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The Effect of Health Insurance on Young Adults' Labor Market Outcomes: Evidence from the Affordable Care Act's Dependent Coverage Expansion

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by

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1 Introduction

Aimed at expanding health insurance to all Americans, the Patient Protection and Affordable Care Act (ACA) was signed into law in March 2010. On September 23rd, 2010, the first mandate of the ACA to take effect was the expansion of the dependent coverage of young adults, allowing them to remain on their parents' employer-sponsored and private insurance until the age of 26. Prior to the federal mandate, many states had implemented similar policies. However, those expansions are generally considered weaker than the ACA mandate, since they required that young adults be students, unmarried, have no dependents, or be financially dependent on their parents.

Before the expansion, young adults had remained the age demographic with the highest rates of uninsurance (Levine, Mcknight, Heep, & Levine, 2011). Even though young adults represent a relatively healthy population who generally make less use of health insurance coverage, large rates of uninsurance can result in several negative social and economic outcomes. Uninsured young adults are more likely to forego medical care due to cost, and are the most sensitive to out-of-pocket expenses (Barbaresco, Courtemanche, & Qi, 2015). Since young adults also have lower incomes than adults in older age groups, those who are uninured and seek medical care may accrue large medical debts relative to their incomes. The consequences extend to labor supply as well, since young adults may forego further human capital accumulation through higher education to enter the labor market in order to find jobs that offer employer-sponsored health insurance (ESHI), which overwhelmingly require full-time employment (Depew, 2015). They are less likely to shift to other jobs even when they offer a higher salary or better match their skills, an effect known as "job-lock" (Madrian, 1994).

In this paper, I measure the effect of gaining dependent coverage on labor market outcomes of young adults. The ACA's expansion provides a natural experiment where I can exploit the exogenous source of variation in insurance coverage brought on by the law to instrument for health insurance status. In other words, the expansion provides a way to study the effects of gaining health insurance without coverage being linked with individual-level characteristics such as income or labor status, which would've biased any estimates. This instrumentation allows me to mitigate the endogeneity present between health insurance and labor status. I first measure the causal effect of the ACA's dependent coverage expansion on insurance rates of post-college age young adults using a difference-in-differences (DD) design, where the difference of the changes within affected and unaffected groups provides a causal effect of the expansion. I define treated ages as 23-25, and control ages as 27-29, as they are most similar in their demographic characteristics and labor supply decisions. Then using a two-stage residual inclusion control function method, I instrument for insurance coverage status to find a causal effect of dependent coverage on labor force status, labor supply, and income.

I confirm the findings of previous literature, showing that dependent coverage expansion has increased insurance rates among young adults, decreased own-name coverage, and increased dependent coverage. With respect to labor market outcomes, I find that as a result of dependent coverage, young adults are more likely to exit the labor force and less likely to work full-time. I find that the law has led to a significant decrease in income, and I find mixed evidence on its effect on work hours.

Section 2 details a review of the literature on ACA dependent coverage outcomes, state-level expansions, and health insurance and labor supply. In section 3, I present a description of the data used in this paper and how I resolve issues in the data. Following that, in section 4, I explain the difference-in-differences design and control function methodology. In section 5 and 6, I present estimates and a discussion of what can be concluded from this analysis.

2 Literature Review

Since the implementation of the federal dependent coverage expansion, researchers have produced an extensive body of literature that looks at both its intended and unintended effects on the young adult population. The literature covers the expansion's effects on health insurance coverage, health care access and use, health status, labor market outcomes, education, and time allocation. Since the mandate uses only the criterion of age for eligibility, the majority of these studies use a DD framework to measure causal effects on the variables of interest, with a few using the addition of triple differences (DDD) and regression discontinuity (RD) designs. In most studies, a treatment group — the ages affected by the expansion —

are defined as 19-25 year-olds, and the control group is defined as an age group just beyond the expansion's cutoff, generally ranging from 26-34 years old. This study design rests on the assumption that the choices and circumstances of the treatment and control groups are sufficiently similar, and that they exhibited parallel trends prior to enactment of the policy.

2.1 Health Insurance Coverage

Estimating the policy's effect on insurance coverage, studies have found an increase in the probability of having dependent coverage and a decrease in being uninsured across a number of data sets, regression specifications, and age bands for treatment and control groups. Using the Annual Social and Economic Supplement to the Current Population Survey (ASEC or March CPS), Cantor, Monheit, DeLia, and Lloyd (2012) and Sommers and Kronick (2012) find between a 4.3-5.3 percentage point increase in dependent coverage for young adults aged 19-25 relative to those aged 27-30 and 26-34, respectively. Antwi, Moriya, and Simon (2013) use the 2008 Survey of Income and Program Participation (SIPP) panel to show a 7 percentage point increase in dependent coverage under parental employer-sponsored insurance (ESHI), a 3.1 percentage point decrease in own-name coverage, and a 3.1 percentage point increase in the probability of having any insurance. The authors of the study use a control group consisting of ages 16-18 and 27-29, reasoning that the addition of the younger control group "may reflect the changing circumstances of employer dependent coverage." Slusky (2012) uses data from the March CPS and CPS's Educational Supplement (October CPS), the Behavioral and Risk Factor Surveillance Survey (BRFSS), and the Consumer Expenditure Survey (CES) to algorithmically identify dependent coverage under parental insurance. The paper uses treatment and control groups similar to Antwi et al. (2013) and finds a 7-9 percentage point increase in parental dependent coverage, and a 4-5 percentage point drop in own-name coverage.

Sommers, Buchmueller, Decker, Carey, and Kronick (2013), using the 2005-2010 National Health Interview Survey (NHIS) including early release data for three quarters of 2011 and the 2006-2011 March CPS, find a 4.7 percentage point increase in coverage and a 5.1 percentage point increase in private coverage relative to a control group of ages 26-34. The authors also find an increasing rate of coverage throughout 2011 and that coverage increased at a greater rate for young adults who report fair or poor health status. The expansion also reduced the probability of young adults delaying care (4 percentage points) and forgoing care (2.3 percentage points) due to cost. Looking more finely at the changes, O'Hara and Brault (2013) and Shane and Ayyagari (2014) find that while all races have made gain in coverage as a result of this reform, it has done little to close the coverage gap between white and non-white populations.

Overall, the evidence is strong that the ACA dependent coverage expansion works as intended, with a significant effect on coverage rates among young adults, and even an increase in private dental coverage (Shane & Ayyagari, 2015).

2.2 Labor Market Outcomes

There is mixed evidence on the ACA's effect on labor market outcomes for young adults. The earliest work, by Antwi et al. (2013) finds full-time working status dropped by 2 percentage points and working hours are reduced by about 3%. However, results for the change in hours are statistically weaker when using the logarithm of hours worked (log hours), and it should be noted that the paper does not account for year-specific time trends. Slusky (2012) finds a 23-26 percentage point decrease in full-time work, and a 17-18 percentage point increase in part-time work. Slusky attributes this change in the magnitude of the effect to the addition of 2011 data, and a misspecification of some observations into the treatment group in the earlier study (Antwi et al., 2013).

Later work examining labor market outcomes finds no effect. Bailey and Chorniy (2016) closely follow the design of Antwi et al. (2013), with a control group consisting of both a younger age group of 16-18 and older age group of 27-32. With 2008-2013 CPS Basic Monthly data, the paper looks at job mobility through job switches — that is, whether the individual has changed jobs in a given time period. They find no statistically significant effect of the law's enactment on reducing "job-lock," and offer possible explanations, stating that "either the mandate was not strong enough to reduce job lock, or there was no job lock among young adults to begin with." Similarly, Heim, Lurie, and Simon (2015), using administrative records from the 2008-2012 Internal Revenue Service (IRS) Compliance Data Warehouse (CDW) data linked to parental tax data and a narrower control group age band of

27-29 in DD and DDD regressions, shows no change in employment status, job characteristics, or educational enrollment of young adults. Kim (2016) employs both a DD design with a control group of ages 26-32, and an "aging-out-at-26" RD design to find no effect of the expansion on young adult labor supply after accounting for differential time trends. After redefining the treatment and controls as individuals with parents who have own-name ESHI and individual whose parents do not have own-name ESHI, respectively, the study still finds no changes in labor supply. In the paper, Kim also shows graphically that there may be violations of the parallel trends assumptions post-2008, suggesting that the treatment and control groups were differentially affected by the Great Recession.

Slusky (2015) provides evidence that age bands are affected differently by contemporaneous economic shocks, and that the default DD methodology of previous papers may be overstating the effect of the ACA. The study attempts to replicate Sommers and Kronick (2012), Cantor et al. (2012), and Antwi et al. (2013) using their respective sources of data, and constructs placebo tests by using earlier treatment periods (e.g., 2003-2007 set as a pre-treatment period, and 2008 set as a post-treatment period). Slusky's results show that these placebo tests produce statistically significant "effects" of artificial treatments at various points, both with health insurance and labor market outcomes, although the estimate for parental ESHI dependent coverage remains robust against placebo regressions. This raises concerns when studying the secondary effects of the ACA expansion, since the paper shows that the study design may be providing spurious results. With respect to labor market outcomes, this may be stemming from how economic conditions produce differential effects on and create differential responses by a younger, new to the labor market cohort versus an older cohort who already have some working experience. Slusky recommends narrowing age bands for both treatment and controls as a way to ameliorate these estimate biases. Influenced by this study, several papers have used narrowed age bands and included placebo tests (Barbaresco et al., 2015; Antwi, Moriya, & Simon, 2015; Depew & Bailey, 2015; Colman & Dave, 2015; Kim, 2016; Lenhart & Shrestha, 2016; Jung & Shrestha, 2016).

In contrast to the studies that see no labor market changes among young adults in response to the expansion's enactment, Lenhart and Shrestha (2016) do see changes in young adult labor supply. Using the 2008-2013 March CPS and a typical DD design, they show a

48 minute reduction in weekly work hours, a 2 percentage point substitution from full-time to part-time working status. They also study time allocation variables: with data from the American Time Use Survey (ATUS), they find a reallocation of roughly 2 hours per week to leisure activities, with 67% of it spent watching television, and no change in time spent on "human capital accumulating" health or education related activities. Another paper on time-use finds a 17-23 minute decrease in time worked per day, no change in time spent waiting or receiving medical care, 9 minutes more spent on non-television related leisure activities, and 6 minutes more spent on education (Colman & Dave, 2015). When looking at the ACA's effect on education enrollment, Jung and Shrestha (2016), using a DD design with 19-23 year-olds, assigning those whose parents have private coverage to the treatment group, and those with no insurance to the control group (individuals with parents with public insurance are dropped from the analysis), finds a 3 percentage point decrease in full-time college enrollment and no change in part-time college enrollment.

2.3 State Level Expansions

During 2003-2009, prior to the federal policy to expand dependent coverage, several states enacted their own mandates on dependent coverage provisions. These policies generally differed from the federal mandate and are thought of as "weaker" since the expansions were both limited by age and tied to the status of the dependent, but provide a basis for how the ACA may affect the young adult population over a longer time horizon. The age limits for state expansions varied from 24-30 and were conditional on the individual being a student, single, or having no dependents. Dillender (2014) and Hahn and Yang (2016) provide a graphical representation detailing the conditions of each expansion state. Accordingly, researchers exploit the variation in coverage between expansion and non-expansion states using DD design to measure the causal impacts of these state reforms on young adults' coverage and labor.

Monheit, Cantor, DeLia, and Belloff (2011) found with data from the 2000-2008 March CPS that among young adults 19-29, the state expansions increased dependent coverage by 1.52 percentage points, increasing to 3.84 percentage points when the age band is limited to 19-25, but found no effect on overall insurance rates. Levine et al. (2011) saw a similar

2.2 percentage point increase in private coverage and no change in overall insurance rates among 19-24 year olds, but a DDD analysis finds a larger 4.4 percentage point increase in private coverage and a significant 3.3 percentage point increase in the insurance rate.

However, in Burgdorf (2014), by disaggregating dependent coverage into parental and spousal dependent coverage and further differentiating whether it was employment-related or privately purchased, finds that the results from Monheit et al. (2011) were largely driven by spousal coverage. Falsification tests, where treatment periods were shifted up or down in years, or age limits were changed, also found significant effects, suggesting the results may have been spurious. Attempting the same with the specifications of Levine et al. (2011), the paper finds that results were driven by employment-related parental coverage, and the falsification tests suggest the results were valid. After changing specifications of each model to reconcile the differences in falsification test results between the two studies, Burgdorf notes that the data simply may not be sufficient to estimate the subtle changes as a result of these state policies.

Dillender (2014) looks the state reforms' effect on education and wages. The paper finds that, as a result of dependent coverage expansions, men were 2.5 percentage points more likely to have a college degree by age 26. The results for women were insignificant except for high school completion. With respect to wages, Dillender finds that the reforms have increased wages for men 1.6% after they no longer have access to their parent's insurance coverage, which could be driven by the increase in education. Dillender also finds that, among women, wages increased by 3.1% while the cohort eas covered by parental insurance. This effect continues after the loss of coverage as a 2.1% wage increase. Similarly, Depew (2015) uses DDD specifications to find that, although there was no change in labor supply at the extensive margin, there was a 2.65 percentage point decrease in full-time employment, and 2.5% reduction in hours worked, using all eligibility criteria. The paper also finds these effects are stronger among women than men. Hahn and Yang (2016) look at the impact of the state reforms on the work decisions of young adults. The paper finds a decrease in full-time employment of 3.7 percentage points one year after enactment of reforms, 4.5 percentage points two years later, and 2.3 percentage point three years later.

2.4 Health Insurance and Labor Supply

The relationship between health insurance and labor supply has been studied extensively in health economics literature (Gruber & Madrian, 2002). Much of this literature is concerned with three outcomes: "job-lock," labor force participation, and wage determination.

Job-lock, or job-to-job immobility, can manifest as labor market inefficiencies when employees, tied to their jobs in order to receive health coverage, are required to provide more labor than they would prefer (nearly all ESHI coverage requires full-time employment), or earn less than would have if they could change jobs. These effects can be larger if the employee or the employee's dependents require the ESHI due to poor health status (Stroupe, Kinney, & Kniesner, 2000). Cooper and Monheit (1993), Madrian (1994), and Buchmueller and Valletta (1996) find that individuals with ESHI coverage had a decline in the likelihood of job change.

However, job-lock is reduced when continuation benefits, which allow job leavers to stay on the health plans provided by their former employers for a limited time, are provided (Gruber & Madrian, 1994). There are even greater effects of increased job change and changes in labor force participation when examining the effect of spousal ESHI dependent coverage: in married women, it resulted in an increased probability of exiting the labor market, working fewer hours per week, and switching from full-time to part-time employment (Buchmueller & Valletta, 1999). This effect was found in men as well and across races (Wellington & Cobb-Clark, 2000; Buchmueller & Valletta, 1996).

Early work studying the association between ESHI and wages was at first found to be positive (Leibowitz, 1983; Monheit, Hagan, Berk, & Farley, 1985; Olson, 1992). Since then, by exploiting the natural experiment found in the introduction of mandated private insurance or expansions of private insurance, Gruber (1994) and Kolstad and Kowalski (2012) show evidence of a compensating wage differential — that is, jobs offering ESHI pay less in salary than ones that do not offer ESHI. Kolstad and Kowalski also show that the decrease in wage is greater than the cost of the insurance to employers, similar to findings from Eberts and Stone (1985). From a firm's perspective, providing insurance along with pay, where each extra dollar spent on health care plans allows them to decrease pay by more than one dollar, results in an overall lower valued compensation package. This allows firms to remain competitive while still attracting high quality labor, relative to offering no health insurance and paying more than the value of the compensation package (Currie & Madrian, 1999).

Even given this prior literature, the expansion of health insurance to young adults has only been studied recently. Since the ACA's expansion allows young adults to remain on health insurance without being tied to any factor such as employment, income, student status, marital status, claimed as dependents by their parents, or having children of their own, it provides a means of studying the exogenous effect of health insurance gain on a population. This is not without unintended consequences, including fewer young adults marrying and increases in divorce (Abramowitz, 2016), a decrease in precautionary household savings (Lee, 2016), and an increase in reckless behaviors, such as excessive drinking (Qi, 2015).

However, in line with the literature on other populations' labor market outcomes and previous outcomes from state level expansions, there should be a decrease in hours worked, a shift away from full-time employment, and exits from the labor force. Welfare implications of the mandate would include the ability to accumulate more human capital through education and sorting into jobs that better match the individual's skills. The increased likelihood of young adults being on dependent coverage may make the population more attractive to employers and increase salaries, since neither party need be concerned with ESHI.

3 Data

I use the Current Population Survey's Annual Social and Economic Supplement, commonly known as the March CPS, extracted from the Integrated Public Use Microdata Series (IPUMS) at the Minnesota Population Center. The Minnesota Population Center, a co-operative at the University of Minnesota, harmonizes and maintains the March CPS across years, creating an integrated source of CPS data for researchers. The March CPS is a nationally representative, household-based survey of the civilian, non-institutionalized population administered yearly by the U.S. Census Bureau using computer-assisted telephone interviewing. However, it is microdata; each observation in the data represents an individual within the household. Each year of the March CPS contains responses from approximately 100,000 households, with information covering demographics, education, employment, healthcare coverage, poverty, and migration. These characteristics make the March CPS a popular choice amongst social scientists for policy-related analyses.

For the purposes of this paper, the March CPS is propitious in a few ways. It allows me to distinguish between private health insurance in one's own name, and as a dependent on another individual's plan. For young adults who are no longer living in their parents' household, the data still can identify if they are covered as a dependent on the insurance policy of a non-householder. As opposed to looking at the change in private insurance as a single category, I can investigate the substitution effect between own-name and dependent coverage, and the labor supply effects of specifically the expansions of dependent coverage. Although private dependent coverage can be sub-categorized by the source of the coverage, either parental or spousal, the method only works for some observations (those without insurance from outside the household), and can create ambiguities about the categories to which certain observations belong.

Each year's CPS contains data about an individual's demographics, which are asked as point-in-time questions, along with responses to questions regarding their employment and healthcare status for the previous year. For example, the March CPS for 2006 will contain data about an individual's current age, but have answers to questions about their employment and insurance coverage for 2005, the reference year for the data. These retrospective questions raise the issue of recall bias in the response (Antwi et al., 2013; Slusky, 2012). However, as noted in the the Literature Review section, a significant amount of previous literature has found similar estimates for coverage from a variety of sources, including the March CPS.

Another time-related issue is that the dependent coverage expansion took place in September 2010. Prior literature (Slusky, 2012) have resolved this by omitting March CPS 2011, with reference year 2010, from their analyses. In this paper, I've included the 2010 year as a pre-treatment year, which I justify by reasoning that labor supply changes from this expansion would have a delayed effect, especially with labor market outcomes. Since the policy took effect in the last quarter of the year, its effects would increase throughout 2011, as the majority of health insurance plans, particularly ESHI, are renewed in January. Excluding

2010 also results in an increased magnitude effect of the expansion on young adults' insurance rates, as seen in Slusky (2012), which may be an overestimation from the discontinuity in the data.

Additionally, although being 26 years-old during the time of the interview places them in the control group, these respondents are providing information about the status for age 25. Since the data does not include when they turned 26 (i.e., their date of birth) this creates considerable uncertainty about whether they are treated or control observations. As in prior literature, I address this by removing individuals aged 26 from the analysis, leaving the treatment group as ages 23-25, and the control group as ages 27-29 (Slusky, 2012; Antwi et al., 2013; Barbaresco et al., 2015). Since 27-year-old respondents answered questions about their status as 26 year-olds, they fall squarely into the control group.

Lastly, in the March CPS, individuals can be coded as having two types of insurance. I resolve this by giving preference to dependent coverage; that is, if an observation has had 2 types of insurance in the past year, and one of those types is dependent coverage, I code that observation as having dependent coverage, reasoning that the individual most likely switched from another type of coverage into dependent coverage. The age group mostly likely to have gone from dependent coverage to another type of coverage would have been 26 year-olds, since they would have lost the ability to be covered by their parents' insurance in the past year. Since they are excluded from the data, it is reasonable to assume that given the choice between other types of insurance and dependent coverage, an individual prefers dependent coverage.

The design of the March CPS makes it simple to pool across years to create a panel data set, and therefore for this analysis I use the March 2006-2016 surveys. I limit the data to non-active duty status, post-college aged young adults between ages 23-25 and 27-29, the former as the treated group and the latter as the control. I have 160,312 observations from the 11 years of pooled data, and I present unweighted means for demographic control variables in Table 1. Both age groups are quite similar in demographic characteristics, with most indicators falling within 2 percentage points of each other. The largest difference is in the category of the percentage of individuals with a Bachelor's degree and higher, with 24.7% for the treated group, and 30.8% for the control group, and the percentage of individuals reporting excellent health status with 39.2% for the treated group, and 35.9% for the control group. Both differences are to be expected, as the control group is older, and since these are not variables of interest in this paper, it is still reasonable to include them as controls.

4 Methods

I use a differences-in-differences (DD) design to estimate the causal effect of the dependent coverage expansion on insurance rates among young adults, and a control function framework to estimate the labor supply effects of the change in health insurance status. My main identification strategy in my DD estimation uses age-time dimensions. Since the expansion is defined strictly by age, I use a treated group of individuals aged 23-25 and a control group aged 27-29. The use of small age bands for each group is based on the the recommendations of Slusky (2015), and the assumption that post-college age young adults in their mid- to late 20s are sufficiently similar in characteristics and choices, differing only in eligibility for this policy. The policy took effect in September 2010, so I define a pre-treatment period of reference years 2005-2010, and a post-treatment period of years 2011-2015. The multinomial logistic regression model of the DD estimation is of the form below:

$$H_{igst} = \beta_0 + \beta_1(treat_g * post_t) + \beta_2 Age_g + \beta_3 Year_t + \beta_4 State_s + \beta_5 X_{igst} + \varepsilon_{igst} \quad (1)$$

H_{igst} represents the insurance coverage of individual i, of age g, in state s, in year t. This outcome variable is categorical variable, defined as equaling 1 if the individual is uninsured (the reference level), 2 if covered by any type of public insurance, 3 if covered by insurance on which the individual is a policyholder, and 4 if covered as a dependent on the insurance policy of another individual. Age_g, $Year_t$, and $State_s$ are categorical variables that represent fixed effects for age, state, and time. X_{igst} is a vector of individual-level demographic variables. These include categorical variables for gender, race, education level, marital status, health status, disability preventing work, metropolitan area, dwelling ownership, and dependents. The estimate of interest is β_1, which represents the effect of the interaction of the age group an individual belongs to and whether the observation comes from before or after the mandate's enactment. I use the CPS-provided individual-level weights and robust standard errors are clustered that the state level.

To estimate the effect of dependent coverage on labor force participation and labor supply, I implement a control function approach with the use of two-stage residual inclusion method (2SRI). Naively regressing on health insurance status fails to correct for the endogeneity between health insurance status and labor supply, introducing bias in the estimates. Using IV methods such as two-stage least squares (2SLS) are also prone to biased estimates arising from the nonlinear models in the analysis. As described in Terza, Basu, and Rathouz (2008), using the 2SRI method produces consistent estimators in the nonlinear two-stage models. In this case, I instrument health insurance status with dependent coverage eligibility. The estimate of eligibility in equation (1), represented by β_1, carries a causal effect on coverage type that is statistically significant. Since eligibility is exogenous, that is, the dependent expansion was determined by an arbitrary measure (age), there is no reason to presume that any individual-level characteristics that would be correlated with the first-stage estimate. The 2SRI works by calculating the residuals, associated with each category of health insurance status from the first-stage DD regression, by differencing the observed health insurance status and the predicted value of the model. In the second stage, these calculated residuals are included as covariates along with the health insurance status variable, and the estimate on the health insurance status variable gives the causal effect of exogenously gaining health insurance in the form of dependent coverage on labor supply and income outcomes. The second-stage model I use is of the following form:

$$L_{igst} = \beta_0 + \beta_1 H_{igst} + \beta_2 Age_g + \beta_3 Year_t + \beta_4 State_s + \beta_5 \varepsilon_2 + \beta_6 \varepsilon_3 + \beta_7 \varepsilon_4 + \beta_8 X_{igst} + \mu_{igst} \quad (2)$$

L_{igst} are the observed labor market outcomes of individual i, of age g, in state s, in year t. I examine the labor force participation of individuals with a multinomial logistic regression, using a categorical outcome variable which equals 1 if the respondent was not in the labor force, 2 if unemployed, 3 if working part-time, and 4 if working full-time. The labor supply outcomes I use are usual hours worked per week last year, actual hours worked last week, and the log versions of both variables. These are continuous variables that are truncated for more than 99 hours worked, but should not affect estimates since those working 99 or more hours represent about 0.1% of the sample. For wage, I use the continuous variable indicating income from wage, which I also log. H_{igst} is the categorical observed health insurance status variable, where it equals 1 if the individual is uninsured, 2 if covered by any type of public insurance, 3 if the individual has own-name private insurance coverage, and 4 if the individual is has dependent coverage. Age_g, $Year_t$, and $State_s$ are categorical variables that represent fixed effects for age, state, and time. X_{igst} is a vector of individual-level demographic variables identical to the vector in equation (1). The variables ε_2, ε_3, ε_4 represent the residuals calculated based on outcomes except for the reference level from the control function. Here, the estimate of interest is β_1, the effect of coverage type on labor market outcomes. I present in my tables the marginal effect of having β_1 equal 4, that is, the marginal effect of dependent coverage eligibility. I use the CPS-provided individual-level weights and robust standard errors are clustered that the state level for all control function regressions.

5 Results

Table 2 shows descriptive statistics for the coverage and labor market variables in question for the populations. The first three columns show the unweighted means for the control and treatment group, and total sample in the pre-treatment reference years of 2005-2010. The next three columns show the means for the post-treatment period, from 2011-2015. From Table 2, there are some large differences between the change in the groups. For the 23-25 age band, there is a sharper decrease in uninsurance relative to the age 27-29 age band, as well as increases in dependent coverage, decreases in own-name coverage, decreases in full-time employment, and increases in part-time employment. The table shows that both groups have increased income and decreased working hours by a similar magnitude, as well as increased public insurance, and decreased unemployment.

What I attempt to show in this paper is that, despite the change in means, after controlling for the endogeneity of observed coverage using the enactment of the policy, the estimates may show a change in both direction and magnitude, which may deal with any over- or under-estimation arising from DD methods when analyzing the indirect effects of the policy.

In the second column of Table 3, I present the results of the DD regressions showing the causal impact of the expansion on coverage status. I find an approximate 4.02 percentage point increase in the overall insurance rate, a 0.78 percentage point decrease in public insurance coverage (significant at the 5% level), a 9.95 percentage point decrease in own-name private coverage, and a 14.74 percentage point increase in dependent coverage. As a check to measure if the effects in the main specification are being driven by other covariates, I also estimate coverage on only the interaction term and age and time fixed effects, finding a similar result, shown in column 1. Since the main specification resulted in a small but statistically significant change in public insurance coverage, I estimate the specification on a sub-sample limiting the years to 2005-2013, predating the ACA Medicaid expansion that began in 2014, which is presented in column 3. I find that the Medicaid expansion did have significance change on the estimate for public insurance; however, the estimates for dependent coverage remain comparable.

I estimate the control function model on three labor market outcomes. The first outcome is working status, categorically defined as not in labor force, unemployed, employed part-time, and employed full-time. Table 4 presents the results. I find that the dependent coverage eligibility results in a 13.13 percentage point increase in exiting the labor force, largely a substitution from the 12.22 percentage point decrease in full-time employment. It is worth noting that the CPS codes full-time employment status as 35 or more hours per week.

I estimate the effect on hours worked using ordinary least squares (OLS) regressions, since they require fewer strict assumptions to be made about the data to provide consistent estimates. I look at two variables estimating hours worked, one where respondent recorded how many hours they actually worked last week, and another where they reported how many hours they usually worked per work last year. Columns 1 and 2 of Table 5 present the results: there was no statistically significant effect of dependent coverage on hours worked last week, however usual hours worked last year show a decrease of 6.32 hours of work. Columns 3 and 4 show the estimates for log hours, last week and last year, respectively. The estimate for the log of hours worked last week is significant at the 10% level, and shows a 30% decrease in hours worked last week. Using the log of usual hours worked per week last year, I find that dependent coverage leads to a 42.3% decrease in working hours. There may be two mechanisms at work for the conflicting result. First, there may be a measure of recall bias when respondents report the number of hours they worked last year, and due to that bias, they respond with common weekly working hours (e.g., 20 hours for part-time or 40 for full-time). The respondent may have not worked throughout the year, and is responding with the hours per week when at work, without regard for the frequency of work. This lumping of hours may be leading to the significant effect that was found. Second, and much like the frequency of work issue with the usual hours variable, the reported hours worked last week may lead to a bias if a sufficient number of the working population in the data work fewer hours or at a lower frequency during the CPS interview period between March and May. It is also likely that since the reported insurance coverage type was for the previous calendar year, the reported usual hours worked last year may be a better measure of the effects of dependent coverage.

The first column in Table 6 presents control function model estimates for income, and the second column presents estimates for logged income. The estimate for effect of the policy on logged income from wage is -1.34, or a 73.8% decrease (significant at the 1% level). The estimates for income prove to be contrary to previous literature on health insurance using DD design, which have generally shown an increase in income results from access to health insurance not tied to one's own employer.

6 Conclusion

In this paper, I present an alternative approach to estimating the impact of health insurance on labor market outcomes, exploiting the exogeneity of the ACA's dependent coverage expansion by using it to instrument for health insurance status. I find that the federal mandate has led to a decrease in uninsurance and an increase in dependent coverage for post-college age young adults. This, in turn, has impacted labor market outcomes for this population. I find that young adults with dependent coverage sort out of full-time employment and exit the labor force, and have large decreases in income from wage. I also find mixed evidence of young adults reducing hours worked.

Unlike the literature in health economics on the labor market impact of federal dependent coverage expansion which generally finds no impact of the law on labor outcome, I find that the results are mixed. Although the link between health insurance and labor choices is well-researched, measuring a policy's secondary effects on the labor market is complex.

Nonetheless, the ACA's dependent coverage expansion provides researchers with a novel opportunity to study the effects of health insurance access and labor market decisions in a population where it may have long-run welfare implications.

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8 Tables

Table 1: Summary Statistics for Demographic Controls

	Ages 23-25	Ages 27-29	Total
Age	24.02	28.01	26.07
Female	0.522	0.534	0.528
White	0.542	0.566	0.554
Black	0.122	0.111	0.116
Hispanic	0.234	0.223	0.228
Asian	0.0607	0.0631	0.0620
Native	0.0188	0.0169	0.0178
Other Race	0.0231	0.0193	0.0211
Married	0.238	0.468	0.357
12th Grade and Less	0.113	0.116	0.114
HS Diploma or GED	0.297	0.284	0.290
Some College	0.253	0.192	0.222
Associate's Degree	0.0900	0.101	0.0955
Bachelor's degree and higher	0.247	0.308	0.278
Health Status: Excellent	0.392	0.359	0.375
Health Status: Very Good	0.343	0.354	0.349
Health Status: Good	0.217	0.227	0.222
Health Status: Fair	0.0392	0.0475	0.0435
Health Status: Poor	0.00903	0.0114	0.0103
Work Disability	0.0341	0.0385	0.0364
Has Dependents	0.287	0.499	0.396
Owns Home	0.451	0.495	0.474
Rents, No Cash	0.0160	0.0148	0.0154
Rents, With Cash	0.533	0.490	0.511
Observations	160312		

Note: Unweighted means. Data from 2006-2016 March CPS.

Table 2: Summary Statistics for Dependent Variables

	Pre-treatment, 2005-2010			Post-treatment, 2011-2015		
	23-25	27-29	Total	23-25	27-29	Total
Uninsured	0.331	0.272	0.301	0.235	0.234	0.235
Public Ins.	0.116	0.107	0.111	0.142	0.143	0.142
Own-name Cov.	0.347	0.435	0.392	0.249	0.429	0.342
Dependent Cov.	0.206	0.186	0.196	0.374	0.194	0.282
Not In LF	0.209	0.182	0.195	0.231	0.195	0.212
Unemployed	0.0787	0.0637	0.0710	0.0746	0.0577	0.0659
Part-time Work	0.198	0.164	0.180	0.207	0.158	0.182
Full-time Work	0.514	0.591	0.554	0.488	0.590	0.540
Hours Last Week	25.73	28.64	27.23	24.84	28.53	26.73
Hours Last Year	29.77	32.84	31.35	27.95	31.67	29.86
Wage Income	18250.2	26403.8	22442.6	19355.0	28757.1	24182.2

Note: Unweighted means. Data from 2006-2016 March CPS.

Table 3: DD Estimates for the Effect of the Expansion ($treat * post$) on Coverage

	(1) Limited Model	(2) Full Model	(3) Pre-2014 Sub-sample
Uninsured	-0.0392***	-0.0402***	-0.0385***
	(0.00491)	(0.00493)	(0.00567)
Public Insurance	-0.00821	-0.00778*	-0.00383
	(0.00452)	(0.00381)	(0.00340)
Own-name Coverage	-0.103***	-0.0995***	-0.0977***
	(0.00772)	(0.00553)	(0.00575)
Dependent Coverage	0.151***	0.147***	0.140***
	(0.00922)	(0.00769)	(0.00758)
Year Fixed Effects?	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes
State Fixed Effects?	No	Yes	Yes
Demographic Controls?	No	Yes	Yes
Observations	160312	160312	132737

Note: Multinomial logistic regressions in difference-in-differences design. Uses data from the March CPS 2006-2016 (reference years 2005-2015). Demographic controls include race, gender, education level, marital status, health status, work disability, metropolitan area status, dwelling ownership status, and dependents. Weighted using CPS-provided individual-level weights. Robust standard errors clustered at the state level in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Control Function Model Estimates for Work Status

	(1) Effect of Dependent Coverage
Not In LF	0.131***
	(0.0262)
Unemployed	-0.0175
	(0.0166)
Part-time Work	0.00833
	(0.0310)
Full-time Work	-0.122**
	(0.0420)
Observations	160312

Note: Multinomial logistic regression. Uses data from the March CPS 2006-2016 (reference years 2005-2015). Demographic controls include race, gender, education level, marital status, health status, work disability, metropolitan area status, dwelling ownership status, and dependents. Includes control function residuals and age, year, and state fixed effects. Weighted using CPS-provided individual-level weights. Robust standard errors clustered at the state level in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Control Function Model Estimates for Hours Worked

	(1) Hours LW	(2) Hours LY	(3) Log Hours LW	(4) Log Hours LY
Dependent Coverage	-3.098	-6.318***	-0.358	-0.550***
	(2.496)	(1.743)	(0.209)	(0.142)
Observations	160312	160312	160312	160312

Note: OLS regressions. Uses data from the March CPS 2006-2016 (reference years 2005-2015). Demographic controls include race, gender, education level, marital status, health status, work disability, metropolitan area status, dwelling ownership status, and dependents. Includes control function residuals and age, year, and state fixed effects. Weighted using CPS-provided individual-level weights. Robust standard errors clustered at the state level in parentheses.
LY = Last Year
LW = Last Week
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Control Function Model Estimates for Income

	(1) Income	(2) Logged Income
Dependent Coverage	-9084.4**	-1.340***
	(3135.4)	(0.364)
Observations	160312	160312

Note: OLS regressions. Uses data from the March CPS 2006-2016 (reference years 2005-2015). Demographic controls include race, gender, education level, marital status, health status, work disability, metropolitan area status, dwelling ownership status, and dependents. Includes control function residuals and age, year, and state fixed effects. Weighted using CPS-provided individual-level weights. Robust standard errors clustered at the state level in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$