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DEPENDENT COVERAGE AND PARENTAL "JOB LOCK": EVIDENCE FROM THE AFFORDABLE CARE ACT

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ABSTRACT

Coverage for dependents is a common feature of employer-sponsored insurance. While prior work shows that employees trade off job mobility for their own coverage, there is less evidence on the intra-family spillovers of dependent coverage onto parental labor supply. We study this question using a panel of insurance claims that links de- pendent insurance enrollment with a proxy for parental job retention. We develop a regression discontinuity design that exploits variation in coverage eligibility by dependent birth date from the Affordable Care Act, and find that a one percent increase in the dependent enrollment likelihood leads to a 0.2 percent increase in parental job retention. Job lock induced by dependent coverage is greater for parents who were more likely to be on the margin of a job exit, for families who value dependent coverage more, and for employees of firms with a wider range of insurance options.

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1 Introduction

Nearly half of Americans rely on employer-sponsored health insurance for insurance coverage (Kaiser Family Foundation 2022). This tight 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).[1]. 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 – 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 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, like an employee's own coverage, dependent coverage is a form of compensation that should in theory increase the value of employment and induce job lock. On the other hand, it may have a more limited effect since dependents are younger and healthier or parents could already be "job locked" by their own coverage. Measuring the job lock effects of dependent coverage is of critical policy importance both because of the ubiquity of dependent coverage and because many policy reforms specifically target coverage for children, such as the Children's Health Insurance Program (CHIP).

One factor that has limited prior work on the intra-family spillovers of dependent coverage is a lack of data on both insurance take-up and employment outcomes that are linked across different family members. While this is collected in some surveys, these samples are often non-representative and relatively small. For example, the Survey of Income and Program Participation (SIPP) has a sample size of up to 50,000 households per panel, and only

1. Recent empirical work showing evidence of job lock from own coverage includes Boyle and Lahey (2010), Dague, DeLeire, and Leininger (2017), Dave et al. (2015), and Wettstein (2020).

collects insurance coverage information for adult dependents if they reside with their parents (Jun 2023).

We overcome these data limitations by developing a novel application of a dataset that has been widely used in other settings: employer-sponsored health insurance enrollment data. To study the job lock effects of dependent coverage, we leverage three key features of this dataset. First, we have a linkage between planholders and dependents across a large number of households, spanning many years. Second, we have an accurate measure of monthly dependent enrollment. Finally, we have a proxy measure of job retention for the parent: the number of months they retain coverage from any plan offered by their employer, including those from different insurers.[2] The insurance claims data also provide the sample size to support heterogeneity analyses on different subgroups, which is useful for exploring mechanisms. Furthermore, linking dependent and parental outcomes together allows us to scale the parental labor supply responses to dependent take-up. These scaled effects can then be used to calculate how much parental job lock might arise from other expansions of dependent coverage. Future work using these data may provide valuable insights into the intra-family spillovers between health, insurance, and employment outcomes.

We use these data to study the effects of a dependent coverage expansion that occurred as part of the Affordable Care Act (ACA). The so-called "dependent mandate" requires private insurers to extend coverage to adult children up to age 26, whereas previously dependent coverage was provided through age 19, or 23 for full-time students. Prior work on the mandate focused on the dependents themselves, and has found sizable 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; Barkowski, McLaughlin, and Ray 2020; Carpenter et al. 2021; Kim 2022) and documented various health and financial

2. We provide supporting evidence from survey data that this measure is a reliable proxy of job retention. In complementary work, Aouad (2023) uses claims data from one insurer to study intra-family spillovers from dependent coverage to parental "insurance lock" following a sudden dependent illness. Our data allow us to follow employees even if they switch insurers, resulting in a measure which is well-suited for measuring job lock.

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 on dependents and parents, 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, as noted by Slusky (2017). To validate our research design, we demonstrate that the distribution of dependents in our sample evolves smoothly through the RD cutoff, as well as a large of set of demographic and employer characteristics.

Our analysis sample includes dependents born between January 1985 and 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. We find that dependents to the right of the December 1985/January 1986 cutoff, who are eligible for more coverage, are more likely to enroll and are enrolled for a longer period of time once the mandate is in effect. Dependent enrollment increases by 1.8 percentage points at the birth date cutoff, an increase of 9.2 percent over the enrollment rate for dependents born in December 1985. In addition, the enrollment duration increases by 9.7 days (14.6 percent). Turning to their 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) to the right of the cutoff.

Combining these estimates with the effects on dependent coverage, we estimate a 1 percent increase in the share of dependents covered is associated with an increase in the parental job retention rate by 0.20 percent. For duration, a 1 percent increase in dependent coverage duration is associated with a 0.11 percent increase in parental job retention. Applying our results to the effect of the overall ACA dependent mandate, which was estimated to have

increased dependent coverage by 30 percent, implies that about 580,000 parents were "job locked" by the mandate (Akosa Antwi, Moriya, and Simon 2013).

Our main results are consistent with the increased insurance eligibility for adult dependents making parents' current jobs more valuable, leading to parental job lock. This response is particularly notable given that 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. However, dependent coverage would still reduce job mobility if insurance options vary across jobs or employment status. For example, families could face costly switches to different providers under their potential future firm's insurance network (Sabety 2023), insurance generosity and coverage may differ across firms, and outside options like Medicare do not offer access to the same level or type of coverage. Indeed, such factors have been long been established as drivers of job lock in the setting of own insurance coverage (Gruber and Madrian 1997; Wettstein 2020).

Our estimates remain similar under a variety of robustness checks, including dropping controls, excluding weights, clustering on the running variable, using alternate bandwidths, and replacing our linear control function with a local linear specification. We also assess potential threats to our identification assumption that factors other than coverage eligibility do not change at the discontinuity by conducting placebo analyses using cohorts that were either too old or too young to be eligible for the dependent mandate. Reassuringly, we find no effects on dependent enrollment or parental job retention in the placebo cohorts.

We then conduct heterogeneity analyses to explore the mechanisms driving parental job lock. 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. Job lock is also higher for parents who likely value coverage more: those with dependents with prior inpatient care and those who were on fee-for-service (FFS) plans pre-reform, which are typically more expensive. Finally, we find greater job lock in firms that offer a greater diversity of plan types, which suggests that families also

value having option value in plan choice. Taken together, these results demonstrate that job lock is stronger among parents who are more likely to be on the margin of a job exit, parents who may value their coverage more, and parents in firms with a wider ranger of insurance options.

2 Policy Context

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).[3] 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 had laws that extended coverage past age 23 for certain categories of dependents (e.g., full-time students or those claimed as dependents on their parents' tax returns). The state mandates were more limited in scope because they did not apply to self-insured plans, which cover more than half of private sector workers with employer-sponsored health insurance (Levine, McKnight, and Heep 2011; Monheit, DeLia, and Belloff 2011; Akosa Antwi, Moriya, and Simon 2013).

The ACA mandate applied to all insurance plans after September 23, 2010. Dependents must be born on or after January 1985, and therefore turn 26 on or after January 2011, to receive additional coverage under the ACA mandate. Plans could not 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 mandate was a highly salient and largely popular component of the ACA: over 70 percent of the public was aware of the dependent mandate within a month of enactment (Kaiser Family Foundation 2010). The other major provisions of the ACA, including the establishment of healthcare exchanges and the coverage mandates for mid and large-sized firms, were implemented later in 2013 and 2014. As our data end in 2012, these policies should not be a source of confounding in our

3. 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).

analyses.[4]

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. 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." [5] We refer to these plans as "birth month" vs. "end of year" plans, respectively. 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, as discussed in Section 4.2.

The number of additional months of coverage implied by the ACA dependent mandate depended on the beneficiary's plan type and their birth month, as illustrated in gray in Figure 1a. For dependents in birth month plans, the number of additional months increases linearly in birth month. For example, individuals born December 1985 are eligible for 12 months of coverage as they would lose coverage when they turn 26 in December 2011, whereas individuals born January 1986 are eligible for 13 months of coverage as they would lose coverage in January 2012.

In contrast, for those on end of year plans, the number of additional months is the same within a birth year cohort and then 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 one month earlier in December 1985 are eligible for only 12 months of coverage. These dependents should be otherwise similar, which motivates our use of a regression discontinuity design by birth

4. For a full timeline of the implementation of ACA provisions, see: https://www.ncbi.nlm.nih.gov/books/NBK241401/.

5. Source: https://www.healthinsurance.org/faqs/under-the-aca-can-young-adults-still-remain-on-their-parents-health-pl
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/).

month.

With both plan types in the sample, we would expect the discontinuity at January 1986 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. The blue points in 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

3.1 Data Description

Our main source of data is the Truven Health MarketScan CCE Database ("MarketScan Data"), a large panel of employer-sponsored health insurance claims. The data combine detailed information on individual claims, monthly enrollment records, and basic demographic information. The data cover 2000 to 2012 and include 143,969,922 enrollees, of which 69,227,012 are planholders (i.e., the employee) and 74,742,910 are dependents (i.e., their spouse and children). The data include employees between the age of 18 and 64. While the sample disproportionately covers the South, it has wide geographic coverage (Baker et al. 2014; Blewett et al. 2018).

The data were provided to MarketScan by 246 large employers and health insurers ("data contributors"). Most of these employers are Fortune 500 firms, and medium and small firms are relatively underrepresented in the data (Adamson, Chang, and Hansen 2008). We limit our sample to data provided by employers (212 out of the 246 data contributors). 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 plans 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 information on job retention and tenure.

Our sample is a monthly panel of enrollees — each observation represents an enrollee and enrollment month. 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 dependents (spouses and children). Note that we can only track dependents while they remain covered by the same employee. For example, if a child disenrolls from one parent's plan and re-enrolls on another parent's plan, we would not be able to follow them.

We impose several additional sample restrictions. First, we limit the sample to plans that include at most one dependent born between January 1985 to December 1986. Second, to ensure that the relationship between the planholder and dependent is that of a parent-child, we require at least a 16-year age gap between the two. Third, we limit the sample to plans with planholders who are under 65 throughout the sample period, or those born after 1947. As our data do not include employees older than 65, we might otherwise confuse exits from the data with exits from one's employer. Fourth, we require that the planholder and dependent are first observed in the data prior to 2010 (the "pre-period").[6] This step ensures that we avoid endogenous selection into the sample due to enrollment incentives created by the dependent mandate. Fifth, we require that dependents are enrolled for at least one month in the pre-ACA period while younger than 23, to avoid any issues of selection due to the pre-existing state-level mandates that provided coverage beyond 23. In robustness exercises, we show that requiring that dependents are observed under the age of 19, rather than 23, does not alter our main findings, although it does reduce sample size (and, as a result, the power to examine heterogeneous treatment effects). We also require that time-varying control variables (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.

We then limit the sample to the subset of data contributors that participate continuously from 2008-2012. New data contributors are added to the MarketScan sample each year in January, as shown in Appendix Figure A.2. Thus, this step ensures that we avoid selection

6. Although the ACA mandate was officially implemented in 2011, some plans elected to start providing coverage earlier in 2010 to graduating college students, to avoid a summer coverage gap. While our sample cohorts are generally too old to be in college in 2010 (as they are 24-25), we exclude all data from 2010 from our analysis for this reason.

into the sample by dependent birth date that could arise as a result.[7]

The key independent variable in our analyses is dependent birth month. 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. In order to ensure we observe birth month for each dependent, it is necessary to limit the sample to plans in which dependents are enrolled for at least 12 months continuously in the pre-period. Imposing this final sample restriction leaves us with an analysis sample of 393,791 planholder-dependent pairs. Henceforth, we refer to the planholder as the "parent."

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.

It is important to consider what we can measure with regard to dependent coverage. Because we require that all dependents are covered by their parent's plan in the pre-ACA period, our measure of "any enrollment" is in fact an indicator for whether the dependent is still enrolled (or re-enrolled) on any insurance plan provided by their parent's pre-mandate employer. Thus, we do not count adult dependents who enroll in their parent's plan as a result of the ACA mandate but who were not previously covered by the same parent. In addition, we cannot observe coverage provided by that parent if they move to a different employer after 2010. Similarly, we do not observe coverage provided through other sources,

7. Appendix Table A.1 lists, for each birth cohort in our sample (January 1985-December 1986), the range of enrollment months during which we could conceivably observe them enrolled on their parent's plan while under the age of 23. The range starts in January 2000 because that is the first month of our MarketScan sample. Our goal is to avoid differential selection into the sample between December and January birth months. Adding new data contributors in January of each calendar year would result in new sets of dependents with January birth months (as compared to December birth months). Imposing this initial enrollment age restriction limits the sample to plan holders whose data contributors continuously participate in MarketScan from 2008 to 2012.

such as the parent's spouse or the adult dependent's employer.

Summary Statistics Table 1 presents summary statistics for our analysis sample, where each observation reflects a parent-child pair. We report means of our outcome variables and control variables for both the full sample (Column 1) as well as by dependent birth year (Columns 2-3). Of the 393,791 parents in our sample, 46 percent have dependent children born in 1985 and 54 percent have dependent children born in 1986.

Comparing dependents in the 1985 and 1986 birth cohorts, the share enrolled for at least one month during 2011-2012 increases from 0.14 to 0.26, or 86 percent. Similarly, there is a large increase in the total number of coverage days during 2011-2012, from 35.91 to 127.70, or 256 percent. This difference between the two cohorts reflects 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 2011 and 2012.

As 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 2012 (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 initial evidence in favor of the "job lock" hypothesis.

Table 1 also reports means of our control variables for the 1985 and 1986 birth cohorts. All time-varying controls are measured with respect to the pre-period, before 2010. There is little difference across these cohorts in the following: female dependent (50 percent), female parent (40 percent), whether a spouse was added to the plan prior to 2010 (78 to 79 percent), number of dependent children added to the plan prior to 2010 (2.3 to 2.4 percent), and whether the dependent received inpatient care prior to 2010 (0.08 to 0.07 percent). As for parental birth month, 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 point emphasizes the importance of controlling for parental age in our

analyses.

The last set of control variables measure the generosity and flexibility of the parent's pre-period insurance coverage options. The construction of these variables is described in Appendix A. The first is an indicator for whether the parent's pre-period plan is a health maintenance organization ("HMO"), which tend to be less expensive but more restrictive than fee for service (FFS) plans. The second variable is an indicator for whether the parent's pre-period employer offers both HMO and FFS plans. This measure is meant to capture the diversity of plan options offered by an employer, which should increase the option value of staying with that firm. There is no difference in the means of these measures across dependent birth cohorts: 0.23 for an employee's own pre-period HMO coverage, and 0.74 to 0.75 for being at a firm that offers both types of plans.

3.2 Insurance Dis-enrollment as a Proxy for Job Exit

In this subsection, we provide evidence on the validity of our measure of parental job retention. We proxy for job retention in the insurance enrollment data using an indicator for 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 degree of measurement error in our proxy, 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 B 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.

One additional potential concern with this proxy for parental job retention is related to the way it interacts with our sample restrictions. We restrict our sample to families with a dependent who we observe having coverage before the month in which they turn 23. This sample restriction requires families of older cohorts to stay with their employer for longer than families of younger cohorts – we require that dependents born in a given month in 1985 be observed before that same month in 2009, and, likewise, those born in a given month in 1986 to be observed before the same month in 2010. Through job churn and attrition over time, we should expect our job retention measure to be lower among older cohorts, leading to a mechanical difference between different cohorts that is unrelated to the mandate. This should not, however, generate a discontinuity between December 1985 and January 1986. Nonetheless, we can address this potential confounder in several ways. First, we include a linear birth month control to account for any changes which are linear in birth month. Second, we repeat our sample restriction and analysis with a placebo cohort: dependents who were too old for the mandate to be relevant in 2011. We expect that attrition and job churn patterns to be similar among these parents, so if the estimates are due to the sample restriction, they should appear in these as well. We should only find a discontinuity among cohorts for whom the mandate is relevant.

4 When do Dependents Exit Parental Coverage?

In this section, we examine the age and timing of dependent exit from parental coverage. Looking at these patterns provides further evidence that the dependent mandate shifted patterns of coverage across birth cohorts, and also confirms the existence of end of year and birth month plans in our sample.

4.1 Effect of the Dependent Mandate on Age of Disenrollment

Appendix Figure A.1 plots the age at which dependents exit coverage (under their parents) in the post-ACA period (2011-2012). In particular, for each dependent we calculate their age

in months when they last appear on their parent's plan ("exit age"). For the 1983 and 1984 birth cohorts, who were too old to qualify for the dependent mandate, the most common exit age is 23 years and 1 month (15.3 percent and 11.7 percent of the cohorts, respectively). Since full-time students could stay enrolled until they turned 23 under the pre-ACA mandates, this pattern suggests dependents in our sample tend to attend college. Virtually no exits occur in the 26th birthday month (or afterwards). Smaller exit spikes appear in the 24th and 25th birthday months, reflecting state-sponsored mandates that extended coverage through these ages.

In contrast, the distributions for the 1985 and 1986 cohorts, who were younger than 26 when the mandate passed, are consistent with the policy increasing parental coverage. A spike emerges at exactly the 26th birthday month, and for the later cohort it becomes by far the most common exit month.[8]

4.2 Evidence of Birth Month and end of year Plans

As discussed in Section 2, the number of additional months implied by the mandate depends on a dependent's birth date as well as whether they were on a birth month or end of year plan. For birth month plans, which cover a dependent until the month they turn 26, the number of additional months is linear in birth month. For end of year plans, which cover a dependent until the end of the year they turn 26, there is a discontinuity of 12 additional months of coverage between dependents who turn 26 in 2011 or 2012. Thus in order to estimate our empirical design, we need evidence that there are dependents in our sample on end of year plans.

While there is qualitative evidence from insurer manuals and policy documents that both of these plan types exist, we cannot directly observe this plan characteristic in our data.[9]

8. The spike at the 23rd birth month for the 1987 cohort reflects dependents who exited after college prior to the ACA (and never subsequently re-enrolled).

9. Furthermore, we cannot infer plan type from an enrollee's plan or their employer. The former is because the plan identifier is missing for a substantial portion of our sample. The latter is because it appears from looking at each individual firm's exit month distribution that firms in our sample employ a mix of birth month and end of year plan types.

However, we can use the timing of exits from parental coverage to provide evidence on the prevalence of end of year plans in the aggregate. While dependents on birth month plans must exit on or before their birth month, dependents on end of year plans can remain enrolled until December. Figure 1b plots the distribution of exit months for dependents *not* born in December who disenroll in the year they turn 26 in months other than their birth month. Over a quarter of these dependents disenroll in December, consistent with a sizable share of end of year plans. Figures 1c and A.1 also provide evidence of birth month plans, as the spikes in the distributions show that many dependents exit at exactly 19, 23, and 26.

5 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 the 1985 and 1986 cohorts around this particular cutoff because our study period of 2011-2012 includes all of their new months of coverage eligibility. Older cohorts did not qualify for coverage under the mandate, whereas younger cohorts turn 26 after our sample ends.

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 cutoff value ($c =$ December 1985). We define the outcome variable, Y_{ij}, as a measure of either dependent enrollment or parental job retention. Then, we model Y_{ij} as follows:

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

where $f(\cdot)$ is a control function based on dependent birth date. In our baseline regressions, $f(\cdot)$ 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 coverage 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 to

vary on either side of the cutoff c and thus should account for any linear trends by birth month that could arise from our sample restriction, as discussed in Section 3.2. X_{ij} is a set of controls: gender of the parent and dependent; parental birth date (year-month); number of dependents added to the parent's plan before 2010 (the pre-period); whether a spouse was ever added to the plan in the pre-period; whether the dependent ever received inpatient care in the pre-period; whether the parent's pre-period plan was an HMO; and whether the employer offered both HMO and FFS plans to their employees during the pre-period. We weight each observation using triangular weights, which decrease linearly in distance from the cutoff month and cluster standard errors at the individual-level.

The coefficient of interest is β, which measures the effect of additional dependent coverage eligibility on dependent enrollment and parental job retention outcomes in 2011-2012. A positive β on dependent enrollment would indicate that dependents to the right of the cutoff 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 cutoff 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 $f(\cdot)$ to be a local linear function, alternative bandwidth choices, excluding the control variables X_{ij}, and clustering standard errors by the running variable.

Lastly, we perform placebo tests by re-estimating Eq. 1 using two alternative cutoff dates: December 1983, for dependents too old to be eligible for additional coverage under the mandate, and December 1995, for dependents who were too young to be affected during our study period of 2011-2012.

Tests of Identification Assumptions Our RD design estimates causal effects by identifying treatment and control groups that are eligible for different amounts of dependent coverage, but are otherwise comparable. In our case, the treatment group consists of families with dependents born right after the start of 1986, while the control group consists of families

with dependents born right before. The identification assumption is that absent the effects of the dependent mandate, our outcomes would evolve smoothly around the end of year cutoff in dependent birth date. Two common ways to test this assumption are to evaluate whether the density of the running variable is smooth through the cutoff value and to test whether observable characteristics evolve smoothly through the cutoff.

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 cutoff, and also probes for any 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.[10] 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.

We assess this by first examining the smoothness of the distribution of dependent birth month around the cutoff. 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 the cutoff – the discontinuity estimate is -0.01803 with a p-value of 0.17.

Next, we examine whether the observable characteristics of dependents, parents, and employers evolve smoothly through the cutoff. For observable characteristics, we use the 8 control variables shown in Table 1: gender of the parent and dependent; birth date of the parent; spousal and dependent coverage in the pre-period; whether the dependent received any pre-period inpatient care; whether the parent was enrolled in an HMO in the pre-period; and whether the parent's pre-period employer offered both HMO and FFS plans.

Appendix Figure A.4 plots the unadjusted means of these variables by dependent birth

10. Note that this particular scenario seems unlikely in our sample because we define birth month based on enrollment data collected prior to the ACA dependent mandate – thus, parents would have to anticipate the reform years in advance.

month. Visually, these graphs appear quite smooth through the birth date cutoffs. All are relatively flat except for parent's birth date, which is linearly increasing. This reflects the fact that younger children will tend to have younger parents.

We formally test for discontinuities in these characteristics by re-estimating our RD specification (Eq. 1) with the outcome variable Y_{ij} equal to the indicated control variable and omitting the vector of control variables. Estimates of β are reported in Table 2. The magnitudes of the 8 estimates are uniformly small and statistically insignificant at conventional levels. Thus, the combination of results in Appendix Figure A.4 and Table 2 provide support for the validity of our causal design.

6 Results

6.1 Main Results

We first estimate the effects of additional months of dependent coverage on dependent enrollment and parental job retention. For each of our outcomes, we present graphical evidence ("RD graphs") as well as estimates of β from Eq. 1. The RD graphs plot residualized means of our outcome variables that are adjusted 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). Because parental job retention decreases in parental age, the raw trend in parental job retention slopes upward in a way that is unrelated to variation in dependent coverage eligibility.

Figures 2a-2b display RD graphs for dependent enrollment likelihood and duration during 2011-2012. On each section of the graph, we include a linear fit line. In column (1) of Table 3, we report corresponding estimates of β along with their standard errors. We also report the mean of the outcome variables for dependents in the December 1985 (control) cohort, which we use to convert our estimates into percent changes.

We hypothesize that expanded dependent coverage should increase the likelihood a dependent is enrolled on their parent's plans as well as the duration of their enrollment. Accord-

ingly, Figures 2a-2b reveal a discontinuous jump in both enrollment likelihood and duration for dependents at the birth date cutoff. The corresponding regression estimates, along with standard errors, are reported in Table 3. Enrollment likelihood increases 1.8 percentage points (9.2 percent of the December 1985 mean) and the duration of enrollment increases by 9.7 days at the cutoff (14.6 percent). Each of these estimates is statistically significant at the 1 percent level.

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. Table 3 reports that the likelihood a parent retains their job increases by 1.0 percentage points (1.8 percent). Correspondingly, our measure of job duration increases by 5.8 days (1.6 percent). These estimates are statistically significant at the 1% level and 5% level, respectively.

6.2 Robustness and Placebo Checks

We next investigate the robustness of our results to changes in our specification and sample. First, we re-estimate our main effects on dependent enrollment and parental job retention, making the following changes one-by-one: excluding controls; excluding regression weights; clustering the standard errors at the level of birth month, the running variable; employing different bandwidths around the cutoff months; and replacing our linear control function with a local linear specification. The results are reported in Appendix Table A.7, which includes the baseline results in Column (1) for comparison. Reassuringly, there is very little change in the magnitude or precision of our estimates across the columns.

As an additional robustness check, we restrict our sample of dependents to those who are enrolled in the pre-period while under the age of 19 rather than 23 (70% of dependents in our current sample). Recall that prior to the ACA mandate, dependent coverage was provided to all dependents through age 19, whereas only students could remain covered through age 23. While our analysis suggests that dependents in our data tended to exit on their 23rd birthday in the pre-period (indicating a high share of college attendance), adding this requirement

provides a check that there are no confounding policies at the December/January cutoffs for ages 20-22.

A drawback of this approach is that limiting the sample to dependents observed in the pre-ACA period while under age 19 requires us to restrict the set of data contributors to those that continually provided data from from 2004 to 2012 (rather than 2008-2012). This step cuts our sample size from 393,791 to 266,855, as many data contributors joined after 2004 (see Appendix Figure A.2). The reduced sample size limits our power to estimate heterogeneous treatment effects by parent, dependent, and employer characteristics. Column (7) in Appendix Table A.7 re-creates our main results using the subset of dependents first observed prior to 19. Reassuringly, the point estimates are very similar (and in percent terms are nearly identical). In addition, each estimate is statistically significant at the 5 percent level.

Next, we conduct placebo exercises with different cohorts of dependents who were either too old or too young to be affected by the mandate. First, we construct a sample of dependents born from January 1983 to December 1984 and set the cutoff value to be $c =$ December 1983. 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. This placebo test provides further evidence that there are no other factors besides dependent coverage eligibility that change discontinuously at the December vs. January cutoff, either due to non-linearities in characteristics by birth month or due to our sample selection criteria.

Appendix Figure A.7 display the RD graphs for $c =$ December 1983 and Appendix Table A.8 reports the corresponding estimates and standard errors. The graphs appear smooth through the cutoffs and the estimated coefficients are small and imprecise.

Second, we construct a sample of dependents born from January 1995 to December 1996 and set the cutoff value to be $c =$December 1995. We restrict the sample to dependents with parents in the same birth cohorts as the main sample. The dependents in this sample are 10 years younger than those in our main sample and are covered under pre-existing, nationwide

mandates in 2011-2012 (when they were 16-17). Thus, we again expect to find no changes in dependent enrollment or parental job retention at these placebo cutoffs.

Appendix Figure A.8 display the RD graphs and Appendix Table A.9 reports the corresponding estimates and standard errors. We find no evidence of discontinuous changes at the cutoffs, despite a much higher level of dependent enrollment than in the previous placebo sample. In sum, our placebo tests using alternative birth cohorts of dependents provide strong support for the validity of our empirical design and our interpretations of our findings.

Effects by Year We then consider how our results vary by outcome year (2011 vs. 2012). Appendix Figure A.5 and Appendix Table A.3 present results for dependent enrollment. Appendix Figure A.6 and Appendix Table A.3 present corresponding results for parental labor supply by year. Following our main results, we see discontinuous jumps in both dependent enrollment and parental job retention at the RD cutoff. As parents must actively decide whether to re-enroll their dependents in 2011, those eligible for a longer duration of coverage (i.e., with dependents born after January 1986) should be more likely to enroll and have longer enrollment periods.

Importantly, we can use the year-specific enrollment patterns to confirm that dependent enrollment behavior aligns with the variation expected from the coverage expansion across the two years. 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. Variation in enrollment by birth month here could be due to factors like the incidence of finding a job or getting married; if the likelihood of these events increase by age then this will lead to an upward sloping pattern. In the year dependents turn 26 (i.e., 2011 for the 1985 cohort and 2012 for the 1986 cohort), enrollment should increase more steeply in birth month, as "birth month" plans will terminate coverage in the birth month. In the year dependents turn 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.

6.3 Scaling the Job Lock Response by the Change in Dependent Coverage

A unique advantage of our setting and data is that we can observe both parental and dependent outcomes. This allows us to scale the change in parental job retention to the change in dependent coverage in the sample. In particular we convert the effects on dependent coverage and parental job retention in Table 3 to percent changes relative to the average for the December 1985 cohort and then calculate the ratio between the percent change in job retention and insurance take-up.

While this scaled measure is not computed at the individual (i.e., family) level, it nonetheless is informative about the value of additional coverage to parents overall. A large ratio between the parental job retention and dependent coverage responses suggests that, among parents who took up coverage, many were induced to stay at their job to do so. In other words, these parents valued dependent coverage enough to not only incur the cost of re-enrolling, but also the cost of remaining at a job they would have otherwise left. In contrast, a smaller ratio indicates that while parents valued the coverage enough to incur the cost to take it up, most were not willing to meaningfully change their labor supply.

For the full sample, the ratio of the percent change in job retention likelihood with respect to the percent change in dependent coverage likelihood (henceforth, the "job retention likelihood ratio") is 0.20, and the ratio of the percent change in job duration to the percent change in dependent coverage duration (henceforth, the "job retention duration ratio") is 0.11.

We can also use these ratios to make more informative comparisons of labor supply responses across groups, since the ratios adjust for differential take-up of dependent coverage across groups. In Section 6.4 we conduct this heterogeneity analysis to explore the mechanisms driving the parental job lock response.

Furthermore, we can use our estimates to extrapolate the parental job retention effects of policies that change dependent coverage. Since the ACA dependent mandate was estimated

to increase coverage by 30 percent, we can combine this with our job retention likelihood ratio of 0.2 to back out that 580,000 parents were "job locked" by the mandate (Akosa Antwi, Moriya, and Simon 2013).[11]

6.4 Heterogeneity Analysis and Mechanisms

Next, we compare our scaled estimates for different subsets of the data by characteristics of parents, dependents, and plans in order shed light on the mechanisms behind our results. In a simple model of job-to-job transitions, a reform that applies equally across homogeneous employers would not be expected to affect the likelihood of job transitions, and thus the dependent mandate should not result in job lock. However, we might expect job lock to arise if some employees are choosing between staying in the labor force or exiting (e.g., retiring), if there is heterogeneity in the types of plans employers offer and the extent to which employees value them, or if there are frictions associated with leaving one's health insurance plan and re-establishing care under a new plan.

We next discuss and test for these mechanisms by conducting heterogeneity analyses on subgroups. Figure 3 plots the retention ratios for each subgroup, and Appendix Figures A.9 and A.10 plot the separate effects in percent terms. Appendix Tables A.4, A.5, and A.6 report the corresponding coefficients and standard errors.

First, we look for evidence that parents nearing retirement are more likely to be "job locked" by the mandate. Parents approaching retirement age may have a larger job lock response for two reasons. They are more likely to be on the margin of exiting the labor force, and thus may be more responsive to to job retention incentives. Additionally, their outside option is less likely to offer insurance, and even if they can insure themselves through retiree insurance or Medicare, they will not be able to obtain coverage for their dependents. We split parents by whether they are over or under 55, as individuals who retire at age 55 or older

11. Using the SIPP and Census, we calculate that 9.7 million parents were affected by the dependent mandate. 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 extrapolating using the total number of adults from the 2010 Census. The percentage point change in job retention, 1.8, divided by percent change in take-up, 9.0, is 0.20. Multiplying this by 30 implies that 6 percent of affected parents, or about 580,000, were "job locked."

can withdraw from their 401(k) without penalty, thus making it a popular early retirement age.[12]

Dependents of parents eligible for early retirement are less likely to take up coverage, but the job retention effect is larger for these parents. This translates to a job retention ratio of 0.30 for parents over 55 compared to 0.13 for parents under 55; the job duration ratio is also somewhat higher for retirement-age parents than for younger parents. This implies that the dependent mandate is more likely to induce job lock among parents nearing retirement age.

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. These parents would also face a greater cost of exiting to non-employment or re-establishing care at a new job.

We find that parents who cover their spouse or other children are more likely to take up dependent coverage, and, correspondingly, the magnitude of the job retention effect is larger as well. However, once the two effects are scaled relative to each other, job retention ratios are 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.

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. Thus, we consider heterogeneity by a proxy for poor dependent health: whether we observe

12. Indeed, we find that in our sample 55 is the first age at which a significant share of employees leave their jobs.

the dependent receiving inpatient care in the pre-ACA period. 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 ratios: the ratio for likelihood is 0.40 for these parents, compared to 0.19 for parents of children without prior inpatient care.

We would expect that families with access to more "valuable" employer-sponsored insurance would be more likely to be job locked. An employee may value their employer-sponsored insurance because of the generosity of the coverage or the flexibility in provider or plan choice. To measure generosity, we consider whether a family is on a HMO plan or a FFS plan before the ACA. HMO plans limit coverage to doctors within their network, and typically have limited or no coverage out of network. In contrast, fee-for-service plans, such as preferred provider organizations (PPO), are less restrictive. While dependent coverage take-up is similar across the two types of plans, we only find evidence of parental job lock among families previously enrolled in a FFS plan (Table A.4). Thus, the job retention ratios are higher for families who were previously on FFS plans (Figure 3).

One potential concern with looking at each individual family's plan type is that plans also differ in their premia and cost-sharing, which we cannot directly observe. This motivates our third, employer-level measure: the number of plans offered by the parent's employer (i.e., contributor). We expect that employees value having more choice in their insurance plan, and thus expect job lock to be stronger when their firm offers them more options. We split by employers by whether they offer only FFS or both HMO and FFS.[13] We find that dependent coverage take-up is higher when there are more plans available (Table A.6), perhaps because families are more likely to have access to a plan which fits their needs. Correspondingly, the parental labor supply response is stronger among employees of these firms as well. Taken together, the heterogeneity analyses on patient-level prior utilization, plan-level generosity, and plan-level as well as firm-level flexibility are consistent with job lock arising when families

13. In our sample, all employers offer at least one type of FFS plan.

value the insurance at their current employer more.

7 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 linkage is key to understanding the extent to which insurance coverage for one family member distorts job mobility for others.

Leveraging the discontinuous increase in months of dependent coverage eligibility at the January vs. December cutoff, 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 these reduced form estimates to find that a 1 percent increase in dependent coverage take-up is associated with a 0.20 percent increase in parental job retention. 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, those with a dependent in worse health, and those whose employers provide more comprehensive insurance offerings 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.

Still, there are some important caveats to extrapolating from our results to other settings. First, the firms and employees in the Marketscan data are not nationally representative — they are more likely to be large employers and disproportionately located in the South. Second, the ACA dependent mandate was highly salient so our results may be an overestimate. On the other hand, our sample consists of relatively older parents and dependents, so it may be an underestimate of job lock responses for families with younger dependents who are eligible for more years of insurance coverage.

Our findings suggest that the entire package of employer-sponsored health insurance, covering both employees and their families, contribute to labor supply decisions. Thus, policies aimed at expanding dependent health insurance coverage, say through public insurance expansions or private insurance mandates, have important within-family spillover effects on labor supply.

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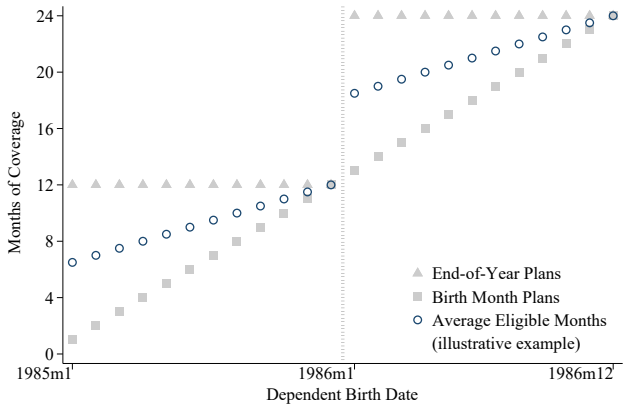
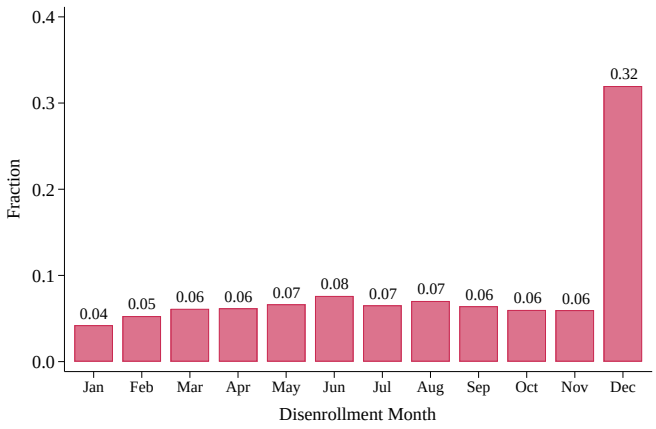
Figure 1: Variation in Additional Months of Coverage

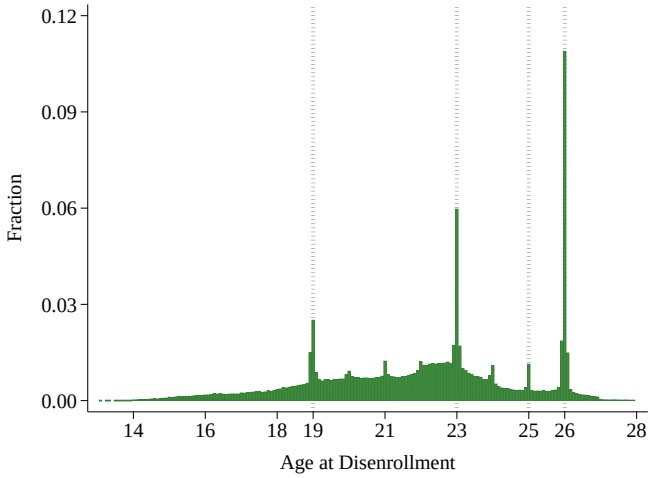
(a) Potential Additional Coverage, by Plan Type



(b) Exit Timing for Dependents Who Disenroll at Age 26

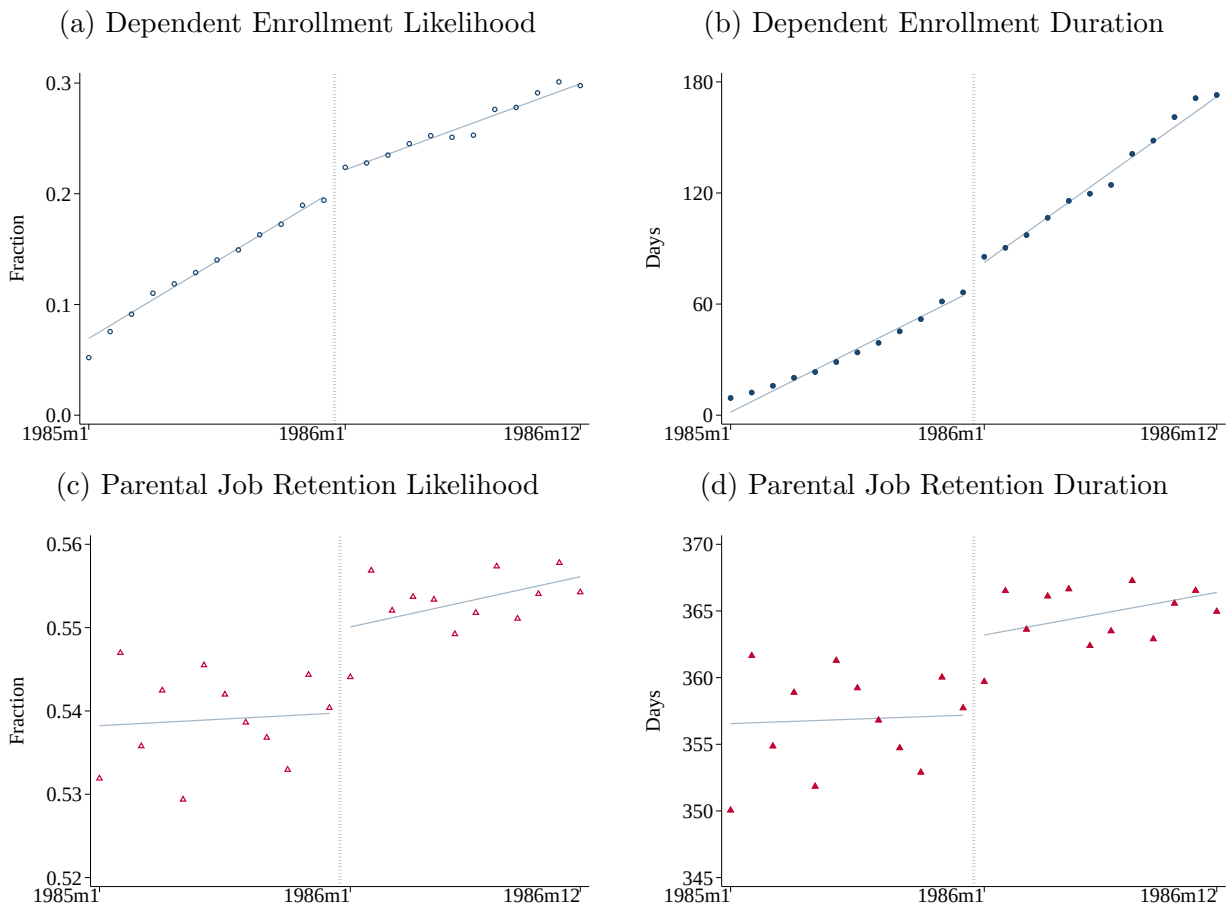


(c) Dependent Age in Months at Exit



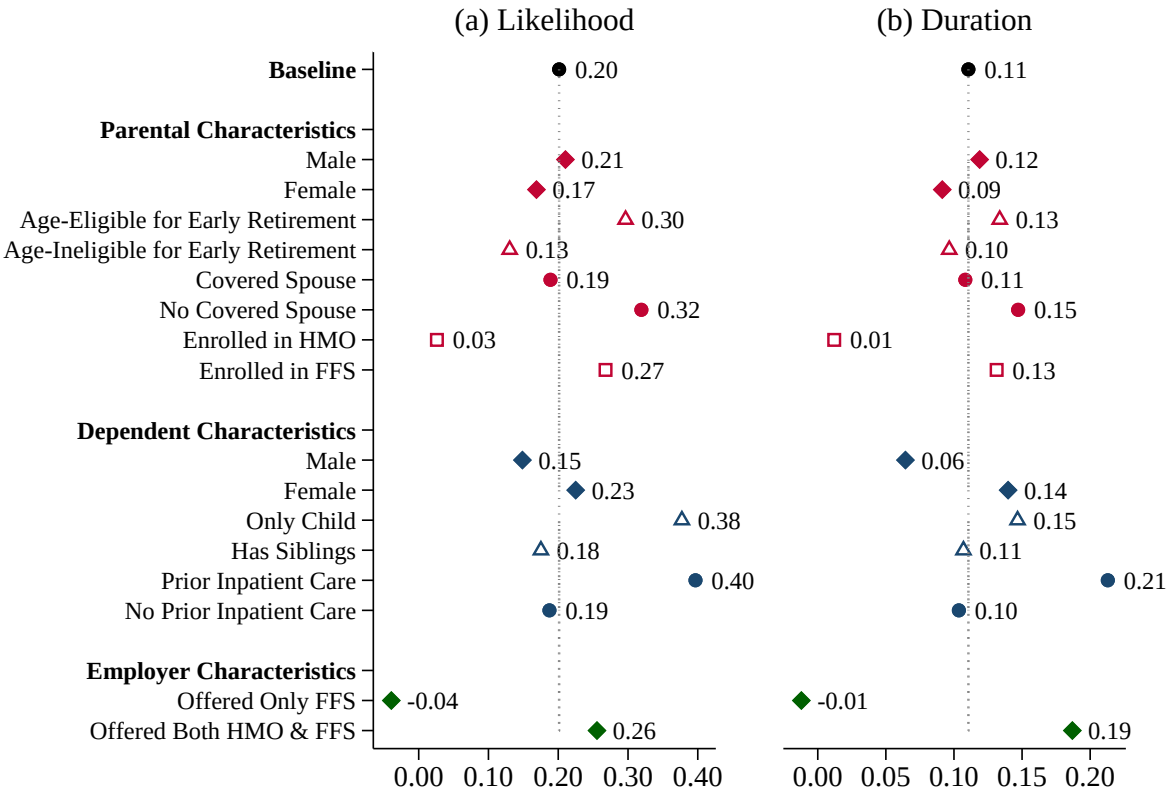
Notes: Subfigure 1a shows the number of months of dependent coverage that cohorts born from January 1985-December 1986 became eligible for under the dependent mandate of the Affordable Care Act. "Birth Month Plans" are those that provide coverage through the month in which the dependent turns 26. "End of Year Plans" are those that provide coverage through December of the year in which the dependent turns 26. The "average eligible months" is constructed under the hypothetical assumption that half of dependents are on "Birth Month Plans" and half are on "End of Year Plans." The vertical line at December 1985 corresponds to the cutoff value used in our regression discontinuity design. We assume that dependents are not eligible for other sources of coverage past age 23 and that plan years start on January 1, as is the case for all plan years in our data. Subfigure 1b displays the share of exits by calendar month for the subset of dependents born in 1985 and 1986 who exit during their 26th year (i.e., post-ACA) but *not* in their birthday month. The sample used to create this figure includes dependents from the 1985 and 1986 birth cohort who (1) are not born in December, (2) disenroll from their parent's plan at age 26, and (3) disenroll in a month other than their birth month. Subfigure 1c displays the distribution of dependents' age in months when they disenroll from coverage provided by their parents' pre-ACA employer. If dependents dis-enroll multiple times, we consider only the last disenrollment. See the notes to Table 1 for more information on the data source and sample construction.

Figure 2: Effects of Dependent Coverage on Enrollment and Parental Job Retention



Notes: This figure displays regression-adjusted means of the dependent enrollment and parental job retention outcomes by dependent birth date. The outcome variable in Figure 2a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure 2b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure 2c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure 2d, the outcome is total days of employment with that employer during 2011-2012. To calculate the regression-adjusted means, we regress these outcomes on our control variables (X_{ij} from Eq. 1), and then calculate the residual means by birth month. See the notes to Table 1 for more information on the data source, sample construction, and variable descriptions.

Figure 3: Ratio of Parental Job Retention Response to Dependent Enrollment Response



Notes: The figures above display our estimates of the ratio between the change in parental job retention and the change in dependent enrollment take-up. In particular, the left panel (a) depicts the percent change in parental job retention likelihood associated with a 1 percent increase in dependent enrollment likelihood. The right panel (b) depicts the percent change in parental job retention duration associated with a 1 percent increase in dependent enrollment duration. We report estimates for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. All characteristics are measured prior to 2010, in the pre-ACA period. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Table 1: Summary Statistics

	(1)	(2)	(3)
	Full Sample	By Dependent Birth Cohort	
		1985	1986
1) Dependent Enrollment, 2011-2012			
Likelihood	0.20	0.14	0.26
Duration (days)	85.40	35.91	127.70
2) Parental Job Retention, 2011-2012			
Likelihood	0.55	0.54	0.56
Duration (days)	361.02	354.30	366.77
3) Parental Characteristics			
Female	0.40	0.40	0.40
Parent's Birth Date	9/1957	4/1957	2/1958
Spousal Coverage	0.78	0.79	0.78
Enrolled in HMO	0.23	0.23	0.23
4) Dependent Characteristics			
Female	0.50	0.50	0.50
Number of Dependents	2.34	2.33	2.35
Prior Inpatient Care	0.07	0.08	0.07
5) Employer Characteristics			
Offer Both HMO and FFS	0.74	0.74	0.75
Observations	393791	181470	212321

Notes: The data source is the Truven Health MarketScan Commercial Claims and Encounters Database, a large panel of employer-sponsored health insurance claims and enrollment records. Our sample spans 2000-2012 and is restricted to a subset of employers that continuously provided data to MarketScan from 2008 to 2012. Each observation represents a dependent-parent pair. To be included in the sample, dependents must: (1) be born from January 1985 to December 1986; (2) be covered on their parent's plan for at least 12 months prior to 2010 (i.e., the "pre-ACA period"); and (3) be covered on their parent's plan while under the age of 23 in the pre-ACA period. Panels 1 and 2 provide summary statistics for our main outcome variables. "Dependent Enrollment" refers to coverage provided by the parent's pre-ACA employer. "Likelihood" indicates that the dependent was covered for at least one month during 2011-2012 ("post-ACA period"). "Duration" measures the total days of coverage in the post-ACA period. "Parental Job Retention" refers to whether (and for how many days) the parent remained with their pre-ACA employer during the post-ACA period. Panels 3, 4 and 5 provide summary statistics for control variables used in our regression. "Parent's Birth Date" refers to the year and month the planholder parent was born. "Spousal coverage" is an indicator for whether the planholder parent provided coverage to a spouse in the pre-ACA period. "Enrolled in HMO" is an indicator for whether the planholder parent was enrolled on an HMO plan in the pre-ACA period. "Number of Dependents" indicates the total dependents covered by the planholder parent in the pre-ACA period. "Prior Inpatient Care" indicates whether the dependent received inpatient care in the pre-ACA period. "Offered both HMO and FFS" is an indicator for whether the parent's pre-ACA employer offered both HMO and FFS plans.

Table 2: Tests for Covariate Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parental Characteristics				Dependent Characteristics			Employer Characteristics
	Female	Birth Date	Spousal Coverage	Enrolled in HMO	Female	Number of Dependents	Prior Inpatient Care	Offer Both HMO&FFS
RD estimate	-0.0035	0.0257	-0.0031	-0.0028	0.0009	0.0139*	-0.0019	0.0012
	(0.0034)	(0.3959)	(0.0028)	(0.0029)	(0.0034)	(0.0078)	(0.0018)	(0.0030)
Mean, left of cut-off	0.41	-28.66	0.79	0.23	0.50	2.36	0.07	0.74
Observations	393791	393791	393791	393791	393791	393791	393791	393791
Controls	No	No	No	No	No	No	No	No
Weighting scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo
Degree of polynomial	1	1	1	1	1	1	1	1

Notes: This table reports estimates of β from a version of Eq. 1 that excludes the vector of control variables (X_{ij}). Each column represents a separate regression in which one of the control variables, as indicated in the column headings, is the dependent variable Y_{ij}. "Parent's Birth Date" is enumerated in months relative to January 1960, so the average value of -29 indicates August 1957. Robust standard errors are reported in parentheses. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1985. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

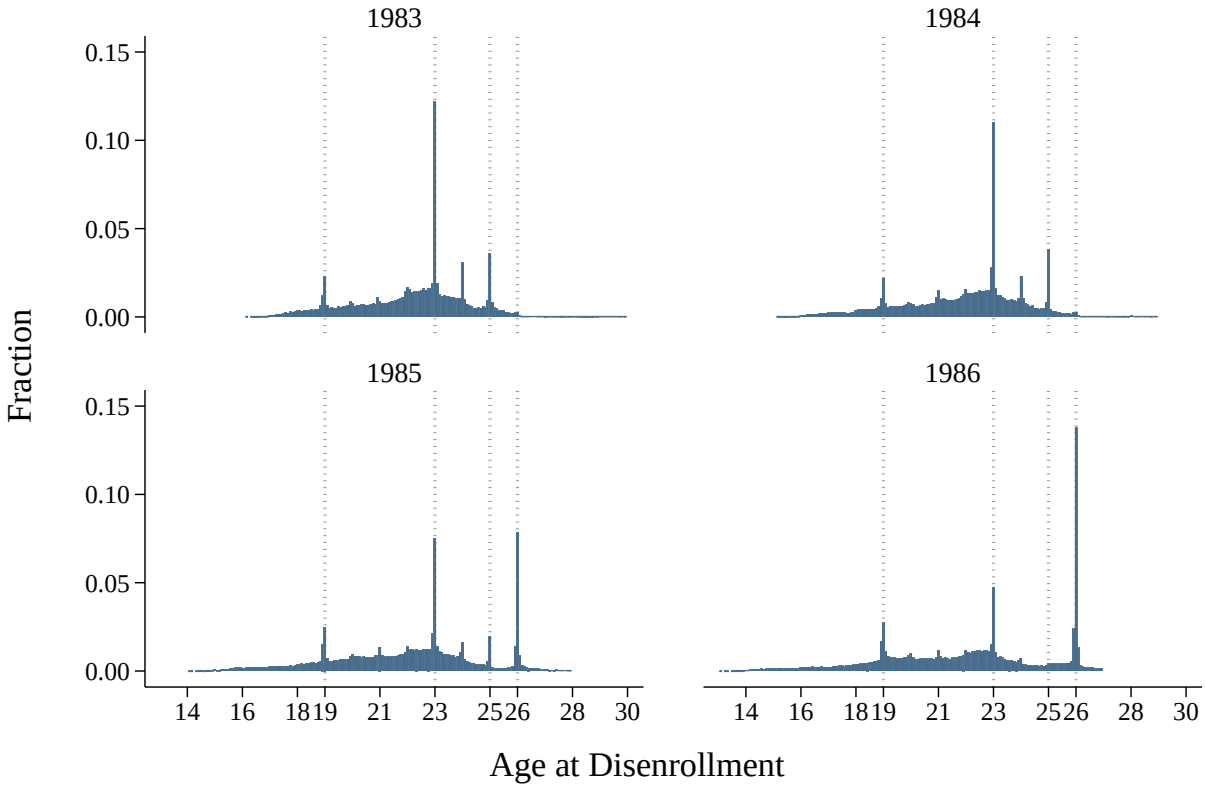
Table 3: Effects of Dependent Coverage on Enrollment and Parental Job Retention

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0175***
	(0.0028)
Mean, left of cut-off	0.19
(2) Duration (days)	9.6811***
	(1.1164)
Mean, left of cut-off	66.48
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	0.0098***
	(0.0034)
Mean, left of cut-off	0.54
(2) Duration (days)	5.7603**
	(2.3791)
Mean, left of cut-off	357.63
Observations	393791
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: The table above reports estimates of β from Eq. 1. Robust standard errors are reported in parentheses. Each coefficient and standard error pair are from a separate regression in which the outcome Y_{ij} is labeled in the first column. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1985. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. The outcome variable, Y_{ij} is reported in the first column. * p<0.10, ** p<0.05, *** p<0.01.

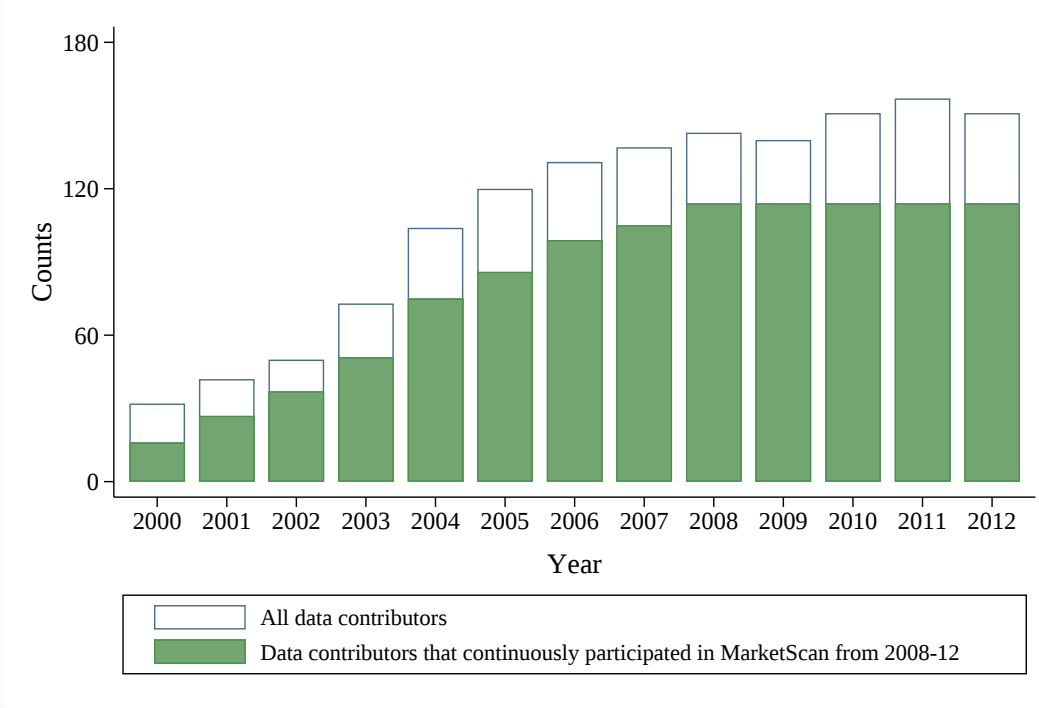
Appendix Figures and Tables

Figure A.1: Distribution of Age in Months at Dis-enrollment by Birth Cohort



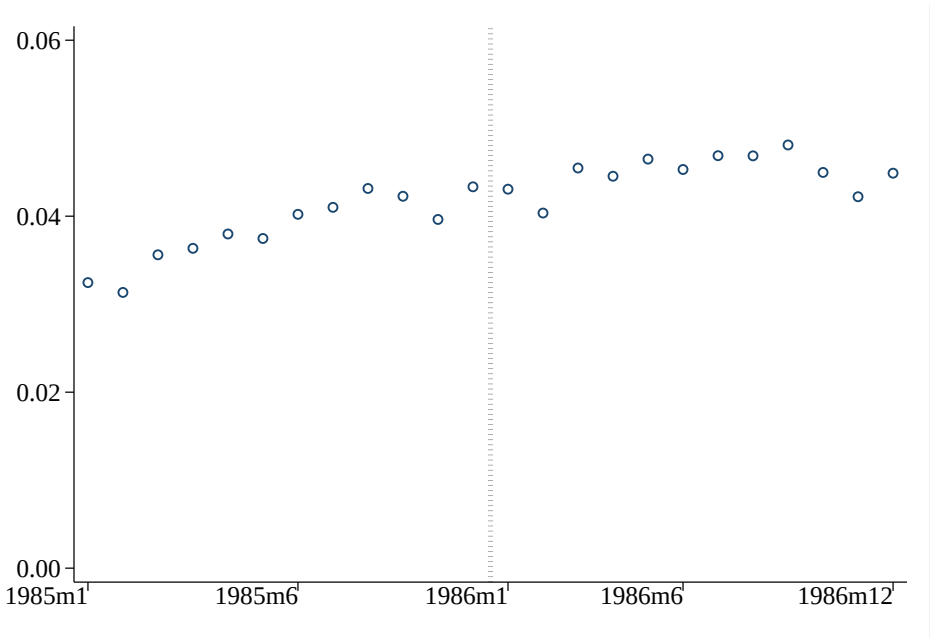
Notes: The figure displays the distribution of dependents' age in months when they disenroll from coverage provided by their parents' pre-ACA employer, separately by birth cohort. If dependents dis-enroll multiple times, we consider only the last disenrollment. The sample is restricted to dependents who are first covered on their parent's plan prior to the ACA (before 2010). The sample is constructed following the same steps used to create our main analysis sample with one exception. Because we include the 1983 and 1984 cohorts in this analysis, we limit data contributors to those that participate continuously from 2006 to 2012, rather than 2008-2012.

Figure A.2: Employers that Contribute Data, Truven MarketScan Panel



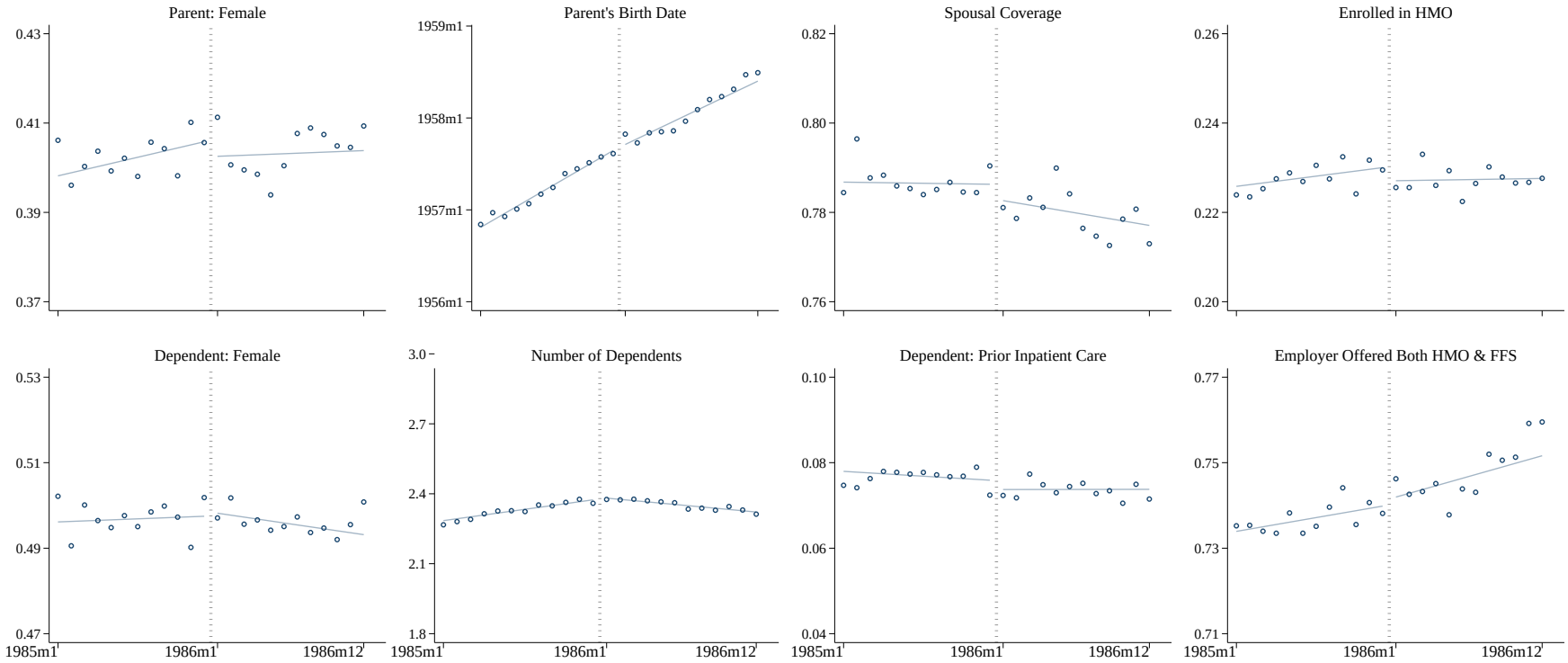
Notes: Ths figure plots the number of employers who contribute in each year of the Truven Marketscan panel from 2000-2012. Of these employers, 114 continuously provided data from 2008-2012 and are thus included in our main sample.

Figure A.3: McCrary Density Test



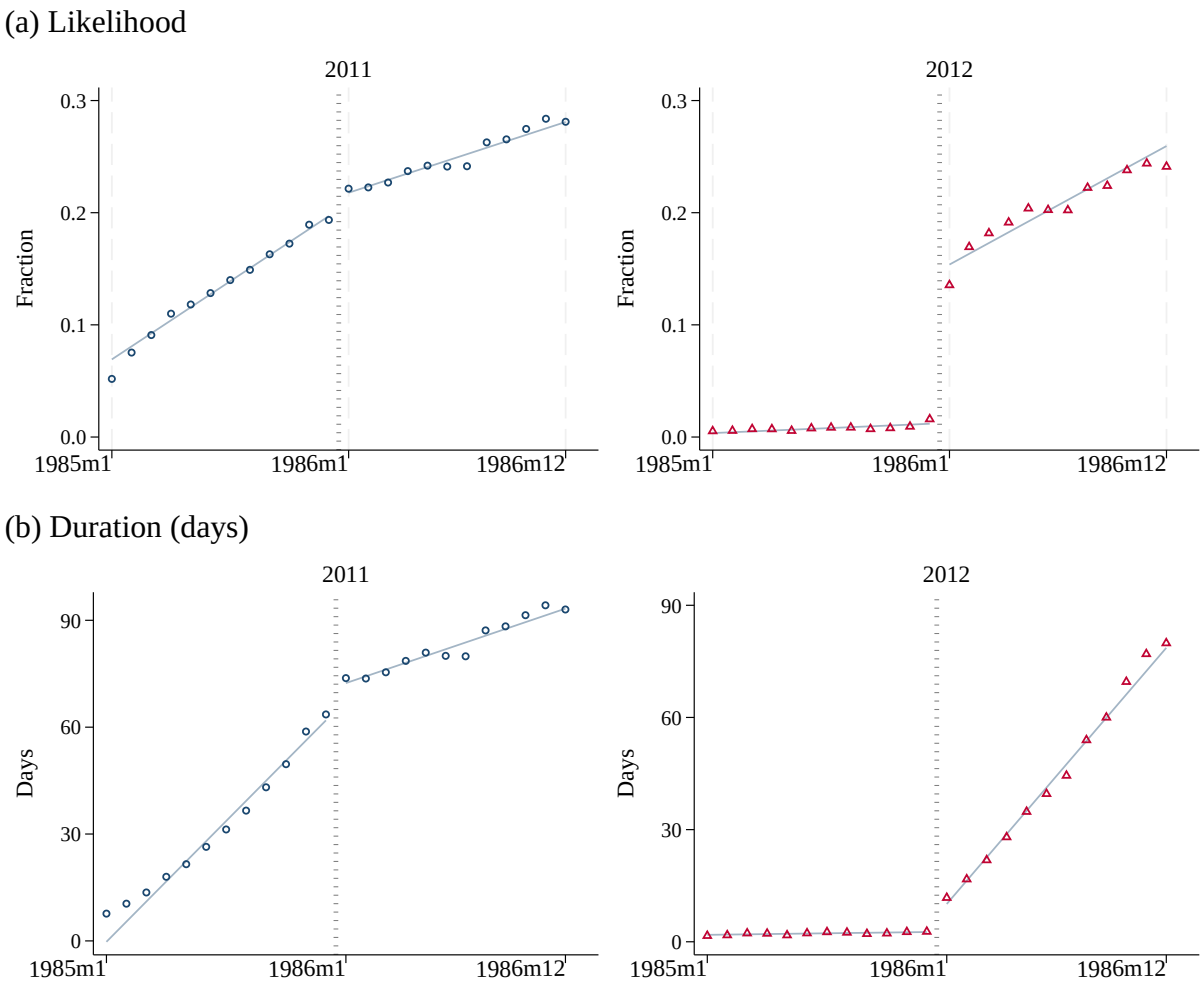
Notes: This figure displays the density of dependents in our analysis sample by their birth month. We conduct a McCrary density test in Stata by using DCDensity.ado, written by Justin McCrary and Brian Kovak. The discontinuity estimates from the McCrary density test are -0.01803 (standard error=0.01191, p-value=0.16848). See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Figure A.4: Characteristics by Birth Month



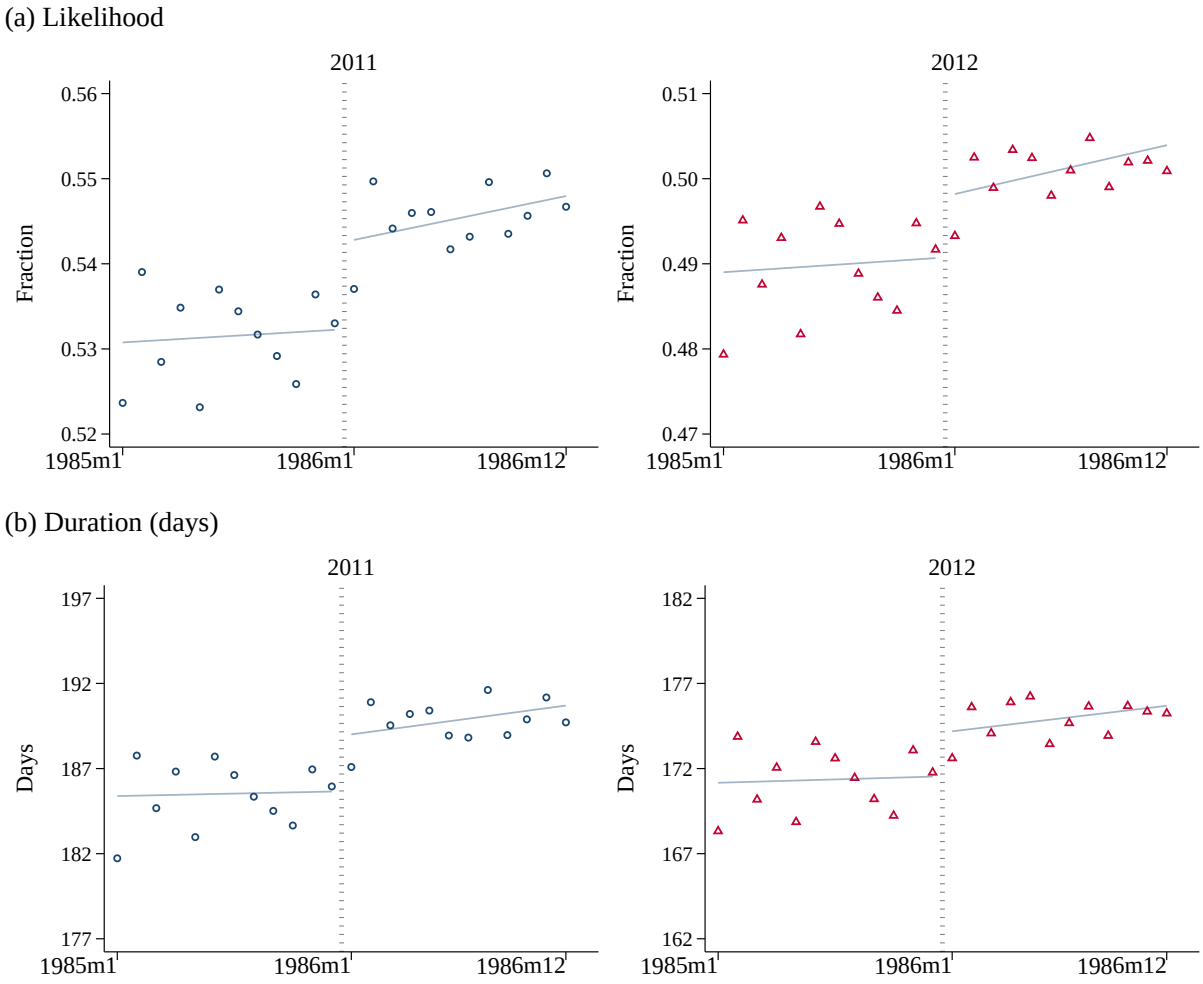
Notes: This figure displays unadjusted means of our control variables by dependent birth cohort. Table 2 reports corresponding regression discontinuity estimates. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Figure A.5: Effect of Dependent Coverage on Dependent Enrollment, by Enrollment Year



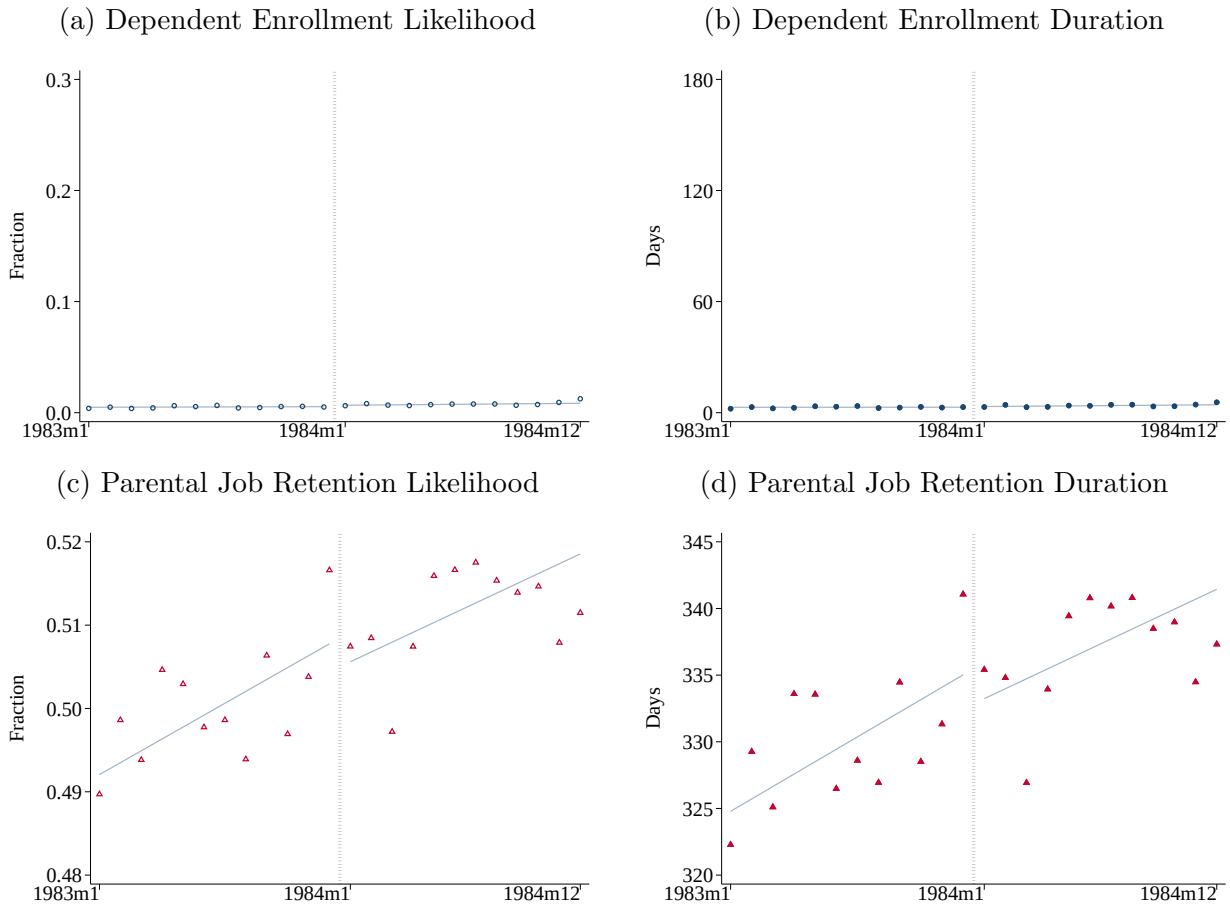
Notes: This figure displays regression-adjusted means of the dependent enrollment outcomes by dependent birth date, separately by enrollment year. The outcome variable in Panel (a) is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer during 2011 or 2012. In Panel (b), the outcome is total days of enrollment during 2011 or 2012. To calculate the regression-adjusted means, we regress these outcomes on our control variables (X_{ij} from Eq. 1) and then calculate the residual means by birth month. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Figure A.6: Effect of Dependent Coverage on Parental Job Retention, by Enrollment Year



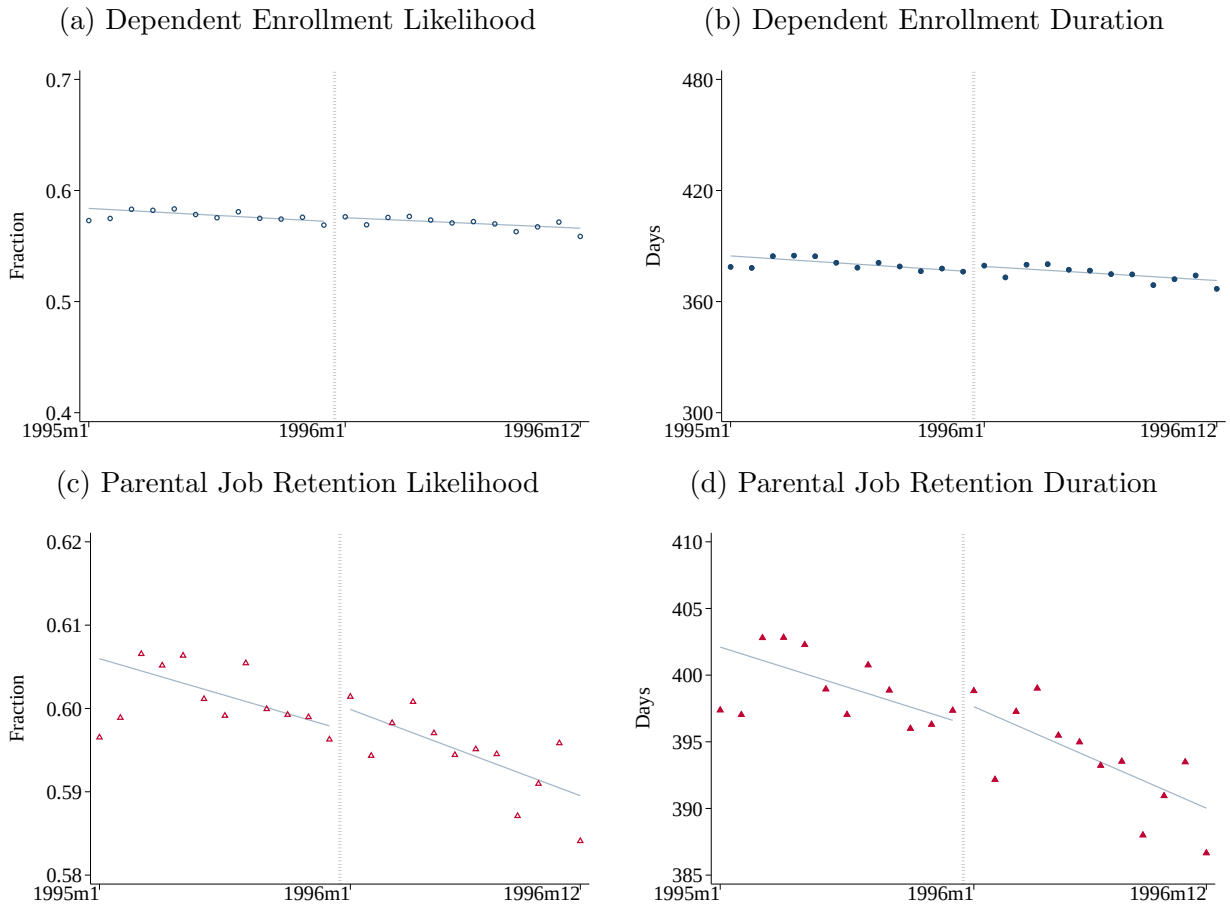
Notes: This figure displays regression-adjusted means of the parental job retention outcomes by dependent birth date, separately by enrollment year. The outcome variable in Panel (a) is an indicator for whether the parent remains with their pre-ACA employer for at least one month in 2011 or 2012. In Panel (b), the outcome is total days of job retention during 2011 or 2012. To calculate the regression-adjusted means, we regress these outcomes on our control variables (X_{ij} from Eq. 1), and then calculate the residual means by birth month. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Figure A.7: 1983-1984 Cohort Placebo Test



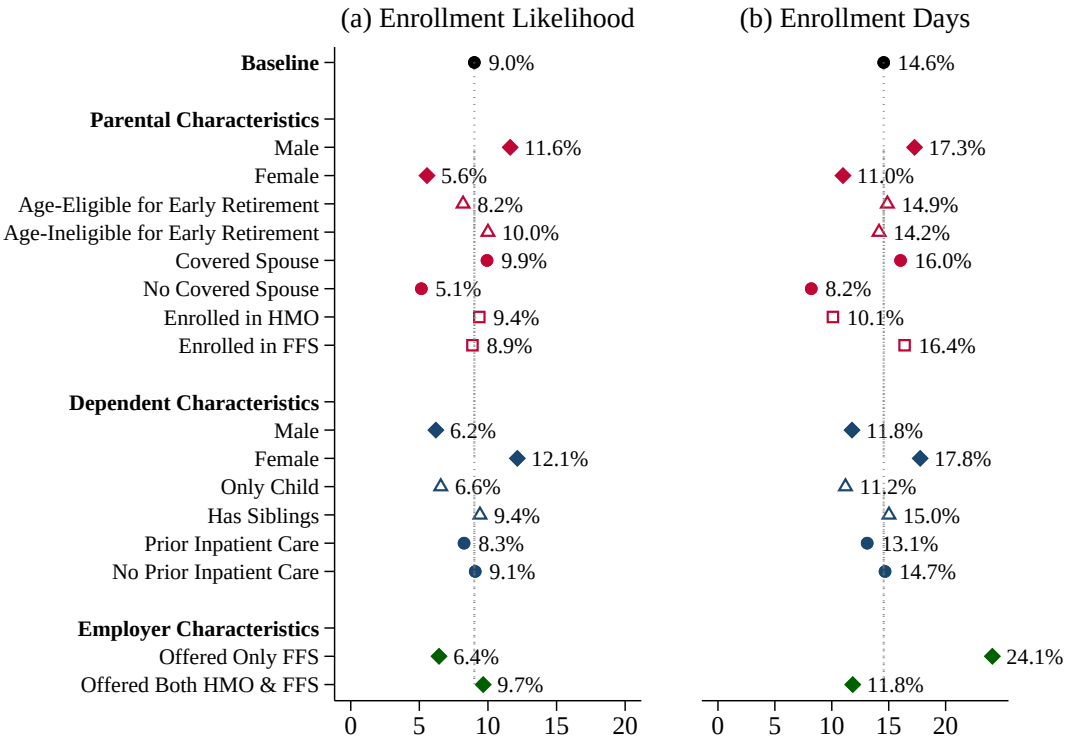
Notes: This figure displays regression-adjusted means of dependent enrollment outcomes by birth month. The sample consists of dependents born between January 1983 and December 1984. The RD cutoff value is December 1983. The outcome variable in Figure A.7a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure A.7b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure A.7c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure A.7d, the outcome is total days of employment with that employer during 2011-2012. The corresponding RD estimates are reported in Appendix Table A.8.

Figure A.8: 1995-1996 Cohort Placebo Test



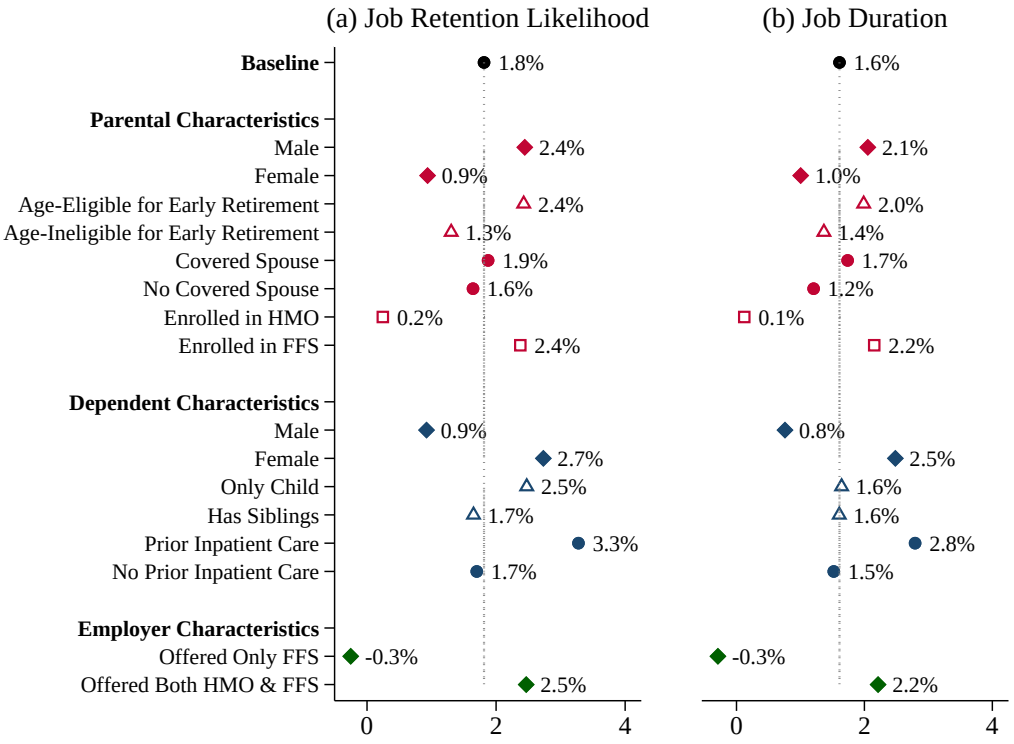
Notes: This figure displays regression-adjusted means of dependent enrollment outcomes by birth month. The sample consists of dependents born between January 1995 and December 1996. The RD cutoff value is December 1995. The outcome variable in Figure A.8a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure A.8b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure A.8c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure A.8d, the outcome is total days of employment with that employer during 2011-2012. The corresponding RD estimates are reported in Appendix Table A.9.

Figure A.9: Percent Change from Baseline: Dependent Enrollment



Notes: The figures above display RD estimates (β from a version of Eq. 1), expressed as a percent of the control mean (i.e., the mean for cohort December 1985). The outcomes are dependent enrollment likelihood and length (days) during 2011-2012. We report effects for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Figure A.10: Percent Change from Baseline: Parental Job Retention



Notes: The figures above display RD estimates (β from a version of Eq. 1), expressed as a percent of the control mean (i.e., for parents of children born December 1985). The outcomes are parental job retention likelihood and length (days) during 2011-2012. We report effects for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions.

Table A.1: Time Range in Our Sample During which Dependent Cohorts are Under 23

Dependent Birth Date While Under 23	In-Sample Dates (Month/Year)
1/1985	1/2000-1/2008
2/1985	1/2000-2/2008
3/1985	1/2000-3/2008
4/1985	1/2000-4/2008
5/1985	1/2000-5/2008
6/1985	1/2000-6/2008
7/1985	1/2000-7/2008
8/1985	1/2000-8/2008
9/1985	1/2000-9/2008
10/1985	1/2000-10/2008
11/1985	1/2000-11/2008
12/1985	1/2000-12/2008
1/1986	1/2000-1/2009
2/1986	1/2000-2/2009
3/1986	1/2000-3/2009
4/1986	1/2000-4/2009
5/1986	1/2000-5/2009
6/1986	1/2000-6/2009
7/1986	1/2000-7/2009
8/1986	1/2000-8/2009
9/1986	1/2000-9/2009
10/1986	1/2000-10/2009
11/1986	1/2000-11/2009
12/1986	1/2000-12/2009

Notes: The table above shows, for each dependent birth month, the range of months during which they could be observed in our sample while under the age of 23. New data contributors are added to the MarketScan sample every January. These annual changes in contributors would result in additional under-23 dependents with January birth months (as compared to December birth months), as illustrated by the above table. To avoid selection into the sample by dependent birth date, we thus restrict our main sample to data contributors that continuously participate in MarketScan from 2008 to 2012.

Table A.2: PSID: Share of Employees Who Remain Employed but Drop Insurance within 2 Years

	Drops Insurance		Total
	Yes	No	
N	84,420	8,001,158	8,008,578
Share	0.01	0.99	1.00

Notes: The source of data is the Panel Study of Income Dynamics, Waves 2011-2013. The sample is limited to heads of household born between 1948 and 1970, who are planholders of an employer-sponsored plan in 2011 and who remain at the same employer by 2013. "Drops Insurance by 2013 " is an indicator for whether the individual is no longer covered by their employer by 2013. Sample counts reflect the use of 2013 PSID cross-sectional individual-level weights. See Appendix Section B for more information on sample and outcome construction.

Table A.3: Results by Enrollment Year

	(1)	(2)
	Enrollment Year	
	2011	2012
(a) Dependent Enrollment, 2011-2012		
(1) Likelihood	0.0152***	0.1324***
	(0.0028)	(0.0019)
Mean, control cohort	0.194	0.016
(2) Duration (days)	8.3972***	1.2246***
	(0.9268)	(0.3358)
Mean, control cohort	63.730	2.750
(b) Parental Job Retention, 2011-2012		
(1) Likelihood	0.0098***	0.0068**
	(0.0034)	(0.0034)
Mean, control cohort	0.533	0.492
(2) Duration (days)	3.1153**	2.4515**
	(1.2195)	(1.2220)
Mean, control cohort	185.928	171.702
Observations	393,791	393,791
Controls	Yes	Yes
Weighting scheme	Triangular	Triangular
Bandwidth	±12 mo	±12 mo
Degree of polynomial	1	1

Notes: This table reports how the effects on dependent enrollment and parental job retention outcomes vary by enrollment year. We estimate our regression discontinuity design (Eq. 1) separately for enrollment during 2011 and 2012 to test whether insurance enrollment drops when each dependent birth cohort turns 26 (the 1985 cohort turns 26 in 2011, and the 1986 cohort turns 26 in 2012). For instance, the 1985 cohort is expected to have a very low enrollment rate in 2012 as they will be over 26 at that time. The corresponding RD graphs are shown in Appendix Figure A.5. Standard errors are adjusted for individual-level heteroskedasticity. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Heterogeneity by Parental Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Gender		Early Retirement		Spousal Coverage		Enrolled in	
		Male	Female	Age-Eligible	Age-Ineligible	Yes	No	HMO	FFS
(a) Dependent Enrollment, 2011-2012									
(1) Likelihood	0.0175***	0.0214***	0.0117***	0.0162***	0.0190***	0.0198***	0.0091	0.0228***	0.0160***
	(0.0028)	(0.0035)	(0.0045)	(0.0038)	(0.0040)	(0.0032)	(0.0056)	(0.0062)	(0.0031)
Mean, left of cut-off	0.19	0.18	0.21	0.20	0.19	0.20	0.18	0.24	0.18
(2) Duration (days)	9.6811***	10.8855***	7.8602***	10.0577***	9.2287***	10.9316***	4.9358**	8.6487***	9.9660***
	(1.1164)	(1.4225)	(1.7952)	(1.5551)	(1.6045)	(1.2859)	(2.2220)	(2.4941)	(1.2442)
Mean, left of cut-off	66.48	63.02	71.56	67.56	65.21	68.15	60.20	85.64	60.77
(b) Parental Job Retention, 2011-2012									
(1) Likelihood	0.0098***	0.0130***	0.0052	0.0122***	0.0076	0.0103***	0.0085	0.0015	0.0122***
	(0.0034)	(0.0044)	(0.0053)	(0.0047)	(0.0049)	(0.0038)	(0.0073)	(0.0069)	(0.0039)
Mean, left of cut-off	0.54	0.53	0.56	0.50	0.58	0.55	0.52	0.63	0.51
(2) Duration (days)	5.7603**	7.2349**	3.6696	6.5533**	5.3409	6.3029**	4.0824	0.5155	7.2840***
	(2.3791)	(3.0801)	(3.7307)	(3.2464)	(3.5025)	(2.6891)	(5.0995)	(4.9401)	(2.7140)
Mean, left of cut-off	357.63	352.31	365.42	329.47	390.79	362.72	338.45	422.86	338.20
Observations	393,791	234,968	158,823	211,907	181,884	308,284	85,507	89,616	304,175
Weights					Triangular				
Controls					Yes				
Bandwidth					± 12 mo				
Degree of polynomial					1				

Notes: This table reports estimates of β from Eq. 1, separately for subsamples by parental characteristics. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Heterogeneity by Dependent Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Gender		Number of Dependents		Prior Inpatient Care	
		Male	Female	Only Child	Has Siblings	Yes	No
(a) Dependent Enrollment, 2011-2012							
(1) Likelihood	0.0175***	0.0126***	0.0225***	0.0112*	0.0189***	0.0189*	0.0174***
	(0.0028)	(0.0040)	(0.0039)	(0.0057)	(0.0032)	(0.0108)	(0.0029)
Mean, left of cut-off	0.19	0.20	0.19	0.17	0.20	0.23	0.19
(2) Duration (days)	9.6811***	8.3204***	11.0817***	6.4587***	10.3574***	10.6651**	9.5896***
	(1.1164)	(1.6042)	(1.5521)	(2.3005)	(1.2739)	(4.5653)	(1.1488)
Mean, left of cut-off	66.48	70.63	62.36	57.65	68.93	81.34	65.32
(b) Parental Job Retention, 2011-2012							
(1) Likelihood	0.0098***	0.0050	0.0147***	0.0130*	0.0090**	0.0185	0.0092***
	(0.0034)	(0.0048)	(0.0048)	(0.0073)	(0.0038)	(0.0122)	(0.0035)
Mean, left of cut-off	0.54	0.54	0.54	0.52	0.54	0.56	0.54
(2) Duration (days)	5.7603**	2.7139	8.8768***	5.6716	5.8044**	10.4637	5.4199**
	(2.3791)	(3.3559)	(3.3735)	(5.1293)	(2.6848)	(8.6158)	(2.4748)
Mean, left of cut-off	357.63	357.87	357.39	345.09	361.11	374.73	356.29
Observations	393,791	198,240	195,551	84,920	308,871	29,499	364,292
Weights				Triangular			
Controls				Yes			
Bandwidth				± 12 mo			
Degree of polynomial				1			

Notes: This table reports estimates of β from Eq. 1, separately for subsamples by dependent characteristics. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Heterogeneity by Employer Characteristics

	(1)	(2)	(3)
	All	Offered	
		FFS Only	Both HMO & FFS
(a) Dependent Enrollment, 2011-2012			
(1) Likelihood	0.0175***	0.0109**	0.0197***
	(0.0028)	(0.0052)	(0.0033)
Mean, left of cut-off	0.19	0.17	0.20
(2) Duration (days)	9.6811***	13.6299***	8.2904***
	(1.1164)	(2.2198)	(1.2910)
Mean, left of cut-off	66.48	56.54	70.01
(b) Parental Job Retention, 2011-2012			
(1) Likelihood	0.0098***	-0.0013	0.0136***
	(0.0034)	(0.0067)	(0.0039)
Mean, left of cut-off	0.54	0.51	0.55
(2) Duration (days)	5.7603**	-0.9670	8.1041***
	(2.3791)	(4.6702)	(2.7629)
Mean, left of cut-off	357.63	333.75	366.10
Observations	393,791	101,246	292,545
Weights		Triangular	
Controls		Yes	
Bandwidth		± 12 mo	
Degree of polynomial		1	

Notes: This table reports estimates of β from Eq. 1, separately for subsamples of employers based on whether they offer FFS only or FFS and HMO plans in the pre-period. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Dependent Enrollment, 2011-2012								
(1) Likelihood	0.0175***	0.0171***	0.0138***	0.0175***	0.0208***	0.0197***	0.0158***	0.0150***
	(0.0028)	(0.0028)	(0.0026)	(0.0022)	(0.0034)	(0.0030)	(0.0032)	(0.0031)
(b) Duration (days)	9.6811***	9.4946***	9.7020***	9.6811***	10.7675***	10.1435***	6.9478***	11.4530***
	(1.1164)	(1.1212)	(1.0874)	(0.9597)	(1.3556)	(1.2144)	(1.2524)	(1.1994)
(b) Parental Job Retention, 2011-2012								
(1) Likelihood	0.0098***	0.0092***	0.0101***	0.0098***	0.0085**	0.0094**	0.0093**	0.0093**
	(0.0034)	(0.0034)	(0.0031)	(0.0028)	(0.0041)	(0.0037)	(0.0041)	(0.0038)
(b) Duration (days)	5.7603**	5.3384**	5.8330***	5.7603***	4.7457	5.4837**	6.0359**	5.3881**
	(2.3791)	(2.4009)	(2.2114)	(1.9401)	(2.8991)	(2.5947)	(2.8882)	(2.7035)
Observations	393,791	393,791	393,791	393,791	269,378	334,369	266,855	393,791
Sample	age $<$ 23	age $<$ 23	age $<$ 23	age $<$ 23	age $<$ 23	age $<$ 23	age $<$ 19	age $<$ 23
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Weighting Scheme	Triangular	Triangular	None	Triangular	Triangular	Triangular	Triangular	Triangular
Linear f()	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Local linear
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±8	±10	±12 mo	±12 mo
Std Error	Robust	Robust	Robust	Cluster(birth month)	Robust	Robust	Robust	Robust

Notes: This table examines the robustness of our estimates to modifications in Eq. 1. Column (1) reports our baseline estimates in Table 3, whereas Columns (2)-(8) report the results of the variations as the following: excluding the control variables; excluding the triangular weights; clustering the standard errors at the level of birth month (the running variable); employing different bandwidths around the cutoff months; restricting the main sample to dependents who were covered at least one month on their parent's plan in the pre-period prior to the age of 19; and replacing our linear control function with a local linear specification. See the notes to Table 1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table A.8: Placebo Test: Dependents born in 1983-1984

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0012*
	(0.0007)
Mean, left of cut-off	0.01
(2) Duration (days)	0.2512
	(0.3711)
Mean, left of cut-off	3.00
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	-0.0037
	(0.0041)
Mean, left of cut-off	0.52
(2) Duration (days)	-2.8011
	(2.8925)
Mean, left of cut-off	341.12
Observations	265752
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: In this table, we report estimates of β from RD specifications that are similar to our main estimating strategy but use the placebo sample of dependents born between January 1983 and December 1984. We modify Eq. 1 so that the cutoff is December 1983 (rather than December 1985). Dependents in the placebo sample are over 26 during 2011-2012 and therefore were ineligible for coverage on their parent's plan in most cases. Similar to the baseline sample. dependents must: (1) be born from January 1983 to December 1984; (2) be covered on their parent's plan for at least 12 months prior to 2010 (i.e., the "pre-ACA period"); (3) be covered on their parent's plan while under the age of 23 in the pre-ACA period, (4) be covered on the plan that include at most one dependent born between January 1983 to December 1984, (5) have parents born between 1948 and 1970 to ensure that both samples have parents from the same birth cohorts, and (6) whose data contributors that continuously participate in MarketScan from 2008 to 2012. The corresponding RD graphs are shown in Appendix Figure A.7. Standard errors are adjusted for individual-level heteroskedasticity. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1983.* p<0.10, ** p<0.05, *** p<0.01.

Table A.9: Placebo Test: Dependents born in 1995-1996

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0042
	(0.0032)
Mean, left of cut-off	0.57
(2) Duration (days)	3.3618
	(2.2786)
Mean, left of cut-off	375.37
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	0.0031
	(0.0032)
Mean, left of cut-off	0.60
(2) Duration (days)	1.8820
	(2.2762)
Mean, left of cut-off	396.79
Observations	438435
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: In this table, we report estimates of β from RD specifications that are similar to our main estimating strategy but use the placebo sample of dependents born between January 1995 and December 1996. We modify Eq. 1 so that the cutoff is December 1995, rather than December 1985. Dependents in the placebo sample were under 19 during 2011-2012 and therefore were eligible for parental coverage under the pre-ACA rules. Similar to the baseline sample. dependents must: (1) be born from January 1995 to December 1996; (2) be covered on their parent's plan for at least 12 months prior to 2010 (i.e., the "pre-ACA period"); (3) be covered on their parent's plan while under the age of 23 in the pre-ACA period, (4) be covered on the plan that include at most one dependent born between January 1995 to December 1996, (5) have parents born between 1948 and 1970 to ensure that both samples have parents from the same birth cohorts, and (6) whose data contributors that continuously participate in MarketScan from 2008 to 2012. The corresponding RD graphs are shown in Appendix Figure A.8. Standard errors are adjusted for individual-level heteroskedasticity. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1995. * p<0.10, ** p<0.05, *** p<0.01.

A Appendix: Measures of Employer Plan Offerings

Our data do not directly report the parameters of all insurance plans offered by employers. Instead, we create proxies using the enrollment data for the characteristics of plans offered to parents by their pre-mandate employer. First, we construct two different measures of the generosity and flexibility of insurance coverage. Our first measure of insurance generosity is an indicator for whether the parent's pre-period plan is an health maintenance organization (HMO) plan or a fee-for-service (FFS) plan. HMO plans limit coverage to doctors within their network, and typically have limited or no coverage out of network. In contrast, fee-for-service plans such as preferred provider organizations (PPO), which make up nearly all other plans in our data, are less restrictive. In particular, we use the "PLANTYP" variable in the Marketscan data, and assign plan types Comprehensive, EPO, POS, PPO, POS with capitation, CDHP, and HDH as FFS. For the 1.7% of individuals in the sample with a missing value, we assign them as 0 for the indicator for HMO coverage. The findings are robust to whether we classify them as HMO or FFS as the share of planholders with the missing plan type information is smooth around the cutoff. If parents are enrolled in multiple types of plans in the pre-period, we use their earliest plan.

One potential concern with measuring generosity or flexibility through plan characteristics is that plans also differ in their premiums, which we cannot observe. This motivates our second measure: an indicator for employers offering both HMO and FFS plans during the pre-period. In contrast to the previous measure, which was at the individual-level, this measure is constructed at the employer-level. In particular, we calculate the annual number of plan holders who maintained their plans for 12 months by employer between 2000 and 2009. We also count the number of plan holders enrolled in HMOs each year. Using these two numbers, we calculate the average share of annual HMO enrollees in a given employer. Plan holders with missing plan type information in a given year are also included in the denominator when calculating the share of HMO enrollees. Employers with a zero annual share of HMO plans are categorized as not offering any HMO plans during the pre-ACA period.

B Appendix: PSID

The PSID is a longitudinal survey with information on both employment and health insurance. We use survey years 2011 and 2013 because it approximately overlaps with our sample and includes insurance information. The PSID is administered every other year during this time period, so our sample combines 3 waves. Observation counts reflect sampling weights provided by the PSID. We then limit the sample to heads of households that participated in the survey in 2011 and 2013 – doing so allows us to observe their employment and health insurance outcomes in both years. We then require that individuals are born from 1948 to 1970, the range of birth cohorts of primary beneficiaries in our MarketScan sample, and that they are observed to have a dependent in 2011. We keep individuals who are employed at the same employer in both 2011 and 2013 and who served as the planholder of an employer-sponsored plan in the 2011.

Our outcome is an indicator for whether the individual is no longer covered by their

employer by 2013. Specifically, we code this as either: 1) no one in the household is covered by health insurance (H61D3), or 2) the individual is not covered by employer-sponsored insurance (H61E), or 3) the individual is covered by employer-sponsored insurance but they are no longer the planholder (H61F).