# The Effects of the Dependent Coverage Mandates on Fathers' Job Mobility and Compensation*

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**Abstract**

Motivated by low rates of health coverage among young adults, some state governments began mandating health insurers to allow adult children to stay on their parents' insurance plans. First implemented in 1995, these mandates aimed to increase health coverage among young adults and reduce their dependence on employment for health insurance. 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 successfully increased insurance rates for young adults, but they may have come with unintended implications for their parents. Parents who place a high value on health insurance for their young adult children may be reluctant to leave jobs with employer-provided health insurance. In addition, employers may offset the increased health care costs by reducing other types of employee benefits or earnings. To assess the extent of these impacts, I study the effects of both the state and federal dependent health insurance mandates on fathers. By analyzing the 2004 and 2008 SIPP panels, which are linked with Detailed Earnings Records and Business Registrar data from the U.S. Census, I examine the mandates' effects on voluntary job separation rates (as job-lock and job-push) and changes in fathers' compensations. After the implementation of the mandates, a 37 percent decrease in the likelihood of voluntary job separation among eligible working fathers aged 45–64 with health insurance is observed. I do not observe any significant effect associated with job-push. Additionally, the implementation of the mandates appeared to decrease annual earnings and total monetary compensation among eligible working fathers.

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**JEL Classification**: I13, I18, J6

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## Introduction

Historically, young adults aged 19–26 experience lower health insurance coverage rates than other groups. The main reason for this may be that young adults are generally healthy, so they may not perceive a need for health insurance (Barkowski and McLaughlin, 2018) and thereby forego health insurance purchases. For those young adults who do need health insurance, however, employer-provided health insurance (EPHI) could affect their job choice because EPHI is significantly more affordable than non-group plans. This is not always a viable option, however, as many young adults work entry level jobs where employers do not offer health insurance. Seeking to increase health insurance for this young adult population, both state and federal policymakers mandated that health insurers expand the age that children could remain covered under their parents' health insurance up to 23–26 years of age depending on the state. 30 states implemented the mandates beginning with Utah and North Dakota in 1995. In 2010, the federal government enacted the young adult dependent coverage mandate that applies to all states.[1] Although many studies confirm the positive effects of these mandates on young adults (Levine et al., 2011; Dillender, 2014; Cantor et al., 2012; Akosa Antwi et al., 2013), the literature still lacks studies detailing the implications for their parents. Because the mandates increase the value of jobs with EPHI for parents who have eligible children, parents' job mobility might be affected. The goal of this research is to determine the extent to which the state and federal mandates cause fathers to experience job-lock, remaining in their job for fear of losing access to EPHI, and job-push, seeking out jobs with EPHI that they would otherwise not have chosen.

A thorough examination of the mandates' effects on parents is needed due to the economic influence of this demographic group. Middle-aged workers (aged 45–64) are in the prime of

[1] The state-level mandates resulted in an increase in the dependent coverage rate by about 11.9 percent among the targeted group—as defined by age, state and year of implementation—compared to a non-targeted group (Burgdorf, 2014). Similarly, Monheit et al. (2011) suggest that there was a 10 percent increase in dependent coverage among the targeted group of young adults after policy implementation, relative to a control group of young adults. Due to the federal mandate, 4.5 million additional young adults who would not otherwise have had coverage have been insured (Furman and Fiedler, 2015).

their careers with longer relevant experience and higher earnings than workers who have just entered the workforce. Thus, the mobility decisions of these workers may have a critical influence on their careers and retirement savings.

Parents' mobility decisions are highly likely to be influenced by these mandates for the following reasons: (1) parents value these mandates as they provide a safeguard for their adult children's health and financial security while promoting their children's career progression, and (2) health insurance enrollment decisions in the United States are often made at the immediate family level as opposed to the individual level (Cutler and Gruber, 1996) in order to be cost-effective. For these reasons, EPHI adds additional value for parents. Thus, the cost of leaving an employer for parents with EPHI increases. Therefore, I expect workers to be less likely to leave their jobs when their children are eligible for dependent coverage mandates. Conversely, for parents without EPHI, these mandates make their current state of employment less attractive; as such, I would expect these people to be more inclined to pursue jobs with EPHI.

In addition to examining an under-studied population affected by these mandates, this paper makes two other contributions to the literature. First, I exploit both the state and the federal dependent coverage mandates to explore their effects on fathers. Second, it sheds light on a decrease in compensation by using data from the Survey of Income and Program participation (SIPP)—a publicly available, self-reported survey—linked with the administrative data from the Detailed Earnings Records (DER) and Business Registrar (BR), made available through the United States Census. While I do not observe any effect solely based on the SIPP, the combination of data from the SIPP, DER and BR provide a new perspective on the effect of the mandates on working fathers' compensation.

I find that working fathers with eligible children experienced a 37 percent decrease in the likelihood of voluntary separation from employers with EPHI. I was not able to detect any job-push effect. The results suggest that these mandates lowered total monetary compensation among eligible fathers with EPHI.

### Institutional Details

In the United States, policymakers have long recognized the low rates of health insurance among young adults and the associated labor market distortions. Before the dependent coverage mandates required insurers to extend the age limit for dependents, most public (e.g., Medicaid and 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 when they turned 19 unless they were enrolled in a college or university as a full-time student.[2] 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 who were not currently in the college without insurance. Moreover, in some states, the tax code defined coverage of dependents 19 years of age or older as a taxable benefit, deterring employers from extending coverage. Due to their limited access to health insurance, some young adults had a greater incentive to choose employment with EPHI (Hahn and Yang, 2015), rather than their ideal jobs.[3]

To increase health insurance and to weaken the link between health insurance and labor supply for young adults, state policymakers expanded access to dependent coverage. In the absence of state funds to expand public programs (Goda et al., 2016), many states required firms that offered dependent coverage to increase their age threshold (generally up to 24–26 years of age). By 2010, 30 state-level dependent coverage expansions were in effect (see Appendix Table 1).[4]

[2] The dependent, in this case, refers to biological, legally adopted, or legally fostered children or to children for whom one has been appointed legal guardian.

[3] These types of jobs, however, often required full-time employment, which may limit young adults' time and prevent them from searching for more optimal jobs (Colman and Dave, 2017).

[4] While all states with state-level mandates increased the age limitations, there were some states with no age limit (i.e., Iowa and Texas) and other states that extended the provision to age 29 (i.e., New York, New Jersey and Pennsylvania). Some states had eligibility criteria like residency within a state or with a parent policy holder. These criteria, however, are not important concerns because the requirement for residency with a parent would be difficult for insurers to enforce (Barkowski and McLaughlin, 2018). Some states might also require student status, single marital status or financial dependency to be qualified as a dependent. Moreover, the parents with EPHI from self-insured firms were exempt from the state mandates under the Employee Retirement Income Security Act of 1974. Finally, most states did not regulate the employee-paid premiums that could be levied for coverage of older dependents, potentially allowing firms to raise prices

Following the states' leads, the federal government enacted the dependent coverage expansion through the 2010 ACA, which required insurers to expand coverage to children up to the age of 26 on their parents' plans. Whereas some state mandates limited eligibility based on factors other than age, the federal law is straightforward: any insurance plan that already offers dependent coverage must offer the same level of benefits at the same price to dependents 26 years of age or younger.[5]

Although the federal mandate has fewer eligibility criteria than some states, both mandates extend the eligibility age. In addition, both the state and federal mandates pursue the same goal: increasing health insurance coverage as well as decreasing dependence on employment for health insurance among young adults. As the implicit value of being employed in a job with EPHI decreases with the mandates, the young adults who become eligible can choose a job aligned with their career aspirations rather than a job that simply provides EPHI.[6] Furman and Fiedler (2015) demonstrate that due to the federal mandate, by 2014 the uninsured rate among young adults dropped by more than 40 percent compared to that of 2009—which translates to 4.5 million additional young adults with coverage. The percent of other non-elderly adults aged 26 to 64 without insurance coverage was stable during that time.[7] Cantor et al. (2012) note that the federal mandate is a "rare public policy success in the effort to cover the uninsured [young adults]."[8] Given the similarities in the state and federal mandates, examining them together is important. If I examined the federal mandate

above what employees could afford.

[5]The requirement does not depend on co-residence with parents, student status, marital status or financial dependency. It applies to all insurance plans including self-insured EPHI, fully-insured EPHI, and plans from the non-group market. The federal law revises the IRS rules so that the benefit offered to the newly eligible young adults would be tax exempt. Dependent coverage under a parent's plan, however, does not extend to the children and spouses of those dependents.

[6]In fact, the Federal Registrar (2015) indicates that one of the most significant aims of this law was to "permit greater job mobility ... as [young adults'] insurance coverage would no longer be tied to [young adults'] own jobs or student status."

[7]These gains eliminate more than two-thirds of the gap in uninsured rates between young adults and other non-elderly adults, even as other non-elderly adults also experienced large coverage gains during 2014 (Furman and Fiedler, 2015).

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

alone, it would lead to a misclassified model since the results would not take into account the effects of the state mandates that had already been enacted. My approach primarily relies on within-state time variation because the state mandates had different ages of eligibility and timing of implementation of mandates.

While the mandates could allow young adults more freedom in choosing their jobs, they may limit job choices for their parents. By staying in their current jobs with EPHI, parents are able to provide more comprehensive plans for their children's health and financial security at a much lower cost. This also enables their children to progress professionally with less concern about their own coverage.[9] Therefore, I expect a decrease in job mobility of these parents after the implementation of dependent coverage mandates. For parents without EPHI, however, the dependent coverage mandates increase the opportunity cost of maintaining their current employment status. In this case, I expect an increase in the job mobility of parents with eligible children. In addition to job mobility, I explore whether eligible working fathers with EPHI experience a reduction in annual earnings or other types of compensation as these mandates raise the relative cost for employers to hire working fathers with eligible adult children.[10] This exploration is critical since the efficiency of these mandates largely depends on the extent to which their costs are shifted to group-specific wages.

### Literature Review

A large body of empirical literature exists regarding job-lock. Most empirical and anecdotal evidence suggests that mobility constraints in the labor market stem from the fear of losing health care coverage. The range of the magnitude for job-lock is from 30 percent to 80 percent and varies depending on the identification strategy or demographic group (Gruber and

[9] For example, Brandeisky (2015) shows that an individual premium costs $486 a month for young adults, on average, in 2015. By adding two or more dependents to the parents' plan, however, a premium costs an average of $1,377 a month. When this is split by three or four, it is still less than an individual plan.

[10] In 2012, the average employer contribution for employees' family plans was about 73 percent, which is converted to $ 11,429 (The Kaiser Family Foundation, 2017). As the dependent children were covered under the fathers' family plan, the financial burden of the insurance premium and health care cost would be transferred not only to the parents, but also to their employers (Chen, 2018). In fact, employers would likely pay the majority of the costs.

Madrian, 2002). For instance, Rashad and Sarpong (2008) find that individuals with EPHI are 60 percent less likely to voluntarily leave their jobs compared to those receiving insurance elsewhere.[11] Many job-lock studies are based on the idea that a worker's unique demographic characteristics—such as proximity to retirement or health status—may lead him or her to value insurance more highly than others, making that worker more vulnerable to job-lock. For the elderly, Kapur and Rogowski (2007) and Blau and Gilleskie (2001) point out that there is unambiguous evidence that health insurance is a central determinant of retirement decisions. Additionally, Bradley et al. (2005) find that EPHI appears to create incentives to remain working and to work at a greater intensity when faced with a serious illness. Gruber and Madrian (2002) and Rashad and Sarpong (2006) have both written comprehensive literature reviews on the topic of job-lock. Compared to the abundant literature about job-lock, however, fewer researchers study job-push. Anderson (1997) wrote one paper studying job-push and suggests that EPHI encourages some workers to leave jobs that are otherwise desirable.

There is considerable literature about the state and federal mandates that focus on decreasing job-lock and job-push for young adults. Two studies by Levine et al. (2011) and Monheit et al. (2011) estimate the effect of the state-level mandates and conclude that the mandates successfully increased coverage among young adults through EPHI received as a dependent. Literature regarding federal mandates including Cantor et al. (2012), Akosa Antwi et al. (2013) and Sommers et al., (2013) finds that when the federal government enacted the ACA, it developed a pathway for more young adults to be insured. According to Colman and Dave (2017), the federal mandate also reduced job-lock among young adults.[12]

Most papers in the literature, however, examine these various outcomes by studying state- and federal-level mandates independently. So far, only one paper analyzes these mandates

[11]The main estimate in this paper—a 37 percent decrease in job mobility among parents—is comparable to theirs. Although my estimate is slightly smaller, this is to be expected because the dependent coverage mandates are not primarily targeted at health insurance coverage of parents.

[12]Colman and Dave (2017) suggest that since young adults feel less pressure to secure a full-time job with EPHI, they work fewer hours and are able to spend extra time on social, educational and job-search activities.

together (Barkowski and McLaughlin, 2018); however, this work focuses on the effects of these mandates on marriage among young adults. Most studies that only focus on the federal mandate justify not accounting for the effects of the state mandates by arguing that these effects were minimal. Instead, they only compare young adults under the age of 26—the cut-off age for the federal mandate—to those above, which is a strategy similar to that of Cantor et al. (2012).

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 it solely uses the federal mandate for identifying variation along with the Health and Retirement Study data. Also, this paper only considers 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 affect parents' voluntary job separation. For instance, Chatterji et al. (2016) find that *the ACA prohibition of the pre-existing condition exclusions for children* increased the likelihood of leaving an employer voluntarily by 0.7 pp among fathers of disabled children relative to fathers of healthy children. Barkowski (2017) also argues 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 may also have decreased job-push, suggesting that unemployed fathers or working fathers without EPHI feel less need to move to jobs that offer insurance.[13] As a caveat, Barkowski (2017) and Hamersma and Kim (2009) focus on low-income workers, while Chatterji et al. (2016) investigate job mobility of parents with disabled children. Since both groups might be systematically different from the general group of middle-aged fathers, a more comprehensive approach for this group merits discussion and is the basis of my work.

[13] This is mainly because expanded eligibility for their children through Medicaid decreases the perceived value of employment with EPHI for those fathers.

In addition to job mobility, several papers examine whether the health benefit mandates affect eligible workers' annual earnings or other types of compensation, as these mandates raise the relative cost for firms to insure them. This exploration is critical since the efficiency of these mandates largely depends on the extent to which their costs are shifted to group-specific wages. There may be several factors, however, that prevent this full group-specific shifting of the total cost of the mandated benefits to beneficiaries. For example, anti-discrimination regulations and workplace relative-pay norms make it difficult for employers to reduce the wages of the targeted group.[14] In spite of these regulations, Gruber (1994) finds substantial shifting of the costs of the group-specific mandates to the wages of the targeted group, suggesting that maternity mandates are an efficient tool of social policy. This study inspired many articles seeking to determine the effects of mandated health insurance on earnings. Monheit and Rizzo (2007) review the relevant literature regarding the costs of various mandates for employees and employers.

Alternatively, the cost of providing additional insurance may be shared by other coworkers. To investigate this, Goda et al. (2016) simulate various cases where the magnitude of the reduction in wages varies by the degree of pooling of employees. Depending on the size of pooling, the wage reduction ranges from $30 to $1,500 per worker.[15] Another strategy that employers may use would be to decrease total monetary compensation (e.g., employer contribution for deferred compensation) for eligible workers instead of directly adjusting their wages.

Many studies explore connections between health insurance and other labor market outcomes. There is also a budding literature devoted to the effects of extending health insurance coverage to young adults, specifically. My paper extends this literature by looking at more comprehensive reforms, at both the state and federal levels, and considering the potential

[14] Anti-discrimination regulations prohibit differential pay for the same job across groups and prevent differential promotion decisions by demographic characteristic. Furthermore, workplace norms that prohibit different pay across groups and union rules about equality of relative pay may have similar effects as the anti-discrimination regulations.

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

consequences of these reforms on parental labor force decisions.

## Data

To assess the effects of policy changes on health insurance outcomes, I leverage detailed information on individuals using the 2004 and 2008 SIPP panels, which are linked to the DER and BR. The SIPP is a nationally representative household survey where each panel is divided into waves of four months. The time period covered in my data is January 2004 to December 2012.[16] This is 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 per month during a 4-month wave. Most SIPP questions ask the respondent to report information regarding the four months prior to the interview (SIPP Users' Guide).

Every SIPP wave includes a core set of questions about labor market outcomes, health insurance coverage and participation in government programs. In this study, I use these core questions to collect information about health insurance, demographic characteristics and employment. 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 also shows the main reason for leaving their employer 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.[17] In addition, there are topical questions included only in selected waves. For example, I use the wave 2 topical module questions that ask for the year when the respondent's last child was born. If the child was the only

[16] Because the Great Recession occurred during this time, labor force decisions might be different in my sample than in those from other periods.

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

child, then I use the question for the year when the respondent's first child was born. My analysis is primarily based on the youngest children because fathers generally need coverage until their youngest children grow up.[18] By subtracting the birth year of the last child from the interview year, I get the youngest child's age and only include fathers with children aged 19–29. Then I link these fathers' information to the core SIPP data which provides detailed demographic characteristics, time of interview and state of residence. None of the variables that I use are imputed.

Although the SIPP provides detailed, self-reported demographic characteristics, the linked dataset between the SIPP and other administrative records (i.e., DER and BR) provides highly accurate measures of earnings and total monetary compensation. First, I use the respondents' Social Security Numbers (SSN) to link them to the DER. The DER includes their W-2 information such as wages and employer contributions to retirement benefits.[19] If the respondents had two or more W-2s that originated from the same parent company, I sum up their earnings to calculate their annual earnings.[20] Determining whether the W-2s are from the same parent company is possible because additionally linked 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.[21] In my research, therefore, linking SIPP data with the administrative data allows me to achieve a more comprehensive understanding of the compensation adjustments caused by the mandates. Furthermore, with this linked data, I can

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

[19] The DER-SIPP linkage is only available until 2012 through the United States Census, so the observations for 2013 cannot be included from the 2008 SIPP.

[20] This analysis considers cases where employees move locations and therefore have two or more W-2s on file. For example, this could include a Walmart worker who is still part of the company but works in two locations within a year. If the W-2s did not come from the same parent company, however, I only consider the W-2 with the highest earnings to link with the corresponding SIPP data. I also run the analysis by omitting all individuals who had two or more W-2s that did not come from the same parent company (instead of using the highest earnings from the W-2s). The results from the samples with and without accounting for these special cases demonstrate no significant difference.

[21] Firms themselves sometimes change or have multiple EINs for payroll or tax purposes or for establishments with different locations within the same firms. In my analysis, about 20 percent of people who have two or more W-2s on file were from the same parent companies.

assess the accuracy of the survey data and adjust for errors in reported earnings.[22] Earnings are inflation-adjusted using the yearly CPI-U indices and 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, which is the tendency for respondents to report higher rates of events between survey waves than within survey waves (Blank and Ruggles, 1996). This means that the analysis is conducted at the father-wave level rather than the father-month level.[23]

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). These data can be represented in Appendix Table 1. I demonstrate the change in eligibility for fathers from three of the states that introduced dependent coverage mandates in the first three rows in Table 1. The last row in Table 1 represents 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. In other words, this is the intersection of the fathers whose children were *ineligible* across all time periods in Table 1. *Ever Eligible* is the group of fathers who were eligible at some point in my analysis. This is the union of fathers with *eligible* children across time periods in Table 1. Both panels in Table 2 include married fathers between the ages of 45 and 64 with

[22]Bridges et al. (2003) find substantial measurement error in SIPP wage and salary data. They argue that the mean SIPP wages were understated by 7.5 percent relative to DER wages. Gottschalk and Huynh (2005) also suggest that respondents with SIPP information but no DER records had lower earnings than respondents with observed earnings in both data sets, possibly reflecting informal work arrangements.

[23]I focus on fathers only because they have more predictable labor force patterns and persistent attachment to their jobs than mothers. The wage-labor supply elasticity of fathers is often much smaller than that of mothers (Blundell and MaCurdy, 1999).

their youngest child between the ages of 19 and 29.[24] They both exclude the states that had no age limit (i.e., Iowa and Texas) and the states that extended the provision to age 29 (i.e., New York, New Jersey and Pennsylvania).

For the job-lock analysis (see Panel A), I include fathers who, in the previous wave, were compensated for their work by an employer, had EPHI under their own name and were not self-employed. The sample includes approximately 14,500 working fathers, 75 percent of whom had an *ever eligible* child. The rate of voluntary job separation within a 4-month wave, on average, is 2 percent. These rates are similar to those reported in other papers based on SIPP data (Barkowski, 2017; Chatterji et al., 2016). Hamersma and Kim (2009) report rates of voluntary employer separation ranging from 3 to 5 percent for employed, low-income parents within the 1996 and 2001 SIPP data. The small deviation may exist between the rates, in part, because I do not limit the sample to low-income respondents and the 2008 SIPP was conducted during a severe recession. Although *always ineligible* fathers within my sample are generally older, less-educated and less likely to be white, almost all other characteristics are comparable. Bansak and Raphael (2008) argue that roughly 18 percent of workers with EPHI separated from their employers within a year. This number is relatively high compared to my data even after considering that their paper focuses on the separation that happened during a year—between wave 1 and wave 4—instead of during a 4-month period. Two additional factors could drive this difference in the means. First, Bansak and Raphael (2008) are considering all separations, not just voluntary ones. Second, they consider between-wave measures for job-mobility while I look for the job separation that happens within a wave.[25]

The fathers in the job-push sample have the same demographic characteristics (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 the *previous* wave, were unemployed or did not have EPHI from their employers. Selecting the sample in this way limits

[24] I do not extend the child's age up past 30 because adult children over 30 are systematically different from adult children in their early 20s in terms of life stage.

[25] Appendix A of Chatterji et al. (2016) explains why measuring separation using 'within-wave' would be more plausible than using 'between-wave.'

the chance that job-push and job-lock would be conflated since individuals without EPHI could not be affected by job-lock.

When I examine the effect of mandates on compensation, I use the job-lock sample subset, which contains fathers who did not voluntarily change their jobs within the wave. This is because the compensation decrease would mainly affect those who stay in their jobs and are eligible to benefit from the mandates. As expected, the descriptive statistics for this sample, which is shown in Appendix Table 3, is remarkably similar to Panel A in Table 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 with youngest children whose ages are at or beneath mandate thresholds and those with youngest children whose ages are above.

The model is specified as

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

where (1) is the probit model in a difference-in-differences framework for individual $i$ in state $j$ and time (wave) $t$. $\Phi(.)$ is a standard cumulative normal distribution function.[26] $y_{ijt}$ is the outcome variables for voluntary job separation and health insurance for the fathers.

To investigate job-lock, $y_{ijt}$ is equal to one if a voluntary job separation happened in that wave. For the health insurance coverage, $y_{ijt}$ indicates whether fathers currently have health insurance that covers their young adult children.

$Elig_{ijt}$ is the main independent variable and is determined by three things: state of res-

[26] Given that I limit my sample to those who are at risk of changing their employment status, this model can be viewed as a standard discrete time hazard model (Bruce Meyer, 1990).

idence, year of interview and a youngest child's age.[27] 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 the case of a father with a youngest child who 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 the Colorado mandate. Other requirements—most importantly, student status—are inappropriate to use for eligibility imputation because they are jointly determined outcomes.[28] I expect the coefficient of $Elig_{ijt}$, $\beta_1$, to be negative for the job-lock analysis.

$X_{it}$ contains other covariates, including father's age and dummy variables for the following: high-school dropouts and high-school graduates, non-Hispanic white respondents, and public sector workers. I include full sets of state and year indicators, denoted with $time_t$ and $state_j$, to focus on within-state variation.[29] In addition, indicators for all the children's ages from 20–29 (the indicator for 19-year-olds serves as the baseline group) are included to account for the time invariant, behavioral difference of fathers of various aged young adults. The regression also contains state-specific, linear time trends.[30]

The empirical strategy I rely on to detect job-push is conceptually similar to the one I use for job-lock, but with one important change: different sample selection criteria. Therefore, for job-push analysis, $y_{ijt}$ is equal to one if fathers voluntarily left their previous jobs without EPHI (or were unemployed in the previous wave). A positive estimate of $\beta_1$ would provide

[27] If the father had a 22-year-old and a 16-year-old, then I do not include him in my sample.

[28] For example, a state mandate might induce individuals into or out of student status, so using it to determine eligibility would introduce bias (Depew, 2015). That being said, in states like Florida where the state mandates required student status, I would consider a father with a child who was a 21-year-old, full-time student as "eligible" **only after** the state mandate was implemented. This empirical strategy may lead to attenuation bias in the point estimate of interest (Depew, 2015).

[29] I did not include the individual-fixed effects since this Difference-in-Differences design addresses possible unobserved characteristics already.

[30] Because my sample consists of people who are at risk of leaving a job and the separation is observed at most once for each person, my specification is equivalent to a discrete time hazard model. Thus, it is not possible to include individual fixed effects because they would soak up all of the variation in the data.

evidence of job-push, meaning that fathers seeking health insurance to cover their child would be more likely to transition from their current employment status.

I also examine the mandates' impact on working fathers' annual earnings or total monetary compensation based on the fathers who stayed in their jobs with EPHI—the aforementioned subset of job-lock sample. Since employers can easily identify the group of working fathers with eligible children, they may respond to the extra cost of providing dependent coverage by reducing other compensations for this group. To examine whether the cost of the mandate was transferred to working fathers with eligible children, I replace $y_{ijt}$ with the natural log of either the annual earnings or the total monetary compensation. This total monetary compensation is the logarithmic value of the sum of annual earnings and deferred compensation. For those working fathers who were eligible for the mandates, therefore, I would expect a negative estimate of $\beta_1$. Compensation was zero for some working fathers and is automatically omitted when I run the regression since I use the natural log of compensation as the dependent variable.[31]

## Results

Table 3 shows the evidence for an increase in job-lock among working fathers due to the dependent coverage mandates. All columns are estimated with a full set of covariates. The results in columns 1–2 are estimated without state-specific linear trends; those in columns 3–4 include these trends. Since multistage-stratified sampling is an important aspect of the SIPP, columns 1 and 3 are weighted while columns 2 and 4 are not.[32] The results in this table indicate that after the mandates took effect, the average probability of leaving an employer for any voluntary reason was 0.7 pp lower for working fathers with eligible children than for

[31] I treat fathers who have a valid SSN but no record in the DER as having zero earnings (Chenevert et al., 2016). Earnings may be absent from the DER because an employer may fail to report an employee's wages to the Social Security Administration. These workers without DER data are 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).

[32] Except Table 9 and Appendix Table 2, I only report the weighted estimates for the remaining tables.

fathers without.[33] The 0.7 pp is a 37 percent decrease in voluntary job separation given that the average separation rate of *ever eligible* working fathers before the implementation was approximately 2.0 percent. This 2.0 percent rate is similar to the magnitudes reported in other papers based on the SIPP. Barkowski (2017), for example, indicates a voluntary job separation of 2.3 percent, although it is important to note that his rate is based on the 1980s and early 1990s SIPP panels. Hamersma and Kim (2009) demonstrate rates of voluntary job separation ranging from 3 to 5 percent for employed, low-income parents in the 1996 and 2001 SIPP panels. The rates in my sample are likely to be lower than those of Hamersma and Kim (2009) since I do not limit the sample to low-income respondents.

The magnitude of my results in Table 3 is comparable to the effects of similar mandates targeting children on the mobility decisions of working fathers. Barkowski (2017) suggests that Medicaid eligibility for one household member results in a 34 percent increase in the likelihood of a voluntary job exit among working member(s) in the household. However, when Barkowski (2017) further restricts the sample to exclude those workers in the top income decile—who are less likely to be affected by Medicaid—the resulting estimates are increased by up to 71 percent. Chatterji et al. (2016) also demonstrate that the *ACA prohibition on pre-existing condition exclusions* increases the probability of job exit by 35 percent for married fathers with disabled children compared to fathers with healthy children. These previous works are comparable to mine because they focus on whether mandates for children can influence parent's reliance on employment; minor differences in magnitudes or sign can be attributed to the different time periods and policies examined.[34] 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 3 of Table 3. 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

[33]Even with alternative regressions such as the linear probability model (LPM) and the logit (Appendix Table 2), similar effect sizes can be observed.

[34]These previous works focus on whether parents' reliance on employment for health insurance decreased, while I examine whether it increased. The primary idea, however, remains the same: health insurance mandates for children affect fathers' labor market decisions.

depending on the range of the control group. In column 3, I also examine whether expanding the time period with the 2001 SIPP panel would alter the results. In this analysis, I omit five more states (besides those five states that were excluded from the main analysis) because they were sampled together in the 2001 SIPP (Wyoming, Vermont, Maine, South Dakota and North Dakota). Because I was unable to verify the 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. All columns are significant and had a similar magnitude as column 1. In column 5, I treat the state mandates that have student status requirements as if they do not have mandates (i.e., Florida, Idaho, Louisiana, Massachusetts, North Dakota, Rhode Island and South Dakota). Although the result in column 5 loses statistical significance, it still has a p-value of about 0.101.[35]

In Table 5, I examine the heterogeneity of the results. Working fathers with higher education might be more responsive to the mandates given their knowledge of dependent coverage mandates. On the other hand, they might also be less likely to have children who need the dependent health coverage, because their children would be better educated and more likely to secure jobs with EPHI. Column 1 indicates the effects of the mandates on working fathers with some college education more, while column 2 shows those on working fathers with less than or equal to a high school education. The results suggest that more educated fathers have less job mobility as a result of the mandates, supporting the former hypothesis.

In Table 6, I perform falsification tests. In this table, my sample is comprised of working fathers in three groups: those with a youngest child between the ages of 8–18, 30–40 and 27–37. This allows me to implement a placebo eligibility among working fathers with youngest children unaffected by the mandates due to their ages. For example, I use a sample that

[35] As mentioned in *Institutional Details*, full-time students aged 19, 20, 21 and 22-years-old before the mandates were implemented were often considered to be eligible under their parent's plan. In my main analysis, however, I assume all working fathers with children aged 19–22 are considered 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 student. By treating these states as states that had no mandate before the ACA, the results in column 5 show the consistency of my main results in Table 3.

is comprised of working fathers whose youngest children were between 8 and 18 years old. Then, I consider the placebo (state or federal) mandates' eligibilities by subtracting 11 from age eligibility criteria (e.g., if the mandate expanded coverage to dependent children up 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 seem to be affected by the mandates. I repeat this process for working fathers with children aged 30 to 40. For these working fathers, I add 11 to the age eligibility (e.g., if the mandate increased the age limit up to 23, I would consider this state's placebo age limit to be 34).

One drawback of the first two falsification tests is that they do not include any fathers who are in my main sample. To rectify this, I use the sample containing working fathers with children aged 27–37 in column 3 of Table 6. With this analysis, I can examine whether there are time-effects (i.e., other circumstances that have changed over time and affect parents differentially based on the age of their children). In this column, I still include the same *Always Ineligible* fathers with children aged 27 to 29 as I use in my main analysis. However, I alter the *Ever Eligible* group by adding 11 to the original age limit, resulting in a sample of fathers with 30 to 37-year-old children. None of the falsification tests have any significant effects and the point estimates are appreciably different from my main findings in Table 3.

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 these fathers were incentivized to leave a job without EPHI due to the increase in opportunity costs of staying in their current employment status. Unlike Hamersma and Kim (2009) and Barkowski (2017) who study the Medicaid expansions, I do not find evidence of job-push for fathers. This might be because of the period in which parents are affected by the policies. While Medicaid influences parents for a long period of time, the dependent coverage mandates only affect parents for a short period when their children are in their early 20s. Parents might be less motivated to change their employment status (in this case, finding a new job with EPHI) for a short-term benefits.

To examine how employers adjust employee compensation in response to rising health insurance costs, I analyze the change in total monetary compensation in addition to the annual earnings. This is because it might be difficult for employers to directly adjust earnings for the eligible working fathers without violating non-discrimination laws. Table 8 presents the effects of this mandate on earnings and other compensation among working fathers who did not leave their jobs with EPHI, based on both the administrative data and the public data.[36] In columns 1–2, I omit the individuals who had zero compensation in the administrative data.[37] It appears that there was a modest decline in earnings and total compensation.[38]

To compare the results in columns 1–2 with those based on the public data, I include the latter results in column 3 of Table 8. Unlike the results from the administrative data, I do not find any significant effect on annual earnings with public SIPP data alone.[39] As another type of falsification test, I run the same analysis for the fathers without EPHI. Results are included in Appendix Table 5. I do not observe any significant change in compensation.

Table 9 shows the change in the dependent coverage rates.[40] The sample I use in this table is the same as the one that I use in the main job-lock analysis. The results from this table demonstrate after the mandates, there was a 32 percent increase in dependent health insurance coverage among working fathers than they would otherwise have been. In some cases, a father may experience job-lock as a result of the mandates even when he is not

[36] This sample is a subset of the job-lock sample, but it excludes those individuals who voluntarily separated from their jobs. I also run the same analysis on the full job-lock sample, but it does not change my results.

[37] To examine whether including fathers with zero compensation would change my results, I also add `one` to both dependent variables and convert them to natural logs (i.e., $ln(AnnualEarnings + 1)$ and $ln(TotalMonetaryCompensation + 1)$). I arbitrarily assign this *one* for the compensation outcome variable to those working fathers with zero compensation. With these adjusted dependent variables, I run the linear regression again with the same specifications as equation (1) and Tobit regressions censored at one (see columns 1-4 of Appendix Table 4).

[38] The results, however, should be interpreted with caution. As mentioned, non-discrimination laws may prevent employers from differentially paying total compensation and earnings. In addition, all workers may bear the cost of the mandate since many non-parents are potential future users of the policy and it may be difficult for firms to implement wage offsets when workers become parents. If the additional costs can be shared by more employees, then my estimates in Table 8 serve as the upper limit in absolute value.

[39] Although the earnings reported in the SIPP are monthly, I aggregate them into annual earnings for each father and use this as an outcome measure in Appendix Table 4.

[40] The outcome variable for this analysis is based on two questions: (1) whether respondents cover children 20 years or older who are living outside their household and (2) whether they cover children between the ages of 19 and 29 living in their household. If the answer to one of these two questions was 'yes,' the outcome variable is equal to one; otherwise, it is zero.

currently adopting EPHI for his young adult child. This might occur, for example, when a father with EPHI anticipates that his young adult child may lose his/her own insurance. The father might choose to forgo other opportunities and stay in his current job, as a safety net for his child.

## Conclusions

While both the state and federal dependent coverage provisions successfully increase health insurance rates among young adults, previous research does not make it clear whether the mandates have possible effects on other sub-populations. In order to fill this gap, I explore whether fathers' dependence on employment for health insurance increased after the implementation of the mandates, as they also heavily value the dependent coverage alongside young adults. I find that eligible working fathers aged 45-64 with EPHI experienced a 37 percent decrease in the rate of voluntary job separation due to the mandates. These results suggest that the dependent coverage mandates may in fact be detrimental to some populations: while they successfully reduce uninsured rates for young adults, they increase the dependence on employment for the middle-aged, thereby weakening the justification for the law itself. This finding was robust among a variety of different specifications for the effect of these mandates.

The general pattern of job mobility for married fathers is consistent with previous findings on the effects of health insurance—specifically that targets children—on parents' job mobility (Bansak and Rapahel, 2008; Chatterji et al. 2016; Hamersma and Kim, 2009). I focus on a more broadly defined population than the other papers: fathers with and without EPHI who have children aged 19-26. This paper contributes to the little that is known about job-lock and change in compensation for eligible fathers whose responsiveness is the key determinant for the policy to be effective. On that note, this paper demonstrates that fathers are willing to adjust their labor market decisions to secure access to high quality health insurance for their children, even when these children are over 19-years-old. My results emphasize how health insurance access can have far-reaching consequences for both targeted individuals and their household members.

Although most related studies only examine the state and federal mandates independently, this paper makes use of both of these mandates as part of its identification strategy. This approach is consistent with that of Barkowski and McLaughlin (2018), which argues that to achieve more credible variation, researchers who study overlapping policies—especially those on health insurance coverage—need to account for both policies. Also, my research demonstrates that both the state and federal dependent coverage mandates may provide a potential source of exogenous variation for researchers seeking to study job mobility decisions and related outcomes among middle-aged men. Besides job-mobility and health insurance enrollment decisions, I investigate only two other possible outcomes: annual earnings and total monetary compensation; however, I believe that additional outcomes of these dependent coverage laws are open for exploration with future data.

## 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 | 19–24 | 25–29 | 19–26 | 27–29 |
| Colorado | . | 19–29 | 2006 | 19–25 | 26–29 | 19–26 | 27–29 |
| Connecticut | . | 19–29 | 2009 | 19–26 | 27–29 | 19–26 | 27–29 |
| Michigan | . | 19–29 | | . | 19–29 | 19–26 | 27–29 |

Table 1: 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 representative states, 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

| | Panel A: Job-Lock Sample | | Panel B: Job-Push Sample | |
|---|---|---|---|---|
| | Always Ineligible | Ever Eligible | Always Ineligible | Ever Eligible |
| Eligible | - | .41 | - | .45 |
| | | [ .49] | | [ .50] |
| Age | 56.30 | 54.09 | 57.29 | 54.03 |
| | [4.60] | [4.60] | [4.38] | [4.75] |
| Highschool dropouts | .05 | .04 | .02 | .04 |
| | [.22] | [.18] | [.15] | [.19] |
| Highschool graduates | .27 | .26 | .16 | .25 |
| | [.44] | [.44] | [.37] | [.44] |
| Some college or higher | .69 | .71 | .81 | .71 |
| | [.46] | [.46] | [.39] | [.45] |
| Non-hispanic white | .81 | .82 | .83 | .79 |
| | [.39] | [.38] | [.38] | [.41] |
| African American | .07 | .07 | .06 | .07 |
| | [.26] | [.26] | [.25] | [.26] |
| Hispanic or Asian | .11 | .11 | .10 | .14 |
| | [.31] | [.31] | [.31] | [.35] |
| Public Sector worker | .21 | .19 | .05 | .08 |
| | [.40] | [.40] | [.23] | [.27] |
| **Dependent Variables** | | | | |
| Voluntary Job Separation rates | .02 | .02 | .02 | .02 |
| | [.13] | [.13] | [.14] | [.15] |
| N. of Individuals [1,000] | .55 | 2.00 | .10 | .45 |
| N. of Observation [1,000] | 3.70 | 11.00 | .50 | 1.90 |
| Ln(Annual Earnings in the SIPP) | 10.97 | 10.85 | 10.49 | 10.53 |
| | [.72] | [.75] | [1.03] | [.86] |
| Ln(Annual Earnings in the DER) | 10.84 | 10.94 | 10.00 | 10.32 |
| | [.97] | [.95] | [1.46] | [1.20] |
| Ln(Tot. Monetary Comp.) | 10.90 | 10.99 | 10.04 | 1.34 |
| | [.98] | [.96] | [1.48] | [1.22] |
| Coverage for Dependent | .02 | .07 | - | - |
| | [.14] | [.25] | . | . |
| N. of Individuals [1,000] | .50 | 1.90 | .10 | .40 |
| N. of Observation [1,000] | 3.50 | 10.50 | .40 | 1.70 |

Table 2: All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands.

Table 3. The Effects of Eligibility on Voluntary Job Separation Rates, Main Results

| | [1] Weighted | [2] Unweighted | [3] Weighted | [4] Unweighted |
|---|---|---|---|---|
| Eligible | -.007* | -.006* | -.007* | -.006* |
| | [.003] | [.003] | [.004] | [.003] |
| Covariates | Y | Y | Y | Y |
| State Differential Time Trends | | | Y | Y |
| N. of Individuals [1,000] | 2.50 | 2.50 | 2.50 | 2.50 |
| N. of Observations [1,000] | 14.5 | 14.5 | 14.5 | 14.5 |
| Dependent variable means | | | | |
| *Ever eligible*, before Mandate | .020 | .017 | .020 | .017 |

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.
All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. Standard errors are clustered at the state level. Observations are weighted using SIPP individual weight.

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

| | [1] | [2] | [3] | [4] | [5] |
|---|---|---|---|---|---|
| | Table 3 [3] | | | | |
| Eligible | -.007* | -.007* | -.006* | -.008* | -.006 |
| | [.004] | [.003] | [.003] | [.004] | [.004] |
| Fathers with Youngest Child Aged 19-33 | | Y | | | |
| Including 2001 SIPP | | | Y | | |
| Excluding States with Unclear Implementation Dates | | | | Y | |
| Treating States with Student-Status as Non-mandated | | | | | Y |
| N. of Individuals [1,000] | 2.5 | 2.4 | 3.6 | 2.4 | 2.5 |
| N. of Observations [1,000] | 14.5 | 18.0 | 21.0 | 13.5 | 14.5 |

Table 4: † 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.

All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weights and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

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

| | [1] Higher Educ. | [2] Lower Educ. |
|---|---|---|
| Eligible | -.009† | -.002 |
| | [.005] | [.005] |
| N. of Individuals [1,000] | 1.7 | 0.8 |
| N. of Observations [1,000] | 10.0 | 4.4 |

Table 5: † 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.
All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weight and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

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

| | [1] Eligible Age-11 | [2] Eligible Age+11 | [3] Eligible Age+11 (only for 19-26) |
|---|---|---|---|
| Eligible | .003 | -.002 | .006 |
| | [.005] | [.007] | [.008] |
| N. of Individuals [1,000] | 1.6 | 1.0 | 1.1 |
| N. of Observations [1,000] | 12.5 | 7.7 | 9.4 |

Table 6: † 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.
All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weight and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

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

| | [1] | [2] |
|---|---|---|
| Eligible | -.006 | .009 |
| | [.005] | [.008] |
| Covariates | Y | Y |
| State Differential Time Trends | | Y |
| N. of Individuals [1,000] | 0.55 | 0.55 |
| N. of Observations [1,000] | 2.4 | 2.4 |

Table 7: † 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.
All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weight. Standard errors are clustered at the state level.

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

| | [1] | [2] | [3] |
|---|---|---|---|
| | SIPP-DER-BR | | Public SIPP |
| | ln(Earnings) | ln(Tot. Comp.) | ln(Earnings) |
| Eligible | -.104† | -.117† | .001 |
| | [.062] | [.062] | [.041] |
| N. of Individuals [1,000] | 2.4 | 2.4 | 2.4 |
| N. of Observations [1,000] | 13.5 | 13.5 | 18.5 |

Table 8: † 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.

In this table, I exclude fathers who voluntarily separated from their jobs among job-lock sample. All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weights and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

Table 9. The Effect of Eligibility on Working Fathers' Health Insurance Coverage Take-up Decisions For Young Adult Dependents

| | [1] Weighted | [2] Unweighted | [3] Weighted | [4] Unweighted |
|---|---|---|---|---|
| Eligible | .021* | .023* | .023* | .025* |
| | [.011] | [.012] | [.011] | [.012] |
| Covariates | Y | Y | Y | Y |
| State Differential Time Trends | | | Y | Y |
| N. of Individuals [1,000] | 2.5 | 2.5 | 2.5 | 2.5 |
| N. of Observations [1,000] | 14.5 | 14.5 | 14.5 | 14.5 |
| Dependent variable means | | | | |
| Ever eligible, before Mandate | .069 | .079 | .069 | .079 |

Table 9: † 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.
All columns show results of the average marginal effect based on probit regressions using the 2004 and 2008 SIPP panels. Standard errors are clustered at the state level. Observations are weighted using SIPP individual weight.

## Appendices

Table A1. Implementation of the Dependent Coverage Laws

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

Appendix Table 1: This table shows the year that federal and state-level mandates were implemented along with age criteria. States that are excluded from the main analyses are ones 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.

Table A2. Alternative Regression Results

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| | Linear | | Logit | |
| | Weighted | Unweighted | Weighted | Unweighted |
| Eligible | -.007 | -.004 | -.008* | -.006* |
| | [.004] | [.003] | [.004] | [.003] |
| N. of Individuals [1,000] | 2.5 | 2.5 | 2.5 | 2.5 |
| N. of Observations [1,000] | 14.5 | 14.5 | 14.5 | 14.5 |
| Dependent variable means | | | | |
| Ever Eligible, before Mandate | .020 | .019 | .020 | .019 |

Appendix Table 2: † 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.
All columns include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level. Observations are weighted using SIPP individual weight.

Table A3. Descriptive Statistics for Table 8

| | Always Ineligible | Ever Eligible |
|---|---|---|
| Eligible | 0 | .41 |
| | | [ .49] |
| Age | 56.25 | 54.07 |
| | [4.57] | [4.59] |
| Highschool dropouts | .05 | .04 |
| | [.22] | [.18] |
| Highschool graduates | .27 | .26 |
| | [.44] | [.44] |
| Some college or higher | .69 | .71 |
| | [.46] | [.46] |
| Non-hispanic white | .82 | .82 |
| | [.39] | [.38] |
| African American | .07 | .07 |
| | [.27] | [.26] |
| Hispanic or Asian | .11 | .11 |
| | [.31] | [.31] |
| Public Sector worker | .20 | .19 |
| | [.40] | [.39] |
| **Dependent Variables** | | |
| Voluntary Job Separation rates | 0 | 0 |
| N. of Individuals [1,000] | .55 | 2.00 |
| N. of Observation [1,000] | 3.60 | 10.50 |
| Ln(Annual Earnings in the SIPP) | 10.85 | 10.97 |
| | [.75] | [.72] |
| Ln(Annual Earnings in the DER) | 10.85 | 10.94 |
| | [.96] | [.95] |
| Ln(Tot. Monetary Comp.) | 10.90 | 11.00 |
| | [.98] | [.96] |
| N. of Individuals [1,000] | .50 | 1.90 |
| N. of Observation [1,000] | 3.60 | 10.50 |

Appendix Table 3: All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands.

Table A4. The Effects of Eligibility on Annual Earnings

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| | SIPP-DER-BR | | | | Public SIPP | |
| | Linear | | Tobit | | Linear | Tobit |
| | ln(Earnings+1) | ln(Tot. Comp.+1) | ln(Earnings+1) | ln(Tot. Comp.+1) | ln(Earnings+1) | ln(Earnings+1) |
| Eligible | -.208† | -.222† | -.213† | -.227† | -.022 | -.022 |
| | [.123] | [.126] | [.126] | [.129] | [.047] | [.047] |
| N. of Individuals [1,000] | 2.5 | 2.5 | 2.5 | 2.5 | 2.4 | 2.4 |
| N. of Observations [1,000] | 14.0 | 14.0 | 14.0 | 14.0 | 19.0 | 19.0 |

Appendix Table 4: † indicates that the p-value is less than 0.1; * indicates that the In this table, I exclude fathers who voluntarily separated from their jobs among job-lock sample. All numbers of observations and individuals are first rounded according to the United States Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weights and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

Table A5. The Effects of Eligibility on Annual Earnings and Total Monetary Compensation
(Job-Push Sample, Falsification Tests)

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| | Linear | | Linear | | Tobit | |
| | ln(Earnings) | ln(Tot. Comp.) | ln(Earnings)+1 | ln(Tot. Comp.+1) | ln(Earnings+1) | ln(Tot. Comp.+1) |
| Eligible | -.109 | -.123 | -.438 | -.451 | -.501 | -.515 |
| | [.243] | [.248] | [.562] | [.565] | [.630] | [.633] |
| N. of Individuals [1,000] | 0.45 | 0.45 | 0.55 | 0.55 | 0.55 | 0.55 |
| N. of Observations [1,000] | 2.1 | 2.1 | 2.4 | 2.4 | 2.4 | 2.4 |

Appendix Table 5: † 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.
All numbers of observations and individuals are first rounded according to the U.S. Census disclosure rules and then are rounded to the nearest thousands. All columns are weighted by SIPP individual weights and include covariates for individual demographics and state differential trends. Standard errors are clustered at the state level.

## Figures

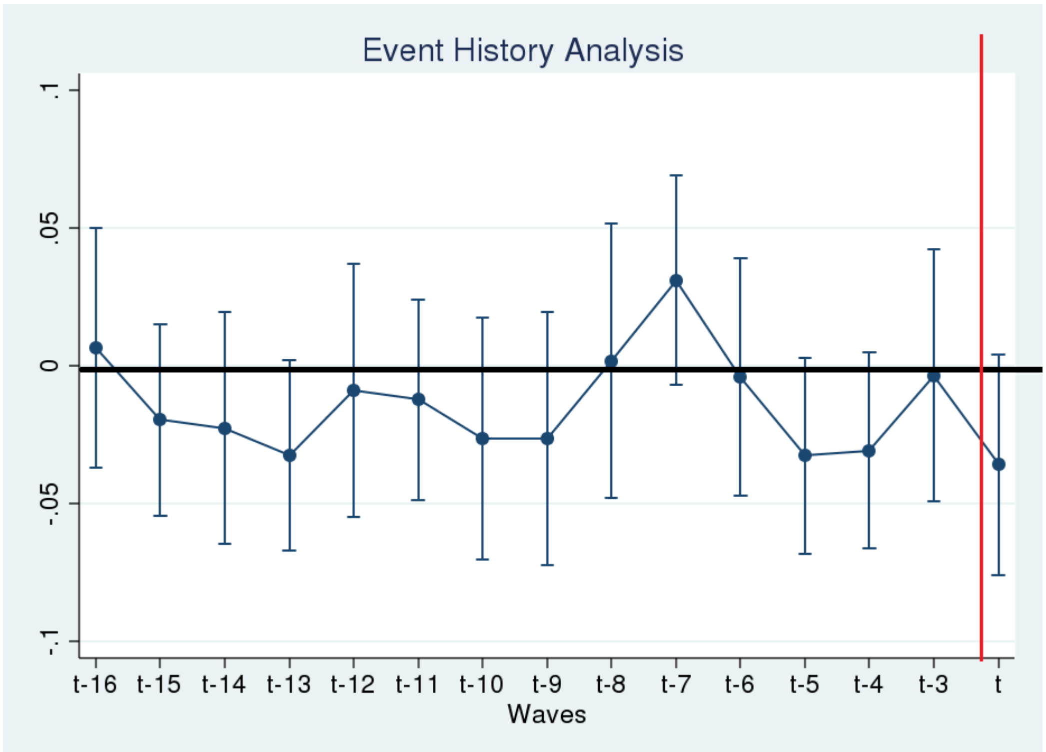


Figure 1: $t-1$ is the baseline time period for this figure. Result for $t-2$ is not reported in this graph due to the Census disclosure rule. Blue lines around the point estimates represent 95 percent confidence intervals.

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