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***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 all 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 socioeconomic outcomes. How health insurance expansions affect young adults through employment, education and family formation have important implications for public finance. Since tax data record access to employer provided fringe benefits in 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 (whether the individual worked at all in the formal sector, self-employment, holding of employer provided retirement and health benefits, educational enrollment, total wages, whether taxes were filed, whether married and whether had children), 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 any impact of the ACA. 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. Sommers et al. 2013, Kolstad and Kowalski 2012, Baicker et al. 2013, Dague et al. 2013, Garthwaite et al. 2014, Heim and Lurie 2014, and Mulligan 2013). 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 less likely to pursue other avenues that would have resulted in health insurance (such as marriage) and more likely to undertake activities that involve medical costs and lower labor force attachment (such as child bearing and full-time enrollment in school). 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, human capital and family formation, 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.<sup>1</sup> 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 is 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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<sup>1</sup> 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 works at all in the formal sector, their self-employment, holding of employer-provided retirement and health benefits, educational enrollment, total wages, whether tax returns were filed, whether they married and whether they had children. We first use the typical DD approach of comparing adults of slightly different ages 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.

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<sup>2</sup>). 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, rather than just compensation, from employment choices, thus preferred jobs may not necessarily have higher wages or benefits. In addition to labor supply, labor demand could also be affected 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,

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<sup>2</sup> 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#](http://www.chicagofed.org/webpages/events/2014/affordable_care_act.cfm#)

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 coverage 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 study that has examined the effect of health insurance on labor supply using 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 {one of which is forthcoming and the others are working papers} shed light on the effect of insurance for young adults on labor market outcomes. Two of these use variation from state laws prior to the ACA, and two examine the ACA provision. 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 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; it is possible that entrepreneurial activity could increase in ways not captured by tax information.

Dillender (2014) studies the long run implications of extending dependent coverage to young adults, on labor market outcomes at later stages in life. Specifically, he compared the outcomes past age 22 of those who spent all their very young adult years (defined as 19-21) with extended dependent coverage to those who did not. Dillender finds that those who had dependent coverage in their young adult years have wages that are 3.1 percent higher for the females and 1.6 percent higher for the males, corresponding to education increases of 0.17 years for men and 0.07 years for women. Note that because this is a question of long run effects, this paper is only able to assess the effect of state laws that happened in a few states (9 or 4, depending on the specification, out of the approximately 30 states that passed such laws) and cannot directly tie these effects to increases in insurance coverage while young adults, because the ACS did not record insurance status until 2008. These effects are also studied for a period during which insurers typically cover full-time students under age 23 even absent state laws, thus any other factors that differ between younger and older cohorts in this set of states vs other states would be captured as a causal effect of the law. This is less stringent of an identification assumption relative to the assumption underlying DD analysis of the ACA YA provision.

Depew (2014) examines the contemporaneous effect of state laws, and finds evidence of reductions in labor supply on the extensive but not intensive margin. The method of

identification used in this paper is to compare outcomes among those who are younger and affected by the law (exact age varies by state) to those of older ages in states with mandates and to those in states without mandates, using data from the American Community Survey (ACS) of 2001-2010 on 19-29 year olds. This triple difference strategy does not require data on parents because of the variation at the state level in adoption of dependent coverage mandates over time; if parental information were available, a fourth level of difference would be possible, to look for effects of state laws only among those whose parents have access to benefits (after first ensuring that the law does not affect parental work decisions). As with our tax data, Depew (2014) does not have data on insurance status in the main data set (ACS), but does conduct a supplementary analysis using the CPS to confirm that the state laws display the expected increases in private insurance status.

Bailey (2014) uses the American Community Survey data from 2005-2011 to study entrepreneurial activity among 19-25 year olds in the first year of the ACA policy (2011) compared to those aged 27-33 years, and finds that receiving coverage increases the probability of starting a business by 13-24 percent. In relation to the number of individuals in the treatment group who received coverage, this effect implies that receiving dependent coverage increases the probability of starting a business by 2 to 3 fold. This result is driven by behavioral changes among women, and the very young (19-22 year olds). The ACS codes an individual as self-employed if they work more hours for their own business for others, so that measure could be considered a more conservative measure of self-employment relative to that in tax data. On the other hand, self-employment may be underreported in tax data relative to survey data (since there are financial benefits from underreporting in tax data), and such underreporting may be more acute among the unincorporated self-employed, the group for which Bailey found an effect.

Bailey and Chorniy (2014) examine job-lock using monthly records of labor market activity available in the CPS for 2008-2013. They examine the rate at which 19-25 yr olds switch jobs during the previous 4 months, relative to 27-32 yr olds. The results indicate that there is no differential rate of switching coverage among the younger vs the older group, pre vs post policy change.

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. Young adults might be less likely to marry, to the extent that marriage provides an additional avenue to insurance coverage. They may also be more likely to have children if the health care costs of a pregnancy were a determining factor, although free

access to contraceptives through parental insurance may also lead to a decline in births. 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 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 1<sup>st</sup> 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.

Figure 1: Difference in Difference Comparisons

	Pre-policy period	Post-policy period
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Treatment age group	A	B
Control ages group	C	D

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

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

Our triple difference estimate starts with a calculation similar to [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 with benefits	A	B
Control ages group with benefits	C	D

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

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.<sup>3</sup> Research using these data is limited because of the highly restricted nature of

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<sup>3</sup> 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.

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 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 recent from recent years.

However, , there exists 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,<sup>4</sup> 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. This structure means that we are limited in conducting pre-trends tests: we cannot go back further than tax year 2008 (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 by birth cohort and year of tax data

		<u>Cohort</u>	
		1995 ..	1979
	<i>2004</i>	9	25
<u>Year</u>	2005	10	26
	2006	11	27

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<sup>4</sup> Almost all children 18 and younger are likely claimed on a parent's tax returns, especially given the tax benefits of claiming children.

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 yr 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 W2s 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,<sup>5</sup> 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 Identification Number (EIN) offers health insurance if any of the workers of that EIN have a W2 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

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<sup>5</sup> Author calculations using data at [http://www.bls.gov/web/cewbd/table\\_f.txt](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](https://ycharts.com/indicators/total_nonfarm_payrolls)).

an alternative, we use information on whether parents have a retirement plan to which the employer contributes. Retirement plan contributions are recorded on W2s 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.<sup>6</sup>

One may be concerned whether the use of tax data means there is selection into the sample due to the policy changes. However, we have information from W2s and some other tax forms regardless of whether the person filed an income tax return. For example, 1098-T forms are filed by all colleges and universities to the IRS, indicating tuition payments were received on behalf of a student. We examine whether the law changed the probability of filing a tax return, as that would otherwise potentially change our sample composition for variables (such as whether they are married, whether they are self-employed, or whether they have children) that are 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 yr 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

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<sup>6</sup> Another proxy would have been full-time employment in a large firm. However, tax data does not contain information on hours of work.

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).<sup>7</sup>

### Results:

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.<sup>8</sup> 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.<sup>9</sup>

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<sup>7</sup> 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.

<sup>8</sup> 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.

<sup>9</sup> 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.

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.<sup>10</sup> 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 “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 significantly different from zero (none are), and a column for whether these outcomes have time trends that differ between the treatment and control groups in the years prior to the policy change (none do). 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. In Table 2, the raw DDD estimates too show that there are no outcomes that are statistically significantly different from 0, and that there is just one pre-trends test that fails (non-filers, at the 10% significance level). The coefficient magnitudes in the DD and DDD calculations are also fairly small, often less than one percent of the base. This lack of statistically significant results occurs despite the fact that our sample sizes are in the hundreds of thousands, even with a 1% sample of the CDW, thus would have the power to catch even more differences than in cases that use a smaller sample, as the CPS or ACS.

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

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<sup>10</sup> We report results that compare 19-25 yr olds to those aged 26-29 years of age. In unreported results, we experimented with alternate treatment groups defined as 23-25 yr olds or just 24-25 yr olds. (In all comparisons, the control group is 27-29 yr olds).

those who do not. We found fairly similar results when 23-25 yr olds were used as an alternate treatment group, and when we used our measure of parental employer health insurance for the DDD estimates, in unreported results. Thus the causal effects of the YA provision on labor market related outcomes appears minimal, at least through 2012.

#### Limitations and caveats of method and data

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 show about a 7 percentage point increase in dependent employer coverage for young adults aged 19-25 in the period after the law, compared to before the law. We do not have a comprehensive DDD estimate for insurance coverage from that paper, but we expect it to be in the 10-14 percentage point range; our estimates indicate that 56% of all young adults have parents with access to retirement benefits (which we take to proxy for health insurance availability).

Our DDD method relies on separating those with access to coverage through parents. Since our variable for employer health insurance through W2s 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.

### Conclusion

There is very little prior evidence on how access to health insurance affects socioeconomic outcomes of young adults relevant for public finance. Tax data provide a unique method to estimate the impact of the YA ACA requirement using a triple difference strategy, as well as a large sample of administratively reported data. Prior evidence 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 worked at all in the formal sector, self-employment, holding of employer provided retirement and health benefits, educational enrollment, total wages, whether income tax returns were filed, whether married and whether had children. We find no statistically significant effects on any of the outcomes we study, and 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 W2, 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. As data become available, we will be able to analyze other features of the ACA. 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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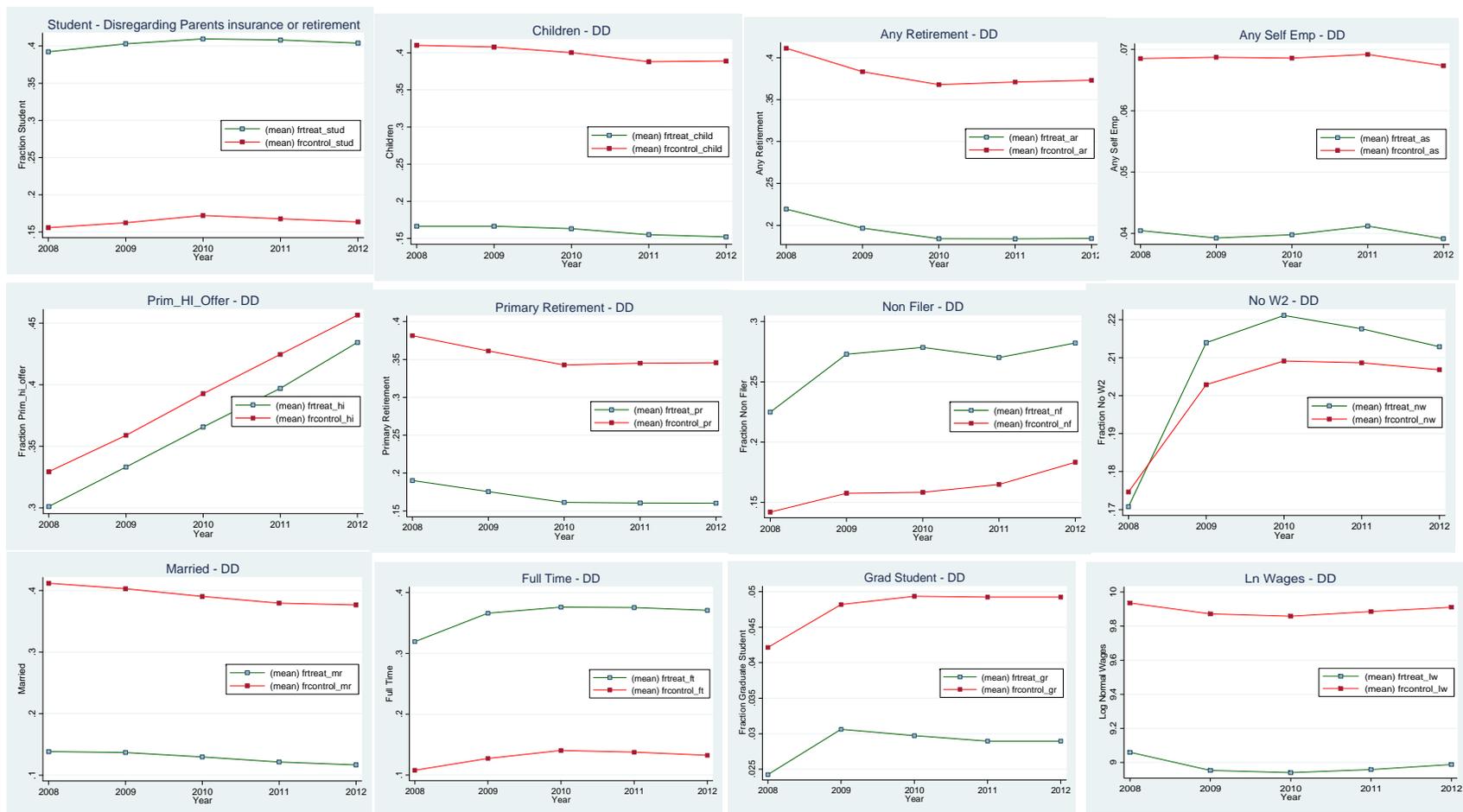
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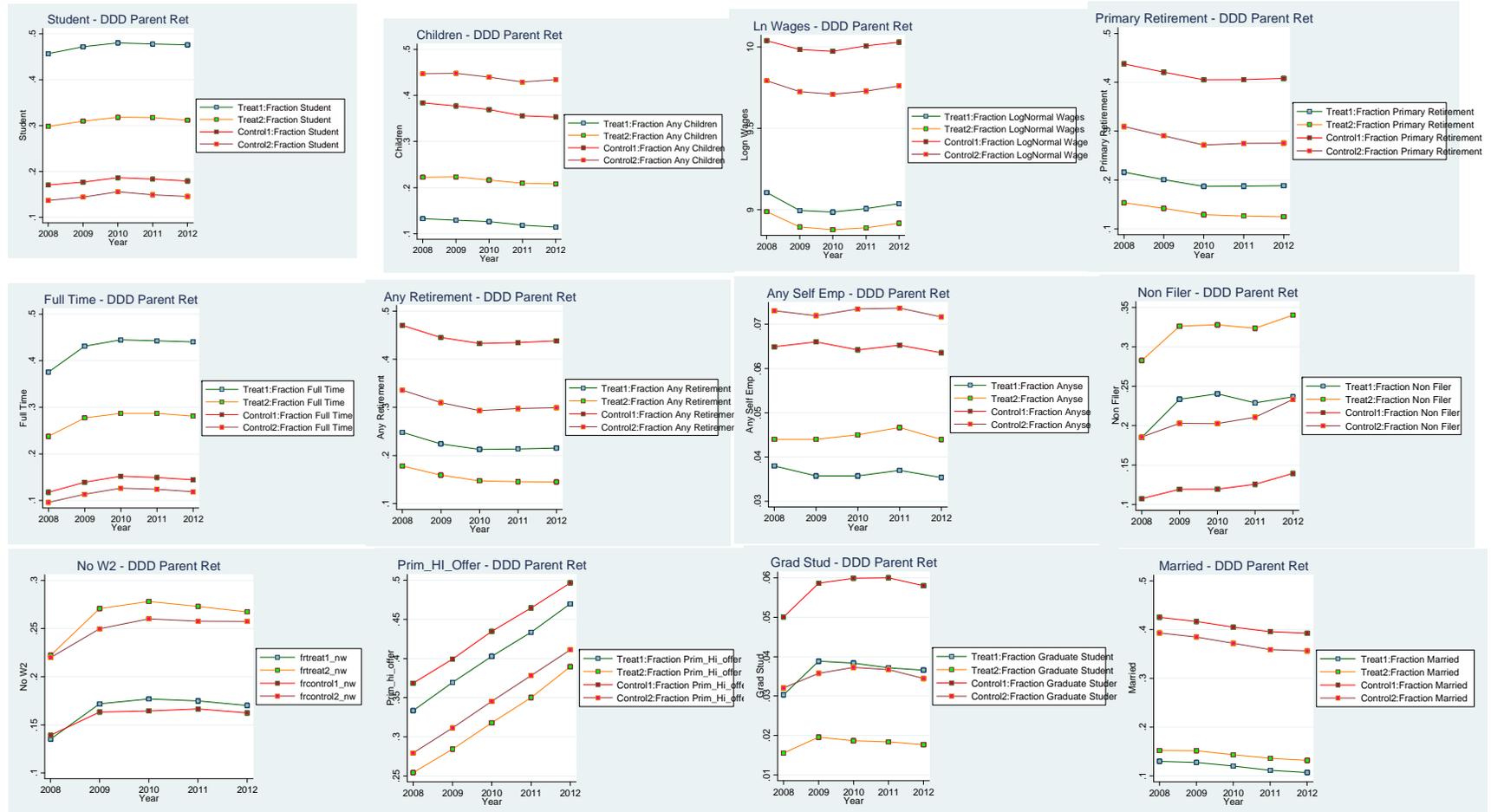
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Figure 3: DD Graphs



Note: Definitions of abbreviations used in the Figure. Student=enrolled in college or university, Children=has any children, LnWage=log annual wages, Primary Retirement=has retirement from primary employer, Full Time=full time student, Any Retirement=has retirement at any job that year, Any Self Emp=has any source of earnings that is self-employment, non-filer=did not file tax return, no W2=did not have a Form W2 that year (non worker), prim\_HI Offer=had an offer of health insurance at primary job, grad student=enrolled as a graduate student, married=married.

Figure 4: DDD Graphs



Note: please see definitions in note to Figure 3.

Table 1: DD Calculations							
Variable	$(\text{Post-Pre})_{\text{treat}} - (\text{Post-Pre})_{\text{control}}$	$(\text{Post-Pre})_{\text{treat}}$	$(\text{Post-Pre})_{\text{control}}$	Mean (post, treatment)	Percent Effect	Statistical significance level	Whether trends are different, pre policy at certain significant level
Student	0.002	0.008	0.007	0.408	0.49		
any children	0.008	-0.013	-0.021	0.16	5		
any retirement	0.001	-0.024	-0.025	0.183	0.54		
any self-employment	0.001	0.0003	-0.0004	0.04	2.5		
full time student	0.013	0.030	0.017	0.375	3.47		
primary retirement	0.004	-0.022	-0.026	0.159	2.52		
Married	0.011	-0.018	-0.029	0.125	8.8		
graduate student	-0.002	0.001	0.003	0.028	-7.14		
Nonfiler	0.003	0.027	0.024	0.277	1.08		
no w2	0.004	0.023	0.019	0.215	1.86		
health insurance offer	0.002	0.099	0.096	0.433	0.46		
log wage	-0.028	-0.034	-0.006	8.97	-0.31		
<p>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.</p>							

Table 2: DDD Calculations					
Variable	DDD	Mean (post, treatment, parents with retirement)	Percent effect (ret)	Statistical significance level	Whether trends are different, pre policy at certain significant level
student	0.001	0.48	0.21		
any children	0.009	0.121	7.44		
any retirement	-0.001	0.212	-0.47		
any self employment	-0.001	0.036	-2.78		
full time student	0.010	0.445	2.25		
primary retirement	-0.0004	0.186	-0.22		
Married	-0.006	0.115	-5.22		
graduate student	-0.001	0.036	-2.78		
non-filer	0.005	0.234	2.14		*
no w2	0.005	0.173	2.89		
health insurance offer	0.002	0.469	0.43		
log wages	-0.011	9.031	-0.12		
<p>Note: The first two columns show the raw DDD estimates that use where parents have retirement benefits. This is <math>(((\text{Post-Pre})\text{treat} - (\text{Post-Pre})\text{control}) \text{ p\_retirement}) - [((\text{Post-Pre})\text{treat} - (\text{Post-Pre})\text{control}) \text{ no\_p\_retirement}]</math>. 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 (for brevity, only the parental retirement version is show. Values are similar for the parental health insurance version). 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.</p>					

Appendix Table 1: Sample Sizes by Age and Year, Internal Revenue Service (IRS) Compliance Data Warehouse (CDW) data 1%  
Sample

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: 2010 data, and data for 26 yr olds, are not used in the differences calculations.