Contingency Plans:

The Effects of Dependent Coverage Mandate on the Contingent Workforce Decisions of Young Adults

A Thesis submitted by

Michael Thomas Gibson

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Advisor: Melissa McInerney

Abstract:

The majority of Americans obtain their health insurance through their job. However, there is a certain type of employment, contingent work, that does not offer health insurance. These jobs are characterized by a lack of relationship between the employer and worker. In 2010, the Dependent Coverage Mandate was passed, allowing young adults to remain on their parents' employer provided health insurance until age 26. This law broke the link between employment and health insurance for young adults and so in theory could have freed young adults to take up more contingent work. Using a difference in difference analysis, this paper will show that the DCM caused a 0.31 percentage point increase in the number of young men aged 23-25 who take up contingent work, an increase of roughly 38.7% of the mean percentage of 23-25-year-old men doing contingent work in the pre-treatment period.

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1. Introduction

The majority of Americans obtain their health insurance through their employers (Bailey & Chorniy, 2015), a trend which began after WWII, when the federal government passed wage controls as a reaction to a tight labor market. As such, companies could not compete for workers by offering higher wages but they were able to compete via the fringe benefits they provided. One of these benefits was the ability to purchase health insurance at a lower price than it was available privately. While this benefit did lead to an increase in the percentage of insured Americans, it also created a phenomenon known as job lock, where a worker felt compelled to remain at a job to maintain insurance coverage. Not every worker was entitled to these benefits, however. In order to qualify the worker had to be an *employee* of the firm; broadly speaking this means the worker had to work full time and have some kind of contracted relationship with the firm.

However, starting in the late 1990s, a growing number of individuals in the labor market did not qualify as employees because they lack the contracted relationship with their firm. The lack of this contracted relationship means that contingent workers do not qualify for benefits such as employer provided health insurance but often have more freedom to choose their hours, the location of their work, and the number of days they work in a week. As the cost of hiring contingent workers to the firm is less than the cost of hiring traditional employees, these jobs may be easier for workers to obtain, but contingent jobs do not offer the benefits and permanence that is traditionally associated with employment. Thus, contingent workers who want benefits such as health insurance must seek sources other than their job in order to obtain them.

For some workers, this relationship between employment and health insurance changed in September of 2010 when the Affordable Care Act was passed. This law included a provision known as the Dependent Coverage Mandate (hereafter referred to as the DCM), which allowed individuals to remain on their parent's employer provided health insurance until age 26. Previously this age cutoff was 18, unless the individual was a full-time student in which case the cutoff was 22. In either case, the DCM represented a sizeable extension of coverage. When the ACA was passed, young adults now had two possible sources of health insurance: the first from their own employer, and the second from their parent's employers. Since young adults no longer had to rely on their own employer for health insurance, it is possible that the DCM caused a change in their labor decisions. Specifically, the DCM may have encouraged young adults to take contingent jobs because they were now free to take jobs without benefits. There is some evidence that decreasing the cost of private health insurance caused an increase in self-employment (Bailey, 2014), so this could indicate that contingent employment may behave in a similar way. Antwi et al. (2013) find that the Dependent Coverage Mandate caused a reduction in full time employment and in hours worked; further evidence that the law may have freed young adults to pursue contingent work.

I will use the 2008 Survey of Program and Participation to conduct a difference in difference analysis to compare treatment and comparison individuals' contingent workforce decisions before and after the Dependent Coverage Mandate took effect. This analysis is known as an age-time difference in difference as I compare treated ages to comparison ages over pre-treatment and treatment time periods. Citing the gender discrepancies in the growth of the contingent workforce (Katz & Krueger, 2018) I will look at the impact of the law separately for men and for women. This paper will show a positive and large effect for men, while an insignificant result for women.

2. .Literature Review

2.1 Contingent Work

The definition of contingent work depends mainly on who is being asked to define it. There is a general consensus on who composes the core contingent workforce, namely temp agency workers, on-call workers, independent contractors, and day laborers (Abraham, Haltiwanger, Sandusky, & Speltzer, 2017). This definition is the most narrow by far; other economists include part-time workers and domestic contractors as members of the core contingent workforce as well (Bernhardt, 2014). The Government Accountability Office also recognizes these groups as the core contingent workforce, while broadening the definition by including self-employed individuals as well (Jeszeck, 2015). The share of the United States labor force that each segment of the contingent workforce represents depends on which social survey is being examined: In 2005 under the narrowest definition of contingent work, the government considered 2.5 to 5.7 million workers to be contingent, roughly 1.8 to 4.1% of the total employed labor force, while the broadest definition of contingent work that percentage increased to 30.6% of the total labor force (Jeszeck, 2015). The discrepancy in the size between social surveys is largely a result of the questions being asked within each social survey as well as the general lack of consensus on which types of employment relationships are contingent relationships (Bernhardt, Batt, Houseman, & Appelbaum, 2015). Furthermore, workers themselves are often unclear whether they are *employees* or *contingent workers*. Compounding this confusion is the fact that the primary government survey to quantify contingent work, the Current Population Survey's Contingent Worker Supplement, was not fielded between 2005 and 2017, though Katz and Krueger (2016) appended the RAND American Life Panel to include a similar supplement in 2015.

Furthermore, there is disagreement on which aspects of a job classify it is a contingent job as opposed to a traditional job. Spreitzer et al (2015) posit that contingent jobs offer flexibility in the length of the employment relationship, flexibility in work schedule, as well as flexibility in the place where the work is accomplished. From this definition, it is clear to see why the core contingent jobs as well as the broader definition of contingent jobs are considered contingent jobs—they all have these types of flexibility. However, other economists argue that it is not this flexibility that defines work as contingent, but is unpredictability (Abraham et al., 2017)—in the length of employment relationship, in the schedule, as well as in hours and earnings, that defines work as contingent It is clear that core contingent workers experience this unpredictability, which is largely determined by the firm who is demanding their work. However, for some members of the broader definitions of contingent work such as self-employed and part-time workers, this unpredictability is either not present or reflects personal choices made by the worker. A self-employed landscaper and a day laborer who looks for work at Home Depot both experience unpredictability in terms of hours worked per week, but the self-employed landscaper surely has more autonomy in determining their hours worked. In comparison, the day laborer's unpredictability in hours worked per week arises from the firm, household, or individual demanding his or her work. Workers who are employed part-time may also experience unpredictability, but as traditional employees they usually operate under some contract that establishes clear terms of employment which is not characteristic of contingent jobs. This paper will focus on the narrowest definition of contingent workers, with broader definitions included as a robustness check.

There is much more agreement on who is doing contingent work. On average, the contingent worker is more likely to be: female, younger, Hispanic, have no high school degree, and an immigrant (Jeszeck, 2015; Katz & Krueger, 2016; Liu & Kolenda, 2012). Katz and Krueger (2016) find that in the 1995 CPS the percentage of contingent workers who were men was 62.3%, decreasing to 61.4% in 2005 but then drastically dropping to 49.7% in 2015. This sudden rise in the share of women in the contingent workforce may be a reason to suspect that the DCM may have a different impact on men than it has on women. Specifically, one might expect the law to have a greater impact for women than for men.

Contingent work is more frequently done by highly educated individuals (Katz & Krueger, 2016)—in 2015 for example, 38.4% of contingent workers held a bachelors or higher while only 6% had less than a high school diploma. Katz &

Krueger (2016) point out that the growth in the contingent workforce over the last ten years has been concentrated in college graduates, and workers who hold multiple jobs. This paper will explore heterogeneous treatment effects among these groups.

2.2: Job Lock

This paper heavily depends on young adults being affected by job lock, a phenomenon well known in labor economic literature. To briefly explain this phenomenon, job lock arises when a worker does not take a “better” job—such as a job that would increase their productivity—because doing so would result in some decrease in the quality of their health insurance, whether that came in the form of higher premiums, reduced coverage, or some other decrease in quality of coverage. There is a variety of evidence supporting the existence of job lock, including both theoretical support of job lock, (Dey & Flinn, 2005), as well as empirical support of job lock (Madrian, 1993) Most relevant to this paper is Bailey (2014) which found that as tax write offs for privately purchasing health insurance rose more people chose to become self-employed. This evidence is important because it shows that for some workers, the benefits offered in traditional employment were incentive enough to remain in traditional employment instead of becoming self-employed. As privately purchase health insurance became cheaper, workers were no longer tied to traditional employment in order to obtain the benefit of health insurance, and so they left traditional employment. The hypothesis in this paper is that the Affordable Care Act’s DCM functions as this tax write off did but for young adults interested in contingent work as opposed to individuals interested in self-employment. As the DCM broke the link between traditional employment and health insurance it should in theory make contingent work more desirable.

2.3: The Dependent Coverage Mandate as a Natural Experiment

Using the Affordable Care Act as a natural experiment is a methodology that many authors have utilized. Much of the literature on the Affordable Care Act has focused on two main outcomes: outcomes related to the labor market, and outcomes related to health insurance. In labor market outcomes, it has been shown that the Affordable Care Act caused young adults to reduce both their hours worked per week by 2.51% (Depew, 2015), as well as decreasing the probability of being full-time employed by 0.81 percentage points (Depew 2015). Both of these effects indicate that young adults have more free time, which may allow them to participate in the contingent economy at a higher rate. While the effect was insignificant, Heim, Lurie, and Simon (2015) showed that young adults were more likely to be a full-time student after the ACA was passed—an interesting result given that many universities offer health insurance to their students. Furthermore, Slusky (2012) also found that the DCM did have a statistically significant effect on the probability of being a full-time student. This increase in school attendance lends credibility to the hypothesis that the ACA caused an increase in young adults taking contingent jobs, as the flexibility inherent in contingent work makes it easier to do while also being a student in comparison to full-time work or traditional part-time work. These results are corroborated by Antwi et. al. (2013) who find that post DCM, young adults were 2.21 percentage points less likely to be full-time employed, reduced hours worked per week by 4.7%, and were 1.2 percentage points more likely to report experiencing variability in their hours. This last effect is key, as variability in hours is a central aspect of contingent work as mentioned in Section 2.1 of this paper.

In terms of health insurance outcomes, across the board it has been shown that young adults did take up their parents' employer provided health insurance (Antwi, Moriya, & Simon, 2013; Bailey & Chorniy, 2015; Slusky, 2017). Slusky (2017) finds that the net insurance rate of young adults increased by 2.6 percentage points, with a 6.9 percentage point increase in young adult take up of parental coverage. Antwi et. al (2013) report that the net gain in young adult insurance rates was 3.1 percentage points, while the percentage of young adults covered by their parents' health insurance increased by 7.0 percentage points. Importantly, in both papers authors find that the decrease in own-name employer provided health insurance is offset by a larger increase in parents' employer provided health insurance; thus, the net effect of the DCM on the insured rate of young adults is positive. These effects are largest for 23-25-year-olds. The effects are also different by gender—women were only 1.9 percentage points more likely to be insured following the DCM while men were 4.2 percentage points more insured, though the two genders took up parental EPHI at the same rate. This indicates that for women the DCM caused a switch in the source of health insurance while for men it caused an increase in the insurance rates. This difference is interesting—especially because this paper will show that the treatment effect explored in this paper is largest for men aged 23-25. This implies that young men and young women value health insurance differently, or at least, that they respond to incentives to obtain health insurance differently. Women were more likely to be insured before the DCM; perhaps this is why there is no change in contingent work among women.

This paper contributes to the literature by looking at a different set of outcomes than the ones that have been previously explored. While Antwi et. al (2013), Slusky (2017), Depew (2015), and Heim et. al. (2015) examined traditional employment and insurance outcomes from various social surveys, contingent work was not a focus of their papers. Their papers all offer evidence that there could be an effect of the DCM on contingent work—broadly speaking, their papers show that young adults were less likely to be employed, worked fewer hours, and were more likely to have insurance. However, prior to this paper there has been no investigation of the impact of the DCM on contingent work, though generally Katz and Krueger (2016) show the size of the contingent workforce has grown between 2005 and 2015. This paper will show that the DCM caused an increase of contingent work in young adults, with the effect concentrated in men.

3. Data

The data used in this paper comes from the 2008 Survey of Income and Program Participation run by the Census Bureau. The SIPP follows households over a 60-month span beginning in September of 2008 and ending in November of 2013, with interviews occurring once every 4^{th} month during which the respondent is asked about the current and 3 preceding months. The sample is composed of 50,000 households and is constructed to be nationally representative. A major advantage of the 2008 SIPP panel is that it began surveying households in August of 2008 and concluded in November of 2013, and it offers monthly observations that begin over two years prior to the passage of the DCM and ends over two years after the DCM is fully implemented. This time range offers the ability to examine a long period of time as well as a high frequency of data.

One issue with the sample was attrition—roughly only 75% of individuals who were surveyed in wave one remained in the sample in wave 16. Furthermore, when an individual moved to a new house, everyone inside that house was interviewed as well, causing some individuals to enter the panel in the middle. This results in the composition of the panel changing from month to month, and as such many researchers who use the SIPP data treat it as a repeated cross section instead of panel data; this paper will follow suit in treating it as a repeated cross section.

4. Methodology

The treatment in this paper is the Dependent Coverage Mandate which is part of the Affordable Care Act. Passed in part as a response to the high rate of uninsured young adults, the mandate extended the age range that an individual could remain on their parents' employer provided health insurance to age 26. Furthermore, there were few eligibility restrictions: individuals could go back on their parents' plans even if they were married, no longer lived at home, or had full-time jobs that offered health insurance plans. The DCM took effect in September of 2010, and required insurance plans to offer extended coverage to young adults under the age of 26 the next time the policy is issued. However, as the majority of insurance plans in the United States renew in either January or June, it is unlikely that the full effects of the law would be seen until June of 2011.

Since the Dependent Coverage Mandate takes effect in the middle of the 2008 SIPP, this survey naturally lends itself to doing a difference-in difference-approach, comparing the difference between treated and comparison group individuals after treatment occurs to the difference between treated and comparison group individuals before the treatment occurs. The first step in identifying the impact of the Dependent Coverage Mandate is to identify the treatment and comparison groups. Since the Dependent Coverage Mandate extends the age an individual can stay on their parents' employer-provided health insurance to age 26, it would be natural compare 19-26 year olds to some age group that is not affected by the program, say 27-32 year olds as they are the nearest unaffected age group. However, issue with the policy that arises is that it is unclear how 26-year olds interact with the policy. Under the policy, 26 year olds should be eligible for coverage. Since health insurance plans usually renew in January or June it is unclear if an individual who turned 27 after their parents' renewal date would still be covered, or for how long they would be covered. Thus, 26 year olds were not included as either treatment or comparison because it is unclear which of group they belong to. This exclusion is done in much of the DCM literature, including Antwi et. al (2013).

Furthermore, a key assumption of the difference in difference approach is that the treatment and comparison groups need to have parallel trends in the outcome variable in the pre-treatment period. Theoretically, it is unlikely that 19-22 year olds are making labor market decisions that are similar to the decisions being made by 27-32 year olds, so I restrict the treatment group to only include 23-25 year olds. In fact, Slusky (2017) shows that a narrow age range comparison is necessary to satisfy the parallel trends assumption when looking at labor market outcomes; I will show later that 23-25 year olds did not have statistically significantly different parallel trends compared to 27-29 year olds.

The resulting baseline sample includes individuals aged 23-25 or 27-29 during each month of the SIPP, resulting in a sample of 5,901 individuals between the ages of 23-25 and 5,415 individuals in between the ages of 27-29. In the pre-treatment period, the two groups are statistically similar on a wide array of characteristics, which can be seen in Table 1. Treated individuals were no more likely to be women nor belong to any particular racial group than comparison individuals, as can be seen in the 4th column of the table. The two groups did not statistically differ in terms of household income, in number of children, or in probability of being employed in the contingent work force. The state-wide unemployment rate was the same across groups as well.

Where there are differences between the two groups are in areas that are expected: As can be seen in Table 1, by construction, the treatment group is 3.99 years younger than the comparison group. Correspondingly, the treatment group is 22 percentage points more likely to live at home, 3 percentage points less likely to be in the labor force, 20 percentage points less likely to be married, 11 percentage points more likely to be enrolled in school, and 4 percentage points less likely to be insured; these differences largely reflect the fact that the treatment group is younger and as such has had less time to develop in their career and lives. However, there are some differences that are concerning, as they may bias the results one way or another. For example, the treatment group is less likely to live in a metropolitan area, while the majority of contingent work occurs in metro areas. This could bias the treatment effect downwards. Furthermore, the age groups in the treatment group face higher unemployment rates than the age groups in the comparison group. This measure was conducted by taking the number unemployed individuals in the SIPP of age *g* and dividing it by the number of individuals aged *g* in the labor force, thus it is the SIPP-national unemployment rate for that age group *g*. This could bias the treatment effect upwards if unemployed individuals were eager to take contingent work while looking for traditional unemployment.

For demographic characteristics, the treatment group was 1 percentage point less likely to be an immigrant than the comparison group. This difference is also concerning because as discussed in Katz &Krueger (2016), much of the growth in the contingent economy in the last ten years is concentrated among immigrants. Since there are fewer immigrants in the treatment group than the comparison group, the treatment effect may be biased towards zero if the immigrants in the comparison group were increasingly taking contingent jobs.

Due to these concerns about the differences between the two groups, demographic characteristics such as race, gender, ethnicity, marital status, as well as economic characteristics such as age-level and state-level unemployment, household income, metro residential status, and immigrant status were included as controls. Student status was not included as a control as Slusky (2017) found the DCM increased the probability of a young adult being an enrolled student; as such it is correlated with the treatment variable and to include it would bias the coefficients.

The time frame of the SIPP can be broken into two periods, the pre-treatment and treatment periods. The pre-treatment period in this paper begins when the SIPP starts data collection, May of 2008 and ends in September of 2010, when the DCM is passed. The treatment time period starts in the following month, October of 2010 and ends when the SIPP ends or November of 2013.

The final step in estimation of the impact of the DCM on contingent work is to identify contingent workers within the SIPP. Contingent workers were identified by looking at respondents' answers to the question "which of the following best describes your employment relationship?", where the possible answers were "work for employer", "self-employed", "alternative work arrangement" or "other". Individuals who responded "alternative work arrangement" in a given month are considered contingent workers in that month. It is important to note that the workers identified in this way should only be core contingent workers. Self-employed individuals should have selected the self-employed option, and part-time employees should have indicated they worked for an employer. As a robustness check, this paper will broaden the definition to include these other types of individuals, but the main analysis will report the results for this narrower definition.

The term difference in difference comes from the fact that this methodology compares two differences. The first difference is the difference between the treatment and comparison groups in the pre-treatment period. The second difference is the difference between the treatment and comparison groups in the treatment period. If the DCM actually incentivized individuals to take up contingent work, then the second difference should be larger than the first.

5. Model:

An underlying assumption required for valid estimation of the treatment effect in a difference in difference assumption is that the treatment and comparison groups were trending the same way in the outcome variable prior to treatment. If the two groups have differing trends before the treatment, then the difference in trends after the treatment could reflect pre-treatment differences instead of the effect of treatment. Following the methodology employed by numerous other authors, I use an event study model to test parallel trends. A formal analysis of parallel trends will be discussed later using an event study, however, the similarity in pre-treatment trends can be seen in Figures 1a, 1b, and 1c in the section of the figure to the left of the dashed red line. In these figures, it appears that for the full sample as well as for women and men that on average there is no difference between the treatment and comparison groups in the pre-treatment period. As the parallel trends assumption holds, I can be confident that the treatment effect is not a result of differing pre-treatment trends between the treatment and comparison groups.

The main regression equation is as follows:

(1) $contingent_{igst} = \beta_0 + \beta_1 Z_g + \beta_2 Post_t + \delta_1(Z_g * Post_t) + \boldsymbol{\phi X_{igst}} + \gamma\ age\ unemp_{gt} + \lambda\ state\ unemp_{st} + \boldsymbol{\tau_t} + \psi_s + \alpha_g + \epsilon_{igst}$

The outcome variable is a dummy variable equal to one if individual *i* living in state *s*, of age *g*, in month *t*, is a member of the contingent workforce and zero otherwise. Z_g is the treatment indicator; and is equal to 1 if the individual is in a treated age group or is between 23 and 25 and zero if the individual is in an untreated age group, or 27-29. $\boldsymbol{\tau_t}$ is a series of dummies equal where the first is equal to one in May of 2008 and zero otherwise, the second is equal to one in June of 2008, zero otherwise, and finally the 66th time dummy is equal to one in November of 2013, when the SIPP ends, and zero otherwise. X_{igst} is a vector of controls including race and ethnicity, gender, marital status, household income divided by the federally determined poverty line for that sized household, and the squared value of that term. There are state-specific linear trends as well a dummy for each calendar month and year. Two other controls are *Age unemp*$_{gt,}$, constructed within each month by taking the sum of individuals aged 23-25 or 27-29 in month *t* who are unemployed divided by the number of individuals aged of those age ranges in month *t* who are in the labor force. Finally, *State_unemp*$_{st}$ is the Bureau of Labor Statistics reported unemployment rate of state *s* in month *t*. τ_t, ψ_s, α_g are calendar monthly and yearly dummies, state fixed effects, and age-group fixed effects respectively, and ε_{igst} is the error term. In the model, β_0.is essentially the average percentage of the comparison group doing contingent work over the months in the pre-treatment period, conditional on the controls and fixed effects. $\beta_{1.}$is the difference between the treatment and comparison groups in the pre-treatment period conditional on controls and fixed effects. $\beta_{2.}$ Is the difference within the comparison group between the treatment period and pre-treatment period. Finally, δ_1 is the main variable of interest, as the interaction between the post variable and the treatment indicator shows the treatment effect. δ_1 represents the difference between the treatment and comparison groups in the treatment period; or, the difference in the average percent of each group doing contingent work each month across the whole treatment period. Theory would indicate that δ_1 should be positive, indicating that the dependent coverage mandate caused 23-25-year-olds to increase their contingent workforce participation. The results, found in Table 2, confirm this hypothesis, at least for men.

6. Results

6.1 Contingent Work

As shown in table 2, the unconditional treatment effect is not statistically significantly different from zero. Unconditionally, the treatment group was 0.17 percentage points more likely to take up contingent work in the treatment period than the comparison group. While this effect is positive it is not statistically significant. Neither is the conditional model when using the full sample; however, the coefficient on $female_i$ was significant which lead me to think there could be differential effects for men compared to women. Splitting the sample into men and women shows an area of sharp heterogeneity: for men, the treatment effect is

0.0031 p.p., significant at a 10% level, while for women the treatment effect is 0.0003 and is insignificant. The effect for men is quite large—0.8% of treated men did contingent work in the pre-treatment period, so a treatment effect of 0.31 percentage points constitutes an increase in contingent work of 38.75%. As a robustness check I explored how senstitive the treatment effect was to different model specifications.

Table 3a shows that this insignificance of the treatment effect for the full sample is robust to a wide range of specifications. No matter what specification is applied to the baseline model, the estimated treatment effect for the full sample is statistically insignificant when the SIPP is treated as a pooled cross section. In column (8), when the SIPP is treated as a panel dataset, there is a 0.4 percentage point increase in the likelihood of contingent work.

Turning to Table 3b, the baseline results reproduced in column (1) show, on average, treated men were 0.31 percentage points more likely than the comparison group to take up contingent work in the post-treatment period. Again, treatment effect is relatively large, as it represents an increase of roughly 38.75% from the mean of the treatment group in the baseline period, or 0.80%. The remaining columns of Table 3b shows that the magnitude of this effect is relatively robust, though the estimates are noisier and, in many cases, fail to achieve statistical significance. While this treatment effect is not robust to weighting, clustering at the state level, using alternative unemployment methods or including an individual fixed effect, the magnitude and sign of the treatment is fairly consistent across models.

In column 2 of Table 3b, student status is included as a control. This increases the coefficient on the treatment effect from 0.0031 to 0.0032, while leaving the standard error unchanged, resulting in a significant treatment effect. As the coefficient on the treatment indicator represents the covariance of the outcome variable divided by the variance in the treatment indicator and this coefficient is changed by including a control for student, this suggests that this control is correlated with the treatment indicator or that it explains the outcome variable. Slusky (2017) shows that the DCM caused an increase in the probability of a 23-25 year old being a full time student, suggesting that student status is correlated with the treatment indicator. There is reason to believe student status can explain an individual's contingent workforce decision; students may have the flexibility and free time to pursue contingent work at a higher rate than non-students. This suggests that some portion the treatment effect is working through an individual's decision to be a student.

Column 3 uses an alternative mode of controlling for unemployment. The baseline model includes a constructed age-group measure of unemployment, or age-specific-unemployment, found by taking the number of unemployed individuals of age g in the SIPP and dividing it by the number of SIPP respondents of that age who are in the labor force, as well as the BLS State-specific unemployment rate. Column 3 uses just the BLS state unemployment rate and includes an interaction term between the treatment indicator and the BLS monthly state-specific unemployment rates; this methodology was employed by both Antwi et al. (2013) as well as Slusky (2017). Like in column 2, where student status was controlled for, this suggests these unemployment rates are capturing some of the covariance between the outcome variable and the treatment indicator. Ultimately, it shows my results are sensitive to the method for controlling for unemployment. In column 3 the BLS unemployment rate is allowed to have a different effect for the comparison and treatment group, and in doing so it pulls away some of the variance in contingent work decisions explained by the treatment indicator. This would suggest that the unemployment rates of other age groups has an impact on the workforce decisions of 23-25 year olds. It may be the case that as older age groups face higher unemployment rates, they crowd younger age groups out of traditional work and into contingent work; thus not controlling for this relationship in column 1 caused an upward bias of the treatment effect.

While weighting in column 4 of table 3b, the treatment effect is insignificant, though the magnitude is almost identical to the baseline model while the standard error has increased, losing the significance. Porter (1973) states that weighting should not be used when determining causal estimates and the magnitude of the treatment effect is similar to the baseline model. In column 5, only the fourth reference month is used—or the month in which the respondent is interviewed and discusses that month and the previous three months. This specification is used to check for seam bias or the tendency to forget as time goes on. However, again the size of the treatment effect is similar to the baseline model while the standard error has grown, resulting in an insignificant effect. Furthermore, one might expect more seam bias with measures with more variability such as hours worked per week. It may be difficult to remember exactly how many hours worked per week when discussing a week that took place four months earlier; but it could be easier to remember if one had done contingent work in a month that was four months ago. Furthermore, this reduces the sample size from 182,214 individual months to 45,438 individual months- thus leading to larger standard errors.

In column 6, the standard errors are clustered at the state level as opposed to the age-group level. Again, while the magnitude of the treatment effect is the same as the baseline model, the standard error is a little bit larger, causing insignificance. However, the p-value of 0.123 shows that this specification is only marginally insignificant. In the baseline, the results are clustered at the year of age level because the treatment was delivered to certain ages as a whole, not certain states. As this shock was delivered to all 23-25 year olds regardless of state, I clustered at the age-group level. In column 7 I expanded the age range of the treatment group to include the younger group affected by the law, or 19-22 year olds. This increased the magnitude of the treatment effect to 0.0033 and increased the significance to a 5% level. This is a positive sign, as my hypothesis would indicate a positive effect for this group as well. However, I do not include 19-22 year olds in my baseline sample as they are in a different labor force stage of their lives compared to 23-25 year olds and 27-29 year olds. Furthermore, Slusky (2017) advocates against including this age range to ensure parallel trends holds.

Finally, in column 8, when including an individual fixed effect, the magnitude and standard error of the treatment effect both grew, resulting in an insignificant treatment effect. In this model, the standard errors are clustered at the individual level. However, as this is not alarming as the majority of authors in this literature do not include an individual fixed effect, and the issues of respondents leaving and entering the SIPP could cause this insignificance. This is why the baseline model treats the sample as a repeated cross section. Furthermore, the identifying variation in Column 9 is the men who age out of the treatment group and into the control group over the course of the five years in the SIPP. There are likely few men who actually do this, so it is not surprising that the treatment effect is insignificant when including an individual fixed effect, though the magnitude is similar to the baseline model.

Table 3c shows that the treatment effect for women is robustly insignificant across all specifications and is quite small. In all but column 5, the point estimate is positive, though the standard errors are quite large relative to the size of the coefficient so the negative point estimate in column 5 should not be seen as different to the positive point estimates in columns 1 through 4 and 6 through 9.

Decisions regarding work are not decisions individuals make lightly, thus there is reason to believe there would be a delay after the DCM took effect before individuals took up contingent work. To investigate this, I decomposed the treatment effect into each month of the SIPP by modifying equation 1 into:

$$(2) \quad contingent_{igst} = \beta_0 + \beta_1 Z_g + \sum_{m=0}^{m=66} \tau_m + \sum_{m=0}^{m=66} \delta_t (Z_g * \tau_m) + \boldsymbol{\phi X_{igst}} + \gamma \; age \; unemp_{gt} + \lambda \; state \; unemp_{st} + \psi_s + \alpha_g + \epsilon_{igst}$$

Instead of only looking at the pre-treatment data, I used all 66 months of the 2008 SIPP, and looked at each δ_t, which is the monthly difference between the treatment and comparison group. I then graphed each δ_t or the monthly differences between the treatment and comparison using first the full sample, then men, and then women in figures 2a, 2b, and 2c respectively. The plotted points are the estimated conditional difference in the percentage of the treatment group and comparison group employed in the contingent workforce in each month, while the grey dashed lines show the 90% confidence interval for that point estimate.

Figures 2a-c also offer evidence for the parallel trends assumption. The pre-treatment time is the area to the left of the blue dashed line. The point estimate is the difference between the treatment and comparison group in that month. For the parallel trends assumption to hold, the treatment and comparison groups should not be trending differently in the pre-treatment time. In each of these figures, the difference between the groups pre-treatment months are not statistically different from zero at a 10% level; visually this implies the parallel trends assumption holds. A formal test was conducted by taking equation (2) and restricting the sample to the pre-treatment time. I then did a joint F-test that jointly, all of the $\delta_t$in the pretreatment time were equal to zero. The results of these F-tests are 0.80 for the full sample, 0.11 for men, and 0.75 for women, indicating that at a 10% level, each group passed the parallel trends assumption.

Figure 2a, illustrates why the treatment effect is on average for the full sample the treatment effect is insignificant. For the first year and a half after the treatment is passed, the estimated difference between the treatment and comparison group is negative and statistically insignificantly different from zero. To the right of the blue dashed line, the point estimate changes from negative to positive around September of 2011, but it is still insignificant at a 10% level. While the treatment effect grows in magnitude, it never achieves a 10% significance level. Thus, on average the treatment effect is insignificantly different from zero.

Figure 2b shows the monthly decomposition of the average treatment effect reported in column 1 of Table 3b. It takes the treatment effect roughly a year before it becomes significantly different for zero for a month. Starting in February of 2012, the treatment effect becomes significant at a 10% level and for the most part, maintains that level of significance for the remainder of the SIPP. The final few months of the SIPP are when portions of the sample are finishing their interviews; the noisiness of the standard errors in this area is likely caused by that. Interestingly, the magnitude of the treatment effect grows over time as well; in the first year after the passage of the DCM, it is statistically insignificantly different from zero. Around February of 2012, it jumps up to 0.15 percentage points, from which it steadily increases to a maximum of 0.6 percentage points in November of 2013.

The delay of the significance of the treatment effect is likely a cause of two phenomenon: firstly, renewal for insurance plans happens in January and June; this means that the full sample was not offered the treatment until the closest June to September of 2010, or June 2011. Secondly, labor market decisions are usually major decisions and as such take time, especially if an individual was moving from fulltime employment to contingent work. However, once the treatment effect becomes significant, is consistently significant and grows in magnitude. Figure 2c offers evidence for why the treatment effect is on average insignificant—the monthly treatment effect is only significantly different from zero for three months during the treated time and is estimated to be negative.

As a robustness check I did placebo analysis on the pre-treatment time for men to confirm the treatment effect was valid. This period was split into control time and placebo treatment time, starting with the fifth month and increasing the control time by five months. Since the treatment effect was only significant for men, this placebo test was only run on the sample of men. In all but one of these placebo tests, the treatment effect was statistically insignificant which can be seen in Table 4. In column 2 of table 4, the treatment effect is significant, though the sign is the opposite of the treatment effect. This suggests the treatment effect reported in the baseline model is not driven by placebo effects. As the model passed the majority of the placebo time tests, this gives confidence that the identified treatment effect is an actual treatment affect as opposed to a statistical anomaly.

This sharp heterogeneity of the treatment effect poses some interesting questions. Several authors including Katz and Krueger (2016) have pointed out that the growth in the contingent workforce has been concentrated in women—so it is the surprising that for women there was no effect. Table 5 offers some insight. Table 5 shows the difference in means for treated men and women for the same demographic characteristics explored in Table 1. The results of this investigation show that men aged 23-25 are at vastly different stages of their lives than women aged 23-25.

For example, treated women were 6 p.p. more likely to have children, 8 p.p. less likely to live with their parents, 10 p.p. more likely to be married, 5 p.p. more likely to be enrolled in school, 9 p.p. less likely to be in the labor force and finally 3 p.p. more likely to be insured by any source compared to treated men. These differences indicate that women aged 23-25 are participating in the labor force in a different way than men aged 23-25. More treated women are parents or spouses—which could limit the choices they make in terms of labor. Furthermore, treated women are more likely to be enrolled in school, which means they may have had an additional source of insurance and thus were less likely to take up their parents' employer provided health insurance. Most importantly, treated women were more likely to be insured than treated men, suggesting again that a law designed to increase insurance among these age groups would have had a lesser effect for women.

Indeed, the law did have a different effect in terms of take up of parental health insurance by gender. Figure 3 shows the percentage of treated men and women in each quarter who are covered by their parents EPHI—in each quarter between 4 and 10 percentage points more men are under their parents plan than women. As pointed out by Antwi et al. (2013), the net increase in insurance rates was higher for men than it was for women, indicating that while men were gaining insurance from the DCM, women were switching from their own EPHI to their parents EPHI. Thus, it is reasonable that as the DCM caused differing patterns in take up of EPHI by gender, any secondary effects of that take up would differ by gender as well.

6.2: Heterogeneity of the Treatment Effect

This paper will also examine heterogeneous treatment effects by additional demographic characteristics. This requires the usage of a triple difference in difference model which, using Hispanic individuals for an example, can be written as:

$$(3)\ Contingent_{igst} = \beta_0 + \beta_1 Z_g + \eta_1 hispanic_i + \beta_2 Post_t + \beta_3 (Z_g * Post_t) + \beta_4 (hispanic_i * Post_t) + \beta_5 \left(Z_g * hispanic_i\right) + \delta_1 \left(Z_g * Post_t * hispanic_i\right) +_{\phi} \boldsymbol{X_{igst}} + \gamma\ age\ unemp_{gt} + \lambda\ state\ unemp_{st} + \boldsymbol{\tau}_t + \psi_s + \alpha_g + \epsilon_{igst}$$

In the triple difference model, the variable of interest is the interaction between *Z, post,* and *Hispanic.* The coefficient on this interaction term is the difference in the effect of the dependent coverage mandate between Hispanic treated individuals and non-Hispanic treated individuals. If the policy affected Hispanic individuals more than non-Hispanic individuals, one should expect δ_1 to be positive. This paper will investigate the heterogeneity of treatment effect for men on gender, student status, race, and ethnicity, immigrant status, education level, marital status and location of residence. A summary of results on the heterogeneity of treatment effects can be found in Table 6. When compared to the full sample of men, some groups of men experienced larger increases in the likelihood of contingent work. Specifically, Asian men, and men who lived at home had larger statistically significant increases in contingent work than the full sample of men. For white men, the effect was rather similar in size though it was statistically different from the treatment effect for the full sample. The treatment effect was insignificant for male students and male immigrants, as well as

Hispanic men. While the treatment effect was significant for men with at least some college education, it was not statistically different from the treatment effect for the full sample, and it was similar in magnitude to the full sample.

Heterogeneity is not seen in areas that might be expected: for instance, neither Hispanic men nor male immigrants had a larger treatment effect despite being groups where there was much growth in the contingent workforce (Katz & Kruger 2016). It makes sense that men who live at home would have a larger treatment effect; Perhaps if they are not paying for their housing they are more willing to take jobs with unpredictable earnings.

6.2 Robustness Checks

As an additional robustness check, the definition of contingent worker was appended to include both self-employed and part time individuals. The results can be found in Table 7. The column 1 of table 7 is the baseline specification found in Table 2 where only core contingent workers are included as contingent workers. In column 2, that definition is expanded to first include those core contingent workers and "moonlighters". The SIPP defines moonlighters as those who report under the table work at another job. Next, Column 3 expands the definition to include core contingent workers and workers who report variable hours. Column 4 includes core contingent workers as well as part time workers, while Column 5 includes core contingent workers and all three previously mentioned types of workers.

When the definition of contingent work is expanded to include moonlighters or workers with variable hours, the coefficient of the treatment effect decreases resulting in an insignificant treatment effect. Furthermore, much more of the sample of men are moonlighting or experiencing variable hours, as evident in the mean dependent variable row. In columns 4 and 5, when including part time workers as contingent work and then including all three new definitions, the treatment effect regains significance, and the magnitudes are much larger. The Dependent Coverage Mandate has been shown to increase the probability that an individual will do part time work (Antwi et al., 2013; Slusky, 2017), so this is unsurprising.

Table 7 shows that the bulk of the treatment effect is concentrated in the core contingent workers. While the magnitude of the treatment effect was largest when looking at part time workers and core contingent workers, the relative size of the treatment effect to the mean of the dependent variable for the treatment group in the pre-treatment period was much larger for the core contingent workers than for core contingent workers and part time workers: 38.7% for core contingent workers and 13.2% for core contingent workers and part-time workers. This suggests that the law motivated core contingent workers more than broader types of contingent worker.

A final robustness check was done by splitting the sample into two subsamples: the first subsample contained individuals who had done no contingent work in the pre-treatment period, and the second subsample contained those who had done at least one month of contingent work in the pre-treatment period. Then a triple differences approach was conducted by using equation (3), where Hispanic was replaced by this indicator for doing any contingent work in the pre-period. Table 8 illustrates the results of this exercise.

For the full sample, the treatment effect for those who did no contingent work in the pre-period is 0.11 percentage points and is insignificant. The triple interaction term is much larger 3.6 percentage points but is also insignificant. This indicates that when looking at the full sample, those who did no contingent work in the pre-period were no more or less motivated by the law in the post period. For women, both treatment indicators are insignificant as well.

For men, the treatment effect for those who did no contingent work in the pre-period is significant at a 10% level and is 0.17 percentage points, while the treatment effect for those who did some contingent work in the pre-period is insignificant. This suggests that it was men who did no contingent work in the pre-period who were the most incentivized to take up contingent work.

7. Conclusion

7.1 Review of Results

This paper showed evidence that by breaking the link between full time employment and health insurance, the DCM freed young men to take contingent jobs. This effect is large, representing a 38.7% increase from the mean of the treatment group in the baseline, and while it is delayed for a year after the treatment takes effect, once it becomes significant it is persistent. Since the parallel trends assumption holds and the placebo time analysis showed no placebo effects, the model meets the assumptions necessary for a valid estimation. There was some heterogeneity of the treatment effect within sub-groups of men, though not in the areas identified by Katz and Krueger (2016) as experiencing rapid growth in their share of the contingent workforce. The treatment effect was not significant for women nor for the full sample.

7.2 Contribution to the Literature

This paper has contributed to the Dependent Coverage Mandate literature because it examined an outcome not previously explored by the literature. Additionally, it showed how men and women react differently to incentives to obtain health insurance and how those differences then have secondary effects on their labor market outcomes. Lastly, it showed that men and women aged 23-25 in the 2008 SIPP are significantly different from each other. These three pieces of information can be used to shape policy affecting young adults in this age group, or help other authors shape their research into the effects of the Dependent Coverage Mandate.

7.3 Limitations to the Data

There were considerable limitations to the data. It would have been interesting to explore all possible definitions of contingent work; however, I was constrained by the data. In my extract of the SIPP there was no variable identifying self-employed individuals, who are included in the broadest definitions of contingent work. Since I could not identify these workers, I could not compare the results of the law on core contingent work to every possible definition of contingent work. Furthermore, it was not possible to tell if the variables on hours worked per week and wages were referring to a respondent's contingent job or a traditional job. Thus it was impossible to study the effects on the law on the income earned and hours worked in contingent work increased from the law. While I could examine the extensive margin—i.e. if the respondent did any contingent work in a month—it would be interesting to examine the effects of the law on the intensive margin.

7.4 Policy Implications

It is not exactly clear what the impact of more young adults doing contingent work would be. Many authors have shown that contingent workers earn less per hour than their traditional counter parts and have fewer benefits (Katz & Krueger, 2016). This effect was particularly strong for low skilled jobs (Arindrajit & Kaplan, 2010). However, other authors have shown that contingent workers do not use contingent work as their main source of income and do not treat contingent work as a full time job, ("Paychecks, Paydays, and the Online Platform Economy: Big Data on Income Volatility," n.d.), so, the net impact of a young adult increasing their contingent employment is not exactly clear.

Another paper broadly claims that contingent work can be an important driving force in the economy as the technology that allows for contingent work can make transactions faster, easier and more efficient (Harris & Krueger, 2015). However, Harris and Krueger make the claim that unless the government is proactive in creating a new legal class of worker, there will be costly legal battles in the world of contingent work. Thus, the impact of an increase in contingent work would largely depend on the response of the government.

Other authors feel the impacts of contingent work should be measured differently. Bernhardt (2014) discussed the previous literature on contingent work at length and found that many papers lacked information on the following: changes in the prevalence of contingent work, changes in the distribution of contingent work, changes in the impact of contingent work, and the presence of threat effects. By this, the author is stating that simply saying dependent coverage mandate has increased the contingent workforce would not tell the full story, which would include measures of how the distribution of who is a contingent worker has changed and if the consequences of being a contingent worker has changed (e.g., are contingent workers earning less after the mandate than before). The threat effect is the extent to which an employer can threaten a traditional worker to replace their job with contingent work. Unfortunately, these effects cannot be measured given the data source, but are areas of study that should be explored in the future.

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Appendix A: Tables and Figures:

Table 1: Balance of Covariates Pre-Treatment

Characteristic	Column 1: Mean of Control	Column 2: Diff (Treat-Control)	Column 3: p-value (Diff=0)
Age	28.01	-4.00	0.002***
Women	0.50	0.00	0.66
White	0.78	0.01	0.14
Black	0.14	0.00	0.657
Asian	0.05	-0.01	0.185
Another Race	0.04	0.00	0.313
Hispanic	0.20	-0.01	0.131
Immigrant	0.13	-0.02	0.0444**
Household Income	5320.61	149.75	0.134
Lives with Family	0.45	0.21	0.003***
Has Children	0.55	0.01	0.807
Married	0.45	-0.19	0.0027***
Lives in Metro Area	0.82	-0.01	0.0954*
Enrolled in School	0.12	0.11	0.003***
In Labor Force	0.83	-0.03	0.0802*
In Contingent Workforce	0.01	0.00	0.762
Insured by Any Source	0.59	-0.04	0.0102**
Age-Group Unemployment	0.30	0.06	0.000***
State Unemployment	0.09	0.00	0.426

Number of individual- months for all covariates is 187,040. Women, White, Black, Asian, Other Races, Hispanic, and Immigrant, are binary variables equal to one if the respondent identifies as that demographic. HH income is measured in amount actually received during the month for all members of family above 15, before deductions for income and payroll taxes. Lives with family, Lives in Metro Area, Enrolled in School, In LF, in Contingent Workforce, Married, and Insured by Any source are binary variables equal to one if the individual meets that condition in a given month. Number of children is the count of respondent's children younger than 18 living with them. Age-Group unemployment is the number of respondents in an age group who are unemployed divided by the number of respondents in that age group who are in the labor force for each month. State Unemployment is the BLS statewide unemployment rate for each month.

Table 2: Main Regression Results

	Column 1	Column 2	Column 3	Column 4
Model:	Uncond. Model	Baseline Model		
	Full Sample	Full Sample	Men	Women
$Treatment_g$	0.0025	0.0023	0.0022	0.0024
	(0.0015)	(0.0014)	(0.0015)	(0.0014)
$Post_t$	-0.0007	0.0078	0.0150	-0.0019
	(0.0025)	(0.0128)	(0.0193)	(0.0026)
($Treatment_g$ * $Post_t$)	0.0017	0.0018	0.0031*	0.0003
	(0.0011)	(0.0011)	(0.0015)	(0.0016)
$Female_i$		-0.0015*		
		(0.0006)		
$Married_{it}$		-0.0004	0.0005	-0.0013
		(0.0006)	(0.0007)	(0.0008)
$Hispanic_i$		-0.0010	-0.0014	-0.0007
		(0.0015)	(0.0018)	(0.0016)
$Kids_{it}$		-0.0009*	-0.0003	-0.0015
		(0.0004)	(0.0006)	(0.0008)
$Immigrant_i$		0.0034*	0.0055**	0.0011
		(0.0016)	(0.0018)	(0.0015)
$Black_i$		0.0013	0.0005	0.0020
		(0.0010)	(0.0011)	(0.0013)
$Asian_i$		-0.0048**	-0.0052*	-0.0045**
		(0.0017)	(0.0022)	(0.0013)
Another $Race_i$		-0.0001	0.0024	-0.0021
		(0.0011)	(0.0029)	(0.0017)
(HH income/Federal poverty $line)_{it}$		-0.0007***	-0.0011***	-0.0004**
		(0.0000)	(0.0001)	(0.0001)
(HH income/Federal poverty $line)_{it}^2$		0.0000***	0.0000***	0.0000
		(0.0000)	(0.0000)	(0.0000)
		-0.0202	-0.0492*	0.0075

Age-Group Unemployment Rate		(0.0143)	(0.0204)	(0.0144)
$State_{st}$ Unemployment $Rate_{st}$		-0.0700	-0.0631	-0.0802
		(0.0599)	(0.0936)	(0.0457)
Mean Dep. Variable	0.0078	0.0078	0.0080	0.0076
Individual-Months	373,553	373,553	182,214	191,339
R^2	0.0029	0.0036	0.0061	0.0049

Reporting results from equation (2). All models include a monthly, state, and age-level fixed effect. Covariates refer to statistics describe in Table 1. Mean of dependent variable is unconditional mean percentage of treatment group doing contingent work in pre-period. age group clustered robust standard errors in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$

Table 3a: Specification Comparison-Full Sample

Specification:	Column 1: Baseline Model	Column 2: Column 1 + Student Status	Column 3: Slusky /Antwi Model	Column 4: Weighted	Column 5: 4th Reference Months	Column 6: Clustered at State Level	Column 7: Expanded Treatment	Column 8: Individual Fixed Effects
$Treatment_g$	0.0023	0.0027*	0.0034*	0.0038	0.0019	0.0023	0.0094***	0.0005
	(0.0014)	(0.0014)	(0.0014)	(0.0022)	(0.0018)	(0.0015)	(0.0018)	(0.0046)
$Post_t$	0.0078	0.0080	0.0085	0.0081	0.0059	0.0078**	-0.0005	-0.0069*
	(0.0128)	(0.0128)	(0.0128)	(0.0154)	(0.0129)	(0.0030)	(0.0136)	(0.0037)
($Treatment_g$ * $Post_t$)	0.0018	0.0018	0.0013	0.0015	0.0017	0.0018	0.0018	0.0037**
	(0.0011)	(0.0011)	(0.0014)	(0.0013)	(0.0013)	(0.0013)	(0.0012)	(0.0018)
P-Value Treatment=0	0.183	0.166	0.402	0.309	0.257	0.189	0.169	0.0418
Mean Dep. Variable	0.0079	0.0079	0.0078	0.0079	0.0079	0.0078	0.0090	0.0078
Individual-Months	373,553	373,553	373,553	373,553	93,128	373,553	651,098	373,553
R^2	0.0036	0.0038	0.0037	0.0051	0.0043	0.0036	0.0026	0.0032
Number of Individuals								18,810

Notes: All models include gender, a binary equal to one if respondent has children, immigrant status, race, ethnicity, age-level unemployment, state-level unemployment, marital status, and respondent's household income divided by the poverty line as well as that term squared. Models were run using OLS, though Probit and Logit had same results. Mean of dependent variable is unconditional mean percentage of treatment group doing contingent work in pre-period Column1 is the model discussed in this paper. Column 2 is the same as column 1 except it includes student status as a control. Column 3 includes an interaction term between state-level unemployment rate and treatment indicator and includes student status as control while omitting the SIPP age-group level unemployment rate. Column 4 is column 1 but weighted using SIPP person weights. Column 5 uses only the 4th reference months of the SIPP. Column 6 uses state clustered standard errors. In column 7, treatment group has been expanded to 19-25-year-olds from 23-25-year-olds. Expanding the comparison group lowers the treatment effect but maintains the standard errors, decreasing the significance. Column 8 includes an individual specific fixed effect and uses individual-clustered standard errors. ∗ p < 0.10, ∗∗ p < 0.05, ∗∗∗ p <0.01 Columns 1-5 have age group clustered robust standard errors in parentheses

Table 3b: Specification Comparison-Men

Specification:	Column 1: Baseline Model	Colum 2: Column 1 + Student Status	Column 3: Slusky/ Antwi Model	Column 4: Weighted	Column 5: 4th Reference Months	Column 6: Clustered at State Level	Column 7: Expanded Treatment	Column 8: Individual Fixed Effects
$Treatment_g$	0.0022	0.0026	-0.0004	0.0036*	0.0004	0.0022	0.0133***	0.0081
	(0.0015)	(0.0014)	(0.0013)	(0.0016)	(0.0021)	(0.0027)	(0.0023)	(0.0075)
$Post_t$	0.0150	0.0153	0.0160	0.0177	0.0143	0.0150***	0.0075	-0.0045
	(0.0193)	(0.0192)	(0.0200)	(0.0213)	(0.0187)	(0.0047)	(0.0152)	(0.0056)
($Treatment_g$ * $Post_t$)	0.0031*	0.0032*	0.0019	0.0028	0.0026	0.0031	0.0033**	0.0041
	(0.0015)	(0.0015)	(0.0015)	(0.0018)	(0.0016)	(0.0020)	(0.0014)	(0.0028)
P-Value Treatment=0	0.0955	0.0956	0.259	0.171	0.175	0.123	0.040	0.144
Mean Dep. Variable	0.0080	0.0080	0.0080	0.0086	0.0077	0.0080	0.0098	0.0080
Individual-Months	182,214	182,214	182,214	182,214	45,438	182,214	322,489	182,214
R^2	0.0061	0.0063	0.0062	0.0070	0.0082	0.0061	0.0038	0.0053
Number of Individuals								9,365

Notes: All models include gender, a binary equal to one if respondent has children, immigrant status, race, ethnicity, age-level unemployment, state-level unemployment, marital status, and respondent's household income divided by the poverty line as well as that term squared. Models were run using OLS, though Probit and Logit had same results. Mean of dependent variable is unconditional mean percentage of treatment group doing contingent work in pre-period Column1 is the model discussed in this paper. Column 2 is the same as column 1 except it includes student status as a control. Column 3 includes an interaction term between state-level unemployment rate and treatment indicator and includes student status as control while omitting the SIPP age-group level unemployment rate. Column 4 is column 1 but weighted using SIPP person weights. Column 5 uses only the 4th reference months of the SIPP. Column 6 uses state clustered standard errors. In column 7, treatment group has been expanded to 19-25-year-olds from 23-25-year-olds. Expanding the comparison group lowers the treatment effect but maintains the standard errors, decreasing the significance. Column 8 includes an individual specific fixed effect and uses individual-clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Columns 1-5 have age group clustered robust standard errors in parentheses

Table 3c: Specification Comparison-Women

Specification:	Column 1: Baseline Model	Colum 2: Column 1 + Student Status	Column 3: Slusky/Antwi Model	Column 4: Weighted	Column 5: 4th Reference Months	Column 6: Clustered at State Level	Column 7: Expanded Treatment	Column 8: Individual Fixed Effects
$Treatment_g$	0.0024	0.0027	0.0066**	0.0040	0.0032	0.0024	0.0051	-0.0066
	(0.0014)	(0.0015)	(0.0026)	(0.0030)	(0.0020)	(0.0019)	(0.0020)	(0.0052)
$Post_t$	-0.0019	-0.0018	-0.0014	-0.0063	-0.0058	-0.0019	-0.0149	-0.0081*
	(0.0026)	(0.0026)	(0.0028)	(0.0034)	(0.0041)	(0.0038)	(0.0356)	(0.0048)
($Treatment_g$ * $Post_t$)	0.0003	0.0003	0.0004	-0.0000	0.0007	0.0003	0.0003	0.0034
	(0.0016)	(0.0016)	(0.0017)	(0.0018)	(0.0020)	(0.0015)	(0.0015)	(0.0023)
P-Value Treatment=0	0.859	0.832	0.817	0.983	0.751	0.838	0.836	0.134
Mean Dep. Variable								
Individual-Months	191,339	191,339	191,339	191,339	47,690	191,339	328,609	191,339
R^2	0.0049	0.0050	0.0050	0.0082	0.0060	0.0049	0.0037	0.0046
Number of Individuals								9,482

Notes: All models include gender, a binary equal to one if respondent has children, immigrant status, race, ethnicity, age-level unemployment, state-level unemployment, marital status, and respondent's household income divided by the poverty line as well as that term squared. Models were run using OLS, though Probit and Logit had same results. Mean of dependent variable is unconditional mean percentage of treatment group doing contingent work in pre-period Column1 is the model discussed in this paper. Column 2 is the same as column 1 except it includes student status as a control. Column 3 includes an interaction term between state-level unemployment rate and treatment indicator and includes student status as control while omitting the SIPP age-group level unemployment rate. Column 4 is column 1 but weighted using SIPP person weights. Column 5 uses only the 4th reference months of the SIPP. Column 6 uses state clustered standard errors. In column 7, treatment group has been expanded to 19-25-year-olds from 23-25-year-olds. Expanding the comparison group lowers the treatment effect but maintains the standard errors, decreasing the significance. Column 8 includes an individual specific fixed effect and uses individual-clustered standard errors. ∗ $p < 0.10$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$ Columns 1-5 have age group clustered robust standard errors in parentheses

Table 4: Placebo Time Analysis on Men

	Column 1	Column 2	Column 3	Column 4	Column 5
Pre-Treatment Time	May '08- Sept '08	May '08 - Feb '09	May '08 - July '08	May '08 - Dec '09	May '08 - April '10
Placebo Treatment Time	Oct '08 - Sept '10	March '09 - Sept '10	Aug 09' - Sept '10	Jan '10 - Sept '10	May '10 - Sept '10
$Treatment_g$	0.0078**	0.0105**	0.0086**	0.0090*	0.0084**
	(0.0025)	(0.0029)	(0.0033)	(0.0036)	(0.0021)
Placebo $Post_t$	0.0059	0.0090	0.0041	0.0039	-0.0000
	(0.0079)	(0.0049)	(0.0036)	(0.0036)	(0.0028)
$Treatment_g$ * Placebo $Post_t$	0.0016	-0.0028**	0.0003	-0.0001	0.0004
	(0.0022)	(0.0008)	(0.0009)	(0.0011)	(0.0016)
Mean Dep. Variable	0.0110	0.0104	0.0084	0.0078	0.0074
Ind.-Months	90,594	90,594	90,594	90,594	90,594
R^2	0.0076	0.0077	0.0076	0.0076	0.0076

Notes: Results come from equation (2) but drop the treated time period and splits the pre-treatment into pre-treatment and placebo-treatment time. The treatment time period was dropped from the sample. All results are from the baseline model, and include immigrant status, a binary equal to one if the respondent has children and zero otherwise in that month, gender, race, ethnicity, age-level unemployment, state-level unemployment, marital status, and respondent's household income divided by the poverty line as well as that term squared. Mean dependent variable is the unconditional mean percentage of men in treatment group doing contingent work in control period. Monthly, state-level, and age group fixed effects are included in each model Age-Group Clustered robust standard errors in parentheses. ∗ $p < 0.10$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$

Table 5: Difference in Means between Treated Men and Women

Characteristic	Column 1: Mean of Men	Column 2: Mean of Women	Column 3: Diff (Women-Men	Column 4: p-value (Diff=0)
Age	24.02	24.00	-0.02	0.377
Black	0.12	0.15	0.03	0.111
White	0.81	0.77	-0.04	0.103
Asian	0.04	0.04	-0.00	0.656
Another Race	0.03	0.04	0.01	0.173
Hispanic	0.19	0.18	-0.01	0.570
Immigrant	0.12	0.11	-0.01	0.0557
Household Income	5635	5304	-331	0.0424**
Has Children	0.53	0.59	0.06	0.0408**
Lives with Family	0.70	0.62	-0.08	0.0109**
Married	0.20	0.30	0.10	0.00286***
Lives in Metro Area	0.80	0.81	0.01	0.348
Enrolled in School	0.20	0.25	0.05	0.000782**
In Labor Force	0.85	0.76	-0.09	0.0170**
Insured by any Source	0.54	0.57	0.03	0.0656*
In Contingent Workforce	0.01	0.01	-0.00	0.203
Age Group Unemployment	0.35	0.35	0.00	0.649
State Unemployment	0.09	0.09	-0.000	0.824

Women, White, Black, Asian, Other Races, Hispanic, and Immigrant, are binary variables equal to one if the respondent identifies as that demographic. HH income is measured in amount actually received during the month for all members of family above 15, before deductions for income and payroll taxes. Lives with family, Lives in Metro Area, Enrolled in School, In LF, in Contingent Workforce, Married, and Insured by Any source are binary variables equal to one if the individual meets that condition in a given month. Number of children is the count of respondent's children younger than 18 living with them. Age-Group unemployment is the number of respondents in an age group who are unemployed divided by the number of respondents in that age group who are in the labor force for each month. State Unemployment is the BLS statewide unemployment rate for each month.

Table 6: Heterogeneity of the Treatment Effect among Men

Demographic	Column 1: All Men	Column 2: White Men	Column 3: Black Men	Column 4: Asian Men	Column 5: Other Men
Treatment Effect	0.0031**	0.0039**	-0.0041	0.0111***	-0.0047
	(0.0015)	(0.0018)	(0.0046)	(0.0019)	(0.0081)
P-value (diff=0)		0.5712	0.1680	0.0097	0.7075
Mean Dep. Variable	0.0080	0.0088	0.0107	0.0058	0.0090

Demographic	Column 5: Hispanic Men	Column 6: Male Immigrants	Column 7: Married Men	Column 8: Male Students	Column 9: Men who live with family
Treatment Effect	0.0051	0.0049	0.0016	0.0026	0.0067**
	(0.0047)	(0.0052)	(0.0012)	(0.0027)	(0.0024)
P-value (diff=0)	0.6248	0.9093	0.0671	0.9316	0.0578
Mean Dep. Variable	0.0139	0.0166	0.0101	0.0055	0.0086

Demographic	Column 10: Men with at least some college education
Treatment Effect	0.0038**
	(0.0013)
P-value (diff=0)	0.9018
Mean Dep. Variable	0.0075

Notes: P-value results from equation (4), testing to see if difference between full sample and sub group is statistically significant. Treatment effect comes from equation (2) run with just subgroup. Regressions only run with men instead of full sample. Within each demographic, p-value is F-test that the treatment is different for this group of men compared to all other men (married men vs single men, for instance). Mean dependent variable is unconditional mean percentage of respondents in that demographic group doing contingent work in the pre-treatment period. All models include a yearly, monthly, state, and age-level fixed effect., as well as controls in baseline model. Age group clustered standard errors in parenthesis. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$

Table 7: Broadening Definition of Contingent Work for Men

	Column 1: Core Contingent	Column 2: Core Contingent and Moonlighters	Column 3: Core Contingent and Variable Hours	Column 4: Core Contingent and Part time	Column 5: Core Contingent and all others
$Treatment_g$	0.0022	0.0050**	-0.0123***	0.0559***	0.0486***
	(0.0015)	(0.0017)	(0.0029)	(0.0029)	(0.0036)
$Post_t$	0.0150	-0.0115	0.0147	0.0466	0.0372
	(0.0193)	(0.0399)	(0.0206)	(0.0566)	(0.0651)
($Treatment_g$ * $Post_t$)	0.0031*	0.0004	0.0014	0.0193**	0.0162*
	(0.0015)	(0.0014)	(0.0025)	(0.0068)	(0.0071)
Mean Dep. Variable	0.0080	0.0232	0.0245	0.1467	0.1700
Observations	182,214	182,214	182,214	182,214	182,214
R-squared	0.0061	0.0083	0.0132	0.0254	0.0196

Notes: Regressions only run with men instead of full sample. Column 1 is baseline specification from Table 2. Core contingent workers are those who report having an "alternative work arrangement" for that month. In column 2, definition of contingent work has been broadened to those who "moonlight" or work off the books at another job as well as core contingent workers. Column 3 includes core contingent workers and workers who report variable hours. Column 4 includes core contingent workers and part time workers. Column 5 includes core contingent workers as well as moonlighters, workers with variable hours, and part time workers. Mean value is unconditional mean percentage of respondents doing that work in baseline period. All models include a monthly, state, and age-level fixed effect, as well as all controls in equation (2). ∗ $p < 0.10$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$ age group clustered robust standard errors in parentheses

Table 8: Marginal Contingent Worker

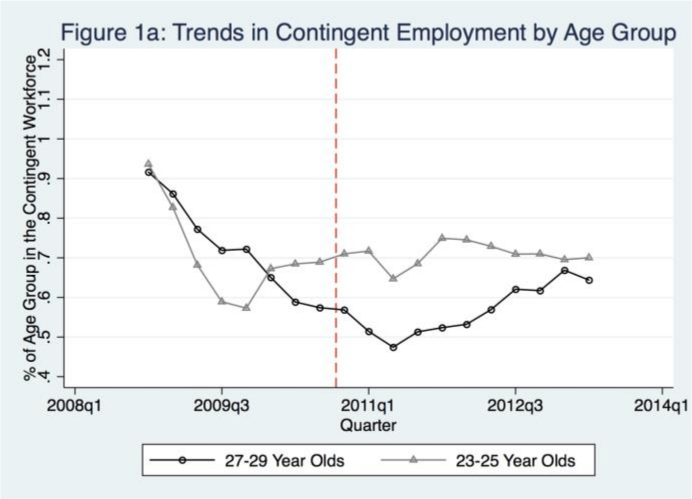
	Column 1: Full Sample	Column 2: Men	Column 3: Women
$Treatment_g$	0.0033**	0.0040	0.0029**
	(0.0011)	(0.0021)	(0.0009)
$Post_t$	-0.0085**	-0.0096	-0.0082*
	(0.0025)	(0.0050)	(0.0040)
($Treatment_g$ * $Post_t$)	0.0011	0.0017*	0.0003
	(0.0007)	(0.0007)	(0.0009)
Some CW in Pre-$Period_i$	0.2297***	0.2395***	0.2169***
	(0.0171)	(0.0181)	(0.0262)
($Treatment_g$ * Some CW_i)	0.0000	-0.0103	0.0132
	(0.0308)	(0.0306)	(0.0377)
(Some CW_i * $Post_t$)	-0.2142***	-0.2144***	-0.2117***
	(0.0207)	(0.0262)	(0.0220)
($Treatment_g$*Some CW_i * $Post_t$)	0.0357	0.0331	0.0351
	(0.0265)	(0.0345)	(0.0295)
Mean Dep. Variable	0.0078	0.0080	0.0076
Individuals	311,671	151,522	160,149
R^2	0.1516	0.1567	0.1483

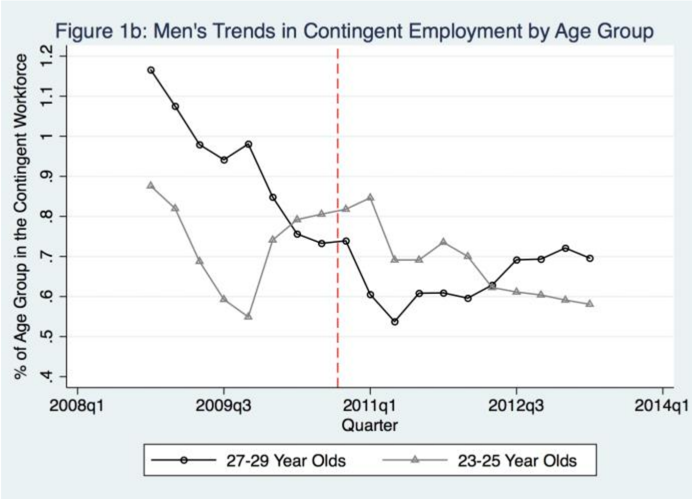
Results from equation (4) replacing Hispanic with a binary equal to 1 if respondent did any contingent work in the pre-period and zero if they did not. All models include a monthly, state, and age-level fixed effect, as well as all controls in equation (2) Age group clustered robust standard errors in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$

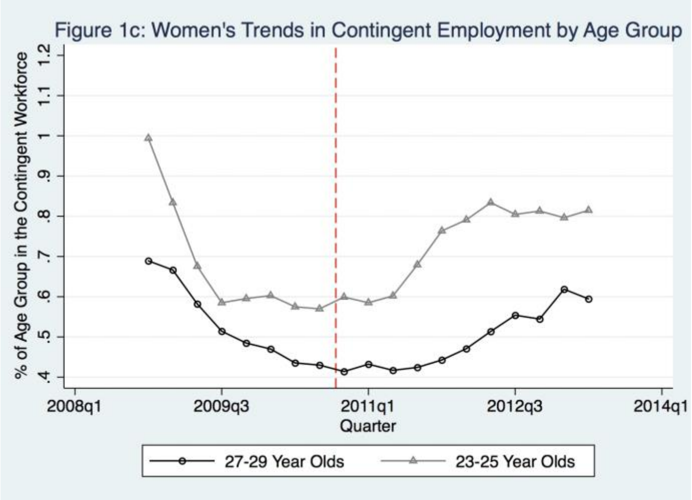
Table 9: Parallel Trends Test

	Column 1: Full Sample	Column 2: Men	Column 3: Women		Column 1 cont. Full Sample	Column 2 cont . Men	Column 3 cont. Women
Treatment_g	-0.00	-0.03	0.02				
	(0.02)	(0.03)	(0.03)				
Month:				Month			
1	0.00	0.01	-0.01	18	-0.00	0.01	-0.01
	(0.01)	(0.02)	(0.01)		(0.01)	(0.01)	(0.01)
2	0.00	0.02	-0.01	19	-0.00	0.01	-0.01
	(0.01)	(0.02)	(0.01)		(0.01)	(0.01)	(0.01)
3	0.00	0.02	-0.01	20	-0.00	0.01	-0.01
	(0.01)	(0.02)	(0.01)		(0.01)	(0.02)	(0.01)
4	0.00	0.02	-0.01	21	-0.00	0.02	-0.01
	(0.01)	(0.02)	(0.01)		(0.01)	(0.01)	(0.01)
5	0.00	0.02	-0.01	22	0.00	0.02	-0.01
	(0.01)	(0.02)	(0.01)		(0.01)	(0.01)	(0.01)
6	0.00	0.02	-0.01	23	0.00	0.01	-0.01
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
7	0.00	0.02	-0.01	24	0.00	0.01	-0.01
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
8	0.00	0.02*	-0.01	25	0.00	0.02	-0.01
	(0.01)	(0.01)	(0.01)		(0.01)	(0.02)	(0.02)
9	0.00	0.02*	-0.01	26	0.01	0.03	-0.01
	(0.01)	(0.01)	(0.01)		(0.01)	(0.02)	(0.02)
10	0.00	0.01	-0.01	27	0.01	0.03	-0.01
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.02)
11	-0.00	0.01	-0.01	28	0.01	0.03	-0.01
	(0.01)	(0.01)	(0.01)		(0.02)	(0.03)	(0.02)
12	-0.00	0.00	-0.01				
	(0.01)	(0.01)	(0.01)	Ind. Months	187,040	90,594	96,446
13	-0.00	0.00	-0.01	p-value	0.803	0.117	0.754
	(0.01)	(0.01)	(0.01)				
14	-0.00	0.01	-0.01				
	(0.01)	(0.01)	(0.01)				
15.	0.00	0.01	-0.01				
	(0.01)	(0.01)	(0.01)				
16	0.00	0.01	-0.01				
	(0.01)	(0.01)	(0.01)				
17.	0.00	0.01	-0.01				
	(0.01)	(0.01)	(0.01)				

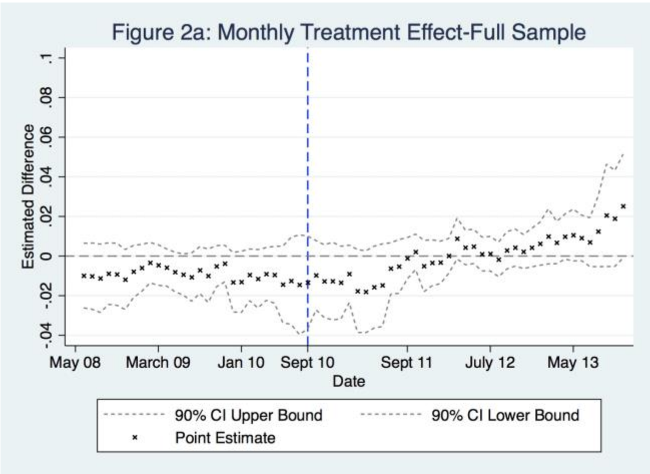
The coefficients on months 1-28 are the conditional difference in trends between treatment and comparison group in contingent work in that month. Coefficients come from Equation (1). P-value is from joint F-test that coefficients on months 1-28 are jointly zero. Non-clustered standard errors. Controls include race, gender, ethnicity, marital status, household income as a share of fed. Poverty line and that term squared. Dummy for year and month, a time trend, state and age-fixed effects.

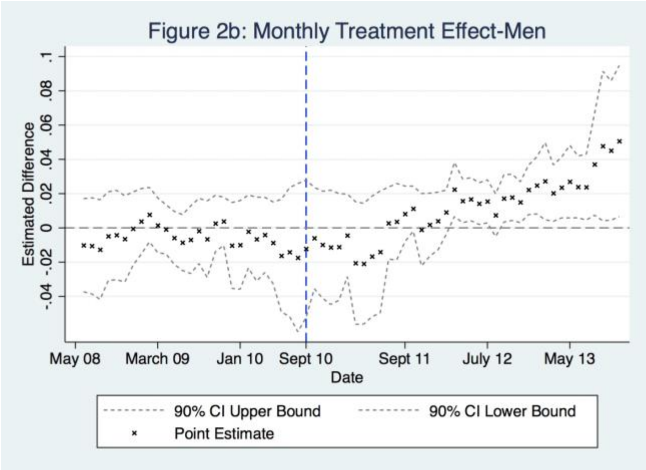


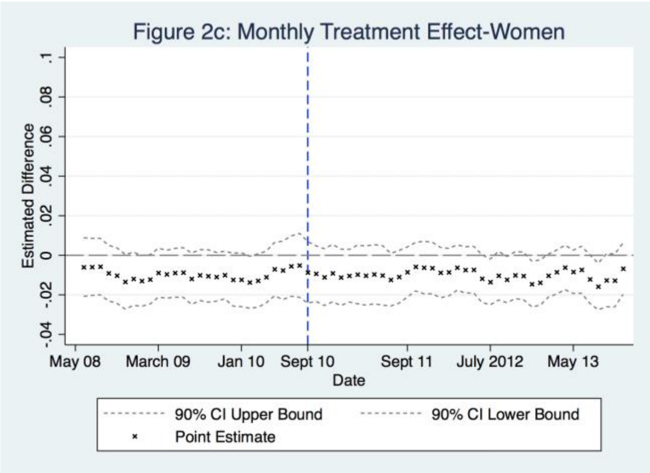




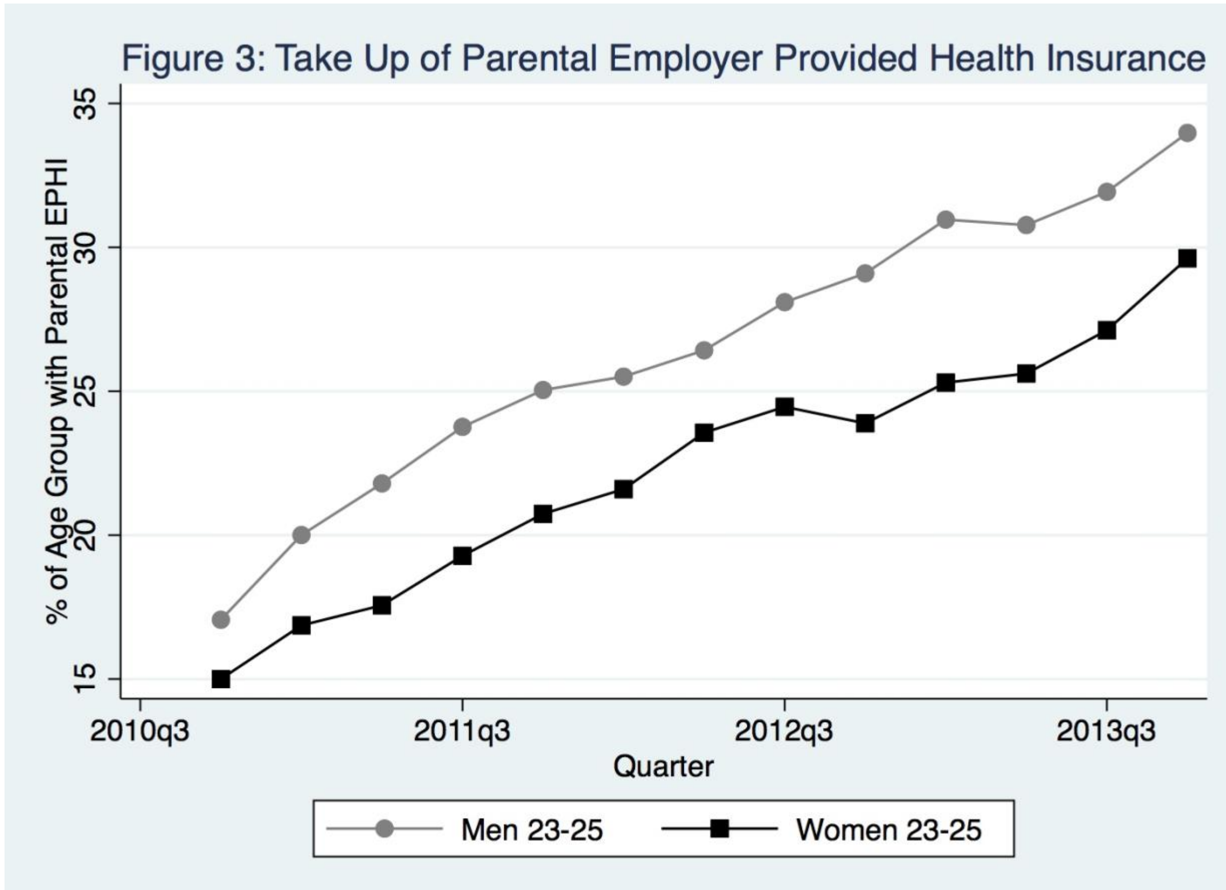
Figures 1a-c were generated by first finding the percent of the treatment groups and comparison groups doing contingent in each quarter. Next a moving average was created by taking the average of each quarter and the 2 preceding and 2following months. The area to the left of the red dashed line represents the pre-treatment period, while the area to the right of the dashed line represents the treatment period. Test for parallel trends resulted in a p-value of 0.80 for the full sample, 0.11 for men and 0.75 for women, confirming trends are parallel.







Figures 2a-2c show the output from equation (3). The dotted grey lines show the 90% confidence intervals while the xs show the conditional point estimate of the difference between treatment and comparison groups. The area to the left of the blue dashed line represents pre-treatment time while the area to the right represents treatment time. Controls include race/ethnicity, gender, marital status, age-group and state specific unemployment, monthly and yearly dummies, age-fixed effects, state-fixed effects. Non-clustered standard errors. Test for parallel trends resulted in a p-value of 0.80 for the full sample, 0.11 for men and 0.75 for women, confirming trends are parallel.



Notes: Figure 3 depicts the percentage of men and women aged 23-25 who took up their parent's employer provided health insurance in that quarter. Only the quarters that took place after enrollment was possible are displayed.

Appendix B: Explanation of the SIPP

The data in this paper comes from the 2008 Survey of Income and Program Participation run by the Census Bureau. The SIPP surveys families over a 64-month span and breaks individuals into a rotation group. Interviews began in September of 2008 and ended in December of 2013. This time line was broken into 16 waves corresponding to four months each and waves overlapped on the rotation group axis: For example, September of 2008 was part of wave one but only rotation group one was interviewed during September. For rotation group one, September 2008 is reference month one. The next month both rotation group one and two are interviewed, but for rotation group two this is reference month one while for rotation group one this is reference month two. After every rotation group has been interviewed they transition into wave two; for rotation group one wave two began in January of 2009, while for rotation group four wave two began in April of 2009. Therefore, data collection spanned a total of 66 months despite the fact that each rotation group was only followed for 64 months. From this data a sample of individual-months can be created.