The Impact of the Affordable Care Act Young Adult Provision on Labor Market Outcomes: Evidence from Tax Data

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Abstract

We use a panel data set of U.S. tax records spanning 2008-2012 to study the impact of the Affordable Care Act (ACA) requirement to allow young adult dependents to be covered by their parents' insurance policies on labor market-related outcomes. How health insurance expansions affect young adults through employment and education have important implications for public finance. Since tax data record access to employer provided fringe benefits on W-2 forms, we are able to examine the impact of this coverage expansion by comparing young adults whose parents have access to benefits to other similar aged young adults, before and after the law, and to young adults who are slightly older than the age threshold of the law. The use of tax data to identify families who have fringe benefits through their employer is an important advantage because the law was implemented during a labor market recovery in which outcomes could differ by age, even absent the law. Despite sizable increases documented elsewhere in insurance coverage resulting from this law, we find no meaningful changes in labor market related outcomes. We examine a comprehensive set of outcomes (including measures of employment status, job characteristics, and post-secondary education), and are the first to use a triple difference strategy to examine labor market effects of this law; we are also the first we know of to use tax data to examine the impact of the ACA on labor market outcomes. Although it is possible that labor market outcomes have changed in ways not captured by tax data (e.g. a change in hours of work while holding total wages constant, or a change in non-reported self-employment), our evidence suggests that the extension of health insurance to young adults did not substantially alter their labor market outcomes thus far.

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Introduction

A growing literature draws attention to the effects of health insurance expansions on outcomes other than health insurance and access to medical care (e.g., Kolstad and Kowalski 2012, Baicker et al. 2013, Dague et al. 2013, Mulligan 2013, Sommers et al. 2013, Dave et al 2014, Garthwaite et al. 2014, and Heim and Lurie 2014,). Specifically, important questions around the labor market consequences of expanding health insurance continue to be debated. In this paper, we examine whether providing young adults with a source of health insurance unconnected to their own employment (through their parent's insurance plans) may reduce work attachment and job-lock, and encourage educational enrollment.

Theories regarding the connection between health insurance and labor supply suggest that the young adult health insurance expansion may reduce the incentive to work in employment arrangements that offer employer health insurance by substituting parental health insurance for own-name employer health insurance, and may also reduce labor supply through an income effect. In addition to reducing the incentive for full-time work, the availability of an alternative form of health insurance might make young adults more likely to undertake activities that involve lower labor force attachment (such as enrollment in post-secondary education). The effects have implications for public finance, especially when young adults are the target of the expansions. Expanding health insurance could affect the formative years of employment and human capital, affecting earnings and tax-paying capacity, as well as social safety net needs for several decades.

As an early provision of the Affordable Care Act (ACA), insurers and sponsors of self-insured plans were required to allow dependents to remain on the private health insurance policies of their parents until they reached age 26. Effects of this law on health insurance

coverage are already documented, but there is only one limited prior examination of labor market outcomes. Studying the ACA young adult (YA) provision is also important for understanding the response of a key age group to the health insurance exchanges and Medicaid expansions that followed in 2014; young adults (generally defined as 19-25 year olds) represented the most uninsured age group prior to the reforms.

Several studies exist on the insurance and medical care access effects of the YA provision (e.g. Monheit et al. 2011, Sommers et al. 2013, Akosa Antwi et al. 2013, Barbaresco et al. 2014, Akosa Antwi et al. 2014). Almost all studies examine this provision using a difference-in-difference (DD) design comparing outcomes after the law, to outcomes before the law, for those in the affected age group (usually 19-25 year olds) vs. those outside (usually 27-29 year olds). Time trends are shown to be similar for the older and younger groups in the period leading up to 2010, which suggests that differences in trends after 2010 are due to the change in policy, rather than underlying differences between age groups. However, the period after the YA policy change coincides with a labor market recovery, and any differential recovery patterns by age could be incorrectly estimated as caused by the YA provision, even if pre-trends tests did not signal concerns. Some researcher explore the use of further control groups, for example, to compare the effect of the law in states with some type of prior law extending coverage to young adults to that in states without such laws, but have found that the states with prior laws experience just as large an impact of the federal provision as those without, if not more (Akosa Antiwi et al 2013, Cantor et al 2012). Another dimension of difference in the impact of the law exists, but has rarely been exploited in the literature; the law only affected young adults with parents who have private insurance, thus young adults whose parents do not have employer benefits form an additional

control group.[1] Our research design, which uses a triple difference strategy with information on parents (DDD), will also be informative about the reliability of estimates from DD designs that lack parental data.

Most data sets that have been used in prior studies do not contain information on non-co-resident parents. A limited exception is the SIPP 2008 Panel; since that is a four year panel survey that follows new households that form out of the originally-sampled households, researchers have information on current health insurance options of parents, as long as parent and child resided together at the start of the panel. We use the word 'limited' here because those whose parental information is available are non-representative in terms of being younger, and more likely to have lived with parents until older ages, than other young adults. Akosa Antwi et al. (2013) use this SIPP parental information to provide DDD estimates for the main outcomes examined in their paper (insurance status), but not for their cursory analysis of labor market outcomes. DDD estimates regarding insurance outcomes show employer coverage through parents increased almost 10 percentage points in the limited sample, compared to about a 7 percentage point increase in employer dependent coverage in the full DD sample; however, these estimates are not statistically different from each other. Thus, based on the insurance literature, there is suggestive evidence that DDD methods could produce larger percentage point effects, which is reasonable because the DD effect measures the average of the effect on a treated and an untreated group.

Longitudinally-linked tax data provide a unique opportunity to study labor market outcomes of the YA provision with data reported by employers as well as data self-reported under penalty

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[1] The use of this additional control group rests on the assumption that there was not selection caused by the law; there is evidence in the literature that the YA provision did not cause parents to seek insurance or change parental labor supply (Akosa Antwi et al 2013; Depew 2014).

of law, for the universe of individuals who either file tax returns, receive a wage statement (Form W-2) or a tuition statement from a college or university (Form 1098-T). Starting in 2012, employers are required to report employer provision of health insurance on W-2s. Tax data represent a rich resource for examining other aspects of the ACA as well; since the first ACA provisions were passed in 2010 and because tax data are available with roughly a 2-year lag, our paper represents the first of likely many papers that will examine ACA impacts with tax data.

In this paper, we examine whether an individual files a tax return, works at all in the formal sector, or is self-employed; their total annual wages and receipt of employer-provided retirement and health benefits; and educational enrollment. We first use the typical DD approach of comparing adults of slightly different ages over time as in the prior literature. We show that prior to the policy change, there were statistically similar trends in outcomes between young adults with and without access to parental benefits, suggesting that any estimated DD effects would be attributable to the policy change. However, DD effects may mask causal effects that occur only among those whose parents have employer fringe benefits, thus we move next to a triple difference approach that further compares young adults whose parents have access to employer provided fringe benefits to those who do not, after testing that trends prior to policy change support identifying assumptions.

We provide estimates using all young adults aged 19-25 years as the treatment group (while those aged 27-29 are the control group), but find our results hold even when we look only at the older members of the treatment group. Although it is possible that labor market outcomes have changed in ways not captured by tax data (e.g. hours of work, or non-reported self-employment), our evidence suggests that the extension of health insurance to young adults did not substantially

alter labor market outcomes thus far, despite sizable changes in insurance status.

Background and Literature Review

In this section, we briefly survey the large literature on the effect of insurance on labor outcomes in general (see Dave et al. 2014 for a longer literature review), and the much smaller literature focused on young adults.

A long literature in health economics studies the connection between health insurance and the labor market (Madrian 2006; Gruber 2014[2]). One strand of this work focuses on the "job lock" effect from public and private insurance (Madrian 1994; Cooper and Monheit 1993; Yelowitz 1995; Ham and Shore Sheppard 2005; Strumpf 2011; Decker and Selck 2011; Pohl 2014; Dave, et al 2014; Hamersma and Kim 2009), and finds mixed evidence on whether having an outside source of health insurance leads to improved job outcomes or changes in labor supply. A few papers have also examined whether the availability of health insurance affects the decision to be self-employed, with Madrian and Lefgren (1998) and Wellington (2001) finding a significant effect, though Holtz-Eakin et al. (1996) find no significant impact.

Another strand examines the effect of receiving health insurance on wages, as compensating differentials suggests that workers would accept lower wages in response to health insurance (e.g. Gruber 1994; Kolstad and Kowalski 2012; and others). This too has produced mixed evidence. This could partly be due to the fact that workers maximize utility from employment choices, rather than just compensation, and thus preferred jobs may not necessarily have higher wages or benefits. In addition to labor supply, labor demand could also be affected

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[2] See, for example, Jonathan Gruber's review presentation at 2014 Chicago Fed Conference "The Affordable Care Act and the Labor Market", available at http://www.chicagofed.org/webpages/events/2014/affordable_care_act.cfm#

by a health insurance expansion. For example, employers may now find young adults with access to parental insurance cheaper to hire, and may increase their demand for such workers. However, under the premise that workers always bear the full incidence of employer-sponsored health insurance, such labor demand incentives should not change. Instead, we simply expect that young adult workers receive higher pay when the employer no longer has to provide them with this fringe benefit when they gain coverage under a parent's plan, and that this adjustment in wages means the labor demand does not change.

Receiving health insurance through a parent may affect labor market incentives in a way similar to receiving Medicaid, in that both are not received from one's own employer and both are relatively "free". On the other hand, Medicaid eligibility is contingent on low income, and thus may have additional disincentives on labor supply that are absent in the receipt of parental coverage. Several recent papers look at the effect of Medicaid expansions in Oregon, Massachusetts, Tennessee, and Wisconsin (Garthwaite et al. 2014; Kowalski and Kolstad 2012, Dague et al. 2013; Baicker et al. 2013) to anticipate possible effects of the ACA. The study with the most rigorous design, a random assignment experiment in Oregon that expanded Medicaid to adults below the federal poverty level, shows very little effect on labor supply or earnings as a result of receiving Medicaid (Baicker et al. 2013). Dague et al. (2013) find in Wisconsin that those who were not enrolled in the program due to a cap worked more than those who were able to enroll in Medicaid, but effect size ranged from modest to medium. Garthwaite et al. (2014) use the case of Tennessee and find extremely large increases in labor supply upon losing Medicaid coverage.

While this literature on health insurance and labor has looked at subgroups such as those near retirement (Blau and Gilleskie 2008; Strumpf 2010; Kapur and Rogowski 2007), those who

are unemployed (Gruber and Madrian 1995) and married women who have health insurance available through their husbands (Buchmueller and Valetta 1999), no prior work existed on young adults before the ACA despite the importance of implications for public finance from knowing responses among this age group. This is mostly due to lack of policy variation that could be used to design a causal study. A prior published study that has examined the effect of health insurance on labor supply using federal policy variation is Akosa Antwi et al. (2013), who show initial estimates that suggest providing dependent coverage has reduced labor supply on the intensive (reduced or varying work hours) but not extensive margin (no effect on probability of being employed).

Four additional papers shed light on the effect of insurance for young adults on labor market outcomes. Two of these (one forthcoming and one working paper) use variation from state laws prior to the ACA, and two examine the ACA provision (two working papers). Although more than half the states had some provision in place prior to the ACA, those laws are thought to be much weaker than the ACA YA provision. More than half of employer provided health insurance is self-insured, and thus exempt from state mandates. State mandates also do not change IRS tax obligations, thus employers would not be able to provide tax-exempt compensation when covering those over age 18 (unless disabled or under age 23 and a full-time student), forming a further obstacle to the effectiveness of state laws. State laws also often had other stipulations, such as requiring that the dependents be full-time students or not married.

Dillender (2014) looks at the long run labor market consequences of experiencing young adulthood with insurance due to state dependent coverage laws while Depew (2014) estimates the impact of state laws on contemporaneous outcomes of work and education. Dillender's results relate to the very long run effects of the law, thus cannot be compared with our findings

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that relate to data covering up to approximately one year after the effective date. Depew's findings are consistent with Akosa Antwi et al.'s results in that the law appears to cause only a decrease in the hours worked, and not in the probability of any work. Bailey (2014) finds that the ACA YA law increased entrepreneurial activity while Bailey and Chorniy (2014) finds that the law did not have any impact on job lock. Bailey's and Chorniy's finding of no job-lock effects are consistent with the findings in our paper, although the findings of increased entrepreneurial activity are not.

This literature review highlights that there is a long literature on the connections between health insurance and the labor market, and a budding literature devoted to studying the labor market effects of extending health insurance coverage to young adults specifically. Results for the ACA YA provision from DD analyses as well as for the state YA mandates from DDD analyses suggest that there appears to be a switch away from forms of labor market activity that entail employer provided health insurance (such as reduced full-time work and increased educational and entrepreneurial activity) but not of increased job mobility.

Hypotheses

Economic intuition formalized in prior literature suggests that providing access to health insurance through a parent would reduce the demand for health insurance through employment, and thus reduce labor supply on extensive and intensive margins, or lead to changes in the nature of work. Young adults would be more able to pursue all alternatives outside of full-time work at firms that offer benefits, such as self-employment, education, or leisure, as the opportunity costs of such activities have decreased. Those workers who no longer require employer-provided

health insurance could experience wage gains as a result of the compensating wage differential for employer provided benefits, but may also appear in our data to have lower annual wages because of a shift towards other types of labor that maximize utility but do not involve higher financial remuneration. We expect that these effects are present only for those whose parents have access to employer provided health insurance, and that employment effects maybe larger for older ages in the 19-25 range because of the higher likelihood of being in school during the earlier years.

Method

We estimate the impact of the YA provision using both difference-in-differences (DD) and triple difference (DDD) identification methods. We compare those in the treatment age to those outside treatment age (19-25 being treatment age and 27-29 being control age; those aged 26 are excluded), before and after the law, among those with parents who are likely to have access to health insurance compared to those who do not. We exclude all of 2010 as a period of staggered implementation; some insurers complied as early as spring 2010, but as most insurance plans renew on January 1st 2011, we consider that the full implementation date. We conduct graphical and statistical checks that prior trends in labor markets are similar between those in the control and treatment ages, although as we will explain later, we have limited years of data prior to policy implementation for these tests in the case of the DDD model.

We start with a DD model where *a, b, c* and *d* represent labor market outcomes for individuals who belong to the Treatment and Control age groups (19-25 year olds and 27-29 year

olds, respectively) in the Pre- and Post-policy time period (2008 and 2009, vs 2011 and 2012, respectively). In order to capture the effect of the policy on the treatment group apart from national time effects and fixed differences across age, we compute the differences in differences estimate (b-a)-(d-c). This captures the average impact since reform implementation, by comparing insurance coverage during the post period relative to coverage before, among the treatment group relative to the control group. This method has the advantage of parsing out effects that could otherwise be wrongly attributed to the policy if we were only to compare effects among the treatment group over time, while also being a very transparent method of analysis.

Figure 1: Difference in Difference Comparisons

	Pre-policy period	Post-policy period
Treatment age group	a	b
Control ages group	c	d

Note: DD estimate is (b-a)-(d-c).

Our triple difference estimate starts with a calculation similar to that in Figure 1, except we first limit the sample to those whose parents have access to employer-provided benefits. We expect that causal effects of the YA policy will only occur in this population. We argue that the availability of YA benefits does not change parental labor supply in ways that would change the composition of the treatment group (evidence in support of this is found in Akosa Antwi et al. 2013). Thus, to complete the DDD we also estimate a version of the DD calculation among those whose parents do not have employer fringe benefits. This would capture other ways by which the labor supply of young adults may have changed, relative to older adults, transitioning from the

pre- to post-policy time period. Evidence in Hoynes et al. (2012) suggests this is possible, as they find that the recession affected young adults worse than older adults. Our DDD estimate is the difference between the DD estimate for those with parents with employer benefits and the DD for those whose parents do not have benefits, and relies on fewer assumptions than the DD estimate.

Figure 2: Difference in Difference in Difference Comparisons

	Pre-policy period	Post-policy period
Treatment age group with benefits	a	b
Control ages group with benefits	c	d
Treatment age group without benefits	A	B
Control ages group without benefits	C	D

Note: DDD estimate is [(b-a)-(d-c)]-[(B-A)-(D-C)]

In all cases, we test the statistical significance of the raw DD and DDD estimates, after clustering our standard errors at the age by year level. We have also conducted regression adjusted versions of the DD and DDD calculations in which we account for the national annual unemployment rate, each year of age fixed effects, and an interaction of age fixed effects and the national unemployment rate. We find that some of the DD estimates are statistically significant when we include the additional variables, but that the results of our DDD comparisons are robust to the regression adjustments and thus do not change our conclusion.

Data

We use the Internal Revenue Service (IRS) Compliance Data Warehouse (CDW) data, which

contains most of the population of U.S. federal tax documents. We use years 2008-2012 because

of the ages of individuals we wish to have linked to parental records, but the CDW is available

for 1996-2012.[3] Research using these data is limited because of the highly restricted nature of

access. The CDW has been used in prior tax and labor papers including Chetty et al. (2013)

although ours is the first paper to use these data to study the labor market impacts of the

Affordable Care Act.

Since non-dependent children are not listed on their parents' tax form(s), and vice versa,

it is not possible to link young adults and parents using only tax returns from recent years.

However, the Treasury Department has available a file that contains, for the universe of filers in

1997, the SSNs of primary and second filers matched to the SSNs of their dependent children, if

any,[4] which makes it possible to match parents' and children's information if those children were

claimed as dependents in 1997. So, to create a dataset from the CDW that links parents' and

children's information, we gather tax information from 2008-2012 for both the children and their

parents, and merge each young adults' information to those of the individual(s) that were listed

as their parent(s) in 1997 (provided that those parents appeared in the 2008-2012 tax data).

Because individuals who were older than 18 years in 1997, or who were born after 1995,

were less likely to be claimed as a dependent in 1997, we are only able to utilize information on

birth cohorts from 1979 through 1995 when conducting DDD estimation. This structure means

that we are limited in conducting pre-trends tests: we cannot go back further than tax year 2008

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[3] Some data from 2012 will continue to be updated as information from late filers are incorporated. We do not
expect this to affect results substantially.

[4] Almost all children 18 and younger are likely claimed on a parent's tax returns, especially given the substantial tax
benefits of claiming children.

(see the illustration below) and still have the full set of control individuals aged 27-29, which limits our pre-trends tests to 2008-2009.

Illustration: Age of birth cohort by year of tax data

| | Birth Cohort | | |
Tax Year	1995	…	1979
2004	*9*		*25*
2005	10		26
2006	11		27
2007	12		28
2008	13		29
…			
2012	17		33

Note: Oldest age of dependent is assumed to be 18 in 1997, thus in the 1979 cohort. Italics show what we would have if we expanded years of data to before 2005. The last year back in time for which we would have 27-29 year olds represented is 2008. If we go back past 2006, we would not have any age in the control age range.

Ideally, we would know in each year of our data 2008-2012 whether the parent has access to employer health insurance. Since the IRS requirement for W-2s to include data on employer health insurance started only in 2012, we have this information reported just for one year. In addition, this reporting requirement of the ACA only applies to firms with over 250 workers, and approximately 40% of the US workforce is in firms with fewer than 250 workers,[5] though it appears that compliance with this requirement by large firms is high by our (unreported) calculations using publicly available data from the Medical Expenditure Survey Insurance Component.

We backfill data on employer provision of health insurance for years prior to 2012 using the following steps: using 2012 data, we know whether an employer with a certain Employer

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[5] Author calculations using data at http://www.bls.gov/web/cewbd/table_f.txt
That shows that in Q1 2013, 51.6M worked in private firms with fewer than 250 employees, out of a total payroll number of 135.7M (from https://ycharts.com/indicators/total_nonfarm_payrolls).

Identification Number (EIN) offers health insurance if any of the workers of that EIN have a W-2 form that reports health insurance. From that, we create a data set of EINs who are known to offer health insurance in 2012. Under the assumption that these employers have always offered health insurance during 2008-2011, and that they offer insurance to all their workers when they offer to any, we have information on employer health insurance offers for 2008-2012. When we examined these data, we found that the rate of employer health insurance increases sharply over time, as we would expect because our method of imputing works best for more recent data. As an alternative, we use information on whether parents have a retirement plan to which the employer contributes. Retirement plan contributions are recorded on W-2 forms in all years of our data, and there is a high correlation between having employer health insurance and a retirement plan through an employer. Thus, we use this variable as a proxy for availability of health insurance, although we have run estimates with the employer health insurance variable as well and note that results are qualitatively similar.[6]

The outcomes that we analyze fall into four groups: employment status (including whether the individual filed a tax return, whether the individual received a W-2 form, and whether the individual reported any amount of self-employment income), job characteristics (including log annual wage income, whether the individual's primary[7] employer offered health insurance, and whether the individual had a retirement plan at their primary job or at any job), and educational enrollment (including whether the individual was a student at a post-secondary institution, whether they were a full-time student at such an institution, and whether they were a graduate student at such an institution). One may be concerned whether the use of tax data means there is

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[6] Another proxy would have been full-time employment in a large firm. However, tax data does not contain information on hours of work.
[7] We define the primary employer (job) as the employer (job) from which the individual received the most in wages in a given year.

selection into the sample due to the policy changes. However, most of our outcome variables

come from forms that are filed regardless of whether the person filed an income tax return.[8]

Examining whether the law changed the probability of filing a tax return, then, additionally

examines whether the policy changed the sample composition for examining whether individuals

are self-employed, which is only available on individuals who file a return.

We use a 1% sample from the parent-child matched CDW for our analysis, consisting of

571,049 unique individuals who range in age from 13-29. When we limit to those aged 19-29

excluding 26 year olds and use all data on 2008-2012 except 2010, we obtain 1,352,731 person-

year observations, corresponding to about 340,000 unique individuals. We examined the

breakdown of our sample by year and age and observed that the number of individuals does not

change in any systematic way over time as our data are a balanced panel, except that the number

of older aged individuals is consistently lower than those at younger ages. This is likely due to

the fact that in 1997, those who are aged 17 and 18 are more likely to be already living outside of

the parental household and not claimed as a dependent. We conducted statistical tests to ensure

that there are no systematic difference in sample size that aligns with our main identification

method, and found no evidence of systematic difference (for example, between treatment and

control, before and after the policy, for the DD).[9]

Results:

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[8] For example, job characteristic variables come from W-2 forms, which (in addition to being attached to income tax returns) are filed by employers to the IRS, and education variables come from 1098-T, are filed by colleges and universities to the IRS and indicating tuition payments were received on behalf of a student.

[9] Year varies from 2008-2012, and average number of observations in a given age-year cell is 33,942. There is close to a 50/50 split between treatment and control, and post and pre data. Please see Appendix Table 1 for a breakdown of our sample by age and year.

In Figure 3, we display the DD results by plotting the labor market related outcomes over time (2008-2012) for the treatment group and the control group. In almost all the outcomes, it appears that the older and younger groups had similar trends in the period prior to the policy (2008-2009), as well in the post-policy period (2011-2012). There are some exceptions; for example, the non-filer outcome shows a steeper increase for the treatment group than for the control group. [10] In Figure 4, we conduct a similar exercise to show the triple difference results visually, using parental access to retirement benefits as the third level of difference. Figure 4 shows that in terms of levels, those whose parents have access to employer benefits have different levels of labor market outcomes, as would be expected of different socioeconomic status groups. However, here too it is not clear that there is any systematic difference in trends before or after the policy.[11]

Because time trends that look visually the same may mask small differences that would not be apparent to the eye, we next present raw DD and DDD calculations as explained above.[12] These results are reported in Table 1 (DD) and Table 2 (DDD).

In Table 1, the value of 0.002 for the raw DD estimate for the "Post-Secondary Student" outcome indicates that, had the effect been statistically significantly different from 0, the YA provision likely caused an increase of (0.002/0.408)*100=0.49 percent increase in the probability of being a student. We include columns for noting whether the estimates are statistically

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[10] Note, also, that the health insurance variable shows large increases over time for all groups. This is likely an artifact of the way we impute the data for years prior to 2012. For this reason, we have only shown the DDD graphs using the parental retirement plan indicator to measure access to parental employer benefits.

[11] We conducted unreported tests of statistical trends in outcomes prior to the policy, between the different control and treatment groups, and describe them in the tables of DD and DDD results. In the case of the DD estimate, we have also conducted tests using data that go back to 2004 and find that the result is qualitatively the same.

[12] We report results that compare 19-25 year olds to those aged 26-29 years of age. In unreported results, we experimented with alternate treatment groups defined as 23-25 year olds or just 24-25 year olds following concerns in Slutsksy 2013 (although our pre-trends tests support the use of wider age bands). We did not find our conclusion affected by the choice of sub-age group.

significantly different from zero. We also include a column for whether time trends in the outcomes differed between the treatment and control groups in the years prior to the policy change. These 'pre-policy trends tests' indicate whether there had been divergent trends in outcomes even before the policy, which would signal unreliability of the DD estimates. Reassuringly, for none of the outcomes is the difference in pre-policy trends statistically significant. However, none of the differences-in-differences estimates are statistically significantly different from zero, either.

In Table 2, the difference in pre-policy trends is again generally statistically insignificant, in that there is just one pre-trends test that fails (for no tax return being filed, at the 10% significance level), suggesting that the DDD estimates will not be biased by difference in pre-existing trends. However, the DDD estimates again show that there are no outcomes that are statistically significantly different from 0.

This lack of statistically significant results occurs despite the fact that our sample sizes are in the hundreds of thousands, and thus would have the power to more precisely identify effects than in studies that use smaller samples, such as the CPS or ACS. Further, the coefficient magnitudes in the DD and DDD calculations are generally fairly small, often less than one percent of the base.

Even though our estimates are not statistically significant, in Table 3 we compare the implied elasticity of the DDD outcome with respect to gaining insurance through an alternate source to the largest statistically significant estimates available in the literature to see if our results would be surprising, were they statistically significant. We do this for four outcomes for

which we could find a relevant literature: No W-2 Forms (employment), Any Self-Employment Income (self-employed), Log Annual Wages, and Full-Time Student.

Each of four literatures includes several papers (except for the Full-Time Student outcome, where Jung et al 2013 is the only paper we were able to find), and in almost all cases papers exist that find no statistically significant effects on relevant outcomes, thus from that standpoint our estimates are not surprising. Often the papers cited did not explicitly state an elasticity, and as mentioned, our paper did not estimate an insurance effect itself, thus these numbers should be viewed as back-of-the envelope calculations. But Table 3 nonetheless shows that compared to the largest estimates available in the literature, our implied elasticity estimates are extremely small, in addition to being statistically not different from 0. Specifically, our implied elasticity for whether an individual works, is 0.1, whereas the largest estimate in the literature (Garthwaite et al 2014) finds an elasticity of 0.63. For self-employment, our estimate is opposite signed from the largest estimate in the literature (-0.09 vs. 0.8). For log wages and for full time employment, our estimates are extremely small compared to the largest existing estimates (-0.004 vs -0.20 and 0.08 vs. 0.22)

Taken as a whole, the results presented in the Figures 3-4 and the Tables 1-2 suggest that there is no evidence to indicate that labor market outcomes were affected by the YA provision, even when we look within a group who had access to parental employer benefits compared to those who do not. The findings were fairly similar when 23-25 year olds or 24-25 year olds were used as an alternate treatment group, and when we used our measure of parental employer health insurance for the DDD estimates, in results not reported here. Thus the causal effects of the YA provision on labor market related outcomes appears minimal, at least through 2012.

<u>Limitations and caveats of method and data</u>

There are several caveats to our measures in the tax data. Most notably, we are unable to examine dependent health insurance as an outcome, while almost all prior papers on the YA requirement have been able to ascertain some effect on dependent coverage, or private health insurance. Having that outcome in our data could enable us to provide a definitive DDD estimate on the insurance impact of the law and ascertain an IV equivalent within the same data set. However, we take as given that were this information reported, our method would also show an increase in dependent coverage. Estimates in Akosa Antwi et al. (2013, Table 2) show about a 30 percent increase in dependent employer coverage for young adults aged 19-25 in the period after the law, compared to before the law.

Our DDD method relies on separating those with access to coverage through parents. Since our variable for employer health insurance through W-2s is not reported for all employers and only actually reported in 2012, we rely on data regarding retirement plans. Although there is a high correlation between the two, the use of retirement plan availability introduces some measurement error into the DDD estimate.

Another limitation is that tax data only provide annual observations. Thus, we cannot split the 2010 year into pre and post periods as we could if the data were available quarterly.

<u>Conclusion</u>

There is very little evidence prior to the ACA on how access to health insurance affects labor market and socioeconomic outcomes of young adults that are relevant for public finance. Tax data provide a unique method to estimate the impact of the ACA YA provision using a triple difference strategy, as well as a large sample of administratively reported data. Prior evidence on the YA provision showed a reduction in hours, and an increase in entrepreneurial activity, but no effect on being in the labor market or other outcomes. The outcomes we study: are whether the individual filed a tax return, whether they worked at all in the formal sector, whether they were self-employed, total annual wages, the holding of employer provided health and retirement benefits, and educational enrollment. We find no statistically significant effects on any of the outcomes we study, and the implied magnitudes are small. This finding holds even in our more stringent specification, our DDD approach. As a caveat, our data on holding of employer-provided health insurance is potentially weak as it was only recently mandated as a reporting requirement on W-2, and only for large employers. However, we do not find qualitatively different results from the use of retirement benefits as a proxy for health insurance. Although it is possible that labor market outcomes have changed in ways not captured by tax data (e.g. hours of work may change even while total wages do not, and non-reported self-employment may change), our evidence suggests that the extension of health insurance to young adults did not substantially alter labor market outcomes thus far.

Aside from the labor supply effects of the young adult provision, there are also distributional implications of expanded insurance as a dependent: the provision spreads costs to parents and co-workers of the parents. Employer health insurance is partially taxpayer financed because of the generous exclusion it entails. Medicaid expansions and exchange subsidies, on the other hand, are almost entirely taxpayer financed.

Tax data have been used to study the labor market impacts of health insurance in the past

(e.g. Heim and Lurie 2010); however, we are the first to use tax data to look at the effect of the

ACA on labor market outcomes. Researchers will be able to analyze other features of the ACA

with public finance consequences as more years of these tax data become available. For example,

we will be able to study how labor market outcomes and health insurance changes for young

adults who do not have access to parental benefits, in response to Exchange and Medicaid

expansions that occurred in 2014.

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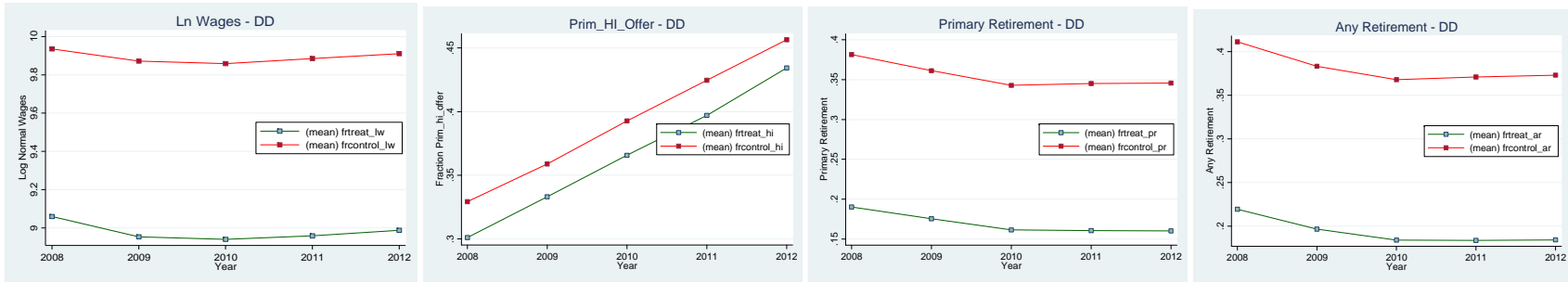
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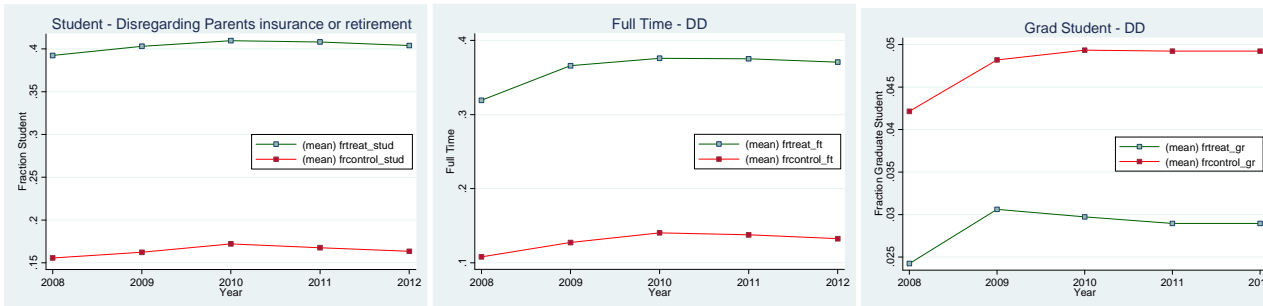
Figure 3: DD Graphs

A. Employment Status
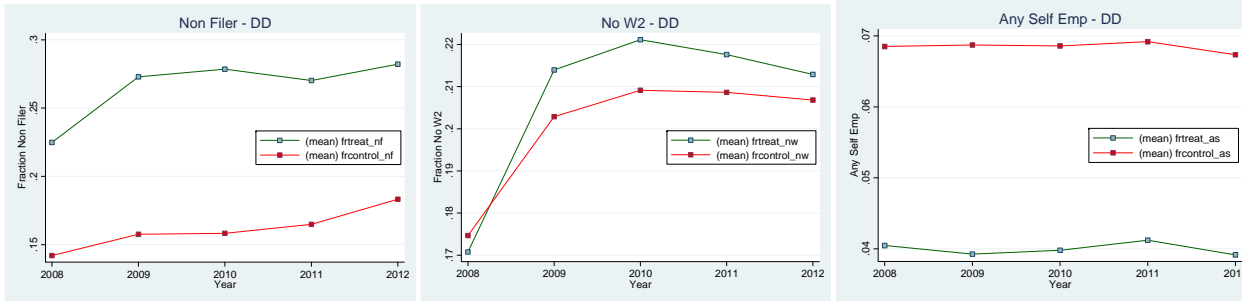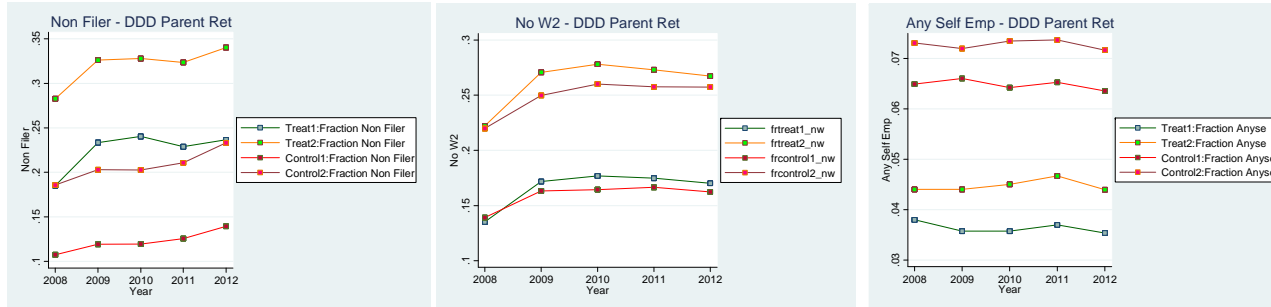

B. Job Characteristics
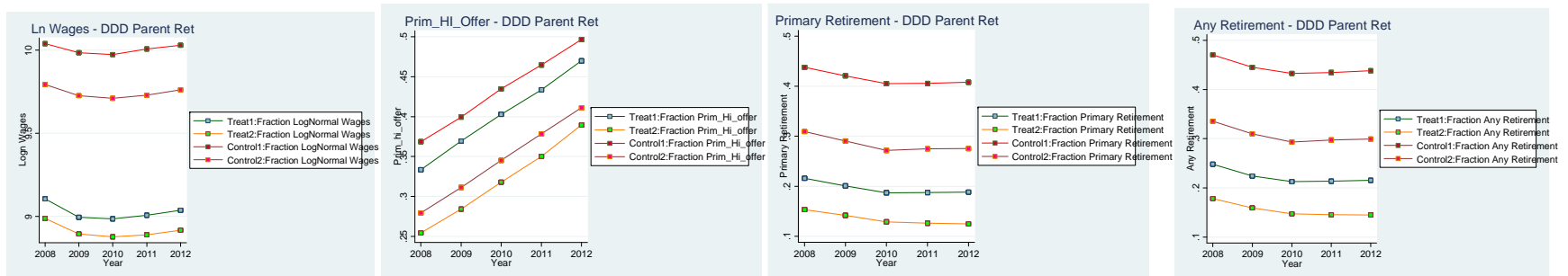

C. Educational Enrollment

Note: Definitions of abbreviations used in the Figure: non-filer=did not file tax return, no W-2=did not have a Form W-2 that year (non worker), Any Self Emp=has any source of earnings that is self-employment, LnWage=log annual wages, prim_HI Offer=had an offer of health insurance at primary job, Primary Retirement=has retirement from primary employer, Any Retirement=has retirement at any job that year, Student=enrolled in college or university, Full Time=full time student, grad student=enrolled as a graduate student.
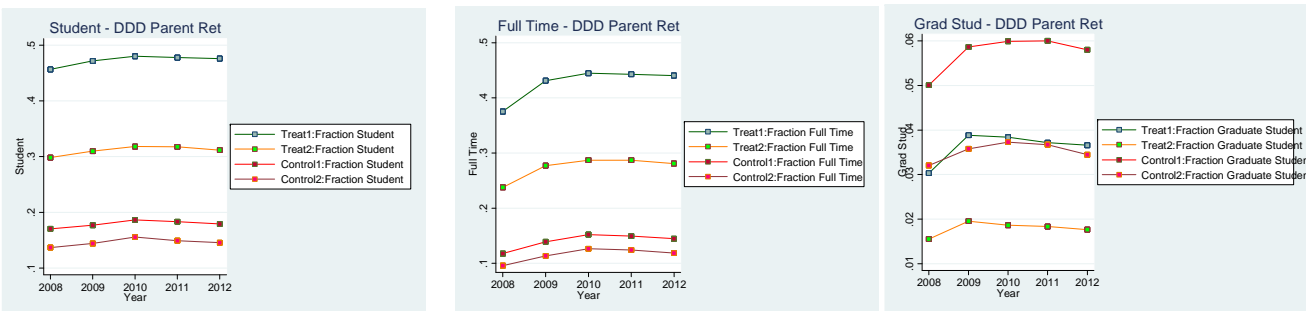
Figure 4: DDD Graphs

A. Employment Status

B. Job Characteristics

C. Educational Enrollment

Note: Please see definitions in note to Figure 3.

Table 1. Difference-in-Differences Calculations

Variable	(Post-Pre)$_{treat}$	(Post-Pre)$_{control}$	(Post-Pre)$_{treat}$-(Post-Pre)$_{control}$	Mean (Post, Treatment)	Percent Effect	Statistical Significance level	Statistical Significance of Difference in Pre-Policy Trends
Employment Status							
No Tax Return Filed	0.027	0.024	0.003	0.277	1.1		
No W-2 Forms	0.023	0.019	0.004	0.215	1.9		
Any Self-Employment Income	0.0003	-0.0004	0.001	0.040	2.5		
Job Characteristics							
Log Annual Wages	-0.034	-0.006	-0.028	8.970	-0.3		
Employer Offered Health Insurance	0.099	0.096	0.002	0.433	0.5		
Retirement Plan at Primary Job	-0.022	-0.026	0.004	0.159	2.5		
Retirement Plan at Any Job	-0.024	-0.025	0.001	0.183	0.5		
Educational Enrollment							
Post-Secondary Student	0.008	0.007	0.002	0.408	0.5		
Full-Time Student	0.03	0.017	0.013	0.375	3.5		
Graduate Student	0.001	0.003	-0.002	0.028	-7.1		

Note: The "Mean" column shows the mean of the outcome, for the treatment group, in the post period. The "Percent Effect" column shows the first column (the DD estimate), divided by the mean, and multiplied by 100, thus showing the effect size as a percent of the mean. The "statistical significance level" column shows whether the raw DD estimate is statistically significant. The last column notes whether the outcome in question showed statistically significantly different pre-policy time trends when comparing the treatment group to the control group, and if so, at what level of statistical significance. In both the last 2 columns, *** indicates statistical significance at the 1% level; ** indicates the 5% level, and * indicated the 10% level. These columns show nothing meets conventional levels of statistical significance.

Table 2. Triple Difference Calculations

Dependent Variable	DDD	Mean (Post, Treatment, Parents with Retirement)	Percent Effect (Post, Treatment, Parents with Retirement)	Statistical Significance Level	Statistical Significance of Difference in Pre-Policy Trends
Employment Status					
No Tax Return Filed	0.005	0.234	2.14		*
No W-2 Forms	0.005	0.173	2.89		
Any Self-Employment Income	-0.001	0.036	-2.78		
Job Characteristics					
Log Annual Wages	-0.011	9.031	-0.12		
Employer Offered Health Insurance	0.002	0.469	0.43		
Retirement Plan at Primary Job	-0.0004	0.186	-0.22		
Retirement Plan at Any Job	-0.001	0.212	-0.47		
Education					
Post-Secondary Student	0.001	0.480	0.21		
Full-Time Student	0.01	0.445	2.25		
Graduate Student	-0.001	0.036	-2.78		

Note: The first column shows the raw DDD estimates that use whether parents have retirement benefits. This is $[((\text{Post-Pre})_{treat}-(\text{Post-Pre})_{control})_{p\_retirement}]-[((\text{Post-Pre})_{treat}-(\text{Post-Pre})_{control})_{no\_p\_retirement}]$. For brevity, only the parental retirement version is shown; values are similar for the parental health insurance version. The "Mean" column shows the mean of the outcome, for the treatment group, in the post period, for those whose parents have access to retirement benefits. The "Percent Effect" columns shows the raw DDD estimates, divided by the mean, and multiplied by 100, thus showing the effect size as a percent of the mean. The "Statistical Significance Level" column shows whether the raw DDD estimate is statistically significant. The last column notes whether the outcome in question showed statistically significantly different pre-policy time trends when comparing the treatment group to the control group in the DDD context, and if so, at what level of statistical significance. In both the last 2 columns, *** indicates statistical significance at the 1% level; ** indicates the 5% level, and * indicated the 10% level. The values of these two columns are blank except for one entry, indicating that none of the results are statistically significant, and that only one of the pre-trends tests showed any statistically significant differences in the DDD context.

Table 3: Comparisons to Largest Estimates in Literature

	DDD percent	Coverage percent	Implied elasticity	Largest estimate in literature	Paper
No W-2 Forms	2.89	30	0.10	0.63	Garthwaite et al (2014)
Any Self-Employment Income	-2.78	30	-0.09	0.80	Bailey (2014)
Log Annual Wages	-0.12	30	-0.004	-0.20	Dague et al (2013)
Full-Time Student	2.25	30	0.08	0.22	Jung et al (2013)

Note: The first column contains the DDD estimates from Table 2. Note that they are all statistically not different from 0 at conventional significance levels. The 30 percent increase in dependent coverage estimate comes from Akosa Antwi et al (2013). The estimate of comes from taking the 24% upper end increase estimate in Bailey (2014) and dividing it by the 30% increase in dependent coverage from Akosa Antwi et al (2013).

Appendix Table 1: Sample Sizes by Age and Year,

	Year				
Age	2008	2009	2010	2011	2012
19	35,453	36,457	35,845	35,161	34,679
20	34,323	35,453	36,457	35,845	35,161
21	33,981	34,323	35,453	36,457	35,845
22	33,176	33,981	34,323	35,453	36,457
23	33,353	33,176	33,981	34,323	35,453
24	32,756	33,353	33,176	33,981	34,323
25	32,678	32,756	33,353	33,176	33,981
26	33,120	32,678	32,756	33,353	33,176
27	32,441	33,120	32,678	32,756	33,353
28	31,961	32,441	33,120	32,678	32,756
29	27,952	31,961	32,441	33,120	32,678

Note: Data from Internal Revenue Service (IRS) Compliance Data Warehouse (CDW) 1% Sample. Data from 2010, and data for 26 year olds, are not used in the differences calculations.