1 Introduction

Nearly half of Americans rely on employer-sponsored health insurance for insurance coverage (Kaiser Family Foundation 2022). This close linkage between insurance and employment in the U.S. has been shown to generate "job lock" in the labor market: that is, employer-sponsored health insurance availability can distort labor supply decisions and reduce job mobility (Madrian 1994; Gruber and Madrian 1995; Gruber and Madrian 1997; Garthwaite, Gross, and Notowidigdo 2014; Dave et al. 2015). This literature primarily focuses on the effects of an individual's *own* coverage on their employment. Yet a common feature of employer-sponsored health insurance is that coverage can also extend to an employee's children and spouse – their "dependents." 96 percent of employers offering health benefits to their employees also provide coverage to their dependents, and 50 percent of children under 19 in the U.S. are covered under employer-sponsored plans (Kaiser Family Foundation 2020; 2023).

Despite its prevalence, relatively little is known about whether dependent coverage affects parental labor supply decisions, or the extent of these distortions. On the one hand, dependent coverage is a form of non-wage compensation similar to own coverage, and thus, by increasing the value of employment, should lead to greater job lock. On the other hand, dependent coverage may have a more limited effect because dependents are younger and healthier, because planholders are already "job locked" by their own coverage, or because employers reduce other forms of compensation to offset its cost. Understanding the extent to which dependent insurance leads to parental job lock is of critical importance when considering policies that affect coverage for children, such as the Children's Health Insurance Program (CHIP) or insurance coverage mandates.

One factor that may have limited prior work on the intra-family spillovers of dependent coverage is a lack of data on both insurance take-up and employment outcomes for different family members. While these outcomes are reported in some survey data, sample sizes are often too small to support well-powered analyses. An important contribution of our paper is

our use of a large panel of private health insurance enrollment data from employer-sponsored plans that has three key features: (1) a measure of job tenure for the planholder; (2) monthly dependent enrollment information; and (3) dependent birth dates. While the enrollment and claims data do not directly report labor supply outcomes, we proxy for job tenure with the number of months a planholder retains coverage from any plan offered by their employer, including those from different insurers.[1] We provide supporting evidence from survey data that this measure is a reliable proxy of job tenure. To our knowledge, our analysis is the first to use this proxy to analyze job mobility. Future work using this proxy may provide valuable insights into the connection between health, insurance, and employment outcomes, as well as potential spillovers within the household.

The ability to link family members together is important because parental labor supply responses alone are difficult to interpret without knowing the associated dependent insurance take-up. If an increase in parental job retention is associated with a relatively large take-up response, then this implies that most parents who took up did not have to distort their labor supply decisions to do so. But if it is associated with a small take-up response, then it implies larger labor supply distortions among those who took up, and thus more job lock for these parents. Having measures of both parental and dependent outcomes is also useful for two additional reasons. First, it allows us to study heterogeneity in job lock across different subgroups by scaling each group's labor supply response by their coverage take-up. Second, it allows us to use our results to extrapolate the parental labor supply effects of policies for which we know the change in the dependent insurance coverage rate.

We use our data to study the effects of a dependent coverage expansion that occurred in 2011 under the Affordable Care Act (ACA). The so-called "dependent mandate" requires private insurers to extend coverage to adult children up to 26, whereas previously coverage was provided through age 19, or 23 for full-time students. Recent work has found sizable

1. In complementary work, Aouad (2023) uses claims data from one insurer to study intra-family spillovers from dependent coverage to parents. Our data, which include claims for all insurers provided by an employer, allows us to follow employees even if they switch insurers.

increases in insurance coverage among young adults following the dependent mandate (e.g., Akosa Antwi, Moriya, and Simon 2013; Sommers et al. 2013; Barbaresco, Courtemanche, and Qi 2015; Carpenter et al. 2021; Kim 2022) and documented various health and financial impacts on dependents (Sommers et al. 2013; Hernandez–Boussard et al. 2014; Barbaresco, Courtemanche, and Qi 2015; 2015; Daw and Sommers 2018; Blascak and Mikhed 2023).

To identify the effects of the dependent coverage expansion, we develop a regression discontinuity (RD) design which exploits the fact that, on average, adult dependents born in January became eligible for more months of coverage than those born in December. This difference arises because some plans cover dependents through December of the year in which they turn 26, whereas others only cover dependents through their birth month. Using this RD approach allows us to avoid issues associated with difference-in-differences models in the setting of the ACA dependent mandate (Slusky 2017).

Our analysis sample includes dependents born from January 1985 to December 1986 — these cohorts turn 26 by the end of our data in 2012 and thus all coverage added under the mandate is included in our sample period. Our RD design identifies the effects of additional dependent coverage by comparing dependents born in December 1985 to those born in January 1986 under the assumption that factors other than coverage eligibility do not change discontinuously across these cohorts. Reassuringly, we find that characteristics of parents and dependents in our sample evolve smoothly through the birth date cut-off. We also verify that there is no effect at the January/December cutoff in placebo tests using cohorts who are either too old or too young to be affected by the mandate.

We find that dependents eligible for more coverage are more likely to enroll and are enrolled for a longer period of time once the mandate is in effect, in line with prior work on the dependent mandate. Dependent enrollment increases by 1.4 percentage points at the birth date cut-off, an increase of 7.4 percent over the enrollment rate for dependents born in December 1985. In addition, the enrollment duration increases by 12.3 days (18.7 percent). Turning to parents, we find that parental job retention likelihood increases by 1 percentage

point (1.8 percent) and job duration increases by 5.8 days (1.6 percent).

These results are consistent with the increased insurance eligibility for adult dependents making parents' current jobs more valuable, thus leading to greater job retention. Combining these estimates with the effects on dependent coverage, we calculate an elasticity of parental job retention to dependent enrollment of 0.20, meaning that a 1 percent increase in the share of dependents covered leads to an increase in the parental job retention rate by 0.20 percent. For job duration, the elasticity with respect to dependent coverage duration is 0.11. Applying our results to the effect of the overall ACA dependent mandate, which increased dependent coverage by 30 percent, implies that about 400,000 parents were "job locked" by the mandate (Akosa Antwi, Moriya, and Simon 2013).

In heterogeneity analyses, we find evidence of greater job lock among parents who may have otherwise been more likely to leave their jobs: those eligible for retirement benefits, and those who do not provide coverage for their spouse or other children. We also find greater job lock for parents who may value coverage more: those with dependents with prior inpatient care and those more likely to be eligible for dependent coverage through the end of the year. Our estimates remain very similar under a variety of robustness checks, including dropping controls, excluding weights, clustering on the running variable, using alternate bandwidths, replacing our linear control function with a local linear specification, and running the analysis on dependents either too young or too old to be affected by the mandate.

2 Policy Context

2.1 The Dependent Coverage Mandate

Under the dependent coverage mandate, private health insurers were required to extend coverage to adult children through the age of 26 (Cantor et al. 2011).[2] Prior to the mandate, most plans provided dependent coverage through age 19 if the dependent was not a full-time student or through age 23 if the dependent was a full-time student. In addition, some states

2. For more information on the dependent mandate, see: https://obamawhitehouse.archives.gov/sites/default/files/rss_viewer/qa_young_adults_may.pdf (accessed on May 22, 2022).

had laws that extended coverage past age 23 for certain categories of dependents, but these laws did not apply to self-insured plans.

The ACA mandate applied to all plans, including self-insured ones, after September 23, 2010 (i.e., "the 2011 plan year"). Dependents must be born on or after January 1985 to receive additional coverage under the ACA mandate. Plans cannot charge different premiums or offer different benefit packages, and the premiums receive the same tax-favored status as those paid for other dependents.

The dependent coverage mandate is not employer-specific, meaning parents could in principle switch employers and re-enroll their dependents in employer-sponsored health insurance at their next employer. Despite its portability across employers, the dependent mandate could still reduce job mobility if parents would have to switch providers under their potential future firm's insurance network (Sabety 2023), if insurance generosity, coverage, or prices differ between their current and potential future firm, or if their outside option does not have dependent insurance (e.g., they are switching to Medicare or to a period of non-employment between employers).

2.2 Additional Months of Coverage by Dependent Birth Date

While the dependent mandate only requires plans to insure dependents through the month in which they turn 26, some plans choose to provide coverage through the end of the year in which they turn 26.[3] We refer to these plans as "birth month" vs. "end of year" plans, respectively. Figure 1a illustrates the potential additional months of coverage provided under the ACA dependent mandate from January 2011 to December 2012 for dependents born from January 1985 to January 1986. We calculate the additional coverage months separately for

3. Healthinsurance.org, an online consumer resource site, explains: "young adults can remain on a parent's health plan until age 26. Some plans will keep the young adult insured until the end of the plan year (which often corresponds to the calendar year) in which they turn 26, although others will drop them from the plan the month they turn 26." (Source: https://www.healthinsurance.org/faqs/under-the-aca-can-young-adults-still-remain-on-their-parents-health-plans-until-age-26/). As an example, Kaiser Permanente provides the following explanation in response to the question "Will I lose my coverage at age 26?": "if you're a dependent on your parent's plan, you may lose coverage under that plan either at the end of your birth month or end of the calendar year." (Source: https://continuecoverage.kaiserpermanente.org/losing-parents-plan/).

"birth month" and "end of year" plans.

For dependents in birth month plans, the number of added coverage months increases linearly in birth month. In contrast, for those on "end of year" plans, coverage jumps discontinuously between the December 1985 and January 1986 cohorts. Dependents born in January 1986 turn 26 in December 2012, and thus become eligible for 24 months of coverage, whereas dependents born in December 1985 are eligible for only 12 months of coverage. The fact that these dependents should be otherwise similar motivates our use of a regression discontinuity design using birth month to identify the effects of expanded coverage eligibility.

While we cannot directly observe whether a dependent is on a birth month or end of year plan, we find evidence of both types of plans in our data. Figure 1b plots the share of exits by month for dependents not born in December who disenroll at some point during the year in which they turn 26. Over a quarter of these dependents disenroll in December despite it not being their birth month, which is consistent with some of these dependents being on end of year plans. Appendix Figure A.1 plots the age (in months) at which a dependent disenrolls by cohort. The disproportionate share of dependents exiting at exact ages is consistent with some of these dependents being on "birth month" plans, but the mass outside of the exact ages also is consistent with some being on "end of year plans."

With both plan types in the sample, we would expect the discontinuity at January 1986 in added coverage months to be a weighted average of the 12 additional months for dependents on end of year plans and the one additional month for those on birth month plans. Figure 1a shows an illustrative example of the average discontinuity under the assumption that half of dependents are on each type of plan.

3 Data

Our main source of data is the Truven Health MarketScan CCE Database ("MarketScan Data"), a large monthly panel of employer-sponsored health insurance claims. The data combine detailed information on individual claims, monthly enrollment records, and basic

demographic information from 2000 to 2012. For each individual, we observe an enrollee ID, which allows us to follow them over time, and a family ID, which allows us to link planholders with their covered family members.

We limit our sample to data provided by employers, which comprise 212 out of the 246 all "data contributors" in the data. Doing so ensures we can track employees over time as long as they remain with the same employer and do not drop health insurance altogether. Importantly, this means we can track employees across insurers offered by the same employer (Adamson, Chang, and Hansen 2008). This unique feature of our data allows us to use it as a source of employment information.

We restrict to households with at most one dependent born between January 1985 to December 1986 and we require each dependent is first observed on their parent's plan in the pre-ACA period. Appendix A.1 describes the sample construction in full detail. Dependent birth date is not directly reported in the MarketScan data — instead, we back it out using the fact that enrollee age is reported on a monthly basis. Specifically, age is reported as of the 1st of the given enrollment month. Thus, an enrollee's birth month is the month before the one in which their age increases.

Our outcomes of interest measure whether and for how long the parent and dependent are covered by the parent's pre-ACA employer in the post-mandate period. Specifically, our outcomes are enrollment for at least one month ("enrollment likelihood") and total enrollment days ("enrollment duration") in 2011-2012 . These outcomes are our measures of post-mandate insurance coverage for the dependent and job retention for their parent.

We also create several control variables: gender of the parent and their adult dependent; birth date of the parent; total number of dependent children added to the parent's plan before 2010 (a proxy for family size); whether a spouse was ever added to a plan before 2010 (a proxy for marriage); whether the dependent had any inpatient care visits prior to 2010 (a proxy for their demand for care); and whether the parent worked full time prior to 2010. We require that time-varying measures (i.e., family size, marriage, inpatient care,

and full-time status) are observed prior to 2010 to avoid confusing changes in these variables with endogenous responses to the dependent mandate.

3.1 The Link Between Insurance Dis-enrollment and Job Exits

It is important to consider what we are measuring with respect to parental job retention. We proxy for job retention using information on whether parents continue coverage from any plan offered by their pre-mandate employer. If a parent remains with the same employer but elects to forego health insurance coverage, then our proxy would incorrectly code them as having left their job.

To assess the importance of measurement error in our proxy measure, we use 2011-2013 data from the Panel Study for Income Dynamics (PSID) to look at how often employees forgo insurance but stay at their job. Appendix Section A.2 describes the sample construction and analysis in further detail. Using individuals with similar profiles as our sample who do not leave their job by 2013, we construct an indicator for whether the individual is no longer covered by their employer in 2013. Appendix Table A.2 shows the tabulation of these indicators for heads and spouses in our sample. Only one percent of this sample drops their employer-sponsored insurance. Thus, it appears that dropping health insurance while remaining with the same employer is highly unusual for this sample. This suggests that it is reasonable to infer that the end of a planholder's coverage from their employer coincides with the end of their employment with them.

3.2 Summary Statistics

Appendix Table A.3 presents summary statistics for the full sample and by dependent birth year, where each observation is a parent-child pair. Comparing dependents in the 1985 and 1986 birth cohorts, the share enrolled for at least one month during 2011-2012 increases by 86 percent. Similarly, there is a 256 percent increase in the total number of coverage days during 2011-2012. These increases reflect the fact that the 1985 cohort is only eligible for coverage under the dependent mandate in 2011 (when they turn 26), whereas the 1986 cohort

is eligible in both years.

For parents, those with dependents born in 1986 vs. 1985 are slightly more likely to remain with their pre-ACA employer for at least one month in 2011 to 2022 (3.7 percent increase). Similarly, total job days during 2011-2012 increases by 3.5 percent. The fact that parents' job retention is higher for the 1986 cohort provides suggestive evidence in favor of the "job lock" hypothesis.

There is little difference across these cohorts in the control variable means, outside of parental age. Dependents born in 1985 tend to have older parents than dependents born in 1986, as would be expected. Since younger parents will tend to retire later, increased job retention for those with dependents in the 1985 vs. 1985 cohort may reflect the effects of age, rather than job lock. This emphasizes the importance of controlling for parental age in our analyses.

4 Empirical Method

Our empirical strategy is a regression discontinuity (RD) design in which dependent birth date serves as the running variable. We expect dependent coverage eligibility to jump discontinuously from December 1985 to January 1986. We focus on cohorts around this particular cut-off who are born between January 1985 and December 1986 because our study period of 2011-2012 matches their eligibility period.

For a given family, we use i to refer to the parent and j to refer to the dependent. Define B_j as the birth date (year-month) for dependent j and c as the cut-off value ($c = 12/1985$). We denote the outcome variable, Y_{ij}, which is a measure of either dependent enrollment or parental job retention. Then, we model Y_{ij} as follows:

$$Y_{ij} = \alpha + \beta \mathbf{1}[B_j > c] + \mathbf{1}[B_j > c] \cdot f(B_j - c) + f(B_j - c) + X_{ij}\gamma + \epsilon_{ij}, \tag{1}$$

where $f()$ is a control function based on dependent birth date. In our baseline regressions, $f()$ is linear. This choice is motivated by the policy variation depicted in Figure 1a, which

indicates that outside of the discontinuity from December 1985 to January 1986, the additional months of insurance provided by the ACA should increase linearly by dependent birth date. The term $\mathbf{1}[B_{jt} > c] \cdot f(B_j - c)$ allows the slope of the outcome variable in birth month to vary on either side of the cut-off c. X_{ij} is a set of controls: parent and dependent gender; parental age; whether other dependents or spouse was covered in the pre-period; and whether dependent received inpatient care in the pre-period. We weight each observation using triangular weights which decrease linearly in distance from the cut-off month and cluster at the individual-level.

The coefficient of interest is β – a positive β on dependent enrollment would indicate that dependents to the right of the cut-off are more likely to be enrolled or are enrolled for longer during these years. Likewise, a positive β on parental job retention indicates that the parents of dependents to the right of the cut-off are more likely to remain at the pre-mandate employer or work there for longer.

We estimate a number of variations of our main specification to test the robustness of our results. These include dropping the triangular weights, assigning $f()$ to be a local linear function, alternative bandwidth choices, excluding the control variables X_{ij}, and clustering standard errors on the running variable.

We also perform placebo tests by re-estimating Eq. 1 using two alternative cut-off dates. First, we use a sample of older dependents born in 1983-1984 and set the cut-off value to be $c = 12/1983$. Dependents from these cohorts were too old to be eligible for coverage under the dependent mandate when the ACA passed, but are similar in age to those in our main sample. Second, we construct a younger sample of dependents born in 1985-1986 and set the cut-off value to be $c = 12/1995$. The dependents in this sample are 10 years younger than those in our main sample and were covered under pre-existing, nationwide mandates.

Tests of Identification Assumptions The identification assumption in our RD design is that absent the effects of the dependent mandate, dependent and parental outcomes would evolve smoothly around the end-of-year cut-off in dependent birth date. We test this by

evaluating whether the density of the running variable is smooth through the cut-off value and whether observable characteristics evolve smoothly through the cut-off.

Examining the density of the running variable and the smoothness of observable characteristics sheds light on whether there may be manipulation or misreporting around the cut-off, or other reasons for systematic differences that could affect our outcomes. This could occur, for example, if parents with a dependent born in December falsely report a January birth date to receive extra coverage for their child, resulting in more January birth months than December birth months. Another possibility is that birth month is misreported. If a data provider had a practice of replacing all missing birth months with "January," for example, that would violate our identification assumption.

Appendix Figure A.3 plots the density of dependents by birth month. The distribution appears to be smooth through the end of year. We fail to reject the null hypothesis of a smooth density around both cut-offs. We also check for differences at the cutoff in our control variables. Appendix Figure A.4 plots the unadjusted means by dependent birth month. Visually, these graphs appear quite smooth through the birth date cut-offs. Table A.5 tests for discontinuities formally and finds no evidence of statistically significant differences in parent and dependent characteristics.

5 Results

For each of our outcomes, we present graphical evidence ("RD graphs") as well as estimates of β from Eq. 1. We plot residualized means that adjust for our vector of control variables (X_{ij} in Eq. 1). One important reason we do so is to control for parental birth date, which increases linearly in the running variable (as shown in Appendix Figure A.4).

Figures 2a-2b display RD graphs for dependent enrollment likelihood and duration during 2011-2012. In column (1) of Table 1, we report corresponding estimates of β along with their standard errors and the December 1985 mean, which we use to convert our estimates into percent changes.

Figures 2a-2b reveal a discontinuous jump in both enrollment likelihood and duration for dependents at the birth date cut-off. Table 1 shows that enrollment likelihood increases by 1.7 percentage points (9.2 percent of the December 1985 mean) and the duration of enrollment increases by 9.6 days at the cut-off (14.5 percent).

We then turn to the effects of expanded dependent coverage eligibility on parental job retention. Figures 2c-2d show RD graphs for parental job retention likelihood and duration during 2011-2012. The likelihood the parent retains their job increases discontinuously by 1.0 percentage points (1.8 percent). Correspondingly, our measure of job duration increases by 5.8 days (1.6 percent) (Table 1).

Appendix Figure A.5 and Appendix Table A.4 explore how these effects vary by outcome year (2011 vs. 2012). In the year dependents turn 25 (i.e., 2011 for the 1986 cohort) enrollment should be relatively flat in birth month, as all plans must allow dependents to remain covered throughout the year. Any change in enrollment by birth month should only be related to factors outside of their immediate coverage eligibility, like the incidence of finding a job. In the year dependents turn 26 (i.e., 2011 for the 1985 cohort and 2012 for the 1986 cohort), we would expect enrollment to increase more steeply in birth month, as "birth month" plans will terminate coverage in the birth month. In the year a cohort turns 27 (i.e., 2012 for the 1985 cohort), enrollment should be very low, as coverage would only be provided through the few state mandates which exceed 26. Reassuringly, Appendix Figure A.5 confirms these patterns.

Note that a discontinuity between the two cohorts appears in 2011, even though both cohorts would have been eligible for a full year of coverage that year. This is suggestive of anticipatory effects – the 1986 cohort could access up to two years of coverage, whereas the 1985 cohort could only access up to one. Since dependents had to actively re-enroll to take advantage of the mandate, forward-looking families may have only found it worthwhile to do so if they could access more than a year's worth of coverage. The 2011 and 2012 results on parental job retention also provide evidence of forward-looking behavior – parents to the

right of the cut-off, whose children are eligible for more coverage in 2012, are more likely to retain their job in 2011.

Job Lock Elasticity A unique advantage of our setting and data is that we can observe both parental and dependent outcomes. This allows us to calculate the elasticity of the job retention rate with respect to dependent coverage rate – that is, the change in the share of parents who stay at their job with respect to the change in the share of dependents covered. Calculating this elasticity allows us to extrapolate what the parental job retention effects would be of policies where we only know the change in the dependent coverage rate, like the overall effect of the ACA dependent mandate.

We calculate the job lock elasticity by converting the effects on dependent coverage and parental job retention in Table 1 to percent changes relative to the average for the December 1985 cohort, as shown in Appendix Figures A.7 and A.8. The elasticity is then simply the ratio of the two (Figure 3). The elasticity for job retention likelihood with respect to dependent coverage likelihood is 0.20, and the elasticity of job retention duration with respect to dependent coverage duration is 0.11. Since the ACA dependent mandate was estimated to increase coverage by 30 percent, a back-of-the-envelope calculation implies that 400,000 parents were "job locked" by mandate (Akosa Antwi, Moriya, and Simon 2013).[4]

We can also calculate job lock elasticities for different subsamples, which allows us to make informative comparisons across groups because the elasticities adjust for differential take-up of dependent coverage across groups. Figure 3 plots the elasticities for each subgroup, and Appendix Figures A.7 and A.8 plot the separate effects in percent terms. Appendix Tables A.6, A.7, and A.8 report the coefficients.

First, we find evidence that parents eligible for early retirement benefits (i.e., older parents) who take up dependent coverage are more likely to be "job locked." Parents approaching

4. We calculate the number of affected parents, 9.7 million, using the SIPP and Census. We arrive at this number by calculating the share of adults aged 44-63 with children aged 19-25 in the 2008 wave of the SIPP, and then extrapolate using the total number of adults from the 2010 Census. The semi-elasticity of job retention with respect to dependent take-up (the percentage point change in job retention, 1.0, divided by percent change in take-up, 7.4) implied by our results is 0.14. Multiplying this by 30 implies that 4 percent of affected parents, or about 400,000, were "job locked."

retirement age may be more responsive to job retention incentives – they are more likely to be on the margin of exiting the labor force, and their outside option is less likely to offer insurance or coverage for dependents. We split parents by whether they are over or under 55, as individuals who retire at age 55 or older can withdraw from their 401(k) without penalty and thus it is a popular early retirement age. We find a job retention elasticity of 0.30 for parents over 55 compared to 0.12 for parents under 55; the duration elasticity is also somewhat higher for retirement-age parents than for younger parents. This implies that parents nearing retirement are more likely to face job lock induced by the dependent mandate.

Second, we hypothesize that parents who also provide coverage to their spouse or other children will be less responsive to a marginal change in an individual child's eligibility, as they may already be "job locked" by the other family members. We find that parents who cover their spouse or other children are more likely to take up dependent coverage, and the magnitude of the job retention effect is larger as well (Appendix Tables A.6 and A.7). However, once the two effects are scaled relative to each other, the elasticity of job retention with respect to take-up is smaller for parents who cover their spouse or other children versus those who do not (Figure 3). This example highlights the importance of scaling the labor supply effect by the take-up effect – comparing just the magnitudes of the labor supply effects alone would lead to the opposite conclusion. The magnitude of the job retention effect is larger for parents who cover a spouse or other dependents simply because they are more likely to take up coverage. But the job lock they face is actually *smaller* – that is, the ACA dependent mandate did not distort their labor supply decisions as much as it did for parents who were not covering other family members.

Third, we consider heterogeneity by a proxy for dependent health: whether we observe the dependent receiving inpatient care in the pre-ACA period. While it is difficult to assess how much a parent or dependent "values" the additional coverage with our data, a reasonable assumption would be that the value of coverage, and therefore the extent of job lock, should be greater for parents of dependents in worse health. We leverage the fact that we can observe

claims and utilization in the Marketscan data to identify dependents who had at least one inpatient stay from 2000 to 2009. Figure 3 shows that parents of children with prior inpatient care have higher job retention elasticities: the likelihood elasticity is 0.39 for these parents, compared to 0.18 for parents of children without prior inpatient care.

Finally, we consider differences across employees of firms that offer a greater share of "end of year" or "birth month" plans. We do not directly observe the type of plan that families are enrolled in in our data. Instead, we construct a proxy for the prevalence of "end of year" plans provided by each employer: the share of dependents born in January-March who we still observe as being enrolled *past* March of the year they turn 26. We expect that among "birth month" plans this share should be 0, and for "end of year" plans it should be close to 1. Most employers have a share that is far from both 0 and 1, which suggests that they offer a mix of plans with "birth month" and "end of year" policies (Appendix Figure A.9). We divide the sample into employers with an above-average and below-average share, where we expect that employers with an above-average share should have more dependents on "end of year" plans.

We find a larger increase in the duration of dependent enrollment and job retention in the above-average sample compared to the below-average sample. Parental job retention likelihood is relatively unresponsive to the birth month cutoff in the below-average share sample.[5] Combining the two together implies that the above-average sample is much more likely than the below-average sample to change their labor supply as a result of the additional dependent coverage – they have job retention likelihood elasticities of 0.42 and 0.09, respectively (Figure 3). This suggests that parents are more likely to stay at a job for dependent insurance if their child is eligible for a longer duration – January dependents get up to a year on "end of year" plans compared to one month on "birth month" plans. As for job duration, the elasticities

5. The magnitude of the discontinuity in dependent enrollment *likelihood* in the below-average sample is somewhat counterintuitive, as there should be a smaller discontinuity in eligibility months in "birth month" plans. While we cannot be sure, we hypothesize that this response is because parents with a December-born child are not prompted at the end of 2011 to enroll for the next year while those with a January-born child are.

are relatively close in magnitude – parents remain at their jobs for longer when dependent enrollment duration is longer. In other words, the value of *an additional month* of dependent coverage does not appear to vary across parents enrolled in these two types of plans.

6 Conclusion

In this paper, we study the effect of increased coverage for adult dependents under the Affordable Care Act on parental "job lock." While prior research provides evidence of job lock due to own coverage, less is known about the effects of dependent coverage, despite the fact that it is a widely provided benefit. We compare dependent insurance take-up and parental job retention outcomes in families with adult children who, depending on whether they were born in January vs. December, gained access to different amounts of insurance coverage on average.

Our dataset is a large panel of employer-sponsored insurance claims and enrollment records. By linking together parents and their adult children, we can observe both dependent coverage and a proxy for parental job retention. This novel linkage is key to understanding the extent to which insurance coverage for one family member distorts job mobility for others. Scaling the job retention effect by dependent coverage take-up allows us to assess the degree to which labor supply is distorted by job lock, both in the overall sample and across different subgroups.

Leveraging the discontinuous increase in months of dependent coverage eligibility at the January vs. December cut-off, we first show that adult dependents are more likely to take up coverage when they are eligible for more months, and they also remain enrolled for longer. We then find that parents of dependents eligible for more coverage are more likely to remain with their employer, and remain for a longer period of time.

We combine the reduced form estimates to calculate the elasticity of parental job retention with respect to dependent coverage take-up, and find an average elasticity of 0.20. There is evidence of substantial heterogeneity: parents nearing retirement age, those who do not also

cover their spouse's insurance, those with a dependent who is an only child, and those with a dependent in worse health all face more job lock from the additional dependent coverage. These scenarios correspond to cases in which a job exit would be more probable or dependent insurance is more valuable.

Our results suggest that the entire package of employer-sponsored health insurance, covering both employees and their family, plays a prominent role in determining labor supply. Thus, policies aimed at expanding dependent health insurance coverage, say through public insurance expansions or private insurance mandates, may have important within-family spillover effects on labor supply.