Dependent Coverage and Parental "Job Lock": Evidence from the Affordable Care Act*

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Abstract

In 2010, the Affordable Care Act (ACA) mandated that private insurance plans extend coverage to adult dependents under the age of 26. In this article, we hypothesize that this policy may have had the unintended consequence of increasing "job lock" among parents who would otherwise leave their employer. We use a large panel of insurance claims that links members covered by the same plan and follows individuals over time. To identify the effects of additional dependent coverage provided under the ACA, we estimate a regression discontinuity design in dependent birth date that exploits the fact that, on average, adult dependents born in January became eligible for more months of coverage than those born in December. We first show that, compared to their December-born counterparts, dependents with January births were more likely to enroll in their parent's plan and enrolled for a longer period of time. Correspondingly, we find that their parent is more likely to remain with their pre-ACA employer and remain with that employer for longer. Effects are larger for parents approaching early retirement and dependents who are only children. Our findings provide new insight into the welfare effects of mandated insurance coverage and the importance of intra-family spillovers.

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1 Introduction

The dependent mandate of the Patient Protection and Affordable Care Act (ACA) requires private insurers to provide coverage to adult dependents through the age of 26. Prior to this policy, which went into effect in September 2010, private insurers generally provided dependent coverage through age 19, or 23 if the dependent was a full-time student. Like the other provisions of the ACA, the goal of the dependent mandate was to increase insurance rates among individuals with historically low access to coverage — in this case, young adults. Indeed, recent work finds sizable increases in insurance coverage among young adults following the dependent mandate (e.g., Akosa Antwi, Moriya, and Simon 2013; Barbaresco, Courtemanche, and Qi 2015; Sommers et al. 2013; Carpenter et al. 2021; Kim 2022). For example, Barbaresco, Courtemanche, and Qi (2015) estimates an increase of 5.5 to 6.7 percentage points among individuals aged 23-25, up from a base of 68 percent.

The most common source of private insurance in the U.S. is one's employer.[1] A concern with tying health insurance to one's job is that some individuals may remain with their employer solely for the purpose of retaining coverage, even when leaving would be otherwise beneficial — this scenario is called "job lock" (Madrian 1994). Thus, by giving adult dependents access to health insurance outside of their employer (i.e., through their *parent's* employer), the dependent mandate could conceivably have reduced job lock among this population. Indeed, some studies have found a decrease in employment and hours worked among young adults as a result of the mandate, although other studies, using different settings or designs, have found null effects.[2]

In this article, we explore a different, but related, hypothesis. In particular, we investigate

1. In 2018, for example, 67.3% of the U.S. population had private health insurance coverage. Of these individuals, 81.9% received insurance through their employer. Source: https://www.census.gov/content/dam/Census/library/publications/2019/demo/p60-267.pdf (accessed on May 22, 2022).

2. For example, Akosa Antwi, Moriya, and Simon (2013) find that, for 19-25 year olds, the mandate reduces the probability of working full time by 2.21 percentage points and the number of hours worked by 4.75%. Using data from the U.S. Army, Kofoed and Frasier (2019) find a decrease in re-enlistment rates and an increase in college-going for soldiers aged 23-25. In contrast, Bailey and Chorniy (2016), Heim, Lurie, and Simon (2014), and Kim (2022) find no evidence of meaningful changes in labor supply among young adults as a result of the dependent mandate.

whether the dependent mandate increased job lock among *parents* of adult dependents by tying their child's health insurance to their job. Parents may, for example, delay retirement to ensure continued coverage for their adult child.

To analyze this question, we use a large panel of employer-sponsored insurance claims covering 2002 to 2012. These data link individuals covered under the same plan so that we can match primary beneficiaries to their adult dependents. We can also track primary beneficiaries (and anyone on their plan) as long as they remain with the same employer and do not forego insurance coverage altogether. Another benefit of the data is that age is reported at the monthly level, allowing us to back out birth date for individuals in our sample. We define our analysis sample to include pairs of primary beneficiaries and dependents that we first observe in the pre-mandate period (i.e., 2002-2009). This method allows us to avoid concerns with endogenous selection into the sample in the post-mandate period.

To identify the effects of dependent coverage expansions, we estimate a regression discontinuity (RD) design that exploits the fact that, on average, adult dependents born in January became eligible for discontinuously more months of coverage than those born in December. This difference occurs because some plans cover adult dependents through December of the year in which they turn 26, whereas others only cover adult dependents through their birth month. We limit our sample to dependents born from 1/1984 to 12/1986 — these individuals turn 26 during the post-mandate period in our data (2010-2012). We compare individuals born in 1/1985 to those born in 12/1984 and individuals born in 1/1986 to those born in 12/1985.

Our main outcome measures, for both the primary beneficiary and the adult dependent, are derived from monthly enrollment records from 2010-2012. While we do not directly observe employment outcomes for the primary beneficiary, we argue that enrollment in (any) health insurance plan provided by one's employer is a highly accurate proxy for job retention. To illustrate this point, we construct a sample of primary beneficiaries with similar characteristics to those in our claims data using the Panel Study for Income Dynamics (PSID).

Among these individuals, only 0.6% are observed to drop health insurance altogether while remaining with their employer.

Next, we evaluate the validity of our RD design, which rests on the assumption that variables outside of the treatment evolve smoothly through the RD cut-offs in dependent birth date (1/1985 and 1/1986). We provide evidence to support this assumption by showing that demographic attributes of the families in our data evolve smoothly through the cut-off values. In addition, we find no evidence of discontinuities in the distribution of dependents by birth month around the cut-offs.

Having provided support for the validity of our causal design, we first estimate the effects of expanded coverage eligibility on adult dependents. As expected, we find that individuals eligible for more coverage under their parent's plans are more likely to enroll and enroll for a longer period of time. In particular, dependent enrollment from 2010 to 2012 increases by 8.6 percentage points (86% of the sample mean) and 2 percentage points (8%) at the first and second birth date cut-offs, respectively. In addition, the enrollment duration increases by 8 days (26%) and 13.4 days (11%), respectively.

Next, as a placebo check, we estimate similar RD regressions for dependents born in 1994-1996 (i.e., 10 years younger than the dependents in our main sample). In the post-mandate period, these dependents are always under 19 and thus are eligible for coverage on their parents' plans under pre-existing policies. Reassuringly, we do not find any meaningful changes in enrollment outcomes for this sample at the placebo cut-off dates (1/1995 and 1/1996).

Turning back to our main sample, we estimate effects on job retention among primary beneficiaries with adult dependents. We find that additional dependent coverage increases the likelihood that parents remain with their pre-mandate employer and the length of time they remain with that employer from 2010 to 2012. In particular, job retention likelihood among primary beneficiaries increases 0.8 percentage points (1% of the sample mean) and 1.3 percentage points (2%) at the first and second cut-offs, respectively. In addition, job

duration increases by 6.8 days (1%) and 9.3 days (2%) at the two cut-offs, respectively. These results provide support for the hypothesis that the insurance expansion for adult dependents increased "job lock" among their parents by making it more costly to leave their employers.

Next, we probe the robustness of our main results. Reassuringly, our estimates remain highly similar when we make the following alterations to our baseline specification: (1) excluding controls; (2) excluding weights; (3) clustering on the running variable; (4) using alternate bandwidths; (5) employing different bandwidths around the cut-off months; and (6) replacing our linear control function with a local linear specification.

Lastly, we explore how our treatment effects vary with demographic characteristics of adult dependents and their parents. We first examine heterogeneity by parental age. In particular, we hypothesize that parents approaching early retirement will be most responsive to increases in dependent coverage. By comparison, younger parents are less likely to be considering leaving the labor force, while older parents are more likely to have already made the decision whether to retire early.[3] Indeed, we find evidence of stronger effects for parents around the early retirement age of 55 (when employees can start withdrawing from their 401(k) without penalty), compared to those at younger and older ages.

Second, we examine heterogeneity by the number of children ever covered on parents' plans in the pre-mandate period (as a proxy for total fertility). These results reveal that our effects are completely driven by cases in which dependents are only children. We conjecture that parents of only children may be more sensitive to their particular needs than those with multiple children.

Our paper contributes to several areas of the literature. Most directly related are papers estimating the effects of the dependent mandate on adult dependents and their parents. Above, we discussed research on the effects of the mandate on insurance coverage and labor supply among dependents. Other work on the mandate estimates its effects on healthcare

3. Additionally, the opportunity of switching to a better employer is more valuable for younger parents, who would have more years with the new employer.

utilization (Barbaresco, Courtemanche, and Qi 2015; Hernandez–Boussard et al. 2014; Sommers et al. 2013; Daw and Sommers 2018), health status (Barbaresco, Courtemanche, and Qi 2015), and marriage (Abramowitz 2016) among adult dependents. These analyses find little change in overall healthcare utilization, despite greater access to primary care physicians and fewer emergency department visits. Abramowitz (2016) finds that the mandate reduces the likelihood of marriage among young adults who might otherwise rely on their spouse for health insurance.

In a closely related study, Kim and Koh (2022) consider the effects of the ACA on parental employment outcomes. The authors use the American Community Survey (2006-2016) and compare parents of dependents aged 19-25 (who are treated by the mandate) to those 18-19 and 26-28 (who are controls), before and after the mandate went into effect. They do not find evidence of changes in parental employment or hours worked. An important limitation of their analysis is that their sample is restricted to parents living in the same households as their adult children (as otherwise the ACS does not link between parents and adult children).

Slusky (2017) provides a critique of the difference-in-differences strategy employed by Kim and Koh (2022) as well as other studies on the effects of the dependent mandates. He shows that such designs are subject to a failure of the parallel trends assumption unless a narrower age window is used to define the treatment and control groups. Our study addresses this issue by comparing adults dependents born only one month apart.

Outside of the literature on the ACA dependent mandate, our paper contributes to research on labor supply and retirement among older Americans. This literature finds that access to health insurance plays a significant role in retirement decisions (Gruber and Madrian 1995; Madrian 1994; French and Jones 2011; Aslim 2019). For instance, Nyce et al. (2013) finds that workers aged 58 to 64 retired sooner, working 5.6% fewer years, when they gained access to retiree coverage from a former employer. Similar to these findings, our results suggest that individuals delay retirement to retain insurance coverage — even when the coverage is for their adult children.

The paper proceeds as follows. Section 2 describes the dependent mandate in more detail. Section 3 describes our data. Section 4 lays out our empirical methods. Section 5 presents our main results, and Section 6 explores heterogeneous treatment effects. Lastly, Section 7 concludes.

2 The Dependent Coverage Mandate

Under the dependent coverage mandate, private health insurers were required to extend coverage to dependent children through the age of 26.[4] Prior to the mandate, most employer-sponsored insurance plans ended dependent coverage at age 19 if the dependent was not a full-time student or age 23 if the dependent was a full-time student (e.g., Cantor et al. 2011).[5] The new mandate required plans to insure dependents through the month in which they turn 26, but some plans choose to provide additional coverage through December of that year.[6]

The dependent coverage mandate applied to all plan years beginning on or after September 23, 2010.[7] Newly eligible dependents were able to sign up during a special, 30-day enrollment period. In addition, many plans elected to start coverage early, on May 1, 2010, to avoid a coverage gap in summer 2010 for eligible dependents that were graduating college in spring 2010.

After an adult dependent ages out of coverage at 26, they typically are granted a special enrollment period in which they can sign up for a new plan — either under their own employer or, starting in 2014, in a Marketplace plan. In special cases, they may be eligible to purchase

4. 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).

5. A number of states had previously passed laws requiring insurers to cover specific types of adult dependents (e.g., full-time students, individuals living with their parents, or unmarried individuals). These state laws did not apply to self-insured plans, which cover more than half of private sector workers with employer-sponsored health insurance. As such, only a small share of adult dependents were covered under these provisions (Monheit, DeLia, and Belloff 2011; Levine, McKnight, and Heep 2011; Akosa Antwi, Moriya, and Simon 2013). We provide direct evidence below that pre-existing mandates provided coverage to virtually no dependents in our sample.

6. Initially, plans that existed prior to March 23, 2010 were allowed to refuse coverage to eligible dependents who had access to insurance through their employer. The extent to which this provision was enforced is unknown (Akosa Antwi, Moriya, and Simon 2013). This provision was removed in 2014.

7. Most employer-sponsored plans have an enrollment period for a few weeks in October or November and their plan year starts on January 1 (Swartz and Graves 2014).

temporary extended coverage for up to 36 months under COBRA.

Finally, plans are not allowed to differentiate between the newly eligible adult dependents and similarly situated dependents who had coverage prior to the dependent mandate. In other words, they cannot charge different premiums or offer different benefit packages. Premiums paid for newly eligible dependents also receive the same tax-favored status as premiums paid for other dependents.

2.1 Variation in Added Months of Coverage by Birth date

In this section we describe how expansions in insurance coverage due to the dependent mandate created variation in enrollment months by dependent birth date. In particular, we calculate total months of added coverage from 2010 to 2012, the post-mandate period in our sample. We consider dependents born between 1984 and 1986, as these individuals turn 26 during 2010 to 2012.

For a given dependent birth date, there are two sources of plan-based variation in the number of coverage months that became available under the dependent mandate. First, some plans elected to cover dependents through the month of their 26th birthday, while others extended coverage through December of their 26th year. Going forward, we refer to the first type plan as "birth month" plans, and the second type as "end-of-year" plans. Second, some plans elected to start coverage early, in 5/2010, to avoid a coverage gap for eligible dependents graduating college in the spring of 2010 ("gap coverage").[8]

Considering these two sources of variation, we calculate the minimum and maximum coverage months from 1/2010-12/2012 for dependents born between 1/1984 and 12/1986. The results are shown by birth month in Figure 1, denoted "Minimum coverage months" and "Maximum coverage months," respectively. For example, dependents born 1/1985 would be turning 26 in 1/2011, the first month of new coverage under the mandate. Thus, at a minimum, under a "birth month" plan, they would qualify for 1 month of coverage (i.e., 1/2011).

8. For more information on this policy, see the bottom of page 3 here: https://obamawhitehouse.archives.gov/sites/default/files/rss_viewer/qa_young_adults_may.pdf (accessed on May 22, 2022).

At maximum, they would qualify for 12 months of coverage from an "end-of-year" plan (i.e., 1/2011-12/2011) plus eight months of coverage through the "gap coverage" (5/2010-12/2010), for 20 months total.[9] By comparison, those born in 12/1984 would neither receive coverage in "birth month" plans nor "end-of-year" plans, since they turn 26 in 2010 (and thus no gap coverage either). Given variation in policies across plans, the average number of coverage months available will jump between birth month 12/1984 and birth month 1/1985. A similar jump occurs at birth month 1/1986, when a maximum of 32 months of coverage is available, compared to a minimum of 12.

Thus, the number of coverage months available to dependents, on average, increases discontinuously at the birth date cut-offs shown in Figure 1. Below, we explain how we exploit these discontinuities to estimate the causal effects of added coverage on outcomes among dependents and their parents.

3 Data

Our primary data source is the Truven Health MarketScan® Commercial Claims and Encounters Database ("Truven data"), a large panel of health care claims from private insurers. The data was provided to Truven by around 200 large employers and health insurers ("data contributors") per year. We limit our sample to claims data provided by employers — doing so ensures we can track enrollees over time as long as they remain with the same employer and retain health insurance (even if they switch plans). Our sample covers the years 2002 to 2012.[10]

The data are organized as a monthly panel of enrollees — each observation represents an enrollee and an enrollment month. The data include primary beneficiaries (i.e., the employee) as well as any individuals covered under their plan (i.e., their spouse and dependents). Each individual in the data is assigned an enrollee ID, which enables us to follow them over time, and a family ID, which allows us to link primary beneficiaries to individuals covered under

9. We assume all plan years start in January, as is the case in our data sample.
10. From 2002 to 2012, out of 236 data contributors total, 202 are employers.

their plan. We can only track spouses and dependents as long as they remain with the same primary beneficiary.[11] Primary beneficiaries are included in the data as long as they are under the age of 65.

We impose a number of sample restrictions to ensure that we can identify the causal effect of increased dependent coverage on enrollment outcomes. First, we limit the sample to plans with both a primary beneficiary and a dependent and in which the dependent is born from 1/1984 to 12/1986, our cohorts of interest (as shown in Figure 1). Second, to ensure that the relationship between the primary beneficiary 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 primary beneficiaries under 65 throughout the sample period (i.e., born in or after 1/1948). [12] Fourth, we require that the primary beneficiary and dependent are first observed in the data prior to 2010. This step ensures that we avoid endogenous selection into the sample due to the enrollment incentives created by the dependent mandate. Fifth, we keep plans that only added one dependent belonging to the 1984-1986 birth cohorts throughout the sample period (so as to simplify the definition of treatment).

Dependent birth date is not directly reported in the Truven data — instead, we back it out using the fact that enrollee age is reported on a monthly basis.[13] In backing out dependent birth month, it is necessary to limit the sample to dependents enrolled for at least 12 months continuously; otherwise, we cannot necessarily observe an age change across months for all birthdates. We also require that the 12+ month period of continuous enrollment occurs prior to 2010 to avoid endogenous selection into the sample by dependent birth date.

We additionally restrict the sample of dependents to those who are enrolled for at least one month prior to 2010 while under the age of 23. We do so because, prior to 2010, plans were required to cover dependents while they were college students, which results in most

11. For example, if a child disenrolls from one parent's plan and re-enrolls under another parent's plan, they would be assigned a different enrollee ID.

12. This requirement ensures we observe enrollment outcomes for primary beneficiaries throughout our sample period, 1/2002-12/2012, as they are dropped from the data when they turn 65.

13. 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.

dependents in our data exiting coverage on their 23rd birthday.[14] Thus, dependents enrolled for the first time at ages older than 23 would likely be covered by state mandates, some of which feature discontinuous enrollment incentives by birth month. Still, because coverage due to state mandates was very small, any impacts would be highly limited.

Our final sample restriction is to keep a subset of data contributors that participate continuously from 2007-2012. New data contributors are added to the Truven sample each year in January, as shown in Appendix Figure A.2. Thus, this step ensures that we avoid selection into the sample by dependent birth date that could arise as a result.[15]

Next, we construct our primary outcome variables, which are measures of insurance coverage from 2010 to 2012. Although the dependent mandate went into effect for plans starting in September 2010, some plans elected to start enrollment early so that students graduating college in the spring of 2010 would not face a coverage gap. Thus, we count enrollment at any time during 2010 (as mentioned above).[16] Our outcome variables, for 2010-2012 as well as each year separately, are as follows: enrollment for at least one month ("any enrollment") and total enrollment days. We construct these measures separately for the primary beneficiary as well as their dependents.

It is important to consider what we can and cannot observe with regards to adult dependent coverage, given the structure of our data. Because we require that all dependents are covered under their parent's plan at some point prior to 2010, our measure of "any enrollment" is in fact an indicator for re-enrollment in a plan held by that same parent. Thus, we do not count adult dependents who enroll in their parent's plan as a result of the ACA

14. In Appendix Figure A.1, we show that nearly all dependents exiting our sample prior to the ACA did so at age 23.

15. Appendix Table A.1 lists, for each birth cohort in our sample (1/1984-12/1986), 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 2002 because that is the first month of the Truven 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 Truven from 2007 to 2012. If we use age 19 as our cut-off, rather than age 23, the sample of contributors is further limited to those that participate from 2003-2012.

16. We also estimate results separately for enrollment during 2010, 2011, and 2012.

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 job after 2010. Similarly, we do not observe coverage provided through other sources, such as the parent's spouse or the adult dependent's employer.

Table 1 presents summary statistics for our main sample. Our sample includes 455,254 distinct dependents and primary beneficiaries. Of these, 26% were born in 1984, 36% were born in 1985, and 39% were born in 1986.

We report means of our outcome variables and controls for the full sample (Column 1) as well as by dependent birth year (Columns 2-4) . The increase in enrollment among dependents from birth year 1984 to birth year 1986 is striking (2% to 31%, or a 1450% increase). Similarly, the number of days enrolled increases dramatically (8.9 to 181.4, or a 1938% increase). There is also an increase in enrollment likelihood among primary beneficiaries, although much smaller, from 58% for those with dependents born in 1984 to 62% for those with dependents born in 1986 (7% increase). Primary beneficiaries' enrollment days increase from 540.4 to 581.4 (8% increase).

As for the control variables, there is little variation across dependent birth cohorts in: female share of dependents (50%) and primary beneficiaries (41%), the total number of dependents (2.1-2.2), and whether a spouse was added to the plan prior to 2010 (72-74%). The primary beneficiary's birthdate increases (i.e., they get younger) with dependent birth cohort, as would be expected.

3.1 The Link between Insurance Disenrollment and Job Exits

We hypothesize that the expansion in coverage for dependents that occurs under the ACA will increase the value of parental employment. Thus, primary beneficiaries who cover adult dependents may be less likely to leave their employer. To test this hypothesis, we would ideally observe parental employment decisions in the post-mandate period. However, the Truven data does not report employment information. Thus, we proxy for whether (and how long) a parent remains with their pre-ACA employer using an indicator for whether (and the

number of days) they retain coverage from any insurer or plan offered by their pre-mandate employer. If primary beneficiaries remain with the same employer but elect to forego health insurance altogether, then our proxy would incorrectly code them as having left their job.

To assess the importance of measurement error in our proxy measure, we turn to a different data source — the Panel Study for Income Dynamics (PSID). The PSID is a longitudinal survey with information on both employment and health insurance. Detailed questions on health insurance were added in 2011 — therefore, we use survey years 2011 to 2019, which is the most recent wave available. The survey was administered every other year during this time period, so our sample combines 5 waves.

The questions on health insurance are asked of heads and spouses of surveyed households. We thus restrict the sample to individuals who are heads or spouses. We then limit the sample to individuals that participated in the survey continually from 2011 to 2019 – doing so allows us to observe employment and health insurance outcomes in all years. We then require that individuals are born from 1948 to 1969, the range of birth cohorts of primary beneficiaries in our Truven sample. Next, we keep individuals who, in the 2011 survey, are employed, serve as the plan holder of an employer-sponsored plan, and cover at least one family member.

For individuals in this sample, we define two outcomes: (1) whether they ever leave the job that provided the employer-sponsored plan in 2011 or (2) whether they ever drop health insurance coverage entirely. Appendix Table A.2 shows the cross tab of these indicators for heads and spouses in our sample. Observation counts reflect sampling weights provided by the PSID.

Of individuals who remained with their employer from 2011 (12,579,614 weighted), only 0.55% drop their employer-sponsored insurance. Thus, it appears that dropping health insurance while remaining with the same employer is very rare. This suggests that it is reasonable to infer that the timing of the end of a primary beneficiary's health insurance coverage coincides with the end of their employment with their current employer.

Going forward, we refer to our proxies for parental employment (i.e., any enrollment in

a plan offered by their pre-mandate employer and duration of enrollment in plans offered by their pre-mandate employer) as "job retention" and "job duration," respectively.

4 Empirical Method

Our empirical strategy is a regression discontinuity (RD) design in which the dependent's birth date serves as the running variable. Following Figure 1 above, dependent coverage eligibility jumps discontinuously at 1/1985 and 1/1986. Thus, we estimate the change in insurance coverage for primary beneficiaries and dependents during 2010-2012 around these cut-offs.

We estimate effects at each cut-off in separate regressions, restricting the sample to a bandwidth of 12 months on either side of the given cut-off. We use triangular weights to assign more importance to dependent birth months closer to the cut-offs.[17]

For a given plan, index the primary beneficiary by i and the dependent by j. Let B_j denote the birthdate for dependent j. We index the two cut-offs in dependent birthdate by c (i.e., c =1/1985 or 1/1986). Denote our enrollment or employment outcomes by E_{ij}. Then, we model E_{ij} as follows:

$$E_{ij} = \alpha + \beta \mathbf{1}[B_j \geq c] + \mathbf{1}[B_j \geq c] f(B_j - c) + f(B_j - c) + X_{ij}\gamma + \epsilon_{ij} \tag{1}$$

$f()$ is a control function in dependent birthdate — in our baseline regressions, $f()$ is linear. This choice is motivated by Figure 1, which suggests that, on average, the duration of additional months of insurance provided by the ACA should increase linearly. The term $\mathbf{1}[B_j \geq c] f(B_j - c)$ allows the slope of the outcome variable in birth month to vary on either side of the cut-off c. X_{ij} is a set of controls: gender of the primary beneficiary and dependent; birth date (year-month) of the primary beneficiary; number of dependents added to the plan before 2010; and whether the spouse was ever added to the plan for 12+ months before 2010. As noted above, the sample is restricted to plans for which the dependent is

17. Specifically, we construct the weights to equal $1 - \frac{\text{distance to cutoff in months}}{12}$. Thus, births in the December right before and the January right after the cutoff receive a weight of 1.

born between January 1984 and December 1985. We adjust ϵ_{ij} to allow for individual-level heteroskedasticity.

The coefficient of interest is β, which measures the effect of an increase in expected insurance coverage for dependents on enrollment and employment decisions. We hypothesize that our estimates of these parameters will be positive, signalling an increase in the probability and length of both enrollment (for dependents) and employment (for primary beneficiaries).

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

Lastly, we perform placebo tests using a similarly constructed sample of dependents born between 1994 and 1996 (as opposed to 1984 and 1986). We estimate the corresponding RD specifications, with dependent birth date cut-offs at 1/1995 (for the sample born from 1/1994-12/1995) and 1/1996 (for the sample born from 1/1995-12/1996). All children from these birth cohorts are under age 19 and 10-12 years younger than those in our main sample. Employers universally offered coverage to dependents under 19 prior to 2010, so these dependents should not be affected by the dependent coverage mandate. For this reason, we should expect to see no break in our main outcomes around the placebo birth-month cut-offs (c = 1/1995 or 1/1996) in the placebo sample.

4.1 Tests of the Identification Assumption

The RD design estimates causal effects by identifying treatment and control groups that are "seemingly identical." In our case, the treatment group is dependents (and primary beneficiaries with a dependent) with a birthdate in January. The control group is dependents (and primary beneficiaries with a dependent) who are born in December of the previous year. The identification assumption is that, absent the effects of the dependent mandate, our outcomes would evolve smoothly around the end-of-year cut-offs in dependent birthdate. Two common ways to test this assumption are: (1) to test whether the density of the running

variable is smooth through the cut-offs and (2) to test whether the composition of sample based on observable characteristics is smooth through the cut-offs.

Examining the density of the running variable and the smoothness of observable characteristics sheds light on whether there may be manipulation or misreporting around the cut-offs, or other reasons for systematic differences that could affect our outcomes (e.g., another policy that affects insurance take-up by birth month). Manipulation of the running variable may occur, for example, if parents with a dependent born in December falsely report a January birth date to receive extra coverage for their child. Then, there should be more January birth months than December birth months.[18] Another possibility is that birth month is misreported (i.e., observed with measurement error). Misreporting should not matter as long as measurement error is random with respect to the December vs. January cut-off. However, if instances of missing birth months are automatically assigned to January, for example, that would be problematic for our analysis. In this case, we should also see that January is reported more frequently than other birth months.

We start by examining the smoothness of the distribution of dependent birth month in our sample. Figure 2 plots the density of dependents by birth month. The distribution appears to be smooth through the two birth date cut-offs. We do not reject the null hypothesis of a smooth density around both cut-offs (the p-value for first cut-off at 1/1985 is 0.905, and the p-value for the second cut-off at 1/1986 cut-off is 0.119).

Next, we examine whether the composition of our sample varies in observable characteristics across the cut-offs in dependent birth date. For observable characteristics, we use the five control variables shown in Table 2, each of which is measured prior to 2010. Figure 3 plots mean of these variables by dependent birth month (as elsewhere, each observation represents a single dependent-primary beneficiary pair). Visually, these graphs appear quite smooth through the birth date cut-offs. We then test for discontinuities by estimating our

18. Note that this particular scenario seems unlikely in the Truven data because our observations of birth month are taken from enrollment data collected prior to the ACA reform – thus parents would have to anticipate the timing of the reform and its rules correctly.

RD specification (Eq. 1), setting the outcome variable E_{ij} equal to the indicated control variable.[19] Estimates of β are reported in Table 2. The magnitudes of these ten estimates are uniformly small and statistically insignificant at the 5% level. Thus, the combination of results in Figure 3 and Table 2 provide strong evidence in favor of our identifying assumption.

5 Results

In this section, we start by examining the effects of the ACA dependent coverage mandate on dependent enrollment outcomes. We then turn to the effects of the mandate on parental employment, as proxied by their enrollment outcomes. For each outcome considered, we present graphical evidence as well as RD estimates from Eq. 1.

5.1 Dependent Enrollment

Figure 4 displays RD graphs for enrollment likelihood (subfigure a) and duration (subfigure b) from 2010-2012 among dependents in our sample. Above, we hypothesized that dependent enrollment should increase at each cut-off because, on average, the additional months of coverage available under the dependent mandate increases discontinuously (so the incentive to enroll increases). On each section of the graph, we display a linear fit line. In Column (1) of Table 3, we report the corresponding estimates of β along with standard errors. Panel A reports estimates for the first birth date cut-off (1/1985) and Panel B reports estimates for the second birth date cut-off (1/1986).

In accordance with our hypothesis, there are increases in enrollment likelihood and duration at each of the birth month cut-offs. The likelihood of enrollment increases 8.6% at the first cut-off (86% of the sample mean) and 2.0 percentage points at the second cut-off (8%). The duration of enrollment increases by 8 days at the first cut-off (26%) and 13.4 days at the second cut-off (11.3%). Each of these estimates is statistically significant at the 1% level.

Next, we explore how our effects on dependent enrollment outcomes vary by enrollment year (i.e., 2010, 2011, or 2012). Doing so allows us to test that insurance enrollment drops

19. These regressions omit the vector of control variables X_{ij}.

when each cohort (born in 1984, 1985 or 1986) turns 26. The RD graphs are shown in Appendix Figure A.3 and the corresponding estimates are reported in Appendix Table A.3.

Note that the 1984 birth cohort turns 26 in 2010, the 1985 cohort turns 26 in 2011, and the 1986 cohort turns 26 in 2012. Thus, we would expect very little enrollment in 2012 for the first two cohorts (who will be over 26 at the time) as well as very low enrollment in 2011 for the 1985 cohort. Any enrollment for these cohorts in these enrollment years would be provided under the pre-existing state mandates, whose effects are limited.

Indeed, these predictions are borne out in subfigure (a) of Appendix Figure A.3. Correspondingly, in Appendix Table A.3, Panel A (1984-1985 cut-off), there is no effect in 2012 on likelihood or duration, as expected.[20] Thus, these results provide direct evidence that the pre-existing mandates provide virtually no coverage in our sample of dependents.

5.2 Parental Employment

Next, we investigate the effects of increases in dependent coverage eligibility on parental employment decisions in the post-ACA period (2010-2012). In particular, as described above, we proxy for the likelihood a parent remains with their pre-ACA employer with an indicator for whether the parent continues to be enrolled in any health plan offered by that employer. Similarly, we proxy with the length of time they remain with their pre-ACA employer with the number of days they are covered by any plan offered by that employer. As noted above, we refer to these outcomes as "job retention" and "job duration."

In Figure 5, we display RD graphs for job retention likelihood and job duration. The corresponding regression estimates, along with standard errors, are reported in Column (1) of Table 3.

We find that the likelihood a primary beneficiary remains with their employer ("job retention") increases by 0.8 percentage points (1.4% of the sample mean) and 1.3 percentage

20. As for the size of the enrollment jumps, we see a considerably larger increase in enrollment likelihood at the second cut-off (Panel B) in 2012 than the other years (14.4 vs. 1.0 in 2011 and 2.4 in 2010) — it is possible this pattern reflects an increase in saliency of the policy over time. As for enrollment duration, the overall pattern is similar.

points (2.1%) at the first and second cut-offs, respectively. Both of these estimates are statistically significant at the 1% level. Correspondingly, our measure of job duration increases by 6.8 days (1.2%) and 9.3 days (1.6%) at the two cut-offs, respectively. Although the magnitudes are relatively similar, the first estimate is imprecise, whereas the second estimate is statistically significant at the 5% level.

To examine these results further, we estimate our RD specifications separately for enrollment years 2010-2012 (as we did for the dependent outcomes above). The results are shown in Appendix Figure A.4 and Appendix Table A.3. As discussed above, dependents born in 1984 and 1985 are ineligible for coverage in 2012 (as they are over 26). Thus, we would expect to find a smaller difference in parental employment outcomes in 2012 at the first cut-off.[21] In Appendix Table A.3, the RD estimates for the first cut-off are smaller and less precise for 2012 compared to 2010 and 2011, as expected.

5.2.1 Robustness Checks

In this section, we probe the robustness of our findings by altering our baseline specification in a number of ways. In particular, as reported in Appendix Table A.4, we estimate the following variations: 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 cut-off months; and replacing our linear control function with a local linear specification.

Column (1) of Appendix Table A.4 reports our baseline estimates, whereas Columns (2)-(7) report the results of the variations listed above. Across all of these specifications, the RD estimates remain highly similar, providing strong evidence in favor of the robustness of our findings.

21. Differences in parental employment decisions (e.g., in 2011) may persist in the short-run (e.g., in 2012), in which case there may still be a difference in job retention across dependent cohorts in 2012.

5.3 Placebo tests

In this section, we estimate RD specifications that are similar to our main estimating strategy but use the placebo sample of dependents born between 1/1994 and 12/1996 (described above in Section 3). In particular, we modify Eq. 1 so that the first cut-off is 1/1995 (rather than 1/1985) and the second cut-off is 1/1996 (rather than 1/1986). Dependents in the placebo sample were under age 19 during 2010 to 2012 and therefore were eligible for coverage on their parent's plan under pre-existing policies. Thus, we predict that their likelihood and length of coverage during 2010-2012 should not change across the birth cohort cut-offs. Correspondingly, we expect no change in job retention and duration for their parent.

Appendix Figures A.5 and A.6 display the RD graphs and Appendix Table A.5 reports the corresponding estimates and standard errors. Consistent with our predictions, the graphs appear flat through the cut-offs. In Appendix Table A.5, the RD coefficients are small and statistically insignificant. Thus, this analysis provides further evidence in favor of the interpretation of our findings.

6 Heterogeneity Analyses and Mechanisms

In this section, we estimate how our main results vary by demographic characteristics of the primary beneficiary and dependent. Doing so allows us to shed light on potential mechanisms underlying our findings.

6.1 Effects by Birth Year of Primary Beneficiary

We first investigate heterogeneity by the birth year of the primary beneficiary. Primary beneficiaries in our sample are born between 1948 and 1969.[22] Thus, they are between the ages of 41 and 64 during 2010-2012.

We hypothesize that individuals approaching early retirement may be particularly responsive to job retention incentives (such as added dependent coverage), as they are actively

22. The lower bound of 1948 is due to the fact that Truven excludes individuals over 65. Individuals born before 1948 are over 65 during the 2010-2012 plan years. The upper bound occurs because we require that there is at least a 16 year age gap between primary beneficiaries and their dependents.

deciding when to retire. Age 55 is an important early retirement age as individuals who retire at age 55 or older can withdraw from their 401(k) without penalty.[23] Correspondingly, we divide our sample into three groups by birth cohort: 1/1948-6/1952; 7/1952-12/1957, and 1/1958-12/1969. The first group is always over 55 during 2010-2012 and thus will have already decided not to retire at that age. The second group (1952-1957) includes individuals who turn 55. The third group is always under 55.

We display the results from our RD regressions for these three different samples in Figures A.7-A.8 and Appendix Table A.6. For primary beneficiaries, there are noticeably larger jumps in Figure A.8 for the middle group of birth cohorts. Our estimates suggest that, for the middle cohort, at the second cut-off, job duration increases by 14.5 days (2.5%) and job retention likelihood increases by 1.7 percentage points (2.7%). By comparison, the effects are small and statistically imprecise for the older group of primary beneficiaries. For the younger group, the effects are somewhat smaller and generally less precise than for the middle group. Thus, these results suggest that individuals approaching early retirement age are most responsive to job retention incentive provided by the expansion of dependent coverage.

6.2 Effects by Number of Dependents Added to a Plan Before 2010

Second, we examine how our results vary by the number of dependents added to a given plan before 2010. In particular, we examine whether parents are more responsive in their job retention decisions depending on the number of children they have. While we do not observe the exact number of children a parent has, we can proxy for this outcome by counting the number of dependents they cover under their plan prior to 2010.

Parents may be more likely to extend the length of their jobs to provide insurance coverage for their dependent when that dependent is their only child. This conjecture follows from the simple intuition that parents must consider the needs of all of their children when making decisions — thus they can be less responsive to each individual child when there are multiple.

23. Indeed, in the Truven data prior to 2010, the most common age of early retirement (which we define as a job exit between the age of 45 and 64) is 55. The second most common age is 50.

On the other hand, it is possible that parents with multiple children will be more likely to extend their job length to cover those children because the costs may be lower. In particular, many plans charge a lower premium for adding additional dependents after the first one (Akosa Antwi, Moriya, and Simon 2013; Depew and Bailey 2015).

In Appendix Figures A.9-A.10, we display separate RD graphs for the subset of primary beneficiaries who added one or more than one dependents prior to 2010. Appendix Figure A.9 shows RD graphs for dependent enrollment and Appendix Figure A.10 shows RD graphs for job retention for the primary beneficiary. Appendix Table A.7 reports the corresponding regression estimates.

In Appendix Figure A.9, the coverage increases for dependents in single child and multiple child households appear roughly similar. In Appendix Figure A.10, however, the effects on job retention and duration for the primary beneficiary differ starkly across the two graphs, with clear jumps for single-child parents and no discernible jumps for multiple-child parents. Correspondingly, Appendix Table A.7 reports very different effects for the primary beneficiary by number of children, particularly at the second cut-off. In particular, for primary beneficiaries with one child, job retention likelihood increases at the second cut-off by 2.8 percentage points (4.7%), and job retention duration increases by 16.3 days (3.0%). The corresponding effects for primary beneficiaries with multiple children, neither of which are statistically significant, are 0.8 percentage points (1.3%) and 7.0 days (1.2%).

The fact that the likelihood and duration of dependent enrollment increases when there are multiple children indicates that many parents are still responding to additional dependent coverage availability by adding their children. On average, however, parents with multiple children are less than parents with only children likely to stay with their current employer when offered additional dependent coverage.

7 Conclusion

In this paper, we study the effect of increased coverage for adult dependents under the Affordable Care Act on parental "job lock." To do so, we compare job retention outcomes across parents who have adult dependents born in January vs. December, as the former gained access to discontinuously more months of dependent coverage. We also estimate the effects on coverage take-up by the adult dependents.

Our dataset is a large panel of employer-sponsored insurance claims and enrollment records. This unique dataset allows us to link together primary beneficiaries (i.e., parents) and their adult dependents. We can also follow primary beneficiaries over time as long as they remain with the same employer and do not forego insurance coverage altogether. We use this facet of the data to create proxy measures for job retention among parents.

We first show that adult dependents are more likely to take up coverage when they are eligible for more months. They also remain enrolled on their parent's plan for longer. Additionally, we find evidence that parents are more likely to remain with their pre-ACA employer and remain for a longer period of time. These results suggest an increase in "job lock" as a result of tying adult children's insurance coverage to their parent's employer.

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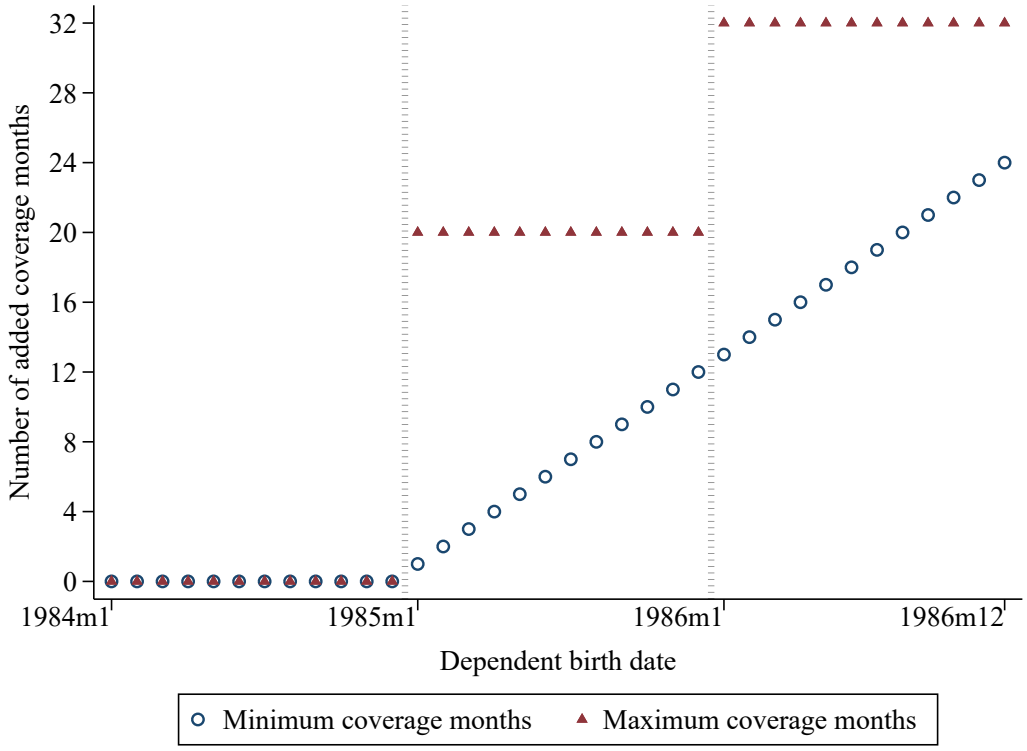
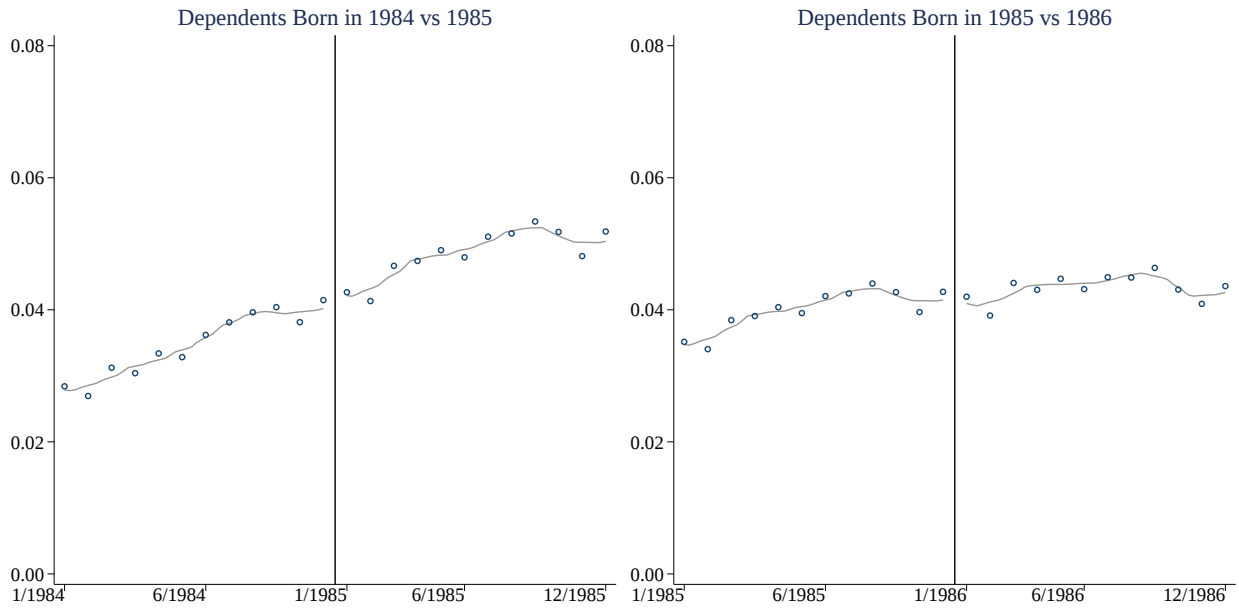
Figure 1: Additional Coverage Months Provided by the ACA, 2010-2012



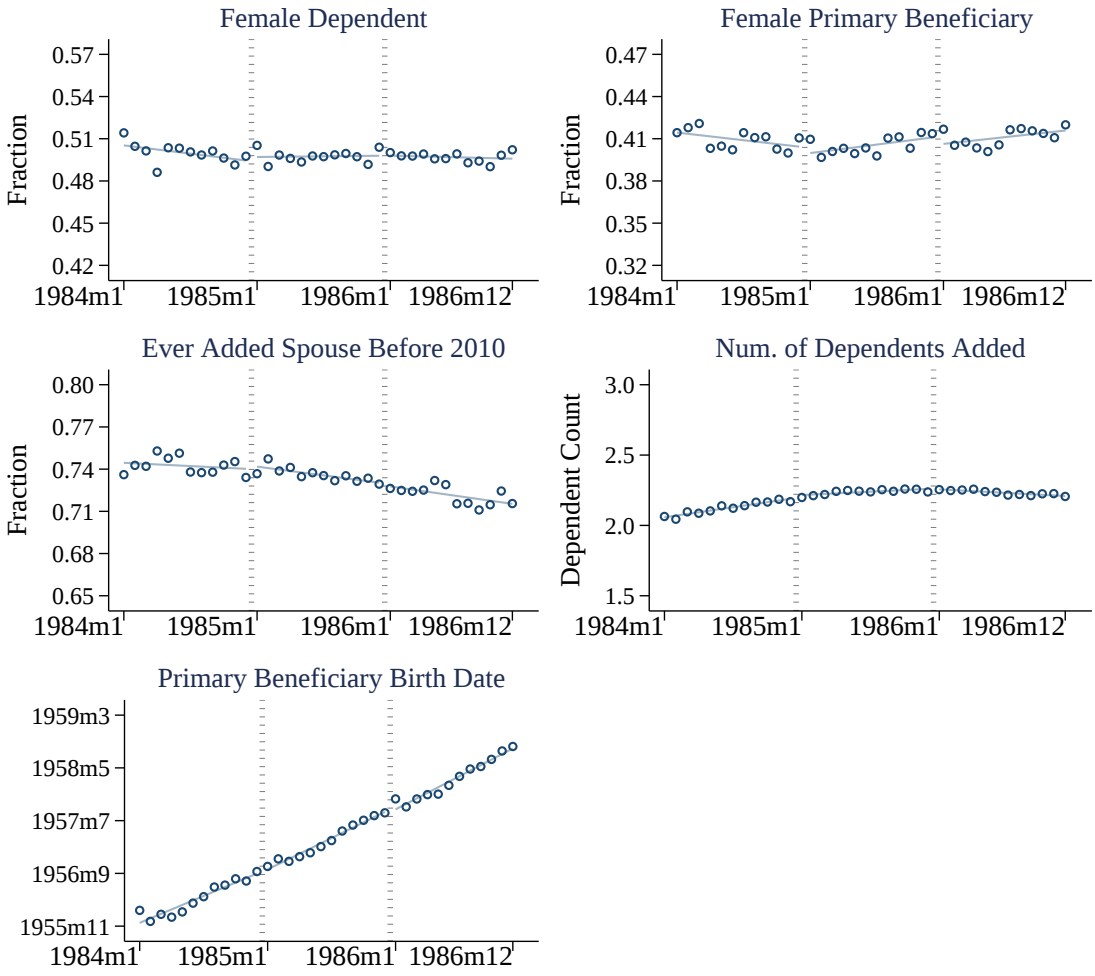
Notes: This graphs displays the range of additional coverage months available to adult dependents, by dependent birth-date, from 2010-2012. The number of months available depends on plan-specific policies. "Minimum coverage months" refers to plans that extend coverage through the month in which the dependent turns 26. "Maximum coverage months" refers to plans in which the dependent is allowed to remain enrolled until the end of the year in which they turn 26. It also incorporates the fact that some plans extended coverage to college students starting in 5/2010. The two vertical lines indicate the following birth dates: 1/1985 and 1/1986, which serve as the RD cut-offs in our econometric model. Please see the text for additional description and examples.

Figure 2: McCrary Density Test



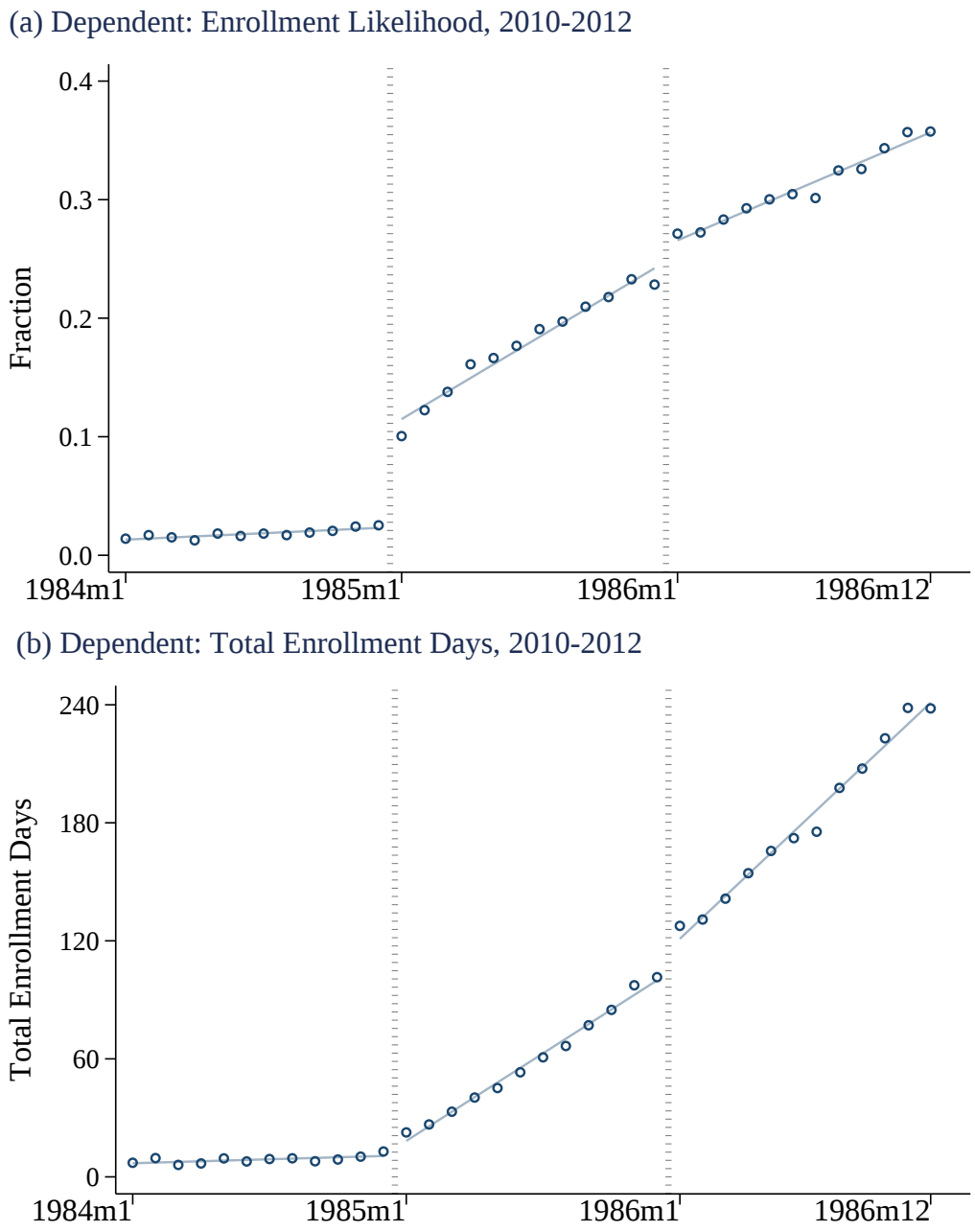
Notes: These figures display the density of dependents by their birth month. We conduct a McCrary density test in Stata by using DCDensity.ado prepared by Justin McCrary and Brian Kovak. The sample used to create the graph shown on the left includes dependents born from 1/1984 to 12/1985, whereas the right panel is based on the sample of dependents born from 1/1985 to 12/1986. The discontinuity estimates from the McCrary density test are -0.0018 (standard error=0.0143, p-value=0.905) for the left-hand graph and -0.0224 (standard error=0.0129, p-value=0.119) for the right-hand graph, respectively. Please see the notes to Table 1 for more information on the data source.

Figure 3: Demographic Characteristics by Birth Month



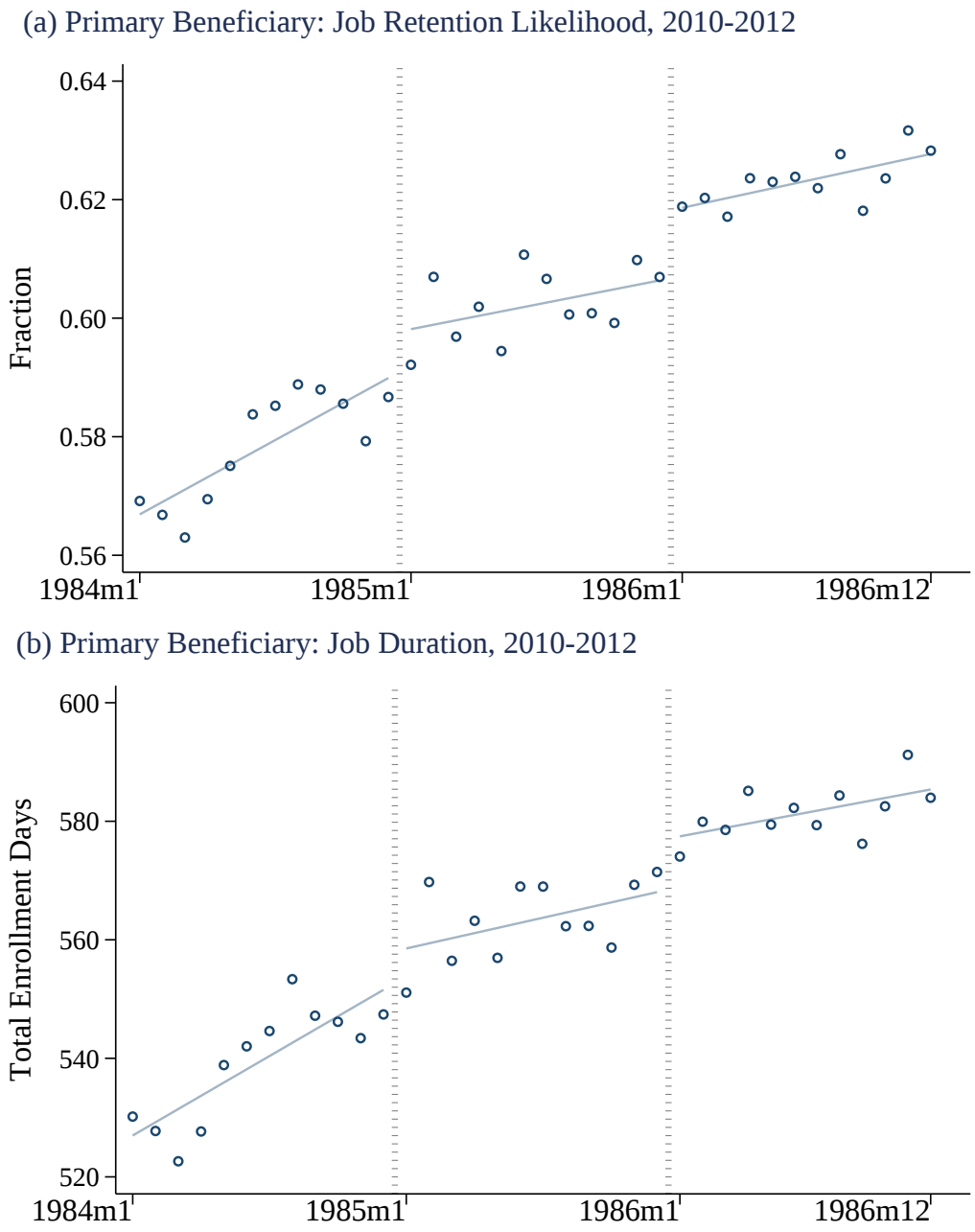
Notes: Demographic measures, which were used as controls in the regressions, are used as outcome variables to test whether covariates are continuous around the birth date cut-offs. These dependent variables include indicator of gender of the primary beneficiary and the dependent, birth year of the plan holder, number of dependents added to the plan before 2010, and whether the spouse was ever added to a plan before 2010. Table 2 reports the results when these measures used as dependent variables in our regressions. Please see the notes to Table 1 for more information on the data source.

Figure 4: Dependent Enrollment Outcomes, 2010-12



Notes: This figure displays RD graphs for two measures of insurance enrollment by adult dependents in our sample. In the top graph, the outcome is an indicator for whether the dependent is covered for at least one month from 2010-2012 on their parent's plan. The outcome in the bottom graph is the total days of enrollment from 2010-2012. Please see the notes to Table 1 for more information on the data source.

Figure 5: Parental Job Retention Outcomes, 2010-12



Notes: This figure displays RD graphs for two measures of job retention by parents (primary beneficiaries) in our sample. In the top graph, the outcome is an indicator for whether the primary beneficiary stays with their pre-ACA employer for at least one month from 2010-2012. In the bottom graph, the outcome is the total days the parent stays at that job from 2010 to 2012. Please see the notes to Table 1 for more information on the data source.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Full Sample	By Dependent Birth Cohort		
		1984	1985	1986
1) Any Enrollment, 2010-12				
Dependent	0.19	0.02	0.18	0.31
Primary Beneficiary	0.60	0.58	0.60	0.62
2) Total Enrollment Days, 2010-12				
Dependent	94.08	8.91	60.54	181.43
Primary Beneficiary	564.44	540.36	563.37	581.36
3) Control Variables				
Female Dependent	0.50	0.50	0.50	0.50
Female Primary Beneficiary	0.41	0.41	0.41	0.41
Spouse Was Added Before 2010	0.73	0.74	0.74	0.72
Number of Dependents Added Before 2010	2.21	2.13	2.24	2.23
Primary Beneficiary Birth Date	5/1957	5/1956	3/1957	3/1958
Observations	455,254	116,510	162,703	176,041

Notes: The data source is privately-insured employed-sponsored plan claims from the Truven Health MarketScan Commercial Claims and Encounters Database. These data include employer-sponsored insurance claims for individuals under the age of 65. The sample is restricted to data contributors who are large employers (i.e., not health insurers) that continuously participated in Truven from 2007 to 2012. Each observation is a dependent-primary beneficiary pair. To be included in the sample, dependents must: (1) be born from 1/1984 to 12/1985; (2) be covered by the primary beneficiary for at least 12 months prior to 2010; and (3) be covered by the primary beneficiary while under the age of 23 prior to 2010. Panel 1 and 2 provide summary statistics for our main outcome variables. "Any enrollment" is an indicator for at least one month of enrollment from 2010-2012 (i.e., the post-ACA period). Panel 3 provides summary statistics for control variables used in our regression. Column (1) shows the descriptive statistics for the full sample and columns (2)-(4) display them by dependent birth cohort.

Table 2: Tests for Covariate Balance Around the Cut-offs

	(1)	(2)	(3)	(4)	(5)
	Female Dependent	Female Primary Beneficiary	Ever Added Spouse Before 2010	Num. Dependent Added Before 2010	Primary Beneficiary Birth Year
	b/se	b/se	b/se	b/se	b/se
Panel A. Dependents born in 1/1984-12/1985					
RD estimate	0.0031	-0.0036	0.0043	0.0149*	-0.3653
	(0.0041)	(0.0041)	(0.0036)	(0.0087)	(0.4520)
Observation	279,213	279,213	279,213	279,213	279,213
Panel B. Dependents born in 1/1985-12/1986					
RD estimate	-0.0002	-0.0064*	-0.0013	0.0028	-0.4253
	(0.0037)	(0.0037)	(0.0033)	(0.0081)	(0.4305)
Observation	338,744	338,744	338,744	338,744	338,744
Controls	Yes	Yes	Yes	Yes	Yes
Weighting scheme	None	None	None	None	None
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo
Degree of polynomial	1	1	1	1	1

Notes: This table reports estimates of β from a version of Eq. 1 that excludes control variables. Each coefficient and standard error is from a separate regression. Each control variable is used as a dependent variable in a separate regression. Robust standard errors are reported in parentheses. Please see the notes to Tables 1 and 3 for more information on the data source and RD specification. * p<0.10, ** p<0.05, *** p<0.01

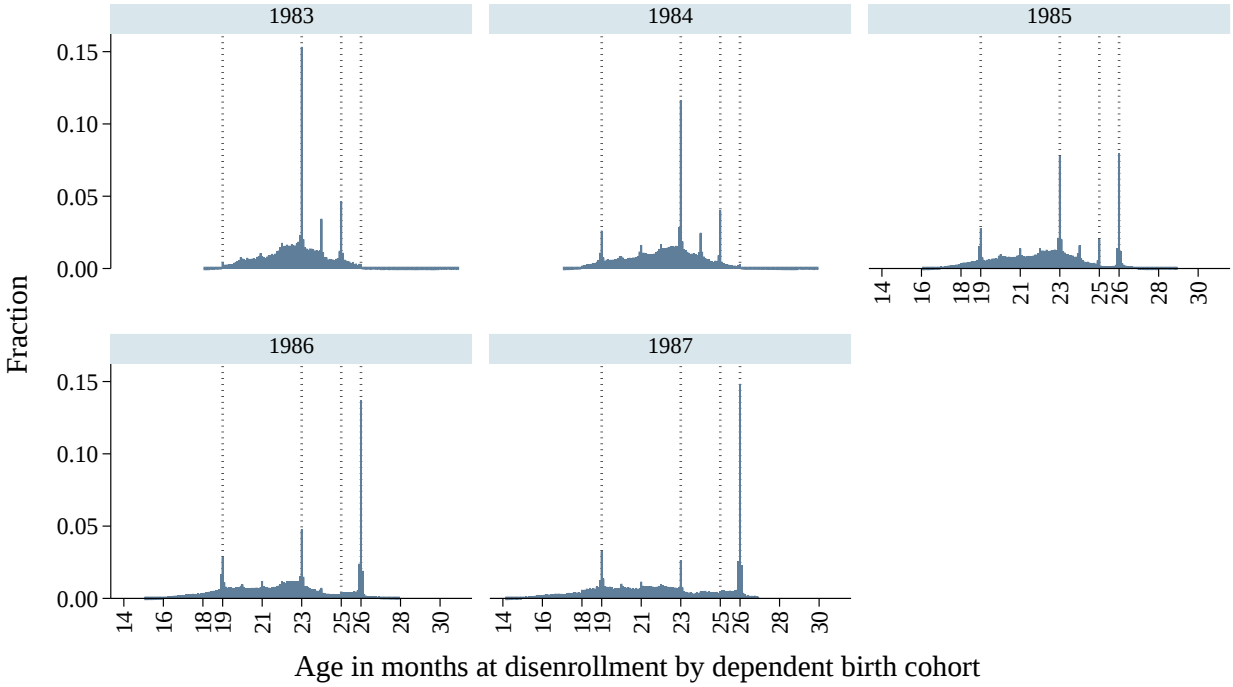
Table 3: Impact of the ACA Dependent Mandate on Parents and Their Dependents

	(1)	(2)
	RD Estimate (b/se)	Sample Mean
Panel A. Dependents born in 1/1984-12/1985		
(1) Dependent: Enrollment Likelihood, 2010-12	0.0856***	0.10
	(0.0019)	
(2) Dependent: Enrollment Duration, 2010-12	7.9972***	30.38
	(0.8588)	
(3) Primary Beneficiary: Job Retention Likelihood, 2010-12	0.0082**	0.59
	(0.0040)	
(3) Primary Beneficiary: Job Duration, 2010-12	6.7777	553.93
	(4.1813)	
Observation	279,213	
Panel B. Dependents born in 1/1985-12/1986		
(1) Dependent: Enrollment Likelihood, 2010-12	0.0204***	0.25
	(0.0032)	
(2) Dependent: Enrollment Duration, 2010-12	13.4573***	118.48
	(1.6911)	
(3) Primary Beneficiary: Job Retention Likelihood, 2010-12	0.0129***	0.61
	(0.0036)	
(4) Primary Beneficiary: Job Duration, 2010-12	9.4727**	572.64
	(3.7497)	
Observation	338,744	
Controls	Yes	
Weighting scheme	Triangular	
Bandwidth	±12 mo	
Degree of polynomial	1	

Notes: In this table, we report estimates of β from our regression discontinuity design, Eq. 1. Each coefficient and standard error is from a separate regression. In Panel A, the cut-off value c is set to 1/1985. In Panel B, the cut-off value c is set to 1/1986. The running variable is the dependent birth month, re-centered at zero at January 1985 in Panel A and January 1986 in Panel B. The sample for each specification is restricted to a bandwidth of 12 months around each cut-off variable. Each regression includes a linear trend in birth month on each side of the RD thresholds. In addition, we add the following controls: gender of dependents and plan holder, birth year of primary beneficiary, number of dependents added before 2010 and an indicator of whether a spouse was ever added prior to 2010. Column (2) reports the sample mean of each outcome variable. Standard errors are adjusted for individual-level heteroskedasticity. The corresponding RD graphs are shown in Figure 4 (dependent enrollment outcomes) and Figure 5 (parental job retention outcomes). * p<0.10, ** p<0.05, *** p<0.01

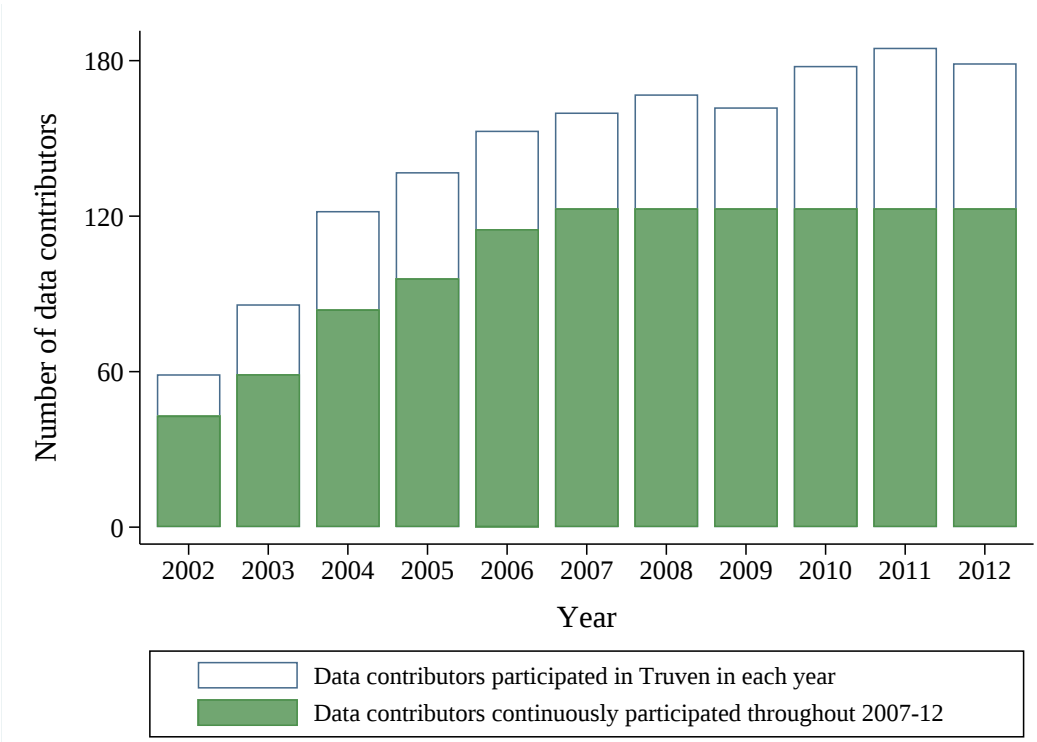
Appendix

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



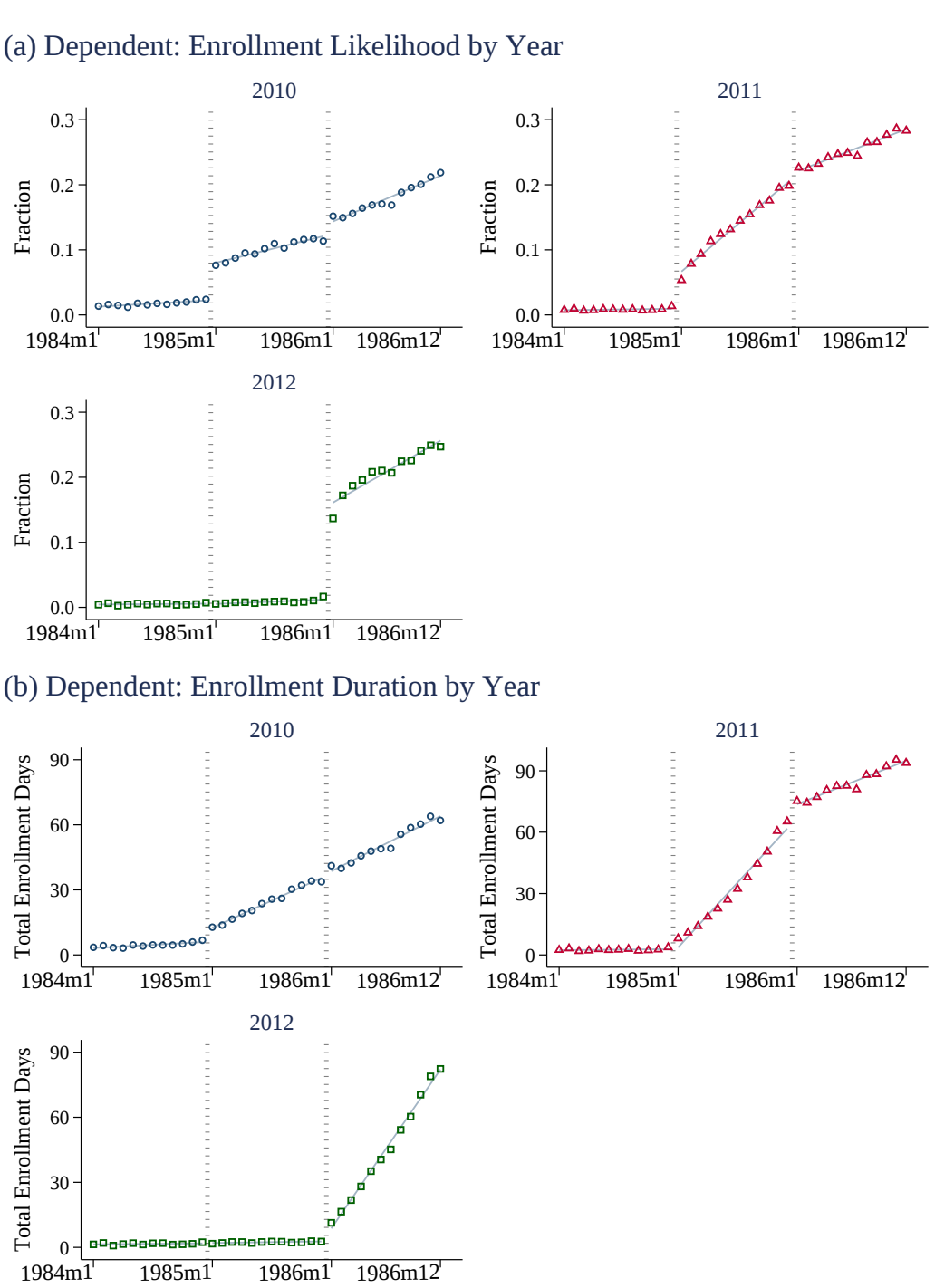
Notes: The figure shows the distribution of dependents' age at disenrolling from their parents' plan by dependent birth year. Dependents born in 1983 or 1984 are more likely to disenroll from their family plan during the month they turn age 23 than those born in or after 1985: 15.3% for 1983 birth cohort, 11.7% for the 1984 cohort, where as 7.8% for the 1985 cohort, 4.8% for the 1986 cohort, and 2.6% for the 1987 cohort. By comparison, dependents born in or after 1985 are more likely to disenroll from their family plans during the month they turn 25 years old or later: 11.1% (1983 birth cohort) 8.4% (1984) VS 16.8% (1985), 25.1% (1986), 27.5% (1987). Please see the notes to Table 1 for more information on the data source.

Figure A.2: Data Contributors, Truven Data



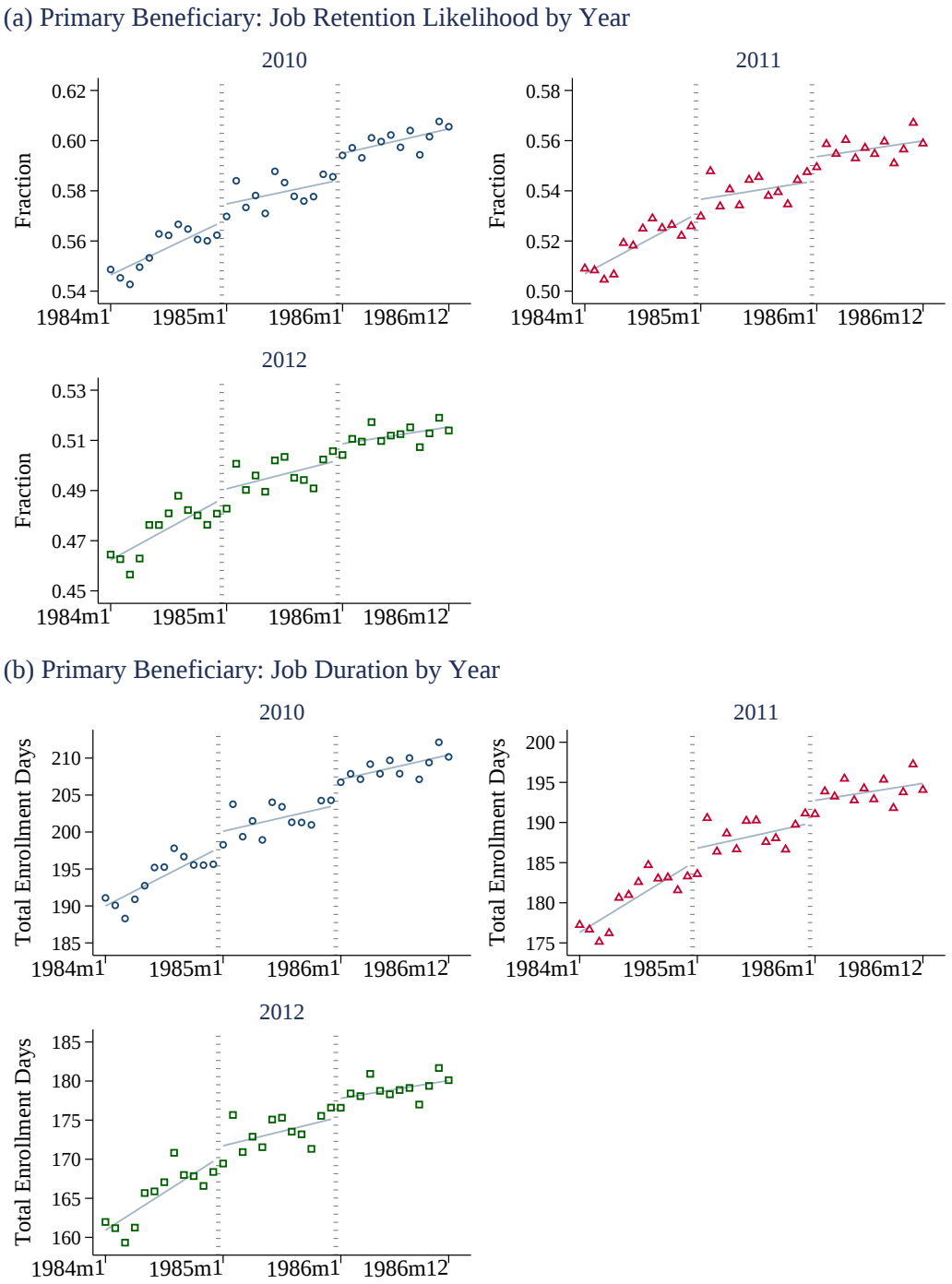
Notes: There are 236 data contributors that ever participated in Truven during 2012-2012. Among 123 data contributors that continuously participated in Truven throughout 2007-2012, 109 of these data suppliers are employers, not insurance companies. The Truven data adds new data contributors every year. Appendix Table A.1 lists, for each birth date, the range of dates during which a dependent must be observed to be under age 23 and enrolled on their parents plan to be included in our analysis sample. This table makes it clear that 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). To avoid selection into the sample by dependent birth date, the sample is restricted to plan holders whose data contributors continuously participate in Truven from 2007 to 2012. Please see the notes to Table 1 for more information on the data source.

Figure A.3: Dependent Enrollment Decision by Enrollment Year



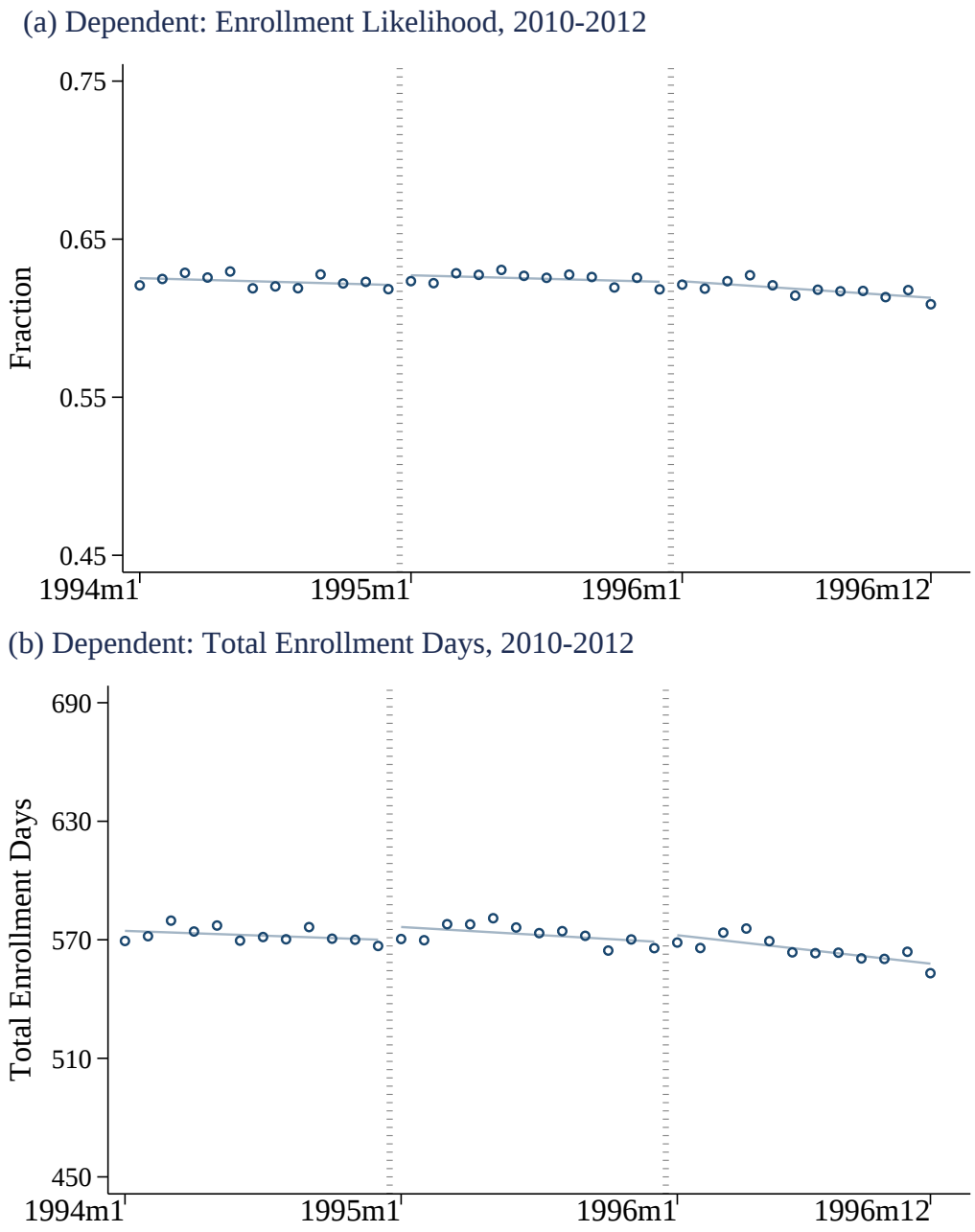
Notes: This figure displays RD graphs for two measures of insurance enrollment by adult dependents by enrollment year (i.e., 2010, 2011, or 2012). We explore how the effects on dependent enrollment vary by enrollment year to test that insurance enrollment drops when each dependent birth cohort turns 26 (the 1984 birth cohort turns 26 in 2010, the 1985 cohort turns 26 in 2011, and the 1986 cohort turns 26 in 2012). In the top graph, the outcome is an indicator for whether the dependent is covered for at least one month from 2010-2012 on their parent's plan. The outcome in the bottom graph is the total days of enrollment from 2010-2012. Appendix Table A.3 reports the results when these measures used as dependent variables in our regressions.

Figure A.4: Primary Beneficiary Job Retention Decision by Enrollment Year



Notes: This figure displays RD graphs for two measures of parental job retention by enrollment year (i.e., 2010, 2011, or 2012). We explore how the effects on dependent enrollment vary by enrollment year to test that insurance enrollment drops when each dependent birth cohort turns 26 (the 1984 birth cohort turns 26 in 2010, the 1985 cohort turns 26 in 2011, and the 1986 cohort turns 26 in 2012). In the top graph, the outcome is an indicator for whether the primary beneficiary stays with their pre-ACA employer for at least one month from 2010-2012. In the bottom graph, the outcome is the total days the parent stays at that job from 2010 to 2012. Appendix Table A.3 reports the results when these measures used as dependent variables in our regressions.

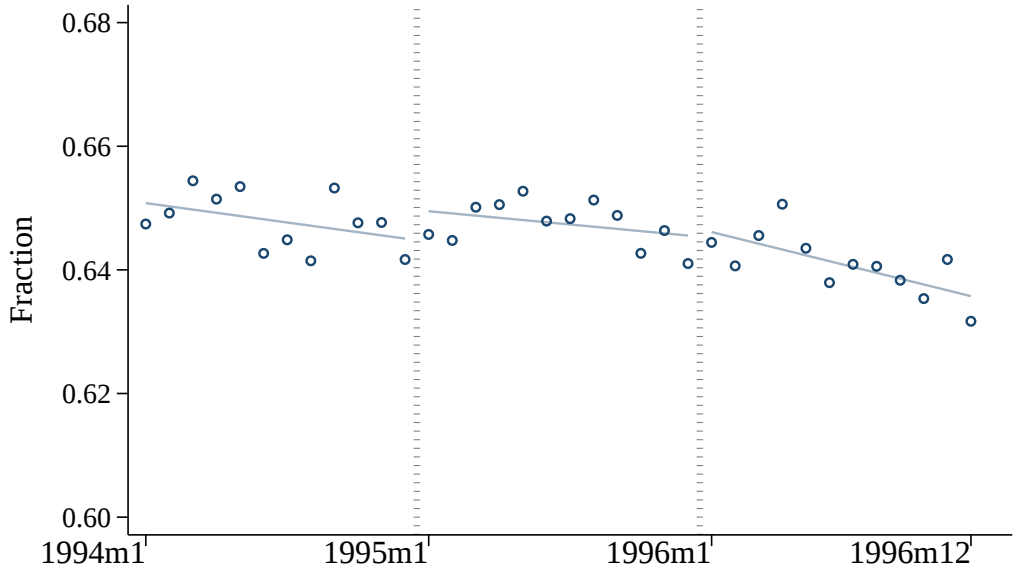
Figure A.5: Placebo test: Dependent Enrollment Outcomes, 2010-12



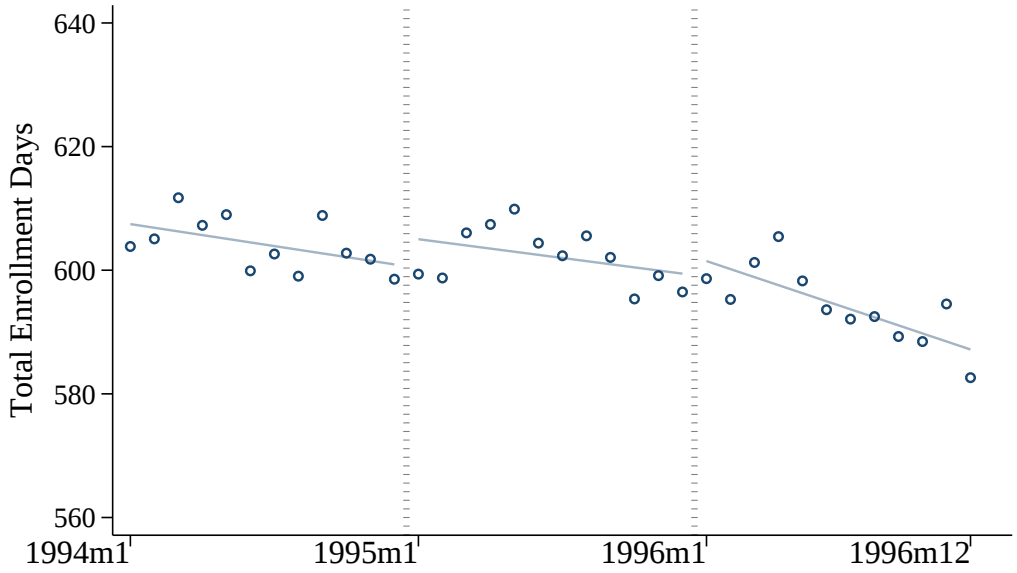
Notes: This figure displays RD graphs for two measures of insurance enrollment by adult dependents in our placebo sample. The sample for placebo test consists of dependents born between 1/1994 and 12/1996. The RD cut-off values are 1/1995 and 1/1996. In the top graph, the outcome is an indicator for whether the dependent is covered for at least one month from 2010-2012 on their parent's plan. The outcome in the bottom graph is the total days of enrollment from 2010-2012. The corresponding RD estimates are reported in Appendix Table A.5.

Figure A.6: Placebo test: Parental Job Retention Outcomes, 2010-12

(a) Primary Beneficiary: Job Retention Likelihood, 2010-2012

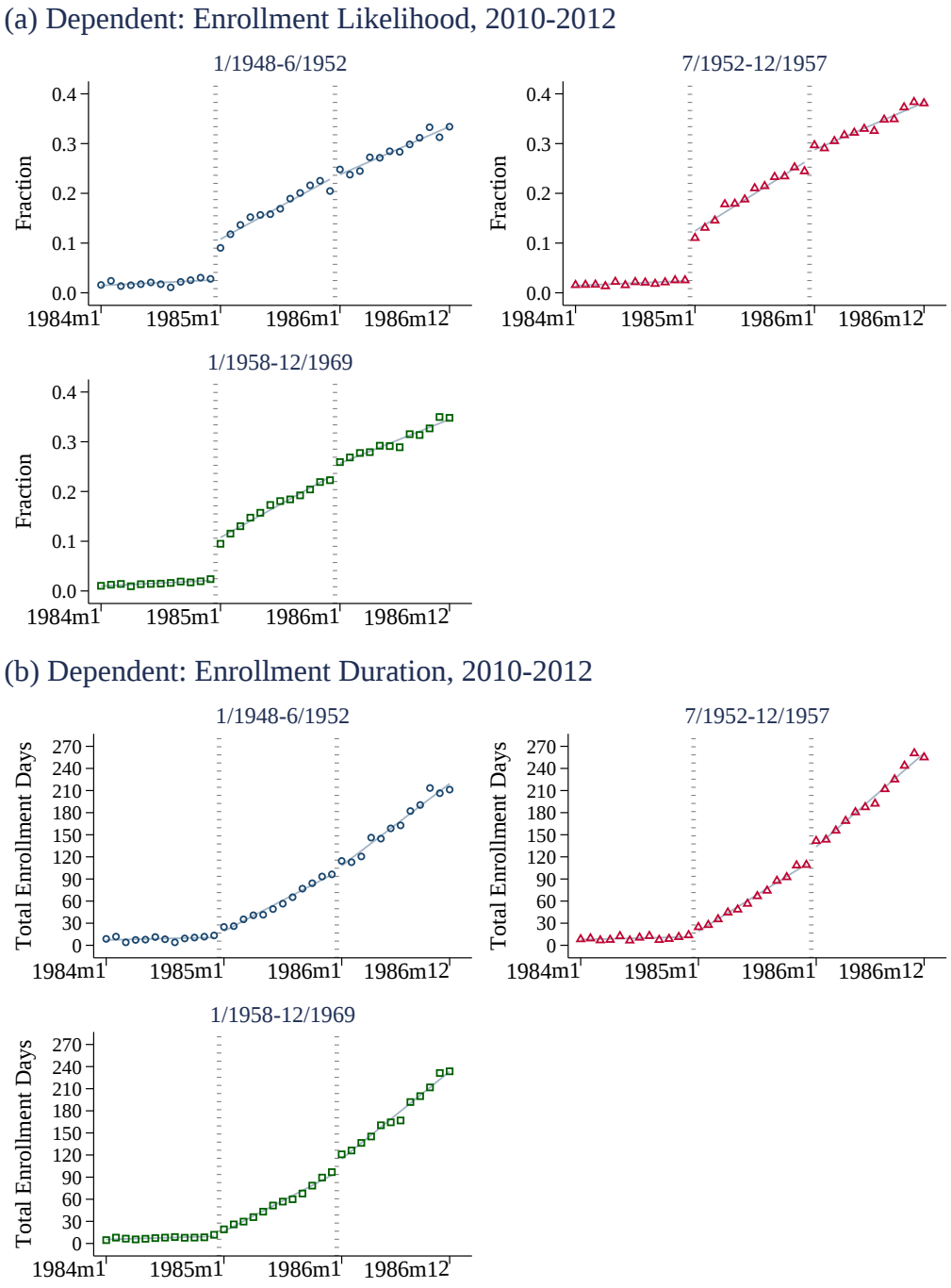


(b) Primary Beneficiary: Job Duration, 2010-2012



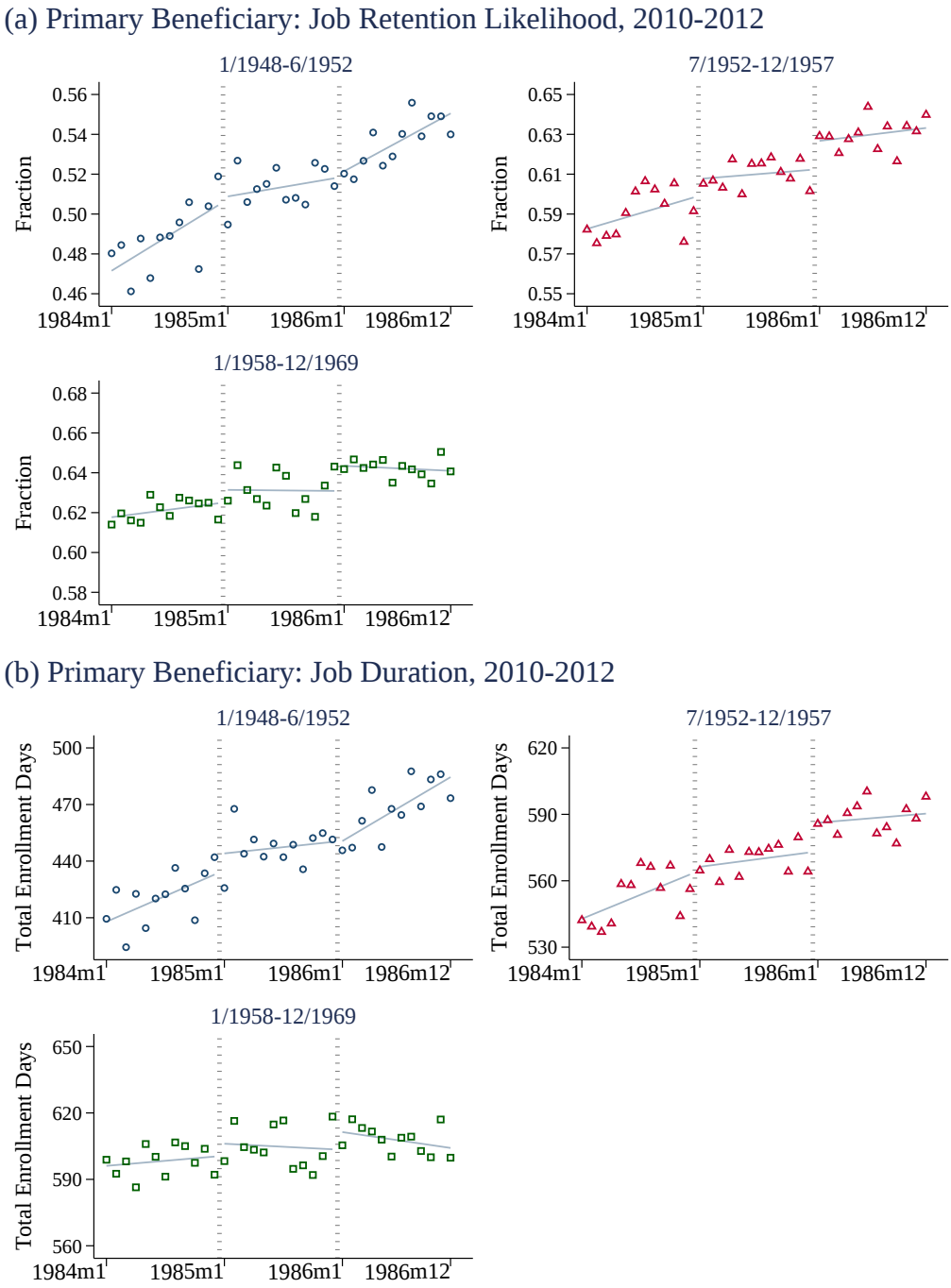
Notes: This figure displays RD graphs for two measures of job retention by parents (primary beneficiaries) in our placebo sample. The sample for placebo test consists of dependents born between 1/1994 and 12/1996. The RD cut-off values are 1/1995 and 1/1996. In the top graph, the outcome is an indicator for whether the primary beneficiary stays with their pre-ACA employer for at least one month from 2010-2012. In the bottom graph, the outcome is the total days the parent stays at that job from 2010 to 2012. The corresponding RD estimates are reported in Appendix Table A.5.

Figure A.7: Dependent Enrollment Decision by Primary Beneficiary Birth Cohort, 2010-12



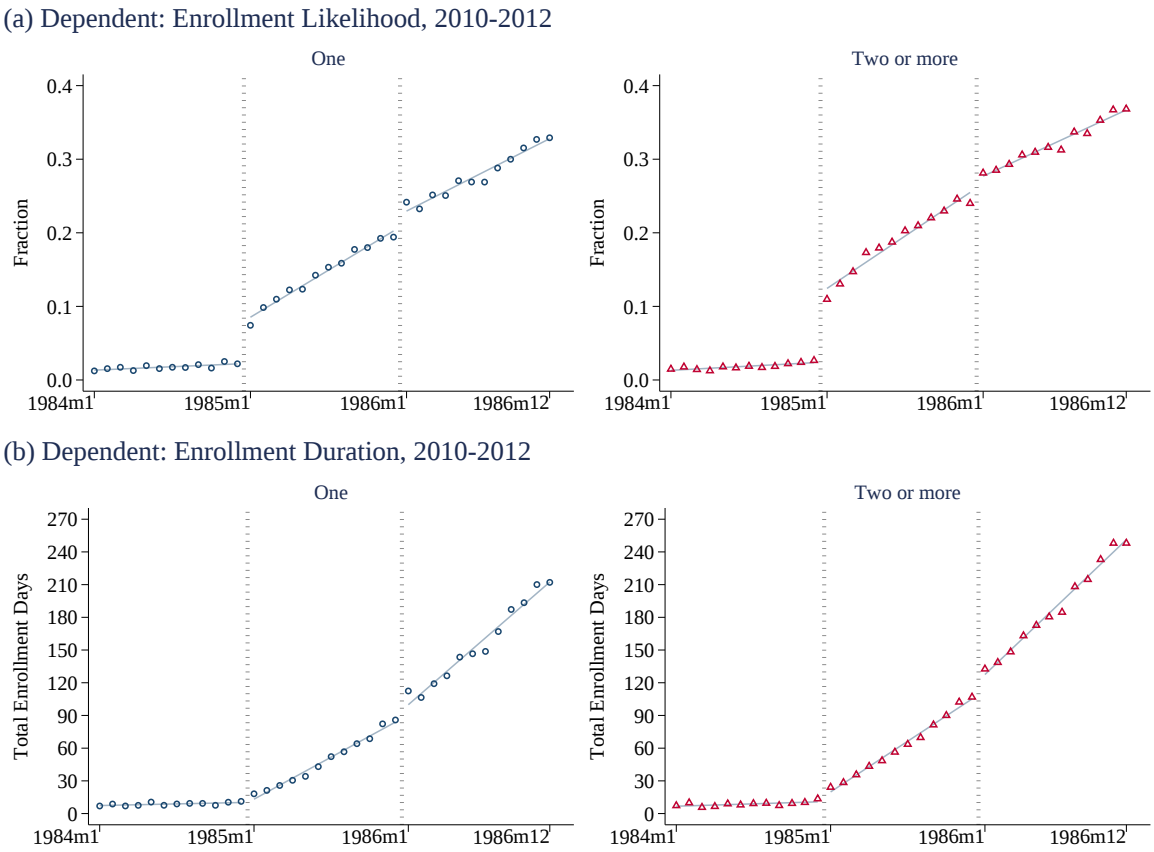
Notes: This figure displays RD graphs for two measures of insurance enrollment by adult dependents across three primary beneficiary birth year groups: 1/1948-6/1952, 7/1952-12/1957, and 1/1958-12/1969. The first group is always over 55 during 2010 to 2012 and thus will have already decided not to retire at that age. The second group includes individuals who turn 55. The third group is always under 55 throughout the sample period. The outcome in the top graph is an indicator for whether the dependent is covered for at least one month from 2010-2012 on their parent's plan. The outcome in the bottom graph is the total days of enrollment from 2010-2012. Appendix Table A.6 reports the results when these measures used as dependent variables in our regressions.

Figure A.8: Primary Beneficiary Job Retention Decision by Primary Beneficiary Birth Cohort , 2010-12



Notes: This figure displays RD graphs for two measures of parental job retention insurance by birth year of primary beneficiaries: 1/1948-6/1952, 7/1952-12/1957, and 1/1958-12/1969. The first group is always over 55 during 2010 to 2012 and thus will have already decided not to retire at that age. The second group includes individuals who turn 55. The third group is always under 55 throughout the sample period. In the top graph, the outcome is an indicator for whether the primary beneficiary stays with their pre-ACA employer for at least one month from 2010-2012. In the bottom graph, the outcome is the total days the parent stays at that job from 2010 to 2012. Appendix Table A.6 reports the results when these measures used as dependent variables in our regressions.

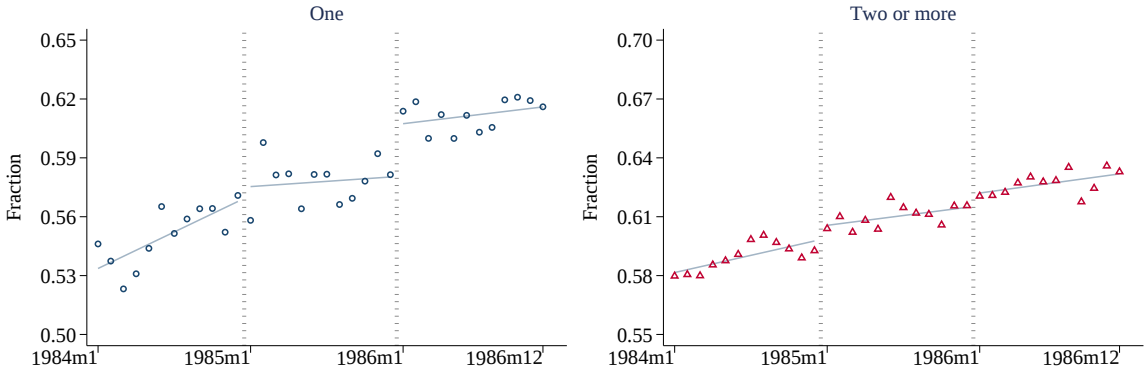
Figure A.9: Dependent Enrollment Decisions by Number of Dependents Added Before 2010, 2010-12



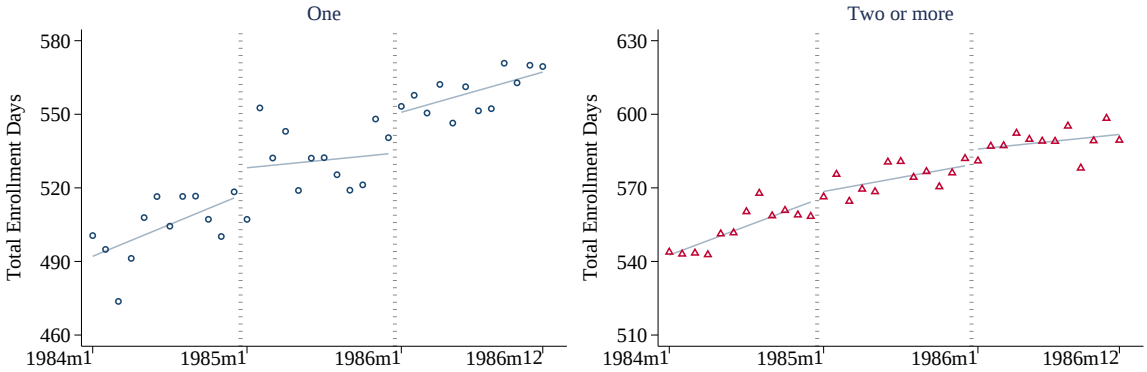
Notes: This figure displays RD graphs for two measures of insurance enrollment by the number of dependents added to a given plan before 2010. We displays separate RD graphs for the subset of primary beneficiaries who added one (left panel) or more than one dependents (right panel). In the top graph, the outcome is an indicator for whether the dependent is covered for at least one month from 2010-2012 on their parent's plan. The outcome in the bottom graph is the total days of enrollment from 2010-2012. Appendix Table A.7 reports the results when these measures used as dependent variables in our regressions.

Figure A.10: Parental Job Decisions by Number of Dependents Added Before 2010, 2010-12

(a) Primary Beneficiary: Job Retention Likelihood, 2010-2012



(b) Primary Beneficiary: Job Duration, 2010-2012



Notes: This figure displays RD graphs for two measures of parental job retention by the number of dependents added to a given plan before 2010. We present separate RD graphs for the subset of primary beneficiaries who added one (left panel) or more than one dependents (right panel). In the top graph, the outcome is an indicator for whether the primary beneficiary stays with their pre-ACA employer for at least one month from 2010-2012. In the bottom graph, the outcome is the total days the parent stays at that job from 2010 to 2012. Appendix Table A.7 reports the results when these measures used as dependent variables in our regressions.

Table A.1: Range of Dates Dependents Under Age 23

Dependent Birth Date (Month/Year)	Enrollment Claim Observation Period Under Age 23 in Truven (Month/Year)
1/1984	1/2002-1/2007
2/1984	1/2002-2/2007
3/1984	1/2002-3/2007
4/1984	1/2002-4/2007
5/1984	1/2002-5/2007
6/1984	1/2002-6/2007
7/1984	1/2002-7/2007
8/1984	1/2002-8/2007
9/1984	1/2002-9/2007
10/1984	1/2002-10/2007
11/1984	1/2002-11/2007
12/1984	1/2002-12/2007
1/1985	1/2002-1/2008
2/1985	1/2002-2/2008
3/1985	1/2002-3/2008
4/1985	1/2002-4/2008
5/1985	1/2002-5/2008
6/1985	1/2002-6/2008
7/1985	1/2002-7/2008
8/1985	1/2002-8/2008
9/1985	1/2002-9/2008
10/1985	1/2002-10/2008
11/1985	1/2002-11/2008
12/1985	1/2002-12/2008
1/1986	1/2002-1/2009
2/1986	1/2002-2/2009
3/1986	1/2002-3/2009
4/1986	1/2002-4/2009
5/1986	1/2002-5/2009
6/1986	1/2002-6/2009
7/1986	1/2002-7/2009
8/1986	1/2002-8/2009
9/1986	1/2002-9/2009
10/1986	1/2002-10/2009
11/1986	1/2002-11/2009
12/1986	1/2002-12/2009
Data contributor with continuous participation in Truven	2007-2012

Notes: We list, for each birth cohort in our sample (1/1984-12/1986), the range of enrollment months during which we could conceivably observe them enrolled on their parent's plan while under the age of 23. Considering new data contributors are included to the Truven sample each year in January as shown in Appendix Figure A.2, 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). To avoid selection into the sample by dependent birth date, the sample is restricted to plan holders whose data contributors continuously participate in Truven from 2007 to 2012. Please see the notes to Table 1 for more information on the data source.

Table A.2: Health Insurance Coverage and Job Exits, PSID

	Drops Insurance		Total
Ever Leave Job	No	Yes	
No	12,510,499	69,115	12,579,614
Yes	20,876,550	2,412,172	23,288,722
Total	33,387,049	2,481,287	35,868,336

Notes: The source of data is the Panel Study of Income Dynamics, Waves 2011-2019. The sample is limited to heads and spouses, born between 1948 and 1969, who are policy holders of an employer-sponsored plan that covers at least one family member. "Ever Leave Job" is an indicator for whether individuals in this sample ever leave the job that provides the employer-sponsored plan. "Drops Insurance" is an indicator for whether individuals ever drop insurance coverage while remaining at their job. 2011 PSID cross-sectional individual weights were used for the analysis. Please see the text in Section 3.1 for additional descriptions.

Table A.3: Results by Enrollment Year (2010-2012)

	(1)	(2)	(3)
	RD Estimate	Standard Error	Sample Mean
Panel A. Dependents born in 1/1984-12/1985			
(1) Dependent: Enrollment Likelihood by Enrollment Year			
2010	0.0541***	(0.0017)	0.06
2011	0.0535***	(0.0015)	0.07
2012	-0.0001	(0.0006)	0.01
(2) Dependent: Total Enrollment Days by Enrollment Year			
2010	5.8327***	(0.4351)	13.78
2011	2.1574***	(0.3499)	14.56
2012	0.0396	(0.2180)	2.04
(3) Primary Beneficiary: Job Retention Likelihood by Enrollment Year			
2010	0.0086**	(0.0041)	0.57
2011	0.0067*	(0.0041)	0.53
2012	0.0058	(0.0041)	0.49
(4) Primary Beneficiary: Job Duration by Enrollment Year			
2010	2.6758*	(1.4562)	198.57
2011	2.0755	(1.4638)	185.28
2012	2.0124	(1.4663)	170.31
Panel B. Dependents born in 1/1985-12/1986			
(1) Dependent: Enrollment Likelihood by Enrollment Year			
2010	0.0243***	(0.0025)	0.14
2011	0.0095***	(0.0030)	0.20
2012	0.1439***	(0.0019)	0.10
(2) Dependent: Total Enrollment Days by Enrollment Year			
2010	1.6685**	(0.7662)	37.86
2011	4.7259***	(0.9921)	62.27
2012	7.0068***	(0.3247)	18.38
(3) Primary Beneficiary: Job Retention Likelihood by Enrollment Year			
2010	0.0111***	(0.0036)	0.59
2011	0.0104***	(0.0037)	0.55
2012	0.0069*	(0.0037)	0.50
(4) Primary Beneficiary: Job Duration by Enrollment Year			
2010	3.5631***	(1.3011)	205.36
2011	3.0723**	(1.3153)	191.18
2012	2.6465**	(1.3216)	176.34

Notes: In this table, we report 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 2010, 2011 and 2012 to test whether insurance enrollment drops when each dependent birth cohort turns 26 (the 1984 birth cohort turns 26 in 2010, the 1985 cohort turns 26 in 2011, and the 1986 cohort turns 26 in 2012). For instance, the 1984 and 1985 cohorts are expected to show very little enrollment in 2012 as they will be over 26 at the time. The corresponding RD graphs are shown in Appendix Figure A.3. Standard errors are adjusted for individual-level heteroskedasticity. Please see the notes to Tables 1 and 3 for more information on the data source and RD specification. * p<0.10, ** p<0.05, *** p<0.01

Table A.4: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Robustness Check					
Panel A. Dependents born in 1/1984-12/1985							
1) Dependent Enrollment Likelihood	0.0856***	0.0858***	0.0913***	0.0856***	0.0799***	0.0828***	0.0818***
	(0.0019)	(0.0020)	(0.0019)	(0.0047)	(0.0023)	(0.0021)	(0.0021)
2) Dependent Enrollment Duration	7.9972***	8.0544***	6.9504***	7.9972***	9.0496***	8.6074***	8.4530***
	(0.8588)	(0.8606)	(0.8350)	(1.6592)	(1.0397)	(0.9328)	(0.9647)
3) Primary Beneficiary Job Retention Likelihood	0.0082**	0.0085**	0.0063*	0.0082*	0.0111**	0.0096**	0.0101**
	(0.0040)	(0.0041)	(0.0037)	(0.0042)	(0.0049)	(0.0044)	(0.0046)
4) Primary Beneficiary Job Duration	6.7777	6.9753*	4.8695	6.7777	9.4041*	7.9789*	8.5042*
	(4.1813)	(4.2370)	(3.8808)	(4.5321)	(5.1059)	(4.5670)	(4.7297)
Observation	279,213	279,213	279,213	279,213	189,279	235,848	279,213
Panel B. Dependents born in 1/1985-12/1986							
1) Dependent Enrollment Likelihood	0.0204***	0.0201***	0.0130***	0.0204***	0.0267***	0.0244***	0.0251***
	(0.0032)	(0.0032)	(0.0029)	(0.0052)	(0.0039)	(0.0035)	(0.0036)
2) Dependent Enrollment Duration	13.4573***	13.3219***	13.7583***	13.4573***	13.8787***	13.6343***	13.5158***
	(1.6911)	(1.6993)	(1.5867)	(2.6543)	(2.0511)	(1.8404)	(1.9034)
3) Primary Beneficiary Job Retention Likelihood	0.0129***	0.0122***	0.0123***	0.0129***	0.0122***	0.0128***	0.0124***
	(0.0036)	(0.0036)	(0.0033)	(0.0024)	(0.0044)	(0.0039)	(0.0041)
4) Primary Beneficiary Job Duration	9.4727**	8.7556**	9.4211***	9.4727**	7.8351*	9.1116**	8.1547*
	(3.7497)	(3.7972)	(3.4835)	(3.6666)	(4.5755)	(4.0924)	(4.2341)
Observation	338,744	338,744	338,744	338,744	230,149	286,703	338,744
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes
Weighting scheme	Triangular	Triangular	None	Triangular	Triangular	Triangular	Triangular
Degree of polynomial	1	1	1	1	1	1	1
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±8 mo	±10 mo	±12 mo
Standard error	Robust	Robust	Robust	Cluster	Robust	Robust	Robust

Notes: We report our findings by altering our baseline specification. Column (1) reports our baseline estimates in Table 3, whereas Columns (2)-(7) 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 cut-off months; and replacing our linear control function with a local linear specification. Across all of these specifications, the RD estimates remain highly similar, providing strong evidence in favor of the robustness of our findings. Regressions are estimated without including control variables. Please see the notes to Tables 1 and 3 for more information on the data source and baseline RD specification. * p<0.10, ** p<0.05, *** p<0.01

Table A.5: Placebo Test: Dependents Under Age 19 During 2010-2012

	(1) RD Estimate (b/se)	(2) Sample Mean
Panel A. Dependents born in 1/1994-12/1995		
(1) Dependent: Enrollment Likelihood, 2010-12	0.0049*	0.62
	(0.0030)	
(2) Dependent: Enrollment Duration, 2010-12	5.5512*	572.89
	(3.1064)	
(3) Primary Beneficiary: Job Retention Likelihood, 2010-12	0.0032	0.65
	(0.0030)	
(3) Primary Beneficiary: Job Duration, 2010-12	2.7982	603.21
	(3.1127)	
Observation	486,698	
Panel B. Dependents born in 1/1995-12/1996		
(1) Dependent: Enrollment Likelihood, 2010-12	0.0012	0.62
	(0.0030)	
(2) Dependent: Enrollment Duration, 2010-12	4.8202	569.76
	(3.1271)	
(3) Primary Beneficiary: Job Retention Likelihood, 2010-12	0.0015	0.64
	(0.0030)	
(4) Primary Beneficiary: Job Duration, 2010-12	3.5860	599.25
	(3.1346)	
Observation	477,325	
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 1/1994 and 12/1996. We modify Eq. 1 so that the first cut-off is 1/1995 (rather than 1/1985) and the second cut-off is 1/1996 (rather than 1/1986). Dependents in the placebo sample were under age 19 during 2010 to 2012 and therefore were eligible for coverage on their parent's plan under mandates that pre-dated the ACA. The corresponding RD graphs are shown in Appendix Figure A.5 (dependent enrollment outcomes) and Appendix Figure A.6 (parental job retention outcomes). Standard errors are adjusted for individual-level heteroskedasticity. * p<0.10, ** p<0.05, *** p<0.01

Table A.6: Results by Primary Beneficiary Birth Cohort

	(1)	(2)	(3)
	Primary Beneficiary Birth Cohort		
	1/1948-6/1952	7/1952-12/1957	1/1958-12/1969
Panel A. Dependents born in 1/1984-12/1985			
1) Dependent - Enrollment Likelihood, 2010-12	0.0744***	0.0935***	0.0820***
	(0.0045)	(0.0031)	(0.0030)
Mean of Dep. Var	0.09	0.10	0.10
2) Dependent - Enrollment Duration, 2010-12	8.5778***	8.2538***	7.2547***
	(2.0308)	(1.3578)	(1.3203)
Mean of Dep. Var	27.87	32.29	29.53
3) Primary Beneficiary - Job Retention Likelihood, 2010-12	-0.0028	0.0099	0.0111*
	(0.0093)	(0.0062)	(0.0065)
Mean of Dep. Var	0.50	0.60	0.63
4) Primary Beneficiary - Job Duration, 2010-12	6.9143	3.5058	8.9894
	(9.1406)	(6.4439)	(6.8655)
Mean of Dep. Var	435.61	563.09	602.54
Observation	53,030	117,374	108,809
Panel B. Dependents born in 1/1985-12/1986			
1) Dependent - Enrollment Likelihood, 2010-12	0.0049	0.0216***	0.0244***
	(0.0079)	(0.0053)	(0.0046)
Mean of Dep. Var	0.23	0.27	0.24
2) Dependent - Enrollment Duration, 2010-12	2.9049	15.4198***	15.3101***
	(4.2309)	(2.8505)	(2.4100)
Mean of Dep. Var	104.77	126.91	115.98
3) Primary Beneficiary - Job Retention Likelihood, 2010-12	0.0036	0.0169***	0.0120**
	(0.0094)	(0.0058)	(0.0052)
Mean of Dep. Var	0.52	0.62	0.64
4) Primary Beneficiary - Job Duration, 2010-12	-0.2520	14.4886**	8.0152
	(9.3064)	(6.0511)	(5.5456)
Mean of Dep. Var	454.29	578.88	606.68
Observation	51,696	130,360	156,688
Controls	Yes	Yes	Yes
Weighting scheme	Triangular	Triangular	Triangular

Notes: In this table, we report how the effects vary by the birth year of the primary beneficiary. We divide the sample into three groups by birth cohort: 1/1948-6/1952 (Column (1)), 7/1952-12/1957 (Column (2)), and 1/1958-12/1969 (Column (3)). The first group is always over 55 during 2010-2012 and thus will have already decided not to retire at that age. The second group includes individuals who turn 55. The third group is always under 55. In Panel A, the cut-off value c is set to 1/1985. In Panel B, the cut-off value c is set to 1/1986. The corresponding RD graphs are shown in Appendix Figure A.7 (dependent enrollment outcomes) and Appendix Figure A.8 (parental job retention outcomes). Please see the notes to Tables 1 and 3 for more information on the data source and RD specification. Standard errors are adjusted for individual-level heteroskedasticity. * p<0.10, ** p<0.05, *** p<0.01

Table A.7: Results by Number of Dependents Added to the Plan Before 2010

	(1)	(2)
	Number of dependents added before 2010	
	One	Two or more
Panel A. Dependents born in 1/1984-12/1985		
1) Dependent Enrollment Likelihood, 2010-12	0.0598***	0.0943***
	(0.0035)	(0.0023)
Mean of Dep. Var	0.08	0.11
2) Dependent Enrollment Duration, 2010-12	3.9752**	9.3230***
	(1.5622)	(1.0225)
Mean of Dep. Var	24.32	32.51
3) Primary Beneficiary Job Retention Likelihood, 2010-12	0.0051	0.0096**
	(0.0079)	(0.0047)
Mean of Dep. Var	0.57	0.60
4) Primary Beneficiary Job Duration, 2010-12	13.0289	4.5294
	(8.0584)	(4.8926)
Mean of Dep. Var	519.76	565.91
Observation	72,975	206,238
Panel B. Dependents born in 1/1985-12/1986		
1) Dependent Enrollment Likelihood, 2010-12	0.0232***	0.0191***
	(0.0061)	(0.0037)
Mean of Dep. Var	0.22	0.26
2) Dependent Enrollment Duration, 2010-12	10.4639***	14.1084***
	(3.2041)	(1.9835)
Mean of Dep. Var	100.78	124.23
3) Primary Beneficiary Job Retention Likelihood, 2010-12	0.0281***	0.0080*
	(0.0073)	(0.0041)
Mean of Dep. Var	0.59	0.62
4) Primary Beneficiary Job Duration, 2010-12	16.7816**	7.1202*
	(7.5049)	(4.3275)
Mean of Dep. Var	544.34	581.83
Observation	83,882	254,862
Controls	Yes	Yes
Weighting scheme	Triangular	Triangular
Bandwidth	±12 mo	±12 mo
Degree of polynomial	1	1

Notes: In this table, we report how the effects vary by the number of dependents added to a given plan before 2010. In Panel A, the cut-off value c is set to 1/1985. In Panel B, the cut-off value c is set to 1/1986. The running variable is the dependent birth month, re-centered at zero at January 1985 in Panel A and January 1986 in Panel B. The sample for each specification is restricted to a bandwidth of 12 months around each cut-off variable. Please see the notes to Tables 1 and 3 for more information on the data source and RD specification. The corresponding RD graphs are shown in Appendix Figure A.9 (dependent enrollment outcomes) and Appendix Figure A.10 (parental job retention outcomes). Standard errors are adjusted for individual-level heteroskedasticity. * p<0.10, ** p<0.05, *** p<0.01