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Abstract

Due to the low rates of health insurance coverage among young adults, some state governments began mandating health insurance companies to allow adult children to stay on their parents' health insurance plans. First implemented in 1995, these mandates aimed to increase insurance coverage among young adults. In 2010, the federal government enacted a more comprehensive version of the dependent coverage mandate as part of the Affordable Care Act. These state- and federal-level efforts increased insurance rates for young adults, but they might have also come with unintended consequences for parents. Parents who placed a high value on health insurance for their young adult children might be reluctant to leave jobs with employer-provided health insurance, and employers might offset the mandate-incurred health care costs by reducing other types of employee benefits or earnings. To assess the extent of such consequences, I study the effects of both the state- and federal-dependent health insurance mandates on fathers' voluntary job separation rates (job-lock and job-push) and changes in their compensation. I observe a significant decrease in the likelihood of voluntary job separation among eligible working fathers aged 45–64 and find weak evidence that the mandates reduced certain fathers' total monetary compensation.

JEL classification: I13, I18, J6

Keywords: Health Insurance, Government Regulation-Public Health, Job Mobility

1 Introduction

Historically, young adults aged 19–25 display lower health insurance coverage rates than other groups in the United States. The main reasons for this might be that (1) young adults are generally healthy so they may perceive less need for health insurance, and (2) they often work in entry-level jobs that are less likely to provide health insurance (Barkowski and McLaughlin, 2018). The alternative to employer-provided health insurance (EPHI) is enrollment in a non-group plan, which can be too expensive for young adults given their lower income compared to that of other working-age groups. Because of these factors, young adults may forego purchasing health insurance. Seeking to increase health insurance for this young adult population, both state and federal policymakers mandated that health insurance companies expand the age that children could remain covered under their parents' health insurance.

Although many studies find positive effects of these mandates on young adults' health insurance coverage rates (Levine et al., 2011; Dillender, 2014; Cantor et al., 2012; Antwi et al., 2013), the literature lacks studies detailing the implications for the parents of those young adults. Because the mandates increased the value of jobs with EPHI for parents who had eligible children, parents' job mobility could be constrained.

Understanding such potential effects is important because middle-aged workers (aged 45–64) are in the prime earning years of their careers. Specifically, the mandates might limit the job choices for parents. The mandates provide parents a more comprehensive and relatively cheaper insurance plan as a valuable safeguard for their adult children's health and financial security while promoting their children's career progression.[1] Thus, the mandates might increase the parents' cost of leaving an employer with EPHI compared to the time period prior to the implementation of the mandates. I, therefore, expect that workers could be less likely to leave their jobs providing EPHI when their children were eligible for depen-

[1] Given that health insurance enrollment decisions in the United States were often made at the immediate-family level as opposed to the individual level (Cutler and Gruber, 1996), covering dependents through parental coverage is a cost effective decision. Brandeisky (2015) shows that, in 2015, an individual premium cost an average of $486 a month for young adults. By adding two or more dependents to the parents' plan, however, a health insurance premium cost an average of $1,377 a month, thus lowering the cost per individual.

dent coverage mandates. Conversely, for parents without EPHI, these mandates made their then-current state of employment less attractive due to the absent opportunity to cover their adult children. These people could therefore be more inclined to pursue jobs with EPHI.

Consequently, this research addresses the extent to which the state- and federal-dependent health insurance mandates caused fathers to experience job-lock and job-push, that is, respectively, remaining in their jobs for fear of losing EPHI and seeking out jobs with EPHI that they would otherwise not have chosen. As many low- and middle-income countries are moving to universal coverage, examining the unintended effects of expanding health insurance coverage becomes more important.[2]

In addition to highlighting the unintended consequences that accompanied the mandates, this paper further contributes to the literature in two important ways. First, this paper exploits the state and federal mandates together, as suggested by Barkowski and McLaughlin (2018). Examining state and federal mandates in tandem is necessary because they had a shared primary objective of increasing coverage among young adults and had similarities in the eligibility criteria. Second, this research uses a comprehensive dataset (the combination of survey and administrative data) to examine whether the implementation of the mandates caused a decrease in annual earnings or other types of compensation. This decrease could occur because the mandates increased the relative costs for employers to hire those parents with eligible adult children.[3]

I observe that working fathers with eligible children experienced a 42 percent decrease in the likelihood of voluntary separation from employers providing EPHI. My results also provide weak evidence suggesting that fathers who decided not to separate from such employers in the current wave could experience a reduction in earnings.[4] Taken together, all of these findings about the potential effects on parents would allow for a more holistic understanding of the mandates' effects.

The following section explains the institutional details of the dependent coverage mandates. Section 3 presents the literature review and Section 4 describes the methods. Section

[2]Elsewhere, insurance is not as prevalently tied to employment as in the case of the United States.

[3]In 2012, the average employer contribution for employees' family plans was about 73 percent or about $ 11,429 (The Kaiser Family Foundation, 2017).

[4]As explained further in the data section, each wave is a four-month period.

5 discusses my results, and Section 6 concludes this paper.

2 Institutional Details

Before the dependent coverage mandates required insurance providers to extend the age limit for dependents, most public health plans (e.g., Medicaid and the Children's Health Insurance Program or CHIP) and private health plans (e.g., self-insured EPHI, EPHI through an insurance company, or plans through the non-group market) removed young adults from their parents' polices.[5] This most commonly occurred when they turned 19 unless they were enrolled in a college or university as a full-time student. If a dependent was a full-time student, then he or she was typically covered through the age of 22. This left many young adults uninsured if they were not attending college. Moreover, in some states, the tax code defined coverage of dependents (19 years of age or older) as a taxable benefit, deterring parents' employers from extending coverage to their adult children. These factors contributed to 31 percent of young adults being uninsured in 2009, which equals approximately 9.2 million people between the ages of 19 and 25 (Busch et al., 2014).

To increase health insurance coverage for this young adult population, state policymakers expanded access to dependent coverage. In the absence of state funds to expand public programs, many states required employers to offer dependent coverage as part of their plans for increasing the age threshold, generally up to 23–25 years of age (Goda et al., 2016). By 2010, 30 state-level dependent coverage expansions were in effect (see Appendix Table 1).[6] Because of the state-level mandates, the dependent coverage rate increased by approximately 11.9 percent (Burgdorf, 2014; Monheit et al., 2011).

Following the states' lead, the federal government enacted the dependent coverage expansion through the 2010 Affordable Care Act (ACA), which required insurers to expand

[5]The dependent, in this case, referred to biological or legally fostered children.

[6]While almost all states with state-level mandates expanded their eligibility to 23–25 years of age, some states extended the provision to age 29 (i.e., New York, New Jersey and Pennsylvania) and other states extended an indefinite age of eligibility (i.e., Iowa and Texas). Some states also required student status, single marital status or financial dependency to be qualified as a dependent. Beyond the differences in eligibility among dependents, the parents with EPHI from self-insured firms were exempt from the state mandates under the Employee Retirement Income Security Act of 1974. Lastly, most states did not regulate the employee-paid premiums that could be levied for coverage of older dependents, potentially allowing employers to raise prices above what employees were willing to pay.

coverage to children through the age of 25 on their parents' plans. Whereas some state mandates limited eligibility based on factors other than age, the federal law was straightforward: any insurance plan that already offered dependent coverage must offer the same level of benefits at the same price to dependents 25 years of age or younger.[7] Furman and Fiedler (2015) find that due to the federal mandate, the uninsured rate among young adults dropped by more than 40 percent from 2009 to 2014, which translates to 4.5 million additional young adults with coverage.[8] Cantor et al. (2012) note that the federal mandate was a "rare public policy success in the effort to cover the uninsured [young adults]."[9]

3 Literature Review

A large body of empirical literature exists regarding job-lock. The majority of empirical and anecdotal evidence suggests that mobility constraints in the labor market stem from the fear of losing health care coverage.[10]. For instance, Rashad and Sarpong (2008) find that individuals with EPHI were 60 percent less likely to voluntarily leave their jobs compared to those receiving insurance elsewhere. Most job-lock studies rely on the idea that a worker's demographic characteristics—such as proximity to retirement or health status—might lead him or her to value insurance more highly than others, making that worker more vulnerable to job-lock (Kapur and Rogowski, 2007; Blau and Gilleskie, 2001; Bradley et al., 2005). Compared to the abundant literature about job-lock, fewer researchers study job-push, and one example of such studies suggests that a lack of EPHI encouraged some workers to leave jobs that are otherwise desirable (Anderson, 1997).

There is a considerable amount of literature about the state and federal mandates that focus on decreasing uninsured rates and job-lock for young adults (Levine et al., 2011; Mon-

[7]The federal mandate did not depend on co-residence with parents, student status, marital status or financial dependency. It applied to all insurance plans including self-insured EPHI, fully-insured EPHI and plans from the non-group market.

[8]These gains alleviated more than two-thirds of the gap in uninsured rates between young adults and other non-elderly adults (Furman and Fiedler, 2015).

[9]There is one caveat that is worth noting: this coverage extension often did not work well for young adults living out-of-state because their parents' plans might only provide expensive, out-of-network coverage (Goldman, 2013; Reinicke, 2018).

[10]Some findings in the literature, however, suggest that there is little evidence of job-lock phenomenon (Gilleskie and Lutz, 2002; Kapur, 1998)

heit et al., 2011; Cantor et al., 2012; Antwi et al., 2013; Sommers et al., 2013; Colman and Dave 2017; Kofoed and Fraiser, 2019). Most papers in the literature, however, study state- and federal-level mandates independently. For example, Antwi et al. (2013) and Cantor et al. (2012) only discuss the federal mandate, omitting the effects of the state mandates by arguing these effects were negligible. Yet, including the effects of the state mandates is necessary to obtain an unbiased estimate given the interactive impacts of both the state and federal mandates on various outcomes such as young adults' insurance rates, marriage rates and educational attainment (Barkowski and McLaughlin, 2018; Gamino, 2018; Barkowski et al., 2018).

Despite the plethora of papers regarding the effects of the dependent coverage mandates on young adults, few researchers consider other populations. There is only one paper that studies the effects of the dependent coverage mandates on parents' retirement decisions (Biehl et al., 2018), but this paper relies solely on the federal mandate for identifying variation by using the Health and Retirement Study (HRS) data. It is also limited by only considering retirement decisions without analyzing other types of voluntary job separation.

Even though little prior work explicitly investigates the link between the dependent coverage mandates and parents, there is some evidence that mandates related to child health insurance affected parents' voluntary job separation. For instance, Chatterji et al. (2016) find that the prohibition of the pre-existing condition exclusions for children under the ACA increased the likelihood of leaving an employer voluntarily by 37 percent among fathers of disabled children relative to fathers of healthy children. Barkowski (2017) also finds that Medicaid eligibility for household members (especially for eligible children) increased the probability of a voluntary job separation by 34 percent among working fathers with EPHI. Hamersma and Kim (2009) find that Medicaid decreased job-push, suggesting that unemployed fathers or working fathers without EPHI felt less need to move to jobs that offer insurance. As a caveat, Barkowski (2017) and Hamersma and Kim (2009) focus on low-income workers, and Chatterji et al. (2016) investigate job mobility of parents with disabled children. Since both studied groups might be systematically different from the general group of middle-aged fathers, a more comprehensive approach for this group merits discussion.

In addition to job mobility, several papers examine whether health benefit-related man-

dates influenced eligible workers' annual earnings or other types of compensation as these mandates raised the relative cost for firms to insure their workers. This exploration is critical since the efficiency of these mandates largely depended on the extent to which their costs were shifted to group-specific wages (Gruber, 1994).[11] In spite of several regulations (e.g., non-discrimination laws) that prevented these shifts, Gruber (1994) finds a substantial shift in the costs of the group-specific mandate to the wages of the targeted group. This study inspired subsequent articles seeking to determine the effects of health insurance mandates on earnings.[12] Alternatively, co-workers might share the cost of providing additional insurance. A study by Goda et al. (2016) suggests that the annual wage reduction of employees (including those who did not benefit from the mandates) could range from $30 to $1,500 per worker depending on the number of coworkers sharing the costs.[13]

4 Methods

4.1 Data

In my analysis, I include married fathers between the ages of 45 and 64 with a youngest child between the ages of 19 and 29. I exclude fathers with a youngest child aged 26—the cutoff age for the federal mandate, because the adult child's exact date of birth is unknown so it is unclear whether such a child would be eligible for coverage under the mandate.[14] Moreover, this sample excludes responses from states that had no age limit and states that extended the provision to age 29. Among family units, I only focus on fathers because they have more persistent attachment to their jobs than mothers. That being said, the wage-labor supply elasticity of fathers is often much smaller than that of mothers (Blundell and MaCurdy,

[11] In examining the efficiency of group-specific mandates, a central consideration is whether the cost of the mandate was shifted to the wages of the group that benefited. Without the ability to adjust wages accordingly, there might be substantial deadweight loss from these mandates even if the benefit was valued by the group (Gruber, 1994).

[12] Monheit and Rizzo (2007) review the relevant literature regarding the costs of various mandates for employees and employers.

[13] Despite the wage reduction caused by the dependent coverage mandates, Goda et al. (2016) do not find any evidence that suggests workers reduced their labor supply in response to lowered wages.

[14] I do not omit fathers with children who were at the cutoff age for the state mandates (generally less than or equal to 25) because they received coverage through the federal mandate in later years.

1999).[15] Finally, one of my sample selection criteria is based on the youngest child because fathers have an incentive to secure health insurance for their children until their youngest child acquires his or her own access to health insurance.[16]

To assess the effects of the dependent health insurance coverage mandates, I leverage detailed information on individuals using the 2004 and 2008 Survey of Income and Program Participation (SIPP) panels, which are linked to the Detailed Earnings Records (DER) and Business Registrar (BR) data. The SIPP is a nationally representative household survey. The time period covered in my data is January 2004 to December 2012—when most state-level dependent coverage provisions and the federal mandate were implemented. The entire sample is divided into four subsamples called rotation groups. One rotation group is interviewed every 4 months, which hereafter is called a wave. Most SIPP questions ask the respondent to report information regarding the four months prior to the interview (United States Census Bureau, 2001).

I use the respondents' health insurance, demographic characteristics and employment records from the core questions of every SIPP wave.[17] The SIPP provides a detailed set of information about current employment for up to two jobs in a given wave. I only include the job that is considered the 'primary job'—the job in which the individual worked the most hours. The data provide the main reasons that fathers left employers within this wave, if applicable, which allows me to separate voluntary versus involuntary job separation. In my analysis, I focus on voluntary job separation—transitioning between jobs, becoming unemployed, leaving the labor force or transitioning from working for an employer to self-employment.[18]

Although the SIPP provides detailed, self-reported demographic characteristics, the linked

[15] In Appendix 4, I do the same analysis for mothers but do not find any significant changes in their labor market outcomes caused by the mandates. This may be attributable to mothers' expectation that they would not remain in their jobs for a long time. If true, this would suggest that they placed less value on the benefits provided by their companies and were less likely to be influenced by the mandates related to their employer-provided benefits.

[16] I use the youngest child to construct my samples, including the sample for job-push analysis. For the job-push analysis, however, any child could affect fathers' job mobility decisions. So I also run the job-push analysis based on the oldest child. This does not change my results.

[17] None of the variables that I use are imputed.

[18] Involuntary job separation includes layoffs, childcare problems, family/personal obligations, illness/injury, school/training, employer bankruptcy/change in ownership, termination of a temporary job, and unsatisfactory work conditions.

dataset between the SIPP and administrative records on earnings—DER and BR—provides highly accurate measures of earnings and total monetary compensation. To construct this combined dataset, I first link the respondents' information from the SIPP to the DER, which includes their W-2 information such as wages and employer contributions to retirement benefits.[19] This SIPP-DER linked data can be extended with BR data based on the EIN information available in the DER. The BR data includes information like type of firms (i.e., single-unit or multi-unit) and the parent company for all companies in the United States.[20] The SIPP-DER-BR data allows me to identify whether some respondents with two or more W-2s worked for the same parent company and to calculate the total compensation from their primary jobs. Over the course of my analysis, an average of 40 percent of the sample had two or more W-2s on file per year. Of these respondents, about 20 percent had W-2s from the same parent companies. Linking SIPP data with the administrative data, therefore, enables a comprehensive understanding of the compensation adjustments caused by the mandates.[21] Earnings and other monetary compensation are inflation-adjusted using the yearly CPI-U indices and by defining 2012 as the base year.

Although most SIPP questions involve asking the respondent to report information for each of the four months prior to the interview month, I only include the responses from the interview month in order to mitigate seam bias.[22] This means that the analysis is conducted at the father-wave level.

To code the eligibility criteria for the mandates, I compile the data regarding state laws (e.g., age limit and timing of implementation) from Depew (2015), Cantor et al. (2012) and the National Conference of State Legislatures (2010). I demonstrate the change in eligibility for fathers from three example states in the first three rows in Table 1. These states introduced state-level mandates before the federal mandate. The last row in Table 1 presents the

[19]The SIPP-DER linkage is only available until the end of 2012 through the United States Census, so the responses for 2013 from the 2008 SIPP panel cannot be included in the analysis. While combining these datasets, I also omit respondents who did not have Social Security Numbers.

[20]Firms themselves sometimes change or have multiple EINs for tax purposes or for multiple locations.

[21]Bridges et al. (2003) find substantial measurement error in SIPP wage data. They conclude that the mean SIPP wages were understated by 7.5 percent relative to the DER wages. Gottschalk and Huynh (2005) also suggest that respondents with SIPP information but without DER records had lower earnings than respondents with observed earnings in both data sets, possibly reflecting informal work arrangements.

[22]The seam bias is the tendency for respondents to report higher rates of events between survey waves than within survey waves (Blank and Ruggles, 1996).

change in eligibility for fathers in states without state-level mandates. I code the fathers in these states as eligible after September 2010 when the ACA was implemented.

In Table 2, I include the sample means for the outcome variables and covariates for job-lock and job-push in two panels. For both panels, the columns titled *Always Ineligible* contain the descriptive statistics for fathers who were not affected by the state and federal mandates. This is the intersection of the fathers whose youngest child was *ineligible* across all time periods in Table 1. *Ever Eligible* is the group of fathers who were affected by the mandates at some point in my analysis. This is the union of fathers, shown in Table 1, whose youngest child was *eligible* during any time period from 2004 to 2012. Although *Always Ineligible* fathers within my sample were generally older and were more likely to not have completed highschool, almost all other characteristics are comparable to *Ever Eligible*.

For the job-lock analysis in columns 1 and 2 of Table 2, I include fathers who, in the previous wave, were employed (but not self-employed) and had EPHI under their own name.[23] The sample includes approximately 11,500 working fathers, 71 percent of whom had an *Ever Eligible* youngest child.[24] The rate of voluntary job separation within a 4-month wave, on average, is 1.7 percent. These rates are similar to those reported in several papers based on SIPP data (Barkowski, 2017; Chatterji et al., 2016). However, some other studies find the rates that differ from my rate of 1.7 percent. For example, Bansak and Raphael (2008), shows that roughly 18 percent of workers with EPHI separated from their employers within a year. This difference can be explained by the fact that they consider all separations, not just voluntary ones.[25]

In columns 3 and 4 of Table 2, I include the fathers in the job-push sample using the same selection criteria (e.g., their ages and the ages of their children) as those in the job-lock sample. The only important difference is that the job-push sample includes fathers who, in

[23] While my analysis considers employment mobility in the current wave, referencing the information from the previous wave enables me to determine a father's employment status at the beginning of the current wave.

[24] Due to the United States Census Disclosure rules, the total number of observations are rounded to the nearest 500.

[25] Another difference is that I look for the job separation that happens within a wave, while Bansak and Raphael use a between-wave measure for job-mobility. Further explanation for the within and between wave measures in the SIPP can be found in Appendix A of Chatterji et al. (2016).

the previous wave, were unemployed or did not have EPHI from their employers.[26] Selecting the sample in this way limits the chance that job-push and job-lock would be misestimated since individuals without EPHI would not be affected by job-lock.

The last two rows in Table 2 provide the average total compensation and annual earnings from linked SIPP-DER data.

4.2 Identification Strategy

This study examines the effects of both the federal- and state-level dependent coverage mandates on fathers. The primary comparison is between two groups of fathers within each state before and after the implementation of the mandates: those who had a youngest child whose age is beneath mandate thresholds and those who had a youngest child whose age is above mandate thresholds.

The model is specified as

$$y_{ijt} = \beta_0 + \beta_1 * Elig_{ijt} + \beta_2 * X_{it} + \beta_3 * time_t + \beta_4 * state_j + \epsilon_{ijt} \tag{1}$$

where (1) is a difference-in-differences (DID) framework for individual i in state j and at time (wave) t.[27]

To investigate job-lock where I only include fathers with EPHI in the previous wave, the outcome variable—y_{ijt}—is set to one if a voluntary job separation happened in the current wave. As this is a binary outcome, I use a probit analog of equation (1).[28] $Elig_{ijt}$ is the main independent variable and indicates whether fathers have eligible children. It is determined by three things—state of residence, year of interview and youngest child's age—and for a given year, fathers are coded as eligible if they were living in a state with a mandate in effect and had a youngest child whose age was at or beneath the mandated age. For instance, in

[26] Compared to those people in the job-lock sample in columns 1 and 2, the fathers in this job-push sample were more likely to not hold a high school diploma, less likely to work in public sectors and less likely to belong to a union. Thus, these fathers in this sample were not randomly selected.

[27] Because my sample consists of fathers who were at risk of leaving a job and the separation was observed at most once for each father, my specification is equivalent to a discrete time hazard model. Thus, it is not possible to include individual fixed effects because there would not be enough variation remaining (Klerman and Haider, 2004).

[28] This also applies to job-push analysis below.

the case of a father whose youngest child was 24 years old living in Colorado in 2006, I would code him as eligible ($Elig_{ijt}$=1) because Colorado enacted a dependent coverage mandate at that time. For another father in Colorado in the same year but with a youngest child who was 25 years old, I would code him as ineligible ($Elig_{ijt}$=0) because the child's age exceeded the limit of Colorado's mandate.[29] If fathers experienced job-lock, I would expect the coefficient of $Elig_{ijt}$, β_1, to be negative.

X_{it} contains other covariates including father's age and dummy variables. These dummy variables are indicators of high-school dropouts, high-school graduates, Black/Hispanic/Asian respondents, public sector workers and union workers. I include full sets of state and year indicators, denoted with $state_j$ and $time_t$, to focus on within-state variation.[30] In addition, I incorporate indicators for all the children's ages from 20–29 (the indicator for 19-year-olds serves as the baseline group) to account for the time-invariant behavioral difference of fathers with young adult dependents of various ages.[31]

As one of the specification checks, I also make use of the variation within state and year that arose due to state and federal policy changes that broadened eligibility to more age groups. I use this variation to examine whether fathers with children whose ages were near but above the maximum limits of the state mandates were affected at the time when they should not have been. I define a placebo group as the fathers in the years prior to the federal mandate—but after the state mandates—who had not been affected by state mandates but would have been affected by the federal mandate had it been in effect. To examine this, I consider the regression below.

$$y_{ijt} = \beta_0 + \beta_1 * Placebo_{ijt} + \beta_2 * Elig_{ijt} + \beta_3 * X_{it} + \beta_4 * time_t + \beta_5 * state_j + \epsilon_{ijt} \qquad (2)$$

[29] Other requirements—most importantly, student status—are inappropriate to use for eligibility imputation because they are jointly determined outcomes. For example, a state mandate might incentivize individuals to pursue or terminate student status, so using it to determine eligibility would introduce bias (Depew, 2015).

[30] As a specification check, I also examine whether including a linear state time trend would affect my results.

[31] As mentioned, I do not include fathers with a 26-year-old child in the sample and so leave out their age indicators in the regression.

Equation (2) is based on the same criteria as equation (1): the child's age, year and state of residence. $Placebo_{ijt}$ is an indicator for the fathers whose youngest child was slightly older than the age eligibility of the state mandate but still younger than that of the federal mandate.[32] For example, in Indiana, fathers with a youngest child aged 19–23 had been eligible under the state mandate since 2008. In this state, therefore, $Placebo_{ijt}$ is equal to one for those fathers with children between the ages of 24–25 from 2008 to 2010.[33] From 2010 to 2012, the duration where my analysis ends, $Placebo_{ijt}$ is equal to zero across all states. $Elig_{ijt}$ is defined in the same way as $Elig_{ijt}$ in equation (1) and captures the actual policy change of state and federal mandates.

The empirical strategy I rely on to detect job-push is conceptually similar to the one I use for job-lock, demonstrated in equation (1). The main difference, however, is that it includes those fathers who did not have EPHI in the previous wave. Therefore, for the job-push analysis, y_{ijt} is equal to one if fathers voluntarily left their jobs without EPHI or indicated a change in employment status from unemployed to employed in the current wave. A positive estimate of β_1 would provide evidence of job-push, meaning that fathers who did not have EPHI from their employers would be more likely to seek new jobs with EPHI to cover their child.

I also examine the mandates' impact on working fathers' annual earnings or total monetary compensation by using the fathers who stayed in their jobs with EPHI during the current wave—a subset of the aforementioned job-lock sample. Since employers could easily identify the group of working fathers with eligible children, they might respond to the extra cost of providing dependent coverage by reducing other types of compensation for this group. To examine whether the cost of the mandate was transferred to working fathers with eligible children, in equation (1), I replace y_{ijt} with the natural log of the annual earnings and total monetary compensation.[34] I analyze total monetary compensation because employers might decrease other compensation (e.g., employer contribution toward deferred compensation)

[32] My interest is not in examining the placebo effect among those fathers whose youngest child was aged 27–29 because they were not targeted by both the state and federal mandates.

[33] This is because fathers were not affected by state mandates; however, they would eventually be affected by the federal mandates.

[34] Because I link the data from the wave-level SIPP with the annual-level W-2, I have to interpret the coefficients and analyze them at the annual-level even though my observation unit is a wave. While there is a mismatch in timing, I expect that my analysis still benefits from combining the SIPP and W-2 data.

instead of directly adjusting eligible fathers' earnings to avoid violating non-discrimination laws (Anand, 2017). I define this total monetary compensation as the sum of annual earnings and deferred compensation. Because my outcome variable is not binary in this analysis, I run a linear regression instead of probit analog of equation (2). If compensation was reduced for those working fathers whose youngest child was eligible for the mandates, I would expect a negative estimate of β_1. An important note in this analysis is that a small number of responses are automatically omitted when zero compensation was reported.[35]

5 Results

5.1 Job-Lock

Table 3 shows the evidence for an increase in job-lock among working fathers due to the dependent coverage mandates. After the mandates took effect, the average probability of leaving an employer for any voluntary reason was 0.8 percentage points lower for working fathers with eligible children than for other fathers. This 0.8 percentage point decrease is a 42 percent decrease in voluntary job separation given that the average separation rate of *ever eligible* working fathers before the implementation was approximately 1.9 percent.[36] Column 1 does not include any control variables, and column 2, the preferred estimate, incorporates all covariates. The last column provides results with control variables and the state time trends. [37]

The magnitude of my results in Table 3 is comparable to the effects of similar child-targeted mandates on the mobility decisions of working fathers. Barkowski (2017) finds that Medicaid eligibility for one household member resulted in a 34 percent increase in the likelihood of a voluntary job separation among working member(s) in the household. Chatterji et

[35]This omission of responses occurs because I use the natural log of compensation for the dependent variable. Thus, I omit the fathers who self-reported that they were employed with EPHI in the SIPP—a primary sample selection criteria for job-lock analysis—but demonstrated zero earnings in the DER. Earnings might be absent from the DER for some working fathers because their employers failed to report the employees' wages to the Social Security Administration. These workers without DER data were more likely to work in private households, construction, agriculture and informal occupations (e.g., street and door-to-door sales work, dancing or bartending) (Roemer, 2002).

[36]Even when I run a logit regression, I observe similar effect sizes (Appendix Table 2).

[37]In Tables 4, 5, 6 and 8 and Appendix Table 3, I only report the results with covariates that do not have state time trends as in column 2 of Table 3.

al. (2016) also demonstrate that the ACA prohibition on pre-existing condition exclusions increased the probability of job separation by 35 percent for married fathers with disabled children compared to fathers with healthy children.[38]

In Table 4, I examine the robustness of the results. To compare these estimates with the main results, column 1 is taken directly from column 2 of Table 3.[39] In column 2, I expand the control group by including working fathers whose children were aged 27–33 to see if the result would vary depending on the age range of the children. In column 3, I also examine whether expanding the time period with the 2001 SIPP panel would alter the results. Here, I omit five states (i.e., Wyoming, Vermont, Maine, South Dakota and North Dakota) because they were sampled together in the 2001 SIPP. As I am unable to verify the exact implementation dates for the mandates in Georgia, Nevada, South Carolina and Wyoming with more than one source (Goda et al., 2016, I exclude fathers from these states in column 4. In column 5, I exclude the state mandates that had student status requirements (i.e., Florida, Idaho, Louisiana, Massachusetts, North Dakota, Rhode Island and South Dakota).[40] In column 6, I compare the real and the placebo effects, and the resulting estimate of placebo is -0.003 with 0.009 standard error. This suggests that there was no effect of the state mandates among those fathers with children who did not meet the state-mandated age criteria. Still, this column shows that the actual policy change (which resulted in a 0.8 percentage point decrease with 0.004 standard error) is contributing to the main result as in column 1. In the last column, I exclude 2008 and 2009 responses because fathers might be affected by the Great Recession, and their labor force decisions during this time could be different compared to those from other periods. For instance, I expect that voluntary job separation would be significantly lower in 2008–2009 because the overall availability of alternative jobs would be lower. Except for column 7, which has a -0.011 estimated coefficient, almost all

[38] Chatterji et al. (2016) and Barkowski (2017) focus on whether parents' reliance on employment for health insurance decreased; I examine whether it increased. The primary idea, however, remains the same: health insurance mandates for children could affect fathers' labor market decisions.

[39] I also repeat the results of Table 3 (column 2) in Tables 5 and 6 for ease of comparison.

[40] As mentioned in *Institutional Details*, full-time students aged 19–22 years old before the mandates were implemented were often considered to be eligible under their parents' plans. In my main analysis, however, I assume all working fathers with children aged 19–22 as eligible **only after** the mandates were implemented. This may raise a concern whether it is valid to consider the states that required student status as mandated states because this only applied to students. I therefore examine whether excluding those states would alter the results significantly.

results in this table have a similar magnitude of around a 0.8 percentage point decrease with statistical significance.[41]

In Table 5, I examine the heterogeneity of the results. Working fathers with lower education might be less responsive to the mandates because of a lack of understanding of the dependent coverage mandates. On the other hand, the working fathers might be more likely to have children who needed the dependent health coverage because their children would generally be less educated and less likely to secure jobs with EPHI. Column 2 shows the effects on working fathers who did not receive a Bachelor's degree, and column 3 shows the effects on working fathers who completed a four-year degree or above. The estimated coefficient on $Elig_{ijt}$ is -0.007 with a standard error 0.003 in column 2. This suggests that fathers with less education had less job mobility as a result of the mandates, supporting the latter hypothesis. Columns 4 and 5 show that fathers whose wives did not have EPHI would be more likely to experience job-lock because they were the only source of health insurance for the household. The estimated coefficient in column 4 is -0.009 with a standard error of 0.004, reflecting less job mobility for fathers with wives lacking EPHI after the mandates.

Table 6 illustrates the results of four falsification tests. In column 2, I examine whether involuntary job separation (e.g., layoff) increased due to the dependent coverage mandates. I use the same sample as that in column 1, but I change my outcome variable from voluntary to involuntary job separation. As involuntary job separation could not be related to the new eligibility for the mandates, no effect would be expected. In columns 3 to 5, I investigate whether there were any contemporaneous changes that affected parents differently based on the age of their children, which could bias my results. In these columns, I consider fathers who were covered by EPHI but whose coverage was not affected by the mandates (due to the ineligible ages of their children) and examine if their behaviors changed when the mandates were implemented. Therefore, my sample in columns 3-5 comprises working fathers in three groups: those whose youngest child was between the ages of 8–18, 30–40 and 27–36, respectively. In column 3, for example, I consider the placebo (state or federal) mandates' eligibility by subtracting 11 from the original age eligibility criteria based on fathers whose youngest child was aged 8–18. If the mandate expanded coverage to dependent children up

[41] Though the result in column 5 is statistically insignificant, it has a p-value of around 0.13.

to the age of 23, I would consider this state's placebo age limit to be 12. By doing this, I can examine whether those working fathers with ineligible children under 19 seemed to be affected by the mandates. I repeat this process for working fathers with children aged 30 to 40 in column 4. For these working fathers, I add 11 to the original age eligibility. That being said, if the mandate increased the age limit to 23, I would consider this state's placebo age limit to be 34).

One drawback of the two falsification tests shown in columns 3 and 4 is that they do not include any fathers who are in my main sample. To rectify this, I use the sample containing working fathers whose youngest child aged 27–36 in column 5 of Table 6. With this analysis, I can examine whether there were any time-effects. This refers to circumstances that had changed over time and affected parents differently based on the ages of their children. In this column, I include the same *Always Ineligible* fathers whose youngest child was aged 27–29 that I use in my main analysis. However, I alter the *Ever Eligible* group—whose children were aged from 19–25—by adding 11 to the original age limit, resulting in an overall sample of fathers with 27- to 36-year-old children. None of the falsification tests have any significant effects, and the point estimates are appreciably different from my main findings in column 1 of Table 6.

5.2 Job-Push

Fathers may not only experience job-lock due to these mandates, but also the twin phenomenon of job-push. In Table 7, I examine whether the fathers without EPHI were incentivized to leave their jobs due to the increase in opportunity costs of staying in their employment status. Unlike Hamersma and Kim (2009) and Barkowski (2017) who study the Medicaid expansions, my 1 percentage point estimate of increased voluntary job separation of the eligible fathers is not significant at a conventional level. As such, I do not find evidence of job-push for these fathers.

One explanation is the time horizon in which parents were affected by the policies. While Medicaid influenced parents for a long period of time, the dependent coverage mandates only affected parents for a shorter period when their children were in their early 20s. Parents,

therefore, might be less motivated to change their employment status (in this case, finding a new job with EPHI) for such a short-term benefit. Additionally, parents who greatly valued insurance for their kids probably would have moved to such jobs already. Because I only use the sample that consists of fathers who did not have EPHI through their jobs, this might mean that they had a lower demonstrated need and value of health insurance.

5.3 Reduction in Compensation

To examine whether employers adjusted employee compensation in response to rising health insurance costs, I analyze the change in total monetary compensation and annual earnings. Table 8 presents the effects of this mandate on earnings and other compensation for working fathers who did not leave their jobs with EPHI, based on both the administrative data and the public data. Column 1 presents no evidence for a reduction in annual earnings while column 2 shows there is weak evidence of decline in total compensation by about 9 percent.[42] This means that employers might try to adjust other monetary compensation instead of directly decreasing the earnings for those fathers with EPHI. Through this examination, I observe that fathers with EPHI not only experienced job-lock, but also could experience compensation reduction. Said another way, because the average total compensation of treatment group was about $70,000 before the implementation of the mandates, a weakly supported 9 percent decrease in total compensation could result in a $6,300 decrease among fathers with eligible children after the mandates.[43]

To compare the results in columns 1 and 2 with the result based on the public data, I include column 3 of Table 8.[44] Unlike the results from the administrative data, I do not find

[42]The results, however, should be interpreted with caution. As mentioned, non-discrimination laws might have prevented employers from differentially compensating employees. In addition, all workers might have borne the cost of the mandate since many non-parents were potential future users of the policy and it would have been difficult for firms to implement wage offsets when workers became parents.

[43]To examine whether including fathers with zero compensation changes my results, I also add one to both dependent variables for those who had zero compensation and examine the same analysis again in Appendix Table 3. This result shows the consistent decrease in compensation, as shown in Table 8.

[44]Although the earnings reported in the public SIPP are monthly, I aggregate them into annual earnings for each father and use this as an outcome measure for column 3.

any significant effect on compensation by using the public SIPP data alone.[45]

6 Conclusions

While both the state and federal dependent coverage provisions successfully increased health insurance rates among young adults, previous research did not make it clear whether the mandates had any effects on their parents. To address this gap, I analyze the effects of both the state and federal mandates on fathers' dependence on employment. My investigation is unique in the literature because I bring together three important analytic features: (1) a focus on fathers (whom are not targeted by the mandates) whose responsiveness is the key determinant for the effectiveness of the mandates; (2) a usage of both the state and federal mandates to achieve more credible variation; and (3) a usage of administrative data on the earnings measure that have not been used in the relevant literature.

I find that the mandates decreased voluntary job separation by about 0.8 percentage points among eligible working fathers (aged 45–64, with EPHI) than would otherwise have been. This decrease in voluntary job separation represents that, on average, 0.8 percentage points of more fathers would stay in their jobs for each wave than prior to the implementation of the mandates. As such, after one year—comprising three waves—2.4 percentage points more fathers would remain in their current jobs providing EPHI.[46] If I assume that each father covers one child, my model suggests that the mandates could increase the young adult dependents who have health insurance coverage by at least 2.4 percentage points. This approximation of increased coverage among young adults aligns with another paper that found an increase of coverage among adult dependents about 5.3 percentage points during 2010–2011 (Cantor et. al., 2012). My estimate, the 2.4 percentage points increase in coverage, is relatively small compared to the 5.3 percentage points and is plausible given that not all fathers considered a job change when they started to cover their newly eligible young adult dependents. While covering a more broadly defined population, my observation that the

[45] Due to data unavailability, I do not examine the effect of the mandates on total monetary compensation from the public SIPP data.

[46] This conversion between wave and year is necessary for my comparison because I report flow changes for job mobility, while other papers report change in dependent coverage rates in stocks.

child-targeted mandates could affect parents' mobility is consistent with previous findings (Bansak and Raphael, 2008; Chatterji et al., 2016; Hamersma and Kim, 2009). Additionally, my estimates are robust to a variety of specification checks, although some effects have a change in magnitude and lose statistical significance. I discover no evidence of job-push, and I find weak evidence of compensation reduction among eligible fathers. My paper provides new insights into the effectiveness of the dependent coverage mandates and emphasizes how health insurance access can have far-reaching consequences for both targeted individuals and their household members.

Since many other insurance-related changes were implemented in 2014 (e.g., premium subsidies for private coverage, the Medicaid expansion and individual mandate), it is important to understand the ways in which the dependent coverage mandates might be affected. For example, people with incomes below 133 percent of the federal poverty level (FPL) could qualify for expanded adult Medicaid while those who have incomes from 133–400 percent of FPL could qualify for premium subsidies for private insurance. Therefore, these people might have chosen an expanded public coverage or newly subsidized form of coverage after 2014. My analysis of the dependent coverage mandates from 2004 to 2012 implies that these two policies could have decreased the EPHI-related constraints that both young adults and their parents faced. Additionally, the repeal of the individual mandate—effective beginning in 2019—could also contribute to further fluctuation in the rates of dependent coverage and the job mobility of fathers.[47] With these subsequent changes, future research might examine how the fathers' incentives to change jobs could be further modified.

Additionally, as there is a clear trade-off—an increase in coverage of young adults and decrease in job mobility among fathers—induced by these mandates, a more thorough welfare analysis is necessary to examine how the mandates affected the economy's overall well-being and to determine if this policy should be continued.

[47]Due to enforcement of the individual mandate in 2014, young adults' health insurance take-up could increase, while simultaneously decreasing job mobility among fathers. The opposite can happen after 2019.

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Declaration of interest: none.

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Tables

Table 1: Examples of Childrens' Age Eligibility by State, Before and After Implementation of Mandates

States	Pre-State Law Period		State Law Period			ACA Period (from 2010)	
	Eligible	Ineligible	Beginning Year	Eligible	Ineligible	Eligible	Ineligible
Indiana	.	19–29	2008	19–23	24–29	19–25	27–29
Colorado	.	19–29	2006	19–24	25–29	19–25	27–29
Connecticut	.	19–29	2009	19–25	27–29	19–25	27–29
Michigan	.	19–29	.	.	19–29	19–25	27–29

Notes: Rows indicate representative states and demonstrate the change in eligibility age of children by state of residence and time period. I choose Indiana, Colorado and Connecticut as examples of states that had implemented state-level mandates prior to the ACA. Unlike the other states in this table, Michigan did not employ any state-level mandates prior to the ACA; thus, its eligibility was only affected by the federal mandate. The change in eligibility for Michigan applies to all other states without state-level mandates.

Table 2: Descriptive Statistics of Fathers for Job-Lock and Job-Push Analyses

	Job-Lock Sample		Job-Push Sample	
Variables	Always Ineligible	Ever Eligible	Always Ineligible	Ever Eligible
Eligible	-	.416	-	.476
		(.494)		(.500)
Age	56.2	53.2	56.6	52.9
	(4.85)	(5.17)	(5.02)	(5.31)
Highschool dropouts	.048	.036	.109	.036
	(.206)	(.188)	(.307)	(.188)
Highschool graduates	.274	.262	.231	.310
	(.444)	(.442)	(.420)	(.456)
Some college or higher	.677	.702	.615	.571
	(.464)	(.459)	(.472)	(.493)
Non-hispanic white	.806	.821	.769	.762
	(.382)	(.386)	(.429)	(.441)
African American	.065	.071	.092	.119
	(.259)	(.257)	(.274)	(.292)
Hispanic	.048	.059	.062	.095
	(.225)	(.235)	(.298)	(.337)
Asian and others	.065	.054	.077	.048
	(.222)	(.224)	(.242)	(.196)
Public Sector worker	.226	.214	.031	.071
	(.424)	(.413)	(.139)	(.233)
Union worker	.258	.226	.046	.071
	(.439)	(.416)	(.195)	(.237)
Dependent Variables				
Voluntary Job Separation rates	.016	.018	.015	.014
	(.120)	(.127)	(.131)	(.131)
N. of Observation [1,000]	3.10	8.40	0.65	2.10
Ln(Annual Earnings in the DER)	10.9	11.0	10.1	10.3
	(.725)	(.785)	(1.19)	(.997)
Ln(Tot. Monetary Comp. in the DER)	11.0	11.0	10.2	10.3
	(.744)	(.793)	(1.21)	(1.02)
N. of Observation [1,000]	3.00	8.20	0.55	1.90

Notes: Standard deviations are in parentheses. All numbers of observations and individuals are rounded to the nearest 500 due to the United States Census Disclosure rules.

Table 3: The Effects of Eligibility on Voluntary Job Separation Rates, Job-Lock

Voluntary Job Separation	[1]	[2]	[3]
Eligible	-.007†	-.008*	-.009*
	(.004)	(.003)	(.003)
Covariates		Y	Y
State Time Trends			Y
N. of Observations [1,000]	11.5	11.5	11.5
Dependent variable means			
Ever eligible, before Mandate	.019	.019	.019

Notes: This table includes fathers with EPHI in the previous wave. Since multistage-stratified sampling is an important aspect of the SIPP, all estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. Column 1 does not include any control variables. Column 2 incorporates covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers. In addition, column 2 also accounts for the state, year and child's age fixed effects. Column 3 includes all covariates from column 2 and the state-specific linear time trends. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Table 4: The Effects of Eligibility on Voluntary Job Separation Rates, Robustness Checks

Voluntary Job Separation	[1] Preferred Table 3 [2]	[2]	[3]	[4]	[5]	[6]	[7]
Eligible	-.008*	-.007*	-.007*	-.008*	-.006	-.008*	-.011**
	(.003)	(.003)	(.003)	(.004)	(.004)	(.004)	(.004)
Placebo						-.003	
						(.009)	
Fathers with Youngest Child Aged 19-33		Y					
Including 2001 SIPP			Y				
Excluding States with Unclear Implementation Dates				Y			
Excluding States with Student-Status					Y		
Estimation Using Equation (2)						Y	
Excluding 2008 and 2009							Y
N. of Observations [1,000]	11.5	15.5	14.5	11.0	10.0	11.5	9.6

Notes: This table includes fathers with EPHI in the previous wave. All estimates are weighted. All number of observations are rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. All columns include covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers in addition to full sets of state, year and child's age indicators. Column 1 repeats the estimates from column 2 of Table 3. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Table 5: The Effects of Eligibility on Voluntary Job Separation Rates by Subgroups

Voluntary Job Separation	[1] Preferred Table 3 [2]	[2] Lower Educ. (Below Bachelor's Degree)	[3] Higher Educ. (With Bachelor's Degree or Above)	[4] No Spouse HI	[5] Spouse HI
Eligible	-.008*	-.007*	-.006	-.009*	-.004
	(.003)	(.003)	(.009)	(.004)	(.006)
N. of Observations [1,000]	11.5	7.80	3.70	9.20	2.30

Notes: This table includes fathers with EPHI in the previous wave. All estimates are weighted. All number of observations are rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. All columns include covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers in addition to full sets of state, year and child's age indicators. Column 1 repeats the estimates from column 2 of Table 3. Samples in columns 2 and 3 are divided based on the education level of the father. Columns 4 and 5 examine the effect of whether the father's spouse has EPHI. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Table 6: The Effects of Eligibility on Voluntary Job Separation Rates, Falsification Tests

	[1] Preferred Table 3 [2]	[2] *Involuntary Job Separation*	[3]	[4] *Voluntary Job Separation*	[5]
			Eligible Age-11	Eligible Age+11	Eligible Age+11 for 19-25
Eligible	-.008*	-.002	.003	-.003	.006
	(.003)	(.003)	(.004)	(.007)	(.007)
N. of Observations [1,000]	11.5	11.5	16.5	6.50	8.00

Notes: This table includes fathers with EPHI in the previous wave. All estimates are weighted. All number of observations are rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. All columns include covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers in addition to full sets of state, year and child's age indicators. Column 1 repeats the estimates from column 2 of Table 3. Column 2 uses the same sample as Table 3. Columns 3, 4 and 5 include fathers with the youngest child aged 8–18, 30–40 and 27–36, respectively. Unlike other analyses, column 2 uses involuntary job separation as the outcome variable. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Table 7: The Effects of Eligibility on Voluntary Job Separation, Job-Push

Voluntary Job Separation	[1]	[2]	[3]
Eligible	.012	.012	.022
	(.022)	(.020)	(.026)
Covariates		Y	Y
State Time Trends			Y
N. of Observations [1,000]	2.80	2.80	2.80

Notes: The sample in this table includes fathers who did not have EPHI in the previous wave. All estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. Column 1 does not include any control variables. Column 2 incorporates covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers. In addition, column 2 also accounts for the state, year and child's age fixed effects. Column 3 includes all covariates from column 2 and the state-specific linear time trends. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Table 8: The Effects of Eligibility on Annual Earnings and Total Monetary Compensation

	[1]	[2]	[3]
	ln(Earnings)	*ln(Tot. Comp.)*	*ln(Earnings)*
	SIPP-DER-BR		Public SIPP
Eligible	-.079	-.091†	-.002
	(.054)	(.054)	(.041)
N. of Observations [1,000]	11.0	11.0	11.0

Notes: The table includes a subset of the job-lock sample in Table 3—excluding those who separated from their jobs providing EPHI within the current wave and those who reported zero compensation. All estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. All columns include covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers in addition to full sets of state, year and child's age indicators. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

Online Appendices

A1. Dependent Health Insurance Coverage Laws

Appendix Table 1: Implementation of the Dependent Coverage Laws

	Full Year Implemented	Maximum age
Federal Mandate	2010	25
State Mandates		
Colorado	2006	24
Connecticut	2009	25
Delaware	2008	23
Florida*	2008	24
Idaho*	2008	24
Illinois	2010	25
Indiana	2008	23
Kentucky	2008	25
Louisiana*	2009	23
Maine	2007	24
Maryland	2008	24
Massachusetts*	2007	25
Minnesota	2008	24
Missouri	2008	24
Montana	2008	24
New Hampshire	2007	25
New Mexico	2003	24
North Dakota*	1995	25
Rhode Island*	2007	24
South Dakota*	2005	23
Utah	1995	25
Virginia	2007	24
Washington	2009	24
West Virginia	2007	24
Wisconsin	2007	26

Notes: This table shows the year that federal and state-level mandates were implemented along with age criteria. In my main analysis, I exclude the states that had no age limit defined in their dependent coverage expansions (Iowa and Texas) or that extended their dependent coverage up to the age of 29 (New Jersey, Pennsylvania, and New York). *indicates states that required student status as part of the eligibility criteria.

A2. Alternative Job-lock Analysis

Appendix Table 2: Alternative Regression Results (Logit)

Voluntary Job Separation	[1]	[2]	[3]
Eligible	-.007†	-.008*	-.010*
	(.004)	(.004)	(.003)
Covariates		Y	Y
State Time Trends			Y
N. of Observations [1,000]	11.5	11.5	11.5

Notes: This table includes fathers with EPHI in the previous wave. All estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. Column 1 does not include any control variables. Column 2 incorporates covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers. In addition, column 2 also accounts for the state, year and child's age fixed effects. Column 3 includes all covariates from column 2 and the state-specific linear time trends. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

A3. Alternative Analysis on Compensation Reduction

Appendix Table 3: The Effects of Eligibility on Annual Earnings and Total Monetary Compensation

	[1] *ln(Earnings+1)*	[2] *ln(Tot. Comp.+1)*
Eligible	-.225†	-.239*
	(.113)	(.114)
N. of Observations [1,000]	11.5	11.5

Notes: The sample size in this table appears to be the same as in Table 3 because I use the approximation of the total number of observations in order to follow the Census rules. This sample, however, still excludes fathers who separated from their jobs with EPHI within the current wave. All estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. All columns include covariates for the fathers' ages, education levels, races, and their statuses as public sector or union workers in addition to full sets of state, year and child's age indicators. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

A4. The Effects of Mandates on Working Mothers

Appendix Table 4: The Effects of Eligibility on Working Mothers' Job Mobility

Voluntary Job Separation	[1]	[2]	[3]
Eligible	.003	.004	.002
	(.005)	(.005)	(.005)
Covariates		Y	Y
State Time Trends			Y
N. of Observations [1,000]	8.80	8.80	8.80

Notes: Mothers in this sample have similar selection criteria as job-lock sample of fathers. They were married mothers between the ages of 45 and 64 and have youngest child between the ages of 19 and 29. They were also employed in the previous wave with EPHI. All estimates are weighted. The number of observations is rounded according to the United States Census Disclosure rules. Standard errors are clustered at the state level. Column 1 does not include any control variables. Column 2 incorporates covariates for the mothers' ages, education levels, races, and their statuses as public sector or union workers. In addition, column 2 also accounts for the state, year and child's age fixed effects. Column 3 includes all covariates from column 2 and the state-specific linear time trends. † indicates that the p-value is less than 0.1; * indicates that the p-value is less than 0.05; ** indicates that the p-value is less than 0.01.

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