either cluster variable. For those pairs that do have common values for a cluster variable, general forms of correlation are permitted, while for those with no common values, the correlation is assumed to be zero. As compared to the alternative of clustering on only one state, this method allows for correlation in two additional categories that would have been assumed to be zero. These additional categories of correlation (rows (4) and (8)) are identified in Table D.3 by row shading.

The primary weakness of using this method of clustering is that one could argue that correlation should be allowed for the categories found in rows (9), (10), and (11), but because there is no common value between observations for either cluster variable, the correlation is assumed to be zero. For example, suppose Person One lived in California for waves one through four then moved to South Carolina, while Person Two lived in South Carolina the first wave then moved to California for the rest of the panel (i.e. the case found in row (9)). These two people both lived in California at the same time for three waves, yet their error terms are assumed uncorrelated. Therefore, to the extent that this type of correlation is truly important in my sample, my standard error estimators will be inconsistent. However, as compared to available alternatives, this method would seem to be an improvement.[23]

[23]Individuals who move more than once over the course of a panel are not included in my sample (see Section 4). However, these individuals could be included, in principle, and this method of clustering extended by creating variables for all of the states where individuals lived and using higher dimension clustering in the same fashion. The weakness regarding the handling of movers living in a place at the same time but without common values for any cluster variable would be compounded, though, so this method may not be justified in some cases.

Appendix Table D.1: State variable creation example – stayer

ID Variable	Wave	Actual State of Residence	First State	Second State
101	1	CA	CA	CA
101	2	CA	CA	CA
101	3	CA	CA	CA
101	4	CA	CA	CA

Example data setup for a hypothetical four-wave panel.

Appendix Table D.2: State variable creation example – mover

ID Variable	Wave	Actual State of Residence	First State	Second State
101	1	CA	CA	SC
101	2	CA	CA	SC
101	3	SC	CA	SC
101	4	SC	CA	SC

Example data setup for a hypothetical four-wave panel.

Appendix Table D.3: Summary of error term correlation assumptions

		Person i		Person j			Correlation
Type of Observation Pair	Row Number	First State	Second State	First State	Second State	Correlation Allowed	of Zero Assumed
Stayer & Stayer	(1)	h	h	h	h	✓	
	(2)	h	h	k	k		✓
Stayer & Mover	(3)	h	h	h	k	✓	
	(4)	h	h	k	h	✓	
	(5)	h	h	k	m		✓
Mover & Mover	(6)	h	k	h	k	✓	
	(7)	h	k	h	m	✓	
	(8)	h	k	m	k	✓	
	(9)	h	k	k	h		✓
	(10)	h	k	m	h		✓
	(11)	h	k	k	m		✓
	(12)	h	k	m	l		✓

All regression standard errors are calculated using two-way clustering, which allows for error term correlation structures as summarized above. Letters i and j indicate distinct individuals and h, k, l, and m represent distinct states of residence. Shaded rows indicate categories of observations in which correlation is allowed using the two-way-cluster method suggested here, but where correlation would be assumed to be zero if one were to cluster only on the first state of residence. More detail regarding the calculation of standard errors can be found in Section 3.3.