Significant Placebo Results in Difference-in-Differences Analysis: The Case of the ACA's Parental Mandate

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The Affordable Care Act lets young adults stay on their parents' insurance. Several recent papers use broad age-time difference-in-differences strategies to argue that this causes significant health insurance and labor effects. Using SIPP and CPS data, I show that difference-in-differences models over "placebo" dates also produce statistically significant "effects" long before ACA implementation, even with conservative standard errors and matching adjustments. This suggests that the effects attributed to the ACA could instead reflect dynamics in the age-structure of the health insurance and labor markets. Reducing the age bandwidth yields more reliable estimates of the increases in parental and overall insurance coverage. The key problem in this literature is therefore potentially overstating the "effects" of the ACA in other dimensions. (JEL I13, I18, J08)

Keywords: *health insurance; labor supply; young adults; ACA; placebo tests; difference-in-differences*

I. INTRODUCTION

The Patient Protection and Affordable Care Act (ACA), signed into law by President Obama on March 23, 2010, includes a provision mandating that as of September 23, 2010 most young adults must be allowed to stay on their parents' health insurance until age 26. Several recent papers study the potential effects of this early provision, making important contributions. Their results are intuitive: an increase in the share of individuals with dependent insurance coverage (Akosa Antwi, Moriya, and Simon 2013; O'Hara and Brault 2013; Cantor et al. 2012b; Sommers and Kronick 2012; Sommers et al. 2013, Chua Sommers 2014), a decrease in the uninsurance rate (Akosa Antwi, Moriya, and Simon 2013; Mulcahy et al. 2013; O'Hara and Brault 2013; Cantor et al. 2012b; Sommers and Kronick 2012, Jhamb et al. 2015), a decreased likelihood of delaying or not obtaining care due to cost (Sommers et al. 2013, Barbaresco et al. 2015), a decrease in out of

pocket costs (Chua and Sommers 2014; Busch et al. 2014) an increased likelihood of having a usual source of care (Sommers et al. 2013; Barbaresco et al. 2015), increased labor market flexibility (Akosa Antwi, Moriya, and Simon 2013), a drop in share of uninsured ER visits (Mulcahey et al. 2013), improved self-reported health (Barbaresco et al. 2015; Carlson et al. 2014), increased use of inpatient and mental health resources (Akosa Antwi et al. 2015; Saloner Le Cook 2014), an increase in dental coverage (Han et al 2014, Shane and Ayyagari 2014), a decrease in emergency room visits (Hernandez-Boussard et al. 2014, Akosa Antwi et al. 2015), and increase in premiums for health insurance plans that cover children (Depew and Bailey 2015).

Econometrically, all of these studies use an age-time difference-in-differences strategy, which has also been used to investigate the impact of other health insurance public policies with sharp age-cutoffs (e.g. Levine, McKnight, Heep 2011). Cantor et al. 2012b, using the years 2005-2010, compares those age 19-25 to those age 27-30, before and after the 2010 implementation of the parental insurance mandate. Unfortunately, this approach does not satisfy the crucial assumption for a difference-in-differences analysis, which is that in the absence of treatment the average outcomes for the affected and comparison groups would have followed parallel trends (Bertrand et al. 2004, Abadie 2005). If this condition is not satisfied, the difference in average trends between the affected and comparison groups in the affected time period can confound the effect of the policy, or even suggest a substantial one when none exists.

While this critique could be applied to any health insurance public policies with sharp age-cutoffs (e.g., Medicare, S-CHIP), it is especially applicable to the ACA's parental mandate because of the age-specific labor market turmoil that was occurring during its enactment and implementation. Specifically, over the past few decades, the United States has undergone substantial shifts in the structure of its labor force (e.g. see Card and Lemiuex 2000; DiCrecio et

al. 2008). Crucially, these shifts have had differential age effects (e.g. see CBO 2004; Bell and Branchflower 2011; O'Higgins 2012), especially during the Great Recession of 2007-2009 (e.g. see Lazear and Spletzer 2013; Dunn 2013), contaminating any age-time difference-in-differences analysis.

[Insert Figure 1 Here]

For example, Figure 1 shows the proportion of individuals who are employed, split into those affected by the expansion of parental coverage under the ACA (those aged 19-25) and comparison age groups not affected (16-18, 27-29). This data is smoothed over the five months before and after the interview month to reduce seasonality. The grey vertical line represents the earliest period to include data from the enactment of the federal mandate (October 2009). The gaps are due to missing data between the end of one panel and the start of the next, which are larger due to the smoothing.

If this were an appropriate application of a difference-in-differences strategy, then while the two groups would have unequal step changes directly after the implementation, the subsequent trends would be the same. However, the two lines to the right of the vertical line are not parallel, suggesting substantial labor market differences between those two groups. Furthermore, one can see in the black boxes that are a many time periods where the two lines have non parallel trends.

To quantify whether these trends are parallel or not one can regress the probability of being employed on month-year-group dummies and then run a joint F-test of whether the coefficients for each month-year until enactment (i.e., through February 2010) are equal across groups (e.g., January#1997#16-18 & 27-29 = January#1997#19-25, February#1997#16-18 & 27-29 = February #1997#19-25). For 186 months in the full dataset, the F-stat is 1.65*** ($p<0.0001$), suggesting that the two trends are not equal. Repeating the analysis with quarter-year-group dummies

(dropping quarters without 3 months of data) gives an F-stat of 2.45*** ($p<0.0001$) for 59 quarters, which is consistent.[1]

[Insert Figure 2 Here]

Figure 2 shows the same picture for the share of individuals working full-time workers (greater than 30 hours/week). This is arguably more relevant to health insurance coverage since usually only full-time employees receive benefits. Analogous to Figure 1, there are periods in the black boxes which have substantially non parallel trends, lending intuition to the claim that young adults of different ages are not ex-ante equivalent. The analogous F-stats are 1.65*** ($p<0.0001$) and 2.78*** ($p<0.0001$).

Since many young adults have health insurance coverage (or the option of it) through their employer, this changing labor market also makes it possible that there were group-specific trends in insurance outcomes during the affected time period that would confound any estimates of policy impacts.

For example, if full-time employment growth were lower for the affected age than the comparison age due to overall labor market conditions, then these individuals would comparatively be losing their own employer sponsored coverage and switching to parental insurance wherever possible, independent of any change in mandated insurance availability. Failing to properly control for this would bias estimated effects away from zero as they would include both the impact of the mandate and the impact of the differentially changing labor conditions.

[1] The regression that generates the coefficients and standard errors for the month-year or quarter-year dummy variables does not cluster standard errors at the state level (as all results below do) because there are fewer clusters (51) than conditions tested (186 or 59). Still, calculating a conservative p value from F(186,51) instead of F(186,593938) gives $p=0.0186<0.05$ and so the result that the trends are not equal is robust. This consistency is the case for all F-statistics in this paper.

It would be extremely challenging to ascertain whether the conclusions in the literature from a pre-2010/post-2010 difference-in-differences are the result of the ACA or from differential trends. Therefore, in lieu of this I will perform earlier-in-time "placebo" regressions tests on three of the most prominent papers on this topic: Sommers and Kronick 2012 (hereafter SK); Cantor et al. 2012b (Cantor); and Akosa Antwi, Moriya, and Simon 2013 (AMS).[2] These papers fostered an entire field of research, and provided rapid, positive feedback on the early consequences of the ACA. Their ingenuity did the field a great service.

My falsification tests will use each paper's specification, shifting their temporal windows (i.e. 2005-2010, 2008-2011) backward in time one year at a time.[3] Using this approach, I find that the differential age-time health insurance and employment effects that appear after the ACA is implemented also appear in other time periods, even with a narrower age bandwidth, thus undermining the conclusion that these effects are causal outcomes of the ACA. Rather, they may be a consequence of the age-differential changes in labor market in the United States.[4]

To attempt to mitigate these differences, I use multiple conservative difference-in-differences statistical methods: adjusting for serial correlation (see Bertrand, Duflo, Mullainathan 2004), adjusting for intra-group correlation (see Donald and Lang 2007) and also weighing the comparison ages to create synthetic control groups (as outlined in Abadie and Gardeazabal 2003). While useful in many contexts, none of these methods substantially reduce statistically significant placebo results in this paper.

[2] AMS (2013) does include limited placebo analysis by randomizing the implementation month between Sept 2008 and Jan 2010, and find no significant results beyond expected Type I errors. Still, trend stability in this short pre-period does not necessarily imply parallel trends in post-period

[3] These placebo tests are not entirely independent since I am rolling a multi-year window backward one-year at a time. Still, the time frame over which I am running placebo regressions is long enough that this overlap cannot entirely explain the placebo results.

[4] This analysis is in the spirit of Ham, Li, and Shore-Sheppard (2009) which dealt with non-comparable control groups and changes in public policy albeit in another context.

I also drastically reduce the age bandwidth which does allow me to more reliably test the hypotheses in the literature. However, only some of the health insurance results remain statistically significant, whereas none of the labor supply results are. A Monte Carlo analysis using the health insurance results confirms this conclusion.

Already, due to the influence of earlier version of this paper, two of the most recent working papers on this topic (Akosa Antwi et al. 2015 and Barbaresco et al. 2015) include a reduced age bandwidth either as their main specification or as a robustness test. Still, many other new papers in the literature continue to use broad age bandwidths with limited if any robustness checks.

II. DATA

For this analysis I use two public microdata sources: the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (aka the "March CPS") covering years 1999-2010[5], and the Survey of Income and Program Participation (SIPP), for 1993-2011[6]. I use the March CPS data to replicate SK's and Cantor's results, whereas for AMS I replicate using SIPP data.[7]

[5] The March Supplement in 2011 underwent a significant change to its imputation procedure so that any non-policy holder in the household can now be coded as a dependent on another household member's plan. Compared to the old routine, estimates derived from the new one reduced the uninsurance rate by 0.5 percentage points (1.5 million people) and increased the rate of any private coverage by 0.5 percentage points (1.7 million people) (Boudreaux and Turner 2011). Microdata going back to only the 2000 survey (reference year 1999) was re-released under this new procedure.

[6] Consistent SIPP data on source of health insurance for dependent individuals is only available from 2001-2011. Additionally, due to the fact that the SIPP is primarily designed as a panel survey, there are significant gaps in the data between the end of one panel and the beginning of the next (i.e. 2000, 2008). As a result, any multi-year placebo time period covering either of these years is incomplete, resulting in fewer potential regressions for comparison.

Furthermore, the 1996 panel does begin until March, and so there is no data for January and February of that year. Rather than dropping the 1993-1996, 1994-1997, and 1995-1998 placebo regressions, I include those with the missing months omitted. The lack of these months should not have a differential effect on those in the affected age group compared to the comparison age groups and therefore should not bias the results.

[7] As a complement to the SIPP, I also use the basic monthly Current Population Survey for 1994-2011 (Census 2013a, 2013b, 2013c) for additional labor supply placebo regressions (see the Online Appendix). The CPS covers every month in the entire sample, and as a result allows for several more placebo regressions. Furthermore, whereas the primary purpose of the SIPP is to quantify numerous outcomes for a longer panel of individuals, the basic CPS is designed to quantify labor supply, making it better suited to this analysis.

For both data sources, I pool across waves (as in AMS, SK, and Cantor) and also across SIPP panels (as in Gruber and Madrian 1997; Ham and Shore-Sheppard 2005; Fujita et al. 2007; and Gruber and Simon 2008). I am able to approximately match the results of all three prior papers, demonstrating that the only difference between my placebo regressions and their main regression is the years used.

The two data sets produce comparable summary statistics:

[Insert Table 1 Here]

While there is a substantial difference in the share of respondents who have ESI in their name, the other health insurance variables (dependent ESI, government health insurance, and overall coverage rates) are comparable.

Despite this, for health insurance questions, I believe that the SIPP data is superior to the March CPS data for the following reasons. SIPP asks a point in time question referring to the interview month. For example, one question is:

> Other than Medicare, Medicaid [State Program Name], or some other public program, are you covered by health insurance in this month?...Other than you, who else was covered by this plan?[8]

The March CPS on the other hand asks a retrospective question about the previous year:

A minor downside is that the labor variables have small definitional differences compared to the ones in the SIPP. These discrepancies, though, are orthogonal to the age and time dimensions of my difference-in-differences strategy and so should not affect the comparison of different CPS placebo regressions to the main regression.

[8] See http://www.census.gov/content/dam/Census/programs-surveys/sipp/questionnaires/2001/SIPP%202001%20Panel%20Wave%2009%20-%20Core%20Questionnaire.pdf

> These next questions are about health insurance coverage during the calendar year 2009. The questions apply to ALL persons of ALL ages. At any time in 2009, (was/were) (you/ anyone in this household) covered by a health insurance plan provided through (their/your) current or former employer or union? In addition to (name/you) who else in this household was covered by (name's/your) plan?[9]

This causes two potential problems: 1) recall bias where the respondent answers as of the interview month, and 2) even if there is no recall bias it is unclear when in the reference year the respondent is referring to (AMS; Ham and Shore-Sheppard 2005). For this reason, this paper will use the SIPP as its primary source of health insurance data.

III. METHOD

Primary Placebo Regressions

The "placebo regressions" in this paper estimate the econometric specifications of the three papers mentioned above on earlier time periods (e.g 1993-1997, 1999-2004). Equation (1) shows SK's relatively simple difference-in-differences structure:

$$(1) \qquad Y_{igst} = \alpha + \gamma\, Treat_g + \delta\, Implement_t + \eta\, (Treat_g * Implement_t) + \mathbf{A}_{ig} + \boldsymbol{\tau}_t + \varepsilon_{igst}$$

[9] See http://www.census.gov/prod/techdoc/cps/cpsmar10.pdf

Here, as below in equation (3), Y_{igst} represents various outcomes for individual *i* in age range *g*, state *s* and time *t*. η is our coefficient of interest. $Treat_g$ represents a dummy for being in the affected age range, which is 19-26, compared to ages 26-34. $Implement_t$ represents a dummy for the year the reform came into effect (2010), compared to years 2005-2009.[10] $\mathbf{A}_{ig}$ and $\boldsymbol{\tau}_t$ represents age and year fixed effects respectively. Standard errors are clustered at the household level, as the panel structure of the CPS results in each household appearing in two adjacent years.

Equation (2) shows Cantor's specification:

$$
\begin{aligned}
& Y_{igst} = \alpha + \gamma Treat_g + \delta\, Implement_t + \eta(Treat_g * Implement_t) \\
& + \phi StateTreat_{igs} + \lambda StatePolicy_{st} + \rho(StateTreat_{igs} * StatePolicy_{st}) \\
& + \mathbf{X}_{igst}\boldsymbol{\beta} + \boldsymbol{\tau}_t + \boldsymbol{\zeta}_s + \varepsilon_{igst}
\end{aligned} \tag{2}
$$

Here, $Treat_g$ equals 1 for the affected ages of 19-23 (excluding full-time students) and all those aged 24-25 and the comparison are 27-30. η is again our coefficient of interest. As above, there are year fixed effects. Cantor adds (in $\mathbf{X}_{igst}$) controls for age (linear in years, not fixed effects), sex, race, education, marital status, poverty ratio, student status, lives with parents, self-reported health, and a linear time trend.

Cantor also adds (also in $\mathbf{X}_{igst}$) numerous controls at the state level. Most worrisome to him and his co-authors are the numerous state mandates implemented before the federal mandate. Analogous to the federal mandate difference-in-differences, this specification includes $StateTreat_{igs}$ (whether an individual is eligible for the state's current or future mandate)[11] and $StatePolicy_{st}$ (whether the mandate is in effect) and their interaction. Cantor also adds the state-

[10] I am using March CPS data for reference year 2010 as the affected year to be comparable to SK and Cantor. This decision does not affect the placebo results which use earlier years.

[11] Eligibility requirements and effective dates for state mandates are as described in Cantor et al. (2012a).

level unemployment (BLS 2013), the share of workers in self-insured employer insurance and share of employers offering health insurance (MEPS 2013a and 2013b) [12], state fixed effects (ζ_s), and state specific time trends. Finally, due to their comprehensive health care programs, Hawaii and Massachusetts are excluded.

AMS's specification is as in equation (3):

$$(3)\quad \begin{aligned} Y_{igst} &= \alpha + \gamma\, Treat_g + \delta\, Implement_t + \theta\, Enact_t + \eta\ (Treat_g * Implement_t) \\ &+ \sigma\,(Treat_g * Enact_t) + \mathbf{X}_{igst}\,\boldsymbol{\beta} + \boldsymbol{\tau}_t + \zeta_s + \varepsilon_{igst} \end{aligned}$$

As opposed to SK and Cantor, AMS's specification is monthly, covering August 2008-November 2011. η (for the post-implementation period) is our primary coefficient of interest, as above. σ is also reported, as AMS additionally include $Enact_t$ (a dummy for March-September 2010) for when the ACA was enacted but not implemented, and its interaction with $Treat_g$ (which here is 19-25, in comparison to 16-18 and 27-29). This is to control for any anticipatory changes in employer-sponsored policies. For example, a firm whose annual plan year began in this six month period might include young adults before it was mandatory to avoid changing twice in the same year.

$\mathbf{X}_{igst}$ here includes age fixed effects, and dummies for sex, race, marital status, student status and a quadratic of household income as a share of federal poverty line. Student status is crucial as students often have access to what is effectively employer sponsored health insurance through their university. AMS also include monthly linear national and state-specific time trends, the monthly state unemployment rate (and its interaction with $Treat_g$) and state fixed effects.

[12] In the data for each of 1999-2002, MEPS pools approximately 10 of the least populated states. Therefore, for placebo regressions including any of these years, these states are assigned the average value for all of the pooled states as opposed to the respective value for the individual state.

Following from the monthly nature of their survey they include calendar month dummies in $\boldsymbol{\tau}_t$ as well as year fixed effects. Standard errors are clustered at the state level.[13]

With these specifications, and the two data sources previously used (CPS ASEC and SIPP), I am able to replicate the specifications of the three major papers in this literature.

Alternative specifications and methods

It is well documented that a vanilla difference-in-differences analysis (even with proper clustering of standard errors) can suffer from two sources of Type I error: serial correlation (see Bertrand, Duflo, Mullainathan (hereafter BDM) 2004) and intra-group correlation (see Donald and Lang (hereafter DL) 2007). The former is from the concern that including multiple time periods in either the pre- or post-implementation period can introduce serial correlation not mitigated for by time trends or fixed effects. The latter is the concern there is correlations among observations within ages and that this is not mitigated by age fixed effects.

Both BDM and DL recommend aggregating the data to pre- and post-time periods and age groups, respectively, since the effective sample size is much smaller and so the standard errors should be larger. Dinkelman and Ranchhod (2012) combine these two approaches, and so I follow their two-stage method as described below.

First, as in equation (4), regress the outcome variable of interest on individual level controls and a fixed effect for each age#period interaction (13 ages x 3 periods = 39 groups). Period here refers to the three time periods in AMS's specification – before enactment, between enactment and implementation, and post implementation. These fixed effects, $\boldsymbol{\Gamma}_{igt}$, correspond to the averages of

[13] AMS also includes a triple-difference specification, with the third dimension being young adults whose parents do and do not have employer sponsored health insurance. See the Online Appendix for methodology and placebo results.

the outcome variable (weighted by sample weights) for each age in each period, adjusted for individual characteristics.

$$Y_{igst} = \mathbf{\Gamma}_{igt} + \mathbf{X}_{igst}\boldsymbol{\beta} + \boldsymbol{\tau}_t + \boldsymbol{\zeta}_s + e_{igst} \tag{4}$$

Secondly, take the coefficients on these fixed effects and regress them on the differences-in-differences coefficients from above, as well as on age fixed effects (since the data is still at the age-level), as in equation (5).

$$\begin{aligned} \hat{\mathbf{\Gamma}}_{igt} &= \alpha + \gamma Treat_g + \delta Implement_t + \theta Enact_t \\ &+ \eta(Treat_g * Implement_t) + \sigma(Treat_g * Enact_t) + \mathbf{A}_{ig} + u_{igt} \end{aligned} \tag{5}$$

The coefficients of interest (η and σ) will be very similar in magnitude to those from equation (3), but the standard errors will be larger to take into account the two sources of correlation.

An alternate approach to improving difference-in-differences estimates is to create synthetic control groups, as outlined in Abadie and Gardeazabal (2003). The approach recognizes that including those aged 16-18 and 27-29 in roughly equal weights as comparison groups for those aged 19-25 is not an ideal strategy. Instead, for each of the seven ages in the affected population, I construct a comparison group from a weighted average of the other age groups. This increasing emphasis on the groups that are more similar should hopefully reduce placebo results from differences in trends due to noncomparability.

First, I collapse the data to averages by ages for the pre-enactment period (weighted by sample weights). Then for each age (a) in each placebo period (p), I minimize the following error function:

$$\min_{\mathbf{W}_{ap}} (\mathbf{X}_{1ap} - \mathbf{X}_{0p}\mathbf{W}_{ap})'\mathbf{V}_{ap}(\mathbf{X}_{1ap} - \mathbf{X}_{0p}\mathbf{W}_{ap}) \tag{6}$$

$\mathbf{W}_{ap}$ is a period and affected age specific vector of weights for the comparison ages. $\mathbf{X}$ is a vector of relevant non-health insurance variables that differ across these ages: share married, student, employed, unemployed, and working full time, as well as average hours worked and household federal poverty ratio. $\mathbf{X}_{1ap}$ is for the age to be matched, whereas $\mathbf{X}_{0p}$ is for all of the comparison ages. $\mathbf{V}_{ap}$ is a normalizing diagonal matrix with the inverse square of each element in $\mathbf{X}_{1ap}$ on the diagonal.

With the $\mathbf{W}_{ap}$ vectors for each age in each placebo period I constructed a synthetic age for each affected age for each month of data. The weights are intuitive: age 19 is ~90% age 18 and ~10% age 27 whereas age 25 is ~90% age 27 and ~10% age 18. Ages 20 and 24 are closer to an 80/20 mix of ages 18 and 27, and ages 21-23 are more equal mixes, sometimes with data from ages 28 and occasionally even 29 included. This method uses almost none of ages 16 and 17 as they are mostly incomparable to any of ages 19-25.

The actual and synthetic ages are run through equation (3) at the age-month level, with age fixed effects common to each synthetic and actual age. Individual controls are excluded since they were already incorporated into the weighing optimization, as our state fixed effects and trends which should average out at this level of aggregation.

IV. RESULTS

The results below are as follows. First are the results of placebo regressions on the three early literature papers, where all statistically significant outcomes are not robust to placebo results. Second is evidence that the results are also not robust to the two different methodological adjustments outlined above. Finally, this paper presents robust results for substantial robust parental and overall health insurance coverage increases using reduced age bandwidths and a supporting Monte Carlo analysis.

Primary Placebo Results

Figures 3 and 4 are analogous to Figures 1 and 2 above, except here they are showing health insurance outcomes.

[Insert Figure 3 Here]

Figure 3 graphs the share of individuals with parental dependent employer sponsored coverage, starting in 2001 due to a change in the interview questions. While the lines crossing on the right side of the graph suggests some impact of the ACA's parental mandate, there are still numerous areas pre-enactment where the trends are not parallel. The corresponding F-stats for the period up to enactment (i.e., through February 2010) are 2.8*** ($p<0.0001$) at the month level and 6.56*** ($p<0.0001$) at the quarter level.

[Insert Figure 4 Here]

Figure 4 graphs overall coverage rates. As in Figure 3, while there is plausibly a major effect after enactment, there are still several non-parallel sections during the placebo periods. The F-stats (for only 102 months / 32 quarters) since this variable is only available from 2001) are 2.41*** ($p<0.0001$) at the month level and 4.55*** ($p<0.0001$) at the quarter level.

Turning to the regression results, Table 2 shows an approximate replication of SK's minimalist regression using CPS ASEC data on health insurance outcomes, where each column represents the same regression on a different 6-year period. For example, the last column (colored grey) – which

gives approximately the same results as Table 1 in SK – has 2010 as the affected year and 2005-2009 as the comparison years.[14]

[Insert Table 2 Here]

For each of the seven variables tested, there are significant results in a placebo regression, some at the 5% or even 1% level. For example, there is a 1.7 percentage point increase in employer provided coverage in one's own name in 2006 vs. 2001-2005, which is statistically significant at the 1% level. With this number of regressions, some spurious results are expected at the 10% (i.e. 1 in 10) or even 5% level (i.e. 1 in 20). Here, though, there are more significant results for them all to be false positives. More likely, they suggest underlying age-group-specific trends in health insurance in the "affected" placebo time period. The average difference between these trends would also be measured by SK's difference-in-differences strategy and so could give a strongly significant coefficient in the absence of a policy change.

[Insert Table 3 Here]

Table 3 shows the same placebo periods using the same data set, but with Cantor's heavily controlled regression and slightly different age buckets (corresponding to Table 3 in Cantor). As above, there are more statistically significant coefficients (4 at the 5% level and 3 at the 10% level) than can be reasonably attributed to spurious false positives.

[14] As described above, the CPS ASEC data for reference year 2010 is considered "affected" since the respondents were answering questions in March 2011 about the previous year and so likely answered with reference to after September 2010 (when the ACA parental insurance mandate took effect).

The placebo results in Tables 2 and 3 cover a relatively narrow time frame.[15] Using the SIPP data would allow earlier placebo regressions, as the basic question regarding insurance coverage has been consistent for longer. Below, Table 4 shows placebo regressions using SIPP based on AMS's specification. Since this source is monthly, the placebo periods all start and end with the same months. For example, the last column in Table 4 (colored grey) — which gives results identical to Table 2 in AMS — has November 2008-February 2010 as the comparison time period, March-September 2010 as the enactment period, and October 2010-November 2011 as the implementation period. The first column, on the other hand, uses November 1993-February 1995, March-September 1995, and October 1995-November 1996, respectively.

[Insert Table 4 Here]

For each of the four health insurance variables studied here, there are significant results in placebo time periods. For example, while AMS found a 3.2 percentage point increase in coverage (i.e. the extensive margin), there is 2.3 percentage point increase in 2006-2007 vs. 2004-2006 and a 2.4 percentage point decrease in 1996-1997 vs. 1994-1996, all significant at the 1% level. The other variables also have multiple statistically significant results at that level (e.g. a 3.2 percentage point decrease in own employer coverage in 2004-2005 vs. 2002-2004), despite the fact that the respective questions were not even asked in their current form until 2001 and so only four placebo regressions are possible. Even so, the number of significant results and the fact that several are significant at the 1% level suggest that there are other economic factors measured by this specification.

[15] As described above, the March CPS was significantly revised in 2010 and only 1999-2009 data was updated to this new procedure, and so placebo regressions with earlier microdata would not be comparable.

There are many possible stories that can be told to explain the placebo results. For example, consider the events of 2006, compared to the years before. The annual unemployment rate was at a local minimum, with a much greater drop in the previous years for those age 20-24 (10% in 2003 to 8.2% in 2006) than those age 30-34 (5.5% to 4.2%) (BLS). An improving labor should increase the number of individuals employed and with their own benefits, and this increase should be larger for younger ages whose unemployment rate had fallen more. Indeed, in this period, SK find a strongly significant increase in insurance from one's own employer policy, Cantor finds an increase in private-self or spouse coverage and a decrease in private non-spouse dependent coverage, and AMS find a drop in parental dependent employer coverage, and increase in employer own coverage, and an overall increase in coverage (see 2004-2007 placebo period).

This story about differential trends in unemployment can be used to try to estimate the direction of the bias in the main estimates. In 2010-2011 the unemployment rate was at a local maximum, analogous to 2003-2004, albeit at a much greater scale (BLS). The column that compares 2001-2003 vs. 2003-2004 finds a statistically significant positive "implementation" effect on parental coverage, and a significant negative effect on own coverage, which are directionally consistent with the main results. Subtracting these placebo results from the main results gives smaller coefficients, and subtracting them scaled up by the relative sizes of the labor market downtown gives coefficients close to zero. A first-order inference from the placebo results would therefore be that the main 2008-2010 vs. 2010-2011 results are bias away from zero.[16]

[16] Results from placebo regressions on a triple difference specification, utilizing one's parents employer sponsored health insurance (ESI) status, are consistent with Table 3. See Online Appendix Table 1, corresponding to Table 5 in AMS. Here the bias is more difficult to untangle, since the placebo results could result from anywhere from one to four pairs of non-parallel trends (i.e., 19-25 with and without parental insurance, 16-18 & 27-29 with and without parental insurance, different ages with parental insurance, and different ages without parental insurance) and the affect group is even more endogenously defined (those 19-25 whose parents have ESI). These multiple factors could easily explain, for example, why in 2001-2003 vs. 2003-2004 the "implementation" coefficient on parental coverage has the same sign as the 2008-2010 vs. 2010-2011 whereas the "enactment" coefficient has the opposite sign.

One may be concerned this analysis is testing multiple hypotheses simultaneously and therefore should adjust the thresholds for statistical significance. However, even with a very conservative Bonferroni correction (as described in Savin 1984), all of the placebo results in Table 4 in the 2001-2007 period (where all hypothesis can be tested) previously statistically significant at the 1% level are still significant. I can therefore conclude that the strongly significant placebo results are not merely due to testing eight correlated hypotheses within each placebo period.[17]

One can use both the SIPP and the CPS to study the labor supply consequences of the federal mandate, since each survey asks respondents whether they and their household members are employed and if so how many hours they work. This analysis, in particular, of the labor market effects builds on the literature of "job lock" (Madrian 1994), which is when an employee who otherwise would quit a job does not because the employee would be unable to get the same level of benefits at another job (e.g. due to preexisting conditions or the new job being fewer than full-time hours). This provision of the ACA severs the link between employment and health insurance for young adults, and therefore it is intuitive that it would impact their labor supply decisions.

[Insert Table 5 Here]

Table 5 shows the results of such a placebo analysis using SIPP, corresponding to Table 7 in AMS. For each of the four labor supply variables studied here (employment, full-time employment, hours varying, and hours), there are significant results in placebo time periods. This is especially true in those that compare substantially different labor markets, such as 2002-2004 compared to 2004-2005. There are also significant results for full-time employment in 1994-1997

[17] Given this result under such a stringent adjustment, there is no need to apply the less conservative methods for multiple hypotheses that are detailed in Finkelstein et al. (2012) and Kling and Liebman (2004).

and 1995-1998. Even the overall probability of being employed, for which AMS's regression does not show an effect from the ACA, has a significant coefficient in 2001-2004.[18]

Alternative specifications and methods

Table 6 shows the SIPP results from Tables 3 using the Bertrand, Duflo, and Mullainathan (BDM) and Donald and Lang (DL) method, as described in Dinkelman and Ranchhod.[19]

[Insert Table 6 Here]

The coefficients in Table 5 are nearly identical to those in Table 3, which is to be expected given the design of this procedure. The main results for the actual enactment and implementation time period persist with most of their original statistical significance. The significant placebo results also remain for any insurance and employer own coverage, though they have largely disappeared for parental ESI and individual coverage. This suggests that while the BDM/DL approach reduces some placebo results, it does not eliminate them sufficiently for broad-age group regressions.[20]

[Insert Table 7 Here]

Table 7 shows the results of applying Abadie and Gardeazabal's synthetic control method. Compared to the regular difference-in-differences results in Table 4, these results have mostly comparable coefficients but more, not fewer significant placebo results. This is potentially due to

[18] Comparable placebo regressions on labor outcomes using the basic monthly CPS can be found in Online Appendix Table 2. The positive coefficients on hours worked in the 1990s could be due to the fact that during the economic expansion of the 1990s young adults would be more likely to take on more hours than those older (who were already working full time) and those younger (who were mostly still in school). The negative coefficient found during the ACA implementation period could also be the result of young adults' hours decreasing more than those older (who have more entrenched jobs) and those younger (who were already working relatively few hours).

[19] Since these methods ultimately do not solve the statistically significant placebo results problem, I do not also apply them or the one that follows to the Cantor and SK regressions using the March CPS. This is also due to the fact that these methods rely on more precise implementation timing than what the annual, backward looking CPS can reasonably accommodate.

[20] Online Appendix Table 3 shows the comparable adjusted labor results, corresponding to Table 4 above.

the fact that if that individuals aged 16-18 and 27-29 are not comparable comparisons to individuals aged 19-25, then no linear combination of them will make them significantly more comparable.[21]

Placebo Results Using Reduced Age-Bandwidth

One strategy for ameliorating the nonparallel trends in the comparison and affected groups is to reduce the bandwidth in the age dimensions, analogous to the strategy of Anderson, Dobkin, and Gross 2012 Eliminating 16-18 year olds from the analysis (perhaps due to concerns about changes in Medicaid eligibility), leaving 19-25 vs. 27-29, is not sufficient to eliminate placebo results (see Online Appendix Tables 4 and 5).

What follows is the same analysis as above, but now only comparing individuals aged 25 to those aged 27. Conceptually, while still a difference-in-differences approach, this has more of the intuition of a regression discontinuity design.[22]

Repeating the F-tests from above gives much smaller F-statistics, all with $p>0.1$.[23] These stats are all much smaller than those for the broad age groups (which are all $p<0.0001$) and so suggest far more comparable pre-treatment trends.

Tables 8 and 9 below parallel Tables 4 and 5 above, repeating the main difference-in-differences regressions.[24]

[21] As the labor supply data was used to calculate the weights for this approach, it will not also be used as a testable regression outcome, and so there is no comparable AG table for Table 5.

[22] Unfortunately, what I gain in robustness, I lose in external validity, as the results below are arguably inapplicable to those in the lower ages of the original affected group (e.g. 19-23).

[23] F= 0.90 (p=0.8407, employment, month level), 0.90 (p=0.6907, employment, quarter level), 0.77 (p=0.9909) / 0.7100 (p=0.9532) (full time), 1.00 (p=0.4798) / 1.02 (p=0.4368) (any health insurance), and 1.12 (p=0.1974) / 1.26 (p=0.1494) (dependent employer coverage through parents).

[24] The alternative specifications are not repeated here, since reducing to a two-by-two difference-in-differences regression with 4 observations has zero degrees of freedom (see Donald and Lang). The synthetic control group results are also not repeated as it is inapplicable with only one affected age and one comparison age.

[Insert Table 8 Here]

Table 8 is comparable to Table 4, showing the results of placebo regressions on health insurance outcomes using SIPP. This table's only robust result is that an increase in coverage through parental employer sponsored health insurance of 4.5 percentage point is significant at the 1% level without any correspondingly statistically significant placebo results. Given that this result has the most direct mechanism to the mandate in the ACA, it is intuitive that it would survive a reduced-bandwidth falsification test when the other results from the literature would not. On the other hand, the extensive margin (i.e. any source of coverage) implementation effect (3.5 percentage points) is only significant at the 10% level, and has an almost identical placebo result for 2005-2006 vs. 2003-2005.[25]

[Insert Table 9 Here]

Table 9 parallels Table5, showing placebo results for an age 25 vs. age 27 regression on labor outcomes using SIPP data. Here only a handful of coefficients are statistically significant, none at the 1% level, suggesting that estimates of the effect of the ACA on labor outcomes presented in the literature are not robust. This weak result is not due to reduced power from the diminished sample size – for example, the magnitude of implementation effect of working full time is only -0.0003.

Online Appendix Table 9 parallels Online Appendix Table 2 for CPS labor outcomes. Here, there are statistically significant results for the main 2008-2011 time period, without any comparable placebo results, but for different outcomes than those found in the literature (e.g. a -

[25] Online Appendix Tables 6 and 7 parallel Tables 2 and 3 for SK and Cantor's March CPS analysis, respectively, whereas Online Appendix Table 8 parallels Online Appendix Table 1 for DDD on SIPP Health Insurance. These results are consistent with those above in Table 8.

2.6 percentage point implementation effect (significant at the 1% level) on the probability of being employed). These results are more convincing than those of SIPP, since the CPS is designed precisely to calculate population employment estimates as opposed to track a variety of variables in a longer-panel structure. The fact that a narrow age bandwidth finds a strongly significant result that a broad bandwidth casts further doubt upon the validity of the literature's broad bandwidth approach.[26]

Supplemental Results Using Monte Carlo Analysis

One other way to test the validity of the main results in Table 4 against the statistically significant placebo results is to run a Monte Carlo analysis. Specifically, assume that the implementation effect coefficients in the placebo insurance regressions are drawn from a multivariate normal distribution, with a mean vector and covariance matrix that can estimated from the (albeit small sample) analogs. Then, with repeated draws from this distribution, one can calculate the probability of drawing a coefficient that is father from zero than the actual coefficient.

The parameters used are in Online Appendix Table 10. The result of 100,000 simulations is that the probabilities of randomly observing a coefficient father from zero than the measured magnitude are: any source of coverage (0.0113), parental dependent coverage (0.0007), individually purchased insurance in own name (0.1200), and coverage from one's own employer (0.1419).

This is entirely consistent with the reduced age bandwidth results in Table 8. The increase in dependent coverage is extremely robust, despite the significant placebo coefficients in Table 4. The increase in overall coverage (i.e. the extensive margin) is also likely robust, though with

[26] It is also doubtful that this drop in employment could be driving the net increase in coverage for 27 year olds shown in Table 8 (and shown to be robust by Monte Carlo Analysis below), as the expected sign would be reversed (i.e., overall coverage decreasing due to a drop in full time employment).

relatively less confidence. The other results could have occurred randomly at a greater than 10% level.

V. CONCLUSION

This paper shows that we should be cautious about using difference-in-differences methods to examine the health insurance and labor market effects of the Patient Protection and Affordable Care Act's provision that young adults must be able to stay on their parents' health insurance. Several recent papers argue that the ACA caused an increase in health insurance coverage, substitution from own to parental coverage, reduction in full-time employment and hours and an increase in the probability of having varying hours. By running placebo regression with both the SIPP and the CPS, this paper finds statistically significant results for all health insurance and labor outcome variables at various points in time predating the ACA. Several of these placebo results are significant at the 1% percent level, suggesting that they are not Type I errors. Furthermore, they occur both during placebo enactment and implementation phases, which preclude any attempt to decompose the results into these two periods.

Reducing the age bandwidth to only compare 25 year olds to 27 year olds does improve the robustness of this approach, though mostly by eliminating the statistical significance of the coefficients. The only result that appears to be extremely robust to placebo regressions, across specifications and data sources, is the one with the most direct mechanism: the increase in employer sponsored health insurance from one's parents.

However, this increase in parental insurance coverage must go somewhere, namely either to an increase in overall coverage or a decrease in other types of coverage. Looking at Table 8 again suggests that the majority of this increase (4.5 percentage points) went into the extensive margin (3.5 percentage points), even though these results are not statistically significant beyond the 10%

level. As mentioned above, however, there are also statistically insignificant extensive-margin placebo results of comparable magnitude (e.g. 3.5 in 2003-2006), casting doubt on the above assertion. Using the Monte Carlo analysis to "break the tie", we can be reasonably confident that the ACA's parental mandate did at least in part cause this overall increase in insurance coverage.

This analysis does call into question the robustness of the other second-degree results listed in the introduction. Some of the most recent papers (Barbaresco et al. 2015, Akosa Antwi et al. 2015, Depew and Bailey 2015), in part due to the influence of early versions of this paper, do use placebo tests or reduced age bandwidths (or both). Their conclusions of positive impacts on having a usual source of care, self-reported health, less delaying care due to cost, increased inpatient admissions, and premium increases are therefore likely robust. But other conclusions regarding out-of-pocket costs, use of emergency rooms (uninsured and overall) and increases in dental coverage come from papers without either of these robustness checks. This does not mean that the conclusions are necessarily spurious, but I remain less convinced than by the more recent comprehensively tested results.

Going forward, therefore, when studying the impacts of a public policy with a sharp age cutoff, it would be more prudent to use a narrower age bandwidth. If this is impossible due to sample size constraints, then a paper should check long-term placebo results, and if needed attempt to improve robustness by using the strategies of Bertrand, Duflo, and Mullainathan, Donald and Lang, and Abadie and Gardeazabal, as well as the Monte Carlo method described above.

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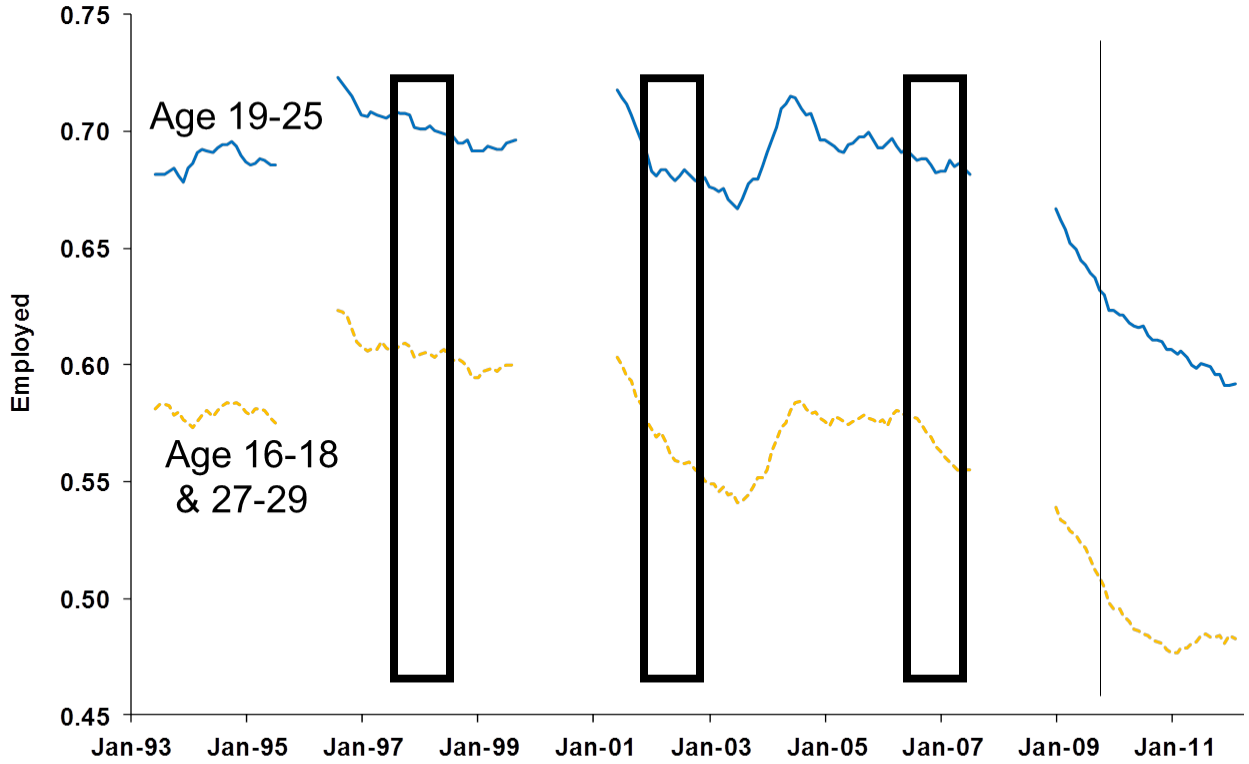
FIGURE 1

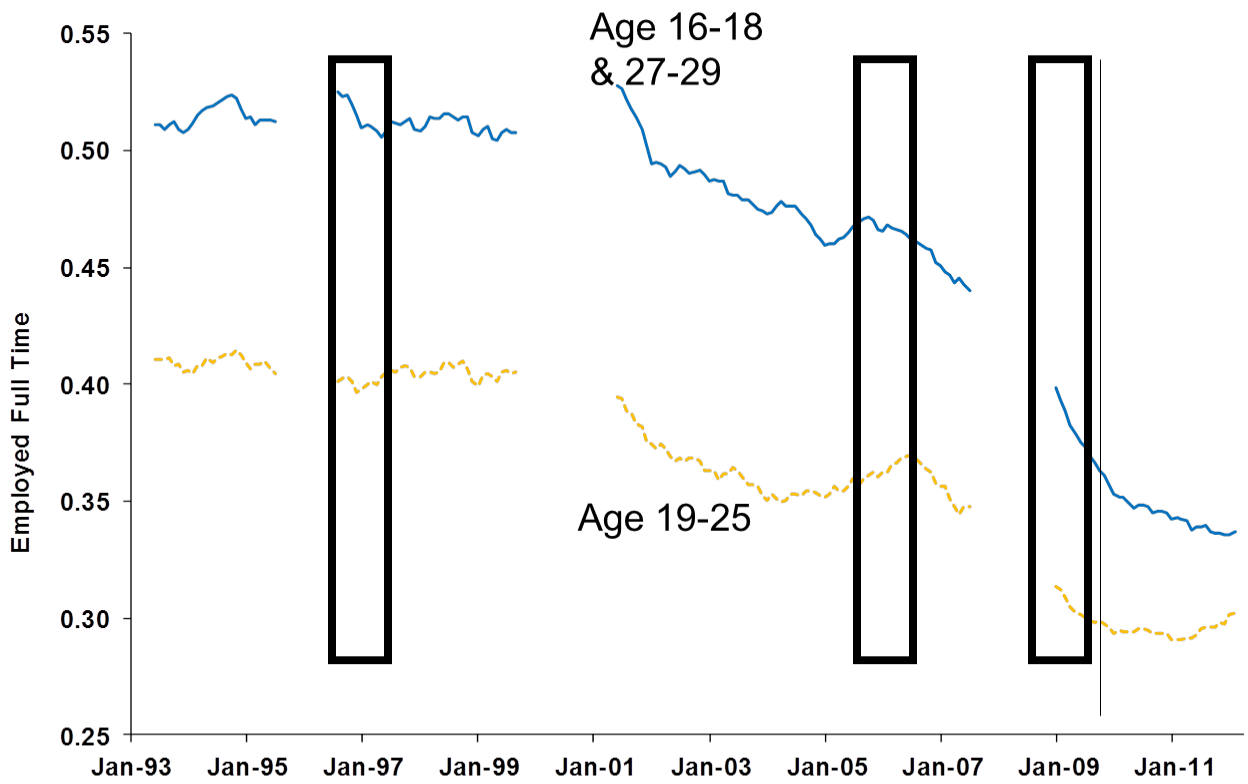
Share Employed, SIPP



Notes: Each point is the average of itself and 5 months before and after. Weighted. Each box is a sample "affected" placebo period where the trends are not parallel. The vertical line is data from the enactment period begins to be including in the moving average window (i.e., October 2009)

FIGURE 2

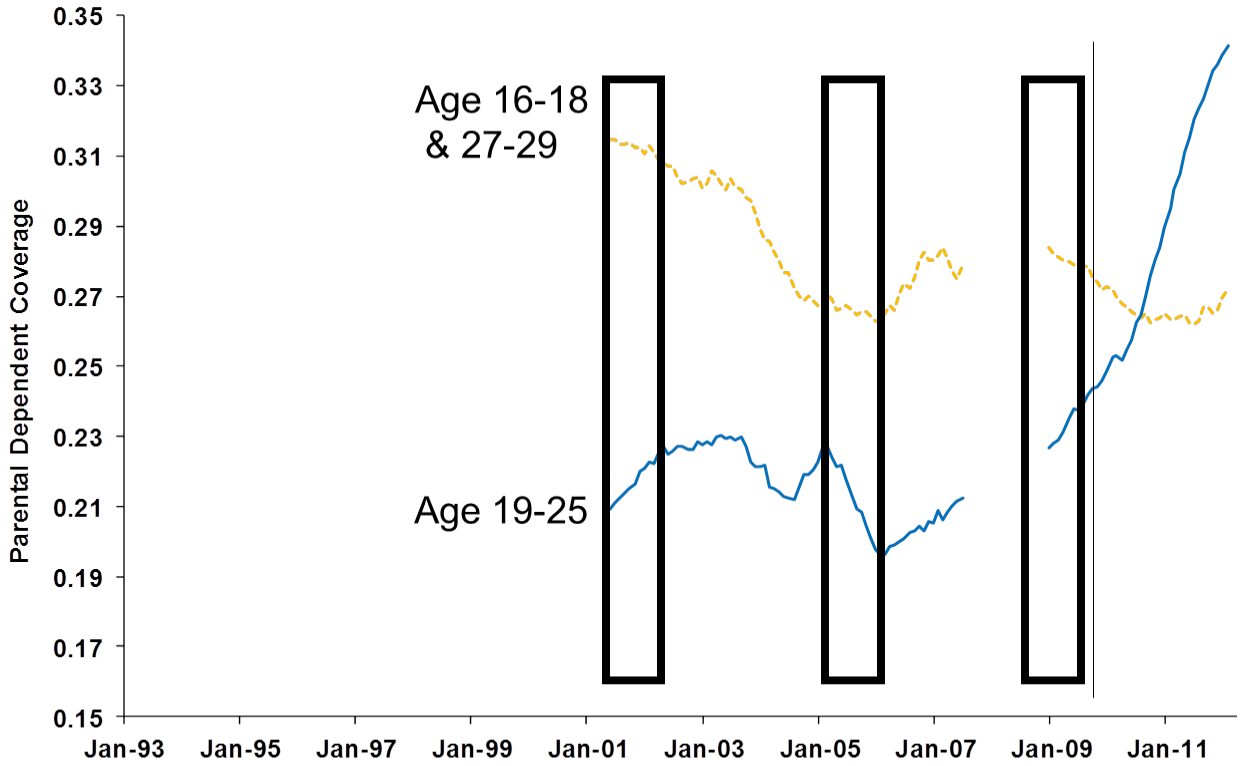
Share Working Full Time, SIPP



Notes: Each point is the average of itself and 5 months before and after. Full time= >30 hours/week. Weighted. Each box is a sample "affected" placebo period where the trends are not parallel. The vertical line is data from the enactment period begins to be including in the moving average window (i.e., October 2009)

FIGURE 3

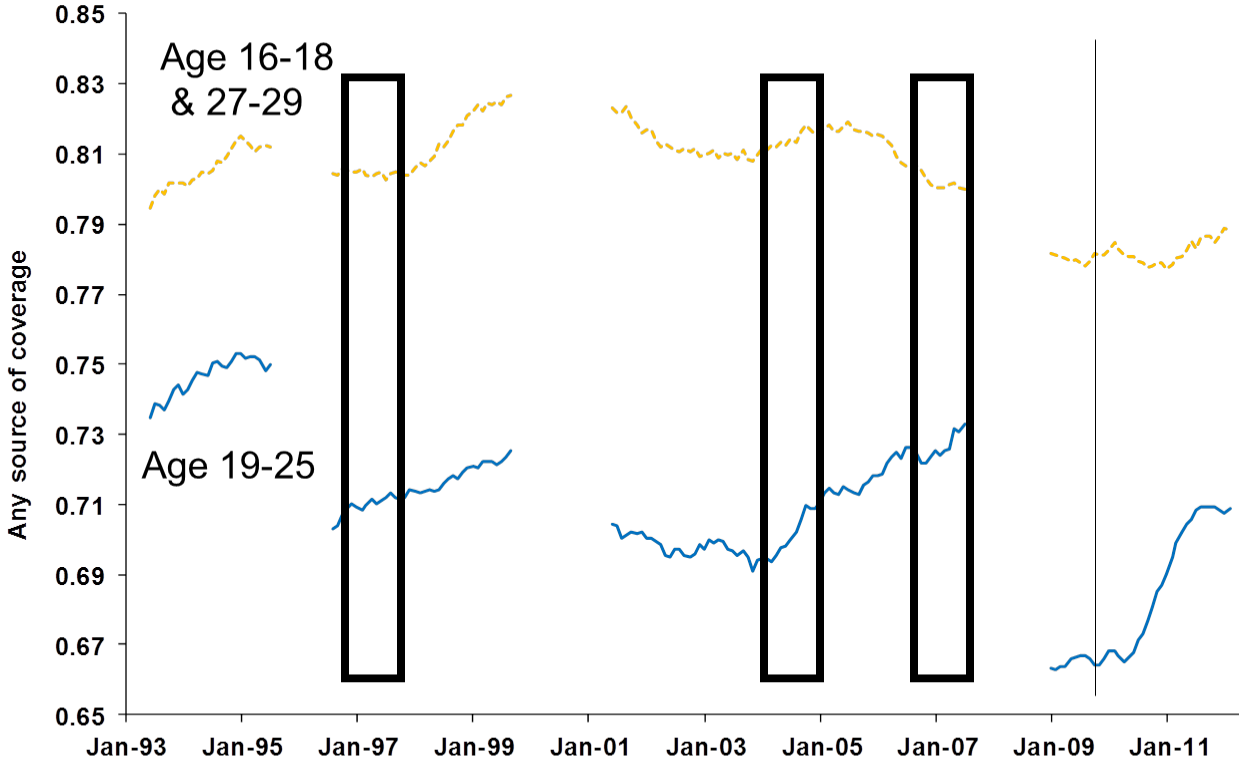
Parental Dependent Employer Sponsored Insurance, SIPP



Notes: Each point is the average of itself and 5 months before and after. Weighted. Each box is a sample "affected" placebo period where the trends are not parallel. The vertical line is data from the enactment period begins to be including in the moving average window (i.e., October 2009)

FIGURE 4

Health Insurance Coverage from Any Source SIPP



Notes: Each point is the average of itself and 5 months before and after. Weighted. Each box is a sample "affected" placebo period where the trends are not parallel. The vertical line is data from the enactment period begins to be including in the moving average window (i.e., October 2009)

TABLE 1

Summary Statistics for the March CPS and SIPP datasets

	March CPS	**SIPP**
Number of observations	197,103	296,805

Is female	0.49	0.50
Is black (non Hispanic)	0.14	0.13
Is Hispanic	0.21	0.13
Is Married	0.31	0.26
Has ESI in own name	0.38	0.30
Has dependent ESI	0.23	0.22
Has government HI	0.14	0.11
Has health insurance	0.68	0.71
Is employed	0.72	0.70

Notes: Weighted. Years 2001-2007 & 2009-2011; Ages 19-25 & 27-29

TABLE 2

Placebo Regression Results using CPS for Health Insurance Outcomes, Minimal Controls (SK)

Comparison period starts	1999	2000	2001	2002	2003	2004	SK Results 2005
Comparison period ends	2003	2004	2005	2006	2007	2008	2009
Affected year	2004	2005	2006	2007	2008	2009	2010
Dependent variable							
Any insurance	-0.004	-0.00002	0.01	0.004	0.011*	0.014**	0.03***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Medicaid	-0.009**	-0.004	-0.007*	0.008*	0.006	0.007	0.007
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Private coverage	0.001	0.008	0.014**	-0.003	0.006	0.01	0.028***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Employer provided (Dependent)	0.002	0.008	0.006	0.007	0.005	0.012**	0.043***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Employer provided (Own policy)	-0.001	0.004	0.017***	-0.006	-0.006	0.001	-0.024***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Directly purchased (Dependent)	-0.004*	0.00003	0.0001	0.0003	-0.0004	0.004*	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Directly purchased (Own Policy)	-0.005	-0.003	0.002	-0.004	0.004	-0.006**	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	244,491	256,998	253,073	249,789	247,558	247,663	247,370

Notes: Data: pooled from 2000-2012 CPS ASEC (i.e. March Supplement), covering reference years 1999-2011. The affected sample is those aged 19-25, whereas the comparison sample is those aged 26-34. Includes age and year fixed effects. Robust standard errors clustered at the household level. Weighted. SK's actual regression highlighted in grey.

Source: Author's calculations using CPS and adapted methodology from SK

*** Significant at the 1 percent level.** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 3

Placebo Regression Results using CPS for Health Insurance Outcomes, Maximal Controls (Cantor)

							Cantor Results
Comparison period starts	1999	2000	2001	2002	2003	2004	2005
Comparison period ends	2003	2004	2005	2006	2007	2008	2009
Affected year	2004	2005	2006	2007	2008	2009	2010
Dependent variable							
Private-non-spouse dependent coverage	-0.004 (0.005)	0.001 (0.005)	-0.011** (0.005)	-0.003 (0.005)	-0.003 (0.005)	0.003 (0.005)	0.052*** (0.006)
Private-self or spouse coverage	-0.013* (0.007)	-0.009 (0.007)	0.013* (0.007)	-0.002 (0.007)	0.008 (0.007)	0.001 (0.007)	-0.024*** (0.007)
Public	-0.004 (0.005)	-0.012** (0.005)	-0.006 (0.005)	0.01* (0.005)	0.005 (0.006)	0.007 (0.006)	0.006 (0.006)
None	0.014** (0.007)	0.014* (0.007)	0.0002 (0.007)	-0.002 (0.007)	-0.011 (0.007)	-0.008 (0.008)	-0.032*** (0.008)
N	131,349	137,895	135,764	134,505	133,930	134,435	134,009

Notes: Data: pooled from 2000-2012 CPS ASEC (i.e. March Supplement), covering reference years 1999-2011. The affected sample is those aged 19-23 who are not full-time students and all those aged 24-25, whereas the comparison sample is those aged 27-30. Hawaii and Massachusetts are excluded. Includes controls for state policies, age, sex, race, education, marital status, poverty ratio, student status, lives with parents, self-reported health, and state-level unemployment, share of workers in self-insured employer insurance and share of employers offering health insurance. Year and state fixed effects also included, as well as a common and state specific linear time trends. Weighted. Cantor et al.'s actual regression highlighted in grey.

Source: Author's calculations using CPS and adapted methodology from Cantor et al.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 4

Placebo Regression Results using SIPP for Health Insurance Outcomes (AMS)

										AMS results
Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Any source	Enactment effect	0.005	-0.032***	-0.007	-0.013*	0.001	0.0002	0.004	0.012	-0.002
		(0.008)	(0.009)	(0.006)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)
	Implementation effect	-0.023***	-0.024***	-0.01	-0.01	-0.004	0.006	0.013	0.023***	0.032***
		(0.008)	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
Employer dep. coverage (through parents)[a]	Enactment effect	No data	No data	No data	No data	-0.001	0.006	0.009	-0.026***	0.024***
						(0.009)	(0.008)	(0.008)	(0.006)	(0.006)
	Implementation effect	No data	No data	No data	No data	0.017***	0.022**	-0.02***	-0.03***	0.07***
						(0.006)	(0.009)	(0.007)	(0.007)	(0.007)
Individually purchased insurance in own name[a]	Enactment effect	No data	No data	No data	No data	0.004	-0.004	-0.001	0.007	0.002
						(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
	Implementation effect	No data	No data	No data	No data	-0.003	-0.008***	0.004	0.014**	-0.008***
						(0.003)	(0.003)	(0.004)	(0.006)	(0.002)
Employer own coverage[a]	Enactment effect	No data	No data	No data	No data	-0.006	-0.007	-0.021***	0.014	-0.017***
						(0.007)	(0.008)	(0.006)	(0.01)	(0.005)
	Implementation effect	No data	No data	No data	No data	-0.018**	-0.032***	-0.004	0.033***	-0.031***
						(0.007)	(0.008)	(0.008)	(0.009)	(0.006)
N		94,526	113,015	128,767	136,873	133,974	146,534	151,933	127,210	150,997

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 19-25, whereas the comparison sample is those aged 16-18 and 27-29. Includes controls for age, gender, race/ethnicity, marital status, student status, household income as a share of federal poverty line, monthly linear national and state-specific time trends, the monthly state unemployment rate, an interaction of the treatment dummy variable and the state unemployment rate, year and calendar month, and state fixed effects. Robust standard errors are clustered at the state level. Weighted. AMS's actual regression highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 5

Placebo Regression Results using SIPP for Labor Outcomes (AMS)

										AMS results
Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Probability of being employed	Enactment effect	-0.006	-0.004	0.007	0.0003	-0.0001	0.011	-0.01	-0.009	-0.002
		(0.008)	(0.008)	(0.006)	(0.009)	(0.009)	(0.008)	(0.011)	(0.007)	(0.006)
	Implementation effect	-0.0003	-0.001	0.003	0.0003	0.016**	-0.003	-0.006	0.012	-0.006
		(0.008)	(0.007)	(0.011)	(0.009)	(0.007)	(0.01)	(0.013)	(0.01)	(0.006)
Probability of working full time	Enactment effect	-0.017*	0.018**	-0.01	-0.002	-0.001	0.016*	-0.004	-0.008	-0.015***
		(0.009)	(0.008)	(0.006)	(0.007)	(0.009)	(0.008)	(0.008)	(0.009)	(0.006)
	Implementation effect	-0.007	-0.01	-0.016**	-0.015	-0.001	-0.015*	-0.019**	-0.012*	-0.022***
		(0.006)	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)	(0.009)	(0.006)	(0.007)
Probability of having hours that vary[a]	Enactment effect	No data	No data	No data	No data	0.0002	0.015***	0.008	-0.006	0.014***
						(0.0005)	(0.003)	(0.005)	(0.004)	(0.005)
	Implementation effect	No data	No data	No data	No data	0.015***	0.02***	0.003	-0.003	0.012**
						(0.002)	(0.003)	(0.004)	(0.005)	(0.006)
Hours worked	Enactment effect	-0.28	1.383***	0.052	-0.526	-0.147	1.227***	-0.305	-0.358	-0.474**
		(0.338)	(0.421)	(0.301)	(0.379)	(0.421)	(0.33)	(0.459)	(0.48)	(0.233)
	Implementation effect	0.953***	0.877**	-0.528	-1.151***	0.454	-0.166	-0.791*	-0.432	-0.807***
		(0.322)	(0.35)	(0.337)	(0.359)	(0.399)	(0.343)	(0.453)	(0.404)	(0.258)
N (Employed, Full Time, Hours Vary)		94,526	113,015	128,767	136,873	133,974	146,534	151,933	127,210	150,997
N (Hours, excludes w/ varied hours)		94,526	113,015	128,767	136,873	130,182	139,185	141,629	117,747	137,841

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 19-25, whereas the comparison sample is those aged 16-18 and 27-29. Includes controls for age, gender, race/ethnicity, marital status, student status, household income as a share of federal poverty line, monthly linear national and state-specific time trends, the monthly state unemployment rate, an interaction of the treatment dummy variable and the state unemployment rate, year and calendar month, and state fixed effects. Robust standard errors are clustered at the state level. Weigthed. AMS's actual regression highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 6

Placebo Regression Results using SIPP for Health Insurance Outcomes, Using BDM/DL (AMS)

										AMS period
Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Any source	Enactment effect	0.005	-0.032***	-0.007	-0.013*	0.002	0.001	0.004	0.012	-0.002
		(0.009)	(0.009)	(0.006)	(0.007)	(0.008)	(0.006)	(0.009)	(0.008)	(0.011)
	Implementation effect	-0.023***	-0.024***	-0.010*	-0.01	-0.004	0.006	0.013	0.023***	0.032***
		(0.007)	(0.008)	(0.005)	(0.007)	(0.006)	(0.006)	(0.01)	(0.007)	(0.009)
Employer dep. coverage (through parents)[a]	Enactment effect	No data	No data	No data	No data	-0.001	0.005	0.008	-0.025	0.024**
						(0.011)	(0.014)	(0.012)	(0.017)	(0.009)
	Implementation effect	No data	No data	No data	No data	0.016	0.021	-0.02	-0.029	0.071***
						(0.013)	(0.017)	(0.019)	(0.018)	(0.012)
Individually purchased insurance in own name[a]	Enactment effect	No data	No data	No data	No data	0.004	-0.004	-0.001	0.007	0.003
						(0.003)	(0.004)	(0.007)	(0.007)	(0.003)
	Implementation effect	No data	No data	No data	No data	-0.003	-0.008	0.005	0.013*	-0.008***
						(0.003)	(0.005)	(0.01)	(0.007)	(0.003)
Employer own coverage[a]	Enactment effect	No data	No data	No data	No data	-0.006	-0.006	-0.021*	0.013	-0.017*
						(0.006)	(0.009)	(0.012)	(0.013)	(0.01)
	Implementation effect	No data	No data	No data	No data	-0.018**	-0.031***	-0.004	0.033**	-0.032**
						(0.007)	(0.01)	(0.013)	(0.015)	(0.011)
N (collapsed)		39	39	39	39	39	39	39	39	39

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 19-25, whereas the comparison sample is those aged 16-18 and 27-29. First stage (not shown) includes controls for gender, race/ethnicity, marital status, student status, household income as a share of federal poverty line, monthly linear national and state-specific time trends, the monthly state unemployment rate, an interaction of the treatment dummy variable and the state unemployment rate, year and calendar month, and state fixed effects. Second stage difference-in-differences at age-time period level includes controls for age. Weighted. AMS's actual regression period highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 7

Placebo Regression Results using SIPP for Health Insurance Outcomes, Using AG Synthetic Controls (AMS)

										AMS period
Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Any source	Enactment effect	-0.003	-0.031***	0.003	-0.012*	0.002	-0.002	0.018***	0.026***	-0.001
		(0.008)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
	Implementation effect	-0.034***	-0.021***	-0.005	-0.003	-0.005	-0.002	0.027***	0.035***	0.040***
		(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Employer dep. coverage (through parents)[a]	Enactment effect	No data	No data	No data	No data	0.003	0.011	0.009	-0.012	0.038***
						(0.007)	(0.007)	(0.006)	(0.009)	(0.005)
	Implementation effect	No data	No data	No data	No data	0.021**	0.018***	-0.020***	-0.017**	0.077***
						(0.005)	(0.006)	(0.007)	(0.008)	(0.005)
Individually purchased insurance in own name[a]	Enactment effect	No data	No data	No data	No data	-0.001	-0.001	0.005*	0.008**	0.002
						(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
	Implementation effect	No data	No data	No data	No data	-0.001	-0.002	0.008***	0.008**	-0.006***
						(0.002)	(0.002)	(0.003)	(0.004)	(0.002)
Employer own coverage[a]	Enactment effect	No data	No data	No data	No data	-0.002	-0.027***	-0.008	0.018**	-0.016***
						(0.006)	(0.006)	(0.006)	(0.007)	(0.005)
	Implementation effect	No data	No data	No data	No data	-0.031***	-0.035***	0.009	0.046***	-0.021***
						(0.005)	(0.005)	(0.006)	(0.008)	(0.004)
N (collapsed)		532	532	532	532	532	532	532	532	532

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 19-25, whereas the synthetic comparison sample is weighted averages of ages 16-18 and 27-29 matching marital status, student status, household income as a share of federal poverty line, employment, unemployment, and full time employment rates. Includes controls for age, monthly linear national trends and year and calendar month dummies. Weighted. AMS's actual regression period highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level

TABLE 8

Placebo Regression Results using SIPP for Health Insurance Outcomes, Reduced Age Bandwidth (AMS)

										AMS period
Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Any source	Enactment effect	-0.008	0.013	-0.001	0.024	-0.006	0.005	0.009	0.051**	0.029
		(0.019)	(0.02)	(0.023)	(0.02)	(0.024)	(0.022)	(0.017)	(0.024)	(0.02)
	Implementation effect	-0.009	0.006	0.01	0.012	0.002	-0.001	0.036*	0.013	0.035*
		(0.018)	(0.022)	(0.023)	(0.021)	(0.017)	(0.022)	(0.022)	(0.027)	(0.02)
Employer dep. coverage (through parents)[a]	Enactment effect	No data	No data	No data	No data	-0.006	-0.001	0.007	0.001	0.013
						(0.008)	(0.005)	(0.008)	(0.009)	(0.01)
	Implementation effect	No data	No data	No data	No data	-0.01*	0.006	-0.003	0.004	0.045***
						(0.006)	(0.008)	(0.009)	(0.009)	(0.01)
Individually purchased insurance in own name[a]	Enactment effect	No data	No data	No data	No data	0.001	-0.005	0.005	0.001	0.003
						(0.007)	(0.007)	(0.006)	(0.008)	(0.006)
	Implementation effect	No data	No data	No data	No data	0.003	-0.005	0.007	-0.013	-0.001
						(0.005)	(0.006)	(0.009)	(0.014)	(0.007)
Employer own coverage[a]	Enactment effect	No data	No data	No data	No data	-0.018	0.001	0.016	0.004	0.018
						(0.023)	(0.019)	(0.016)	(0.026)	(0.016)
	Implementation effect	No data	No data	No data	No data	-0.019	0.004	0.023	0.032	-0.006
						(0.018)	(0.022)	(0.02)	(0.032)	(0.022)
N		14,735	17,687	19,980	20,550	19,080	21,146	21,762	18,022	21,616

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 25, whereas the comparison sample is those aged 27. Includes controls for age, gender, race/ethnicity, marital status, student status, household income as a share of federal poverty line, monthly linear national and state-specific time trends, the monthly state unemployment rate, an interaction of the treatment dummy variable and the state unemployment rate, year and calendar month, and state fixed effects. Robust standard errors are clustered at the state level. Weighted. AMS's actual regression period highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 9

Placebo Regression Results using SIPP for Labor Outcomes, Reduced Age Bandwidth (AMS)

Start comparison period (August)		1993	1994	1995	1996	2001	2002	2003	2004	AMS period 2008
Start enactment period (March)		1995	1996	1997	1998	2003	2004	2005	2006	2010
Start implementation period (October)		1995	1996	1997	1998	2003	2004	2005	2006	2010
End implementation period (November)		1996	1997	1998	1999	2004	2005	2006	2007	2011
Dependent variable										
Probability of being employed	Enactment effect	-0.015	0.02	-0.016	-0.019	-0.004	0.002	-0.014	-0.027	0.028
		(0.022)	(0.019)	(0.016)	(0.017)	(0.019)	(0.016)	(0.012)	(0.02)	(0.022)
	Implementation effect	-0.003	-0.004	-0.014	-0.005	0.008	-0.017	-0.036**	0.046	0.01
		(0.024)	(0.014)	(0.017)	(0.025)	(0.012)	(0.015)	(0.017)	(0.028)	(0.019)
Probability of working full time	Enactment effect	-0.043*	0.028	-0.026	-0.025	-0.016	-0.011	0.021	-0.042*	-0.002
		(0.023)	(0.023)	(0.018)	(0.02)	(0.025)	(0.019)	(0.018)	(0.022)	(0.025)
	Implementation effect	-0.016	-0.001	-0.014	-0.0005	-0.006	-0.025	-0.044*	0.022	-0.0003
		(0.03)	(0.015)	(0.02)	(0.032)	(0.02)	(0.019)	(0.025)	(0.03)	(0.018)
Probability of having hours that vary[a]	Enactment effect	No data	No data	No data	No data	0.0003	-0.006	-0.009	0.003	0.019
						(0.001)	(0.009)	(0.011)	(0.013)	(0.013)
	Implementation effect	No data	No data	No data	No data	0.004	0.006	0.008	-0.004	0.004
						(0.006)	(0.005)	(0.012)	(0.014)	(0.009)
Hours worked	Enactment effect	-1.502	0.532	-0.115	-1.928	0.369	-0.167	-0.061	-1.205	-0.447
		(0.982)	(0.907)	(0.832)	(1.218)	(0.974)	(1.01)	(0.936)	(1.233)	(1.015)
	Implementation effect	-0.385	0.797	-0.457	-1.231	0.553	-1.485*	-1.746	2.253	0.236
		(1.022)	(0.763)	(1.113)	(1.442)	(0.792)	(0.829)	(1.128)	(1.41)	(0.742)
N (Employed, Full Time, Hours Vary)		14,735	17,687	19,980	20,550	19,080	21,146	21,762	18,022	21,616
N (Hours, excludes w/ varied hours)		14,735	17,687	19,980	20,550	18,637	20,246	20,487	16,812	19,759

Notes: Data: pooled waves of the 1993, 1996, 2001, 2004, and 2008 SIPP panels, using only the 4th reference month observations. The affected sample is those aged 25, whereas the comparison sample is those aged 27. Includes controls for age, gender, race/ethnicity, marital status, student status, household income as a share of federal poverty line, monthly linear national and state-specific time trends, the monthly state unemployment rate, an interaction of the treatment dummy variable and the state unemployment rate, year and calendar month, and state fixed effects. Robust standard errors are clustered at the state level. Weighted. AMS's actual regression period highlighted in grey.

Source: Author's calculations using SIPP and adapted methodology from AMS.

[a] Only available from 2001 panel onwards.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.