Stat 156 Final Project

Replicating and Re-Analyzing the Causal Effect of Public Policies on Health Insurance Coverage Among Young Adults

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Abstract

Due to the complexity of the observed data in the field political science, it can be difficult to draw strong causal claims without the ability to recreate large-scale phenomena within controlled experiments. In this study, we apply various methods from Causal Inference to replicate and re-analyze a previous research publication that focuses on analyzing the causal effect of public policy. The paper we replicate and re-analyze is titled *"How Effective Are Public Policies to Increase Health Insurance Coverage among Young Adults?"* [1]. The research was conducted by Phillip B. Levine, Robin McKnight and Samantha Heep, and it was published in the American Economic Journal: Economic Policy in 2011. The abstract of that paper is outlined in the following paragraph. According to our replication and re-analysis, we consider the authors approach to be valid for the research question of interest, and to be consistent with the estimates suggested by methods that rely more heavily on outcome model specification.

This paper assesses the impact of policies to increase insurance coverage for young adults. The introduction of SCHIP in 1997 enabled low-income teens up to age 19 to gain access to public health insurance. More recent policies enabled young adults between the ages of 19 and (typically) 24 to remain covered under their parents' health insurance. We use the discrete break in coverage at age 19 to evaluate the impact of SCHIP, and quasi-experimental variation to evaluate the impact of "extended parental coverage" laws. Our results suggest that both types of policies were effective at increasing health insurance coverage.

1 Paper Summary and Summary Statistics

1.1 Introduction

For the 25 years prior to this paper being published, public policy in the United States strove to increase the rate of health insurance coverage among children and youth. To analyze the impact of public policy on increasing insurance coverage, the original paper focuses on the effects of the State Children's Health Insurance Program (SCHIP) and on the effects of newly implemented extended parental coverage laws on coverage rates among older teenagers. At the age of 19, children generally lose their SCHIP coverage, but some proposals have attempted to increase the age limit to somewhere in the range of 24 to 30 years old (varies by state). Therefore, it is important to analyze the impact of these insurance policies to understand the effectiveness of their intended goal, which is to increase insurance coverage amongst young adults.

1.2 Research Question

The paper addresses the question "What is the impact of insurance policies to increase insurance coverage in young adults?". Specifically, original research assesses the impact of the State Children's Health Insurance

Program (SCHIP) and extended parental coverage laws on the insurance coverage of young adults whose age ranges between 19 and 24. At the time of publication, no past research had focused on the coverage of young adults. Extended parental coverage was also new enough at the time that no prior research had been conducted.

1.3 Research Answer

The paper finds that SCHIP increased insurance coverage by 3 percentage points for older teens as a whole. Those under 150% and those between 150% and 300% of the poverty line experienced 7% and 4% increases, respectively. These estimates reflect roughly a 20% reduction in rates of uninsurance among these two groups. The paper also suggest that there is weaker evidence of crowd-out than in the existing SCHIP literature (crowd out is an economic theory arguing that rising public sector spending drives down or even eliminates private sector spending).

The paper also finds that extended parental coverage laws are effective at increasing rates of insurance coverage. Laws implemented at the state level lead to a 3% point reduction in rates of uninsurance among those eligible, whereas a federal law lead to a 7% reduction. The difference is attributable to a feature of ERISA (Employee Retirement Income Security Act of 1974), which exempts firms that self-insure from state insurance regulation. The research shows that the impact of extended parental coverage laws is at least partially attributable to the increased propensity of young adults to live with their parents when these laws are passed. The results also provide some evidence of reverse crowd out through increases in private insurance coverage.

In conclusion, the estimates in the paper suggest that both policies have been successful in increasing insurance coverage for older teens and young adults.

1.4 Data Cleaning

The paper uses data from the March Current Population Survey (CPS), spanning the years 1992 to 2009. The CPS is a monthly survey of about 50,000 households (although the sample size varies somewhat over time) conducted by the Bureau of Labor Statistics. Each month, information about employment status and demographic characteristics is obtained from individuals aged 16 and older. The March supplement to the CPS asks additional questions, including whether individuals had private health insurance, public health insurance, or were uninsured (in more recent years) in the preceding calendar year.

We note that the data we used in this section comes directly from the paper source. We attempted to download the raw data from multiple sources, including the National Bureau of Ecornomic Research (NBER) and the Integrated Public Use Microdata Series (IPUMS), but we came across issues regarding accessing the appropriate variables necessary to replicate the results of the paper. Thus, because we deferred to using the data directly from the original paper, there was minimal data cleaning that needed to be done.

Before SCHIP	Any Insurance	Public	Private
Full sample	0.76	0.13	0.66
With parents	0.82	0.12	0.73
With parents Poverty < 150	0.64	0.38	0.30
With parents, 150 < Poverty < 300	0.81	0.08	0.76
With parents, Poverty > 300	0.92	0.02	0.91
With parents and group insurance	0.92	0.04	0.91
With parents and group insurance	0.58	0.29	0.32

Insurance Coverage Before SCHIP

After SCHIP	Any Insurance	Public	Private
Full sample	0.77	0.14	0.66
With parents	0.84	0.14	0.73
With parents, Poverty < 150	0.69	0.42	0.31
With parents, 150 < Poverty < 300	0.80	0.14	0.70
With parents, Poverty > 300	0.92	0.04	0.91
With parents and group insurance	0.93	0.06	0.90
With parents and group insurance	0.59	0.35	0.28

Insurance Coverage After SCHIP

With Parent	Any Insurance	Public	Private
1991-1993	0.81	0.11	0.73
1994-1996	0.82	0.13	0.73
1998-1999	0.81	0.11	0.73
2000-2002	0.84	0.12	0.75
2003-2005	0.84	0.15	0.73
2006-2008	0.84	0.16	0.71

Insurance Coverage for sample living with parents

Table 1: Table of Insured Proportion for SCHIP Data, uses data of 16-22 year old young adults

1.5 Summary Statistics and Descriptive Analysis

Given that most of the data is categorical, Table and Table 2 summarizes the dataset by reporting the proportions of individuals with any insurance coverage, public insurance coverage, or private insurance of the full data ($n = 291,118$), or within various subgroups. The included subgroups are sub-samples of the full sample that are subsequently analyzed in the discussion of the effect of the SCHIP eligibility policy and extended parental coverage laws.

Before Parental Coverage	Any Insurance	Public	Private
Full sample	0.69	0.09	0.62
Non student	0.61	0.12	0.51
With parents	0.69	0.09	0.62
With parents and Poverty < 150	0.55	0.21	0.36
With parents and 150 < Poverty < 300	0.66	0.08	0.61
With parents and Poverty > 300	0.83	0.03	0.82
With parents and parents have group insurance, firm size < 100	0.81	0.05	0.78
With parents and parents have group insurance, firm size > 100	0.85	0.04	0.83
With parents and parent have not group insurance	0.58	0.13	0.46

Before Parental Coverage

	Any Insurance	Public	Private
Full sample	0.69	0.11	0.60
Non student	0.60	0.14	0.48
With parents	0.69	0.11	0.60
With parents and Poverty < 150	0.55	0.23	0.35
With parents and 150 < Poverty < 300	0.65	0.09	0.58
With parents and Poverty > 300	0.84	0.04	0.81
With parents and parents have group insurance, firm size < 100	0.81	0.07	0.78
With parents and parents have group insurance, firm size > 100	0.86	0.05	0.83
With parents and parent have not group insurance	0.57	0.16	0.44

After Parental Coverage

Table 2: Proportion insured before and after the Parental Coverage

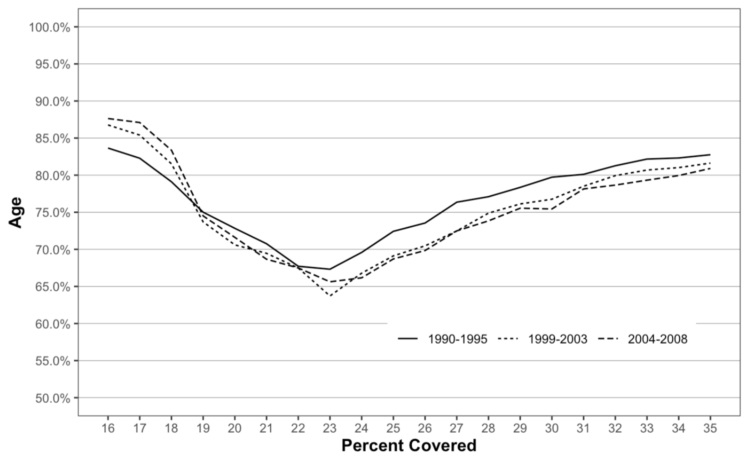

Figure 1: Percent Covered by Insurance

We also replicate plots (Figures 1 - 3) shown in the original paper that show the age pattern of insurance coverage in three different time periods: 1990-1995 (before SCHIP), 1999-2003 (immediately after SCHIP), and 2004-2007 (several years after SCHIP). At most ages, the profile of insurance coverage falls relatively uniformly over time. The exception to this pattern is an increase in insurance coverage for individuals under 19 years of age in the post-SCHIP periods relative to the pre-SCHIP time period. This suggests that the introduction of SCHIP generated a substantive increase in insurance coverage for older teenagers

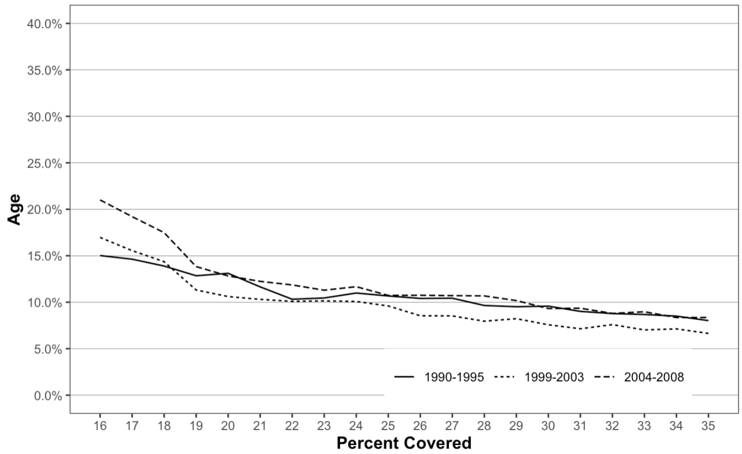

Figure 2: Percent Covered by Public Insurance

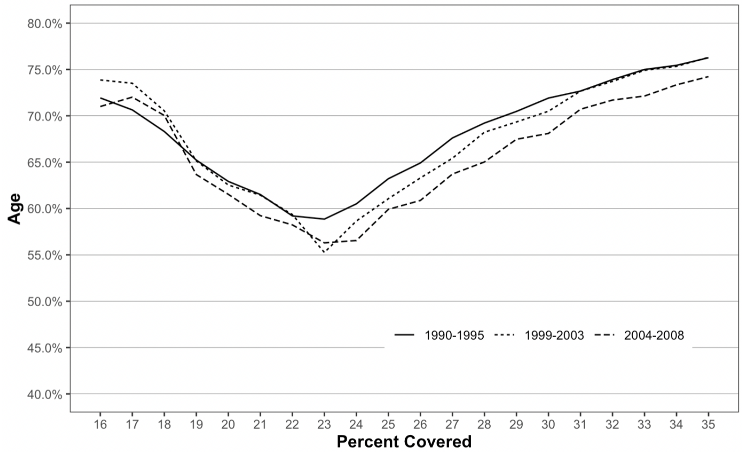

Figure 3: Percent Covered by Private Insurance

2 Replicate the Main Results

2.1 SCHIP Analysis

Between 1997 and 1999, 36 states, including almost all of the large states, increased the age limit of SCHIP eligibility to 19 years old, so the authors conduct a simplified analysis to focus on the difference in insurance coverage between and after 1998 for those who are over and under 19 years old. To address the effect of this change in eligibility, the paper attempts to answer the question "Did an increase in insurance coverage occur beginning in 1999 for those who are under age 19 relative to those who are 19 or older?"

Below is the mathematical formulation of the regression problem.

$$Insurance_{iast} = \alpha + \beta Below19_a \cdot Post1998_t + \delta_a + \tau_t + \pi_s + \rho Unemp_{st} + X_{ist}\theta + \epsilon_{iast}$$

where $Insurance_{iast}$ is a measure of insurance status for individual i in age group a, state s and year t, and the independent variable of interest is an indicator for being under the age of 19 after 1998.

Because this is an observational study in which the treatment variable is the policy change in age (measured by β), the regression had to control for a multitude of covariates. The section below is a direct quote from the paper, explaining the factors that were controlled for in the analysis.

The regression includes a full set of age, year, and state fixed effects. In addition, we control for differences in economic conditions across states, using the state- and year-specific unemployment rate. We also control for individual-level covariates, including gender, marital status, student status, an indicator for residence with a parent, household income as a share of the poverty line, and the square of household income as a share of the poverty line. We use OLS to estimate the regressions and cluster our standard errors at the age level. We limit the sample to ages 16 *to* 22*, so we are including only three years above and below the age cutoff. The implicit identifying assumption of our regression is that, in the absence of SCHIP, there would not have been differential trends in insurance coverages at ages around the age 19 eligibility limit.*

To replicate the results, we re-computed the percentage statistics and regression estimates, and compared the results of the paper (Table 3), to the replicated results (Table). In both of the tables, each cell in columns 2-4 presents the results from separate difference-in-difference regressions models. Control variables include state, year, and age fixed effects, the state- and year-specific unemployment rate, and individual-level covariates. Standard errors are clustered on age. The full sample includes all observations between the ages of 16 and 22, using the 1992- 2009 CPS. In order to compare the results to the original paper, we include one more significant figure in the replicated results in order to give an assessment of precision.

The values in columns $2-4$ are the estimates for the regression coefficient on an indicator for being under the age of 19 after 1998 (mathematically outlined in the equation outlined above). They represent the percentage increase in insurance coverage amongst the specified group.

Across almost all samples, we observe a statistically significant causal effect in overall insurance coverage and public insurance coverage. Within the full sample, we notice a 3% increase in overall insurance coverage, and a 4.5% in public insurance coverage.

We also report several negative coefficients in the rate of private insurance coverage across samples. Given that none of the estimates effects on private insurance coverage are statistically significant and since the estimates are all relatively close to 0, we cannot make any strong claims about the impact of SCHIP on private insurance coverage.

		Estimation results for dependent variable		
	Percent covered by any insurance	Any insurance coverage	Public insurance coverage	Private insurance coverage
Full Sample N = 291,118	76.1	0.032** (0.005)	0.045** (0.005)	-0.004 (0.004)
Sample living with parents N = 218,818	81.8	0.029** (0.010)	0.037** (0.005)	0.001 (0.008)
By Income				
<150 percent of poverty line N=41,168	63.9	0.067** (0.016)	0.084** (0.016)	-0.004 (0.006)
150 - 300 percent of poverty line N=77,562	80.6	0.043** (0.019)	0.058** (0.004)	0.001 (0.018)
> 300 percent of poverty line N=100,088	91.9	0.002 (0.005)	0.005** (0.001)	-0.002 (0.006)

Table 3: Paper Results: The Effect of SCHIP on Insurance Coverage
* significant at 0.1 level
** significant at 0.05 level

		Estimation results for dependent variable		
	Percent covered by any insurance	Any insurance coverage	Public insurance coverage	Private insurance coverage
Full Sample N = 291,118	77.3	0.0316** (0.0030)	0.0447** (0.0025)	-0.0044 (0.0032)
Sample living with parents N = 218,818	82.99	0.0289** (0.0034)	0.0365** (0.0028)	0.0006 (0.0036)
By Income				
<150 percent of poverty line N=41,168	66.85	0.0665** (0.0102)	0.0842** (0.0098)	-0.0037 (0.0093)
150 - 300 percent of poverty line N=77,562	79.77	0.0432** (0.0062)	0.0583** (0.0045)	0.0014 (0.0066)
> 300 percent of poverty line N=100,088	92.12	0.0015 (0.0037)	0.0049** (0.0023)	-0.0022 (0.0038)

Table 4: Replicated Results: The Effect of SCHIP on Insurance Coverage
* significant at 0.1 level
** significant at 0.05 level

Ignoring precision errors, all of the regression coefficients in our replication are consistent with the initial paper, meaning that their main results are reproducible. However, there are some slight inconsistencies in the reported percentage of individuals covered by insurance within each sample. Additionally, the original estimates for the standard error are generally higher than the estimates we found in our analysis. This may be due to some inconsistencies in the method used to compute the clustered standard error (especially since the original analysis was conducted in STATA, whereas our analysis was conducted in R). These results may suggest some crowd out, as SCHIP eligibility is estimated to have increased public insurance coverage and decreased private insurance coverage, but we chose to ignore rigorous crowd-out analysis as we found the data to be insufficient to challenge the crowd-out claims made in the paper.

The regression in original paper controls for a few important covariates, such as gender, marital status, student status, parent status, and geography. The paper importantly focuses on the age 19, the new age limit for eligibility, but it limits the analysis to observations between the ages of 16 and 22. The original hypothesis is that in the absence of SCHIP, there likely would not have been a substantial difference in the insurance coverage trends for individuals further than 3 years away from that cutoff. While this may make sense for the age of 16 (as the paper is only focused on young adults), the paper provides little reasoning for a 3 year interval in the positive direction. Because there were proposals that considered extending the coverage age to at least 24, the analysis may have been more robust if the age range was extended to at least $16-24$, if not even more.

2.2 Extended Parental Coverage Analysis

To analyze the effect of extended parental coverage laws, the original research reports the results from performing ordinary least squares regression for subgroups that may or may not have been affected by the parental coverage law of the state they are living in. The main treatment is the implementation of the parental coverage law in a certain state. The implementation of the extended parental coverage law could be viewed as a quasi-experiment, or in other words, a natural experiment. The regression is formulated as follows:

$$Insurance_{iast} = \alpha + \beta Law_{st} + \delta_a + \tau_t + \pi_s + \rho Unemp_{st} + X_{iast} + \varepsilon_{iast}$$

where $Insurance_{iast}$ is the insurance status for individual i in age group a, state s and year t. Law_{st}, the treatment, is whether the individual lives in a state where the extended parental coverage law had been in place for over a year. $Unemp_{st}$ is the unemployment rate of state s. Other covariates are denoted by X_{iast}, which includes the individual i's gender, marital status, student status, residence with a parent, household income as a share of the poverty line, and the square of household income as a share of the poverty line.

In addition to the approach above, the paper also reports the results of triple difference regression as an additional test, in which ineligible young adults are the third difference. This formulation implies that the causal estimates are a result of regressing the interaction term between eligibility and treatment and the interaction term between eligibility and covariates on insurance coverage rates.

The original authors formulate two key assumptions to perform extended parental coverage analysis:

1. The age reported in the March CPS corresponds to the individual's age in the prior year.
2. The timing of any differential changes in coverage is unrelated to the introduction of extended parental coverage laws.

Given that the authors have verified that the probable age difference cause by assumption 1 wouldn't affect the result, we can safely make the first assumption. It is also unlikely that violation of assumption 2 would occur. More specifically, it is unlikely that unmarried young adults happened to receive greater health insurance coverage than married young adults in the states that implemented extended parental coverage laws, in the years following the implementation. Additionally, because the treatment assignment differs by state, the standard errors of the regressions are all clustered by state. In order to compare the extended parental coverage results results to the original paper, we again include one more significant figure in the replicated results in order to give an assessment of precision.

	Estimation results for dependent variable		
	Any insurance coverage	Public insurance coverage	Private insurance coverage
Panel A. Full Sample			
DiD Full Sample N = 127,106	-4.5e-4 (6.5e-3)	-1.28e-2** (6.21e-3)	1e-2 (7.42e-3)
Eligible Sample N = 100,218	8.36e-3 (6.32e-3)	-1.41e-2** (6.15e-3)	2.17e-2** (6.82e-3)
Triple Difference Full Sample N = 127,106	-2.24e-2* (1.34e-2)	8.86e-3 (1.17e-2)	-2.27e-2* (1.24e-2)
Panel B. Non-students			
DiD Full Sample N=77,861	1.19e-3 (8.37e-3)	-7.96e-3 (8.8e-3)	6.26e-3 (1.03e-2)
Eligible Sample N=54,417	1.79e-2** (8.43e-3)	-5.65e-3 (8.93e-3)	2.62e-2** (1.08e-2)
Triple Difference Full Sample N=77,861	3.21e-2** (1.45e-2)	-1.08e-2 (1.18e-2)	-2.66e-2* (1.39e-2)

Table 5: Results of the Regression on Parental Coverage full sample
* significant at 0.1 level
** significant at 0.05 level

The main results are presented in Table 5 and Table 6. Table 5 presents results for the student/nonstudent subgroup, while Table 6 reports results for observations living with parents. Ignoring precision errors, we again find that all of the regression coefficients in our replication are consistent with the initial paper, which further supports that the results in the original paper are reproducible, but this is relatively unsurprising given that the data and the variable of choice are exactly equivalent.

In both the full sample and the sub-sample of individuals living with parents, the extended parental coverage causes decrease in public insurance coverage for eligible observations, while it causes increases private insurance coverage. The phenomenon is most prevalent in population between 150-300 percent of the poverty line, and is is also prevalent for families with parents working in firms of smaller (< 100) firm size. Consequently, reverse-crowd out effect could be said to exist in these subgroups since private insurance coverage is increasing, but this may be partially offset by the decrease in public insurance coverage. Regardless, we again chose to ignore rigorous crowd-out analysis as we found the data to be insufficient to challenge the crowd-out claims made in the original paper.

	Estimation results for dependent variable		
	Any insurance coverage	Public insurance coverage	Private insurance coverage
All Living with Parents (N=60,550)	1.55e-2** (7.02e-3)	-1.31e-2** (6.51e-3)	2.91e-2** (7.28e-3)
< 150 percent of poverty line N = 8,624	-2.39e-3 (2.81e-2)	-2.03e-2 (2.87e-2)	1.28e-3 (2.3e-3)
150 - 300 percent of poverty line N = 19,847	2.65e-2** (1.22e-2)	-1.35e-2 (1.05e-2)	5.11e-2** (1.42e-2)
>300 percent of poverty line N = 32,079	7.44e-3 (8.78e-3)	-5.44e-3 (5.85e-3)	1.07e-2 (9.74e-3)
Parents' Insurance			
Group Coverage N=127,106	-5.08e-4 (6.53e-3)	-1.34e-2** (6.19e-3)	1.04e-2 (7.42e-3)
And Firm Size < 100 N=10,264	3.64e-2* (2.04e-2)	-3.17e-2** (1.03e-2)	5.52e-2** (1.99e-2)
And Firm Size > 100 N=33,957	3.99e-3 (7.91e-3)	-8.51e-3 (5.21e-3)	1.19e-2* (6.8e-3)
No Group Coverage (N=16,329)	8.81e-3 (1.96e-2)	-6.27e-4 (1.8e-2)	1.84e-2 (1.11e-2)

Table 6: Results of the Regression on Parental Coverage for the population living with parents
* significant at 0.1 level
** significant at 0.05 level

3 Replicating Robustness Checks and Extension

3.1 Extensions to SCHIP Analysis

As an extension to the SCHIP analysis, the paper examines the time pattern of the change in insurance coverage patterns for individuals under the age of 19, to ensure that it corresponds to the timing of SCHIP implementation. More concretely, they measure how the causal effect of SCHIP changes over time for the full sample. The paper does this by conducting two different extensions:

1. Estimating regression coefficients for specific time intervals
2. Estimating regression coefficient estimates for each year, and plot them over time.

Dynamic Specification of the effect of SCHIP on Insurance Coverage

To analyze the first extension (periodized causal coefficients), we re-computed the percentage statistics and regression estimates, and compared the results of the paper (Table 7), to the replicated results (Table 8). In order to compare the precision of the original results, we include an extra significant figure for each of the coefficient and standard error estimates. The notes for the table, from the original paper, are written below:

Each column presents the results from separate difference-in-difference regressions models. The omitted period is 1997. Control variables include state, year, and age fixed effects, the state- and year-specific unemployment rate, and individual-level covariates. Standard errors are clustered on state. The sample includes all observations between the ages of 16 *and* 22*, using the 1992-2009 CPS.* $N = 291,118$.

The paper specifically describes that the coefficient estimates in Table 7 are a result of separate difference-in-difference models. However, we were able to recover the same coefficient estimates (Table 8) by running one regression with all of the dummy variables for each time period, with an interaction for the indicator of being under 19 years old. The formula that we used in our regression is defined below:

$$\begin{aligned} Insurance_{iast} \sim\ & i.stfips + i.year + i.a\_age + ur + povratio + povratio2 + withparent + married \\ & + student + female + under19 + elig\_9193 * under19 + elig\_9496 * under19 \\ & + elig\_9899 * under19 + elig\_0002 * under19 + elig\_0305 * under19 + elig\_0608 * under19 \end{aligned}$$

where the insurance response variable the same as prior regressions, and takes the form of "Any insurance coverage", "Public insurance coverage", or "Private insurance coverage" (3 separate regressions), and the coefficients on interaction terms "$elig\_xxxx * under19$" are the estimates being reported.

	Estimation results for dependent variable		
-	Any insurance coverage	Public insurance coverage	Private insurance coverage
Under age 19			
* Years 1991 - 1993	-0.005 (0.009)	0.001 (0.006)	-0.005 (0.007)
* Years 1994 - 1996	-0.007** (0.002)	-0.004 (0.006)	-0.004 (0.005)
* Years 1998 - 1999	0.006 (0.004)	0.016** (0.004)	-0.006 (0.004)
* Years 2000 - 2002	0.018** (0.003)	0.036** (0.004)	-0.009 (0.005)
* Years 2003 - 2005	0.031** (0.005)	0.047** (0.007)	-0.007 (0.006)
* Years 2006 - 2008	0.038** (0.001)	0.058** (0.005)	-0.009 (0.005)

Table 7: Paper Results: The Effect of SCHIP on Insurance Coverage - Dynamic Specification
* significant at 0.1 level
** significant at 0.05 level

The formula above is derived by taking the original SCHIP regression formula and breaking up the eligibility treatment interaction into different interactions for different time frames. The formula is never explicitly defined in the paper, but based upon the described regressions, existing variables in the cleaned dataset, and the coefficient estimates, we believe this is an appropriate method for computing the time-dependent causal estimates.

	Estimation results for dependent variable		
-	Any insurance coverage	Public insurance coverage	Private insurance coverage
Under age 19			
* Years 1991 - 1993	-0.0052** (0.0083)	0.0007 (0.0065)	-0.0054 (0.0086)
* Years 1994 - 1996	-0.0075** (0.0084)	-0.0036** (0.0067)	-0.0037 (0.0087)
* Years 1998 - 1999	0.0057 (0.0089)	0.0156** (0.0069)	-0.0063 (0.0092)
* Years 2000 - 2002	0.0181** (0.0081)	0.0357** (0.0063)	-0.0086 (0.0083)
* Years 2003 - 2005	0.0310** (0.0080)	0.0467** (0.0063)	-0.0075 (0.0083)
* Years 2006 - 2008	0.0360** (0.0080)	0.0582** (0.0064)	-0.0087 (0.0083)

Table 8: Replicated Results: The Effect of SCHIP on Insurance Coverage - Dynamic Specification
* significant at 0.1 level
** significant at 0.05 level

The replicated regression results in Table 8 are fairly consistent with the original results of the paper (Table 7). Because our replication method may differ from the method used in the original paper, we have evidence supporting the robustness of the original causal estimates in these time periods. However, one point of concern is the statistical significance of these estimates. Neither the original nor the replicated results yielded any significant coefficients for the causal effect of SCHIP on private insurance coverage. Yet, for any insurance coverage or public insurance coverage, the replicated results output different levels of significance, despite the similarity in the results.

Secondly, the standard errors (clustered by state FIPS codes) in the replicated results differed from the standard errors in the original results. However, because we observed a similar phenomena in the main SCHIP results, when using the exact same regression formulation as the original paper, we may question the computational methods used for computing these estimates. There are several existing methods for computing clustered standard errors, so this may be due to computational differences outside of our control. For reference, we computed standard error estimates in R using the 'coeftest' and 'vcovCL' functions from the 'lmtest' and 'multiwayvcov' packages, respectively.

Impact of SCHIP on Insurance Coverage, by year

The paper also uses the same framework to compute causal effect estimates for each individual year from 1991 to 2008. Therefore, our replication uses the regression formulation as the previous extension, except we use interaction terms for eligibility in each individual year, as opposed to a range (again, there is no eligibility estimate for 1997 in this case either since this is the year that the change in coverage was introduced). The paper does not provide the exact coefficient estimates from this regression, but they do display a plot (Figure 4) of the estimate causal effects over time, which we compare to our replicated plot (Figure 5):
Employing the outlined regression strategy allows us to replicate the plot almost exactly:

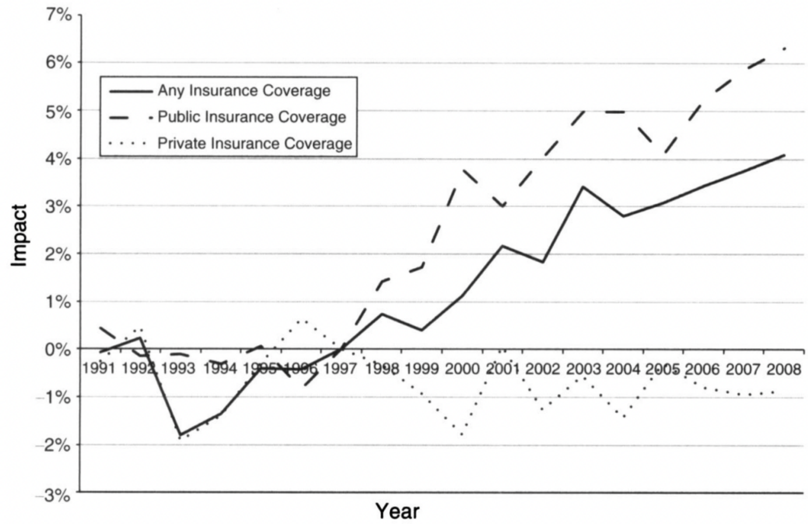

Figure 4: Paper Results: Impact of SCHIP on Health Insurance Coverage, by Year

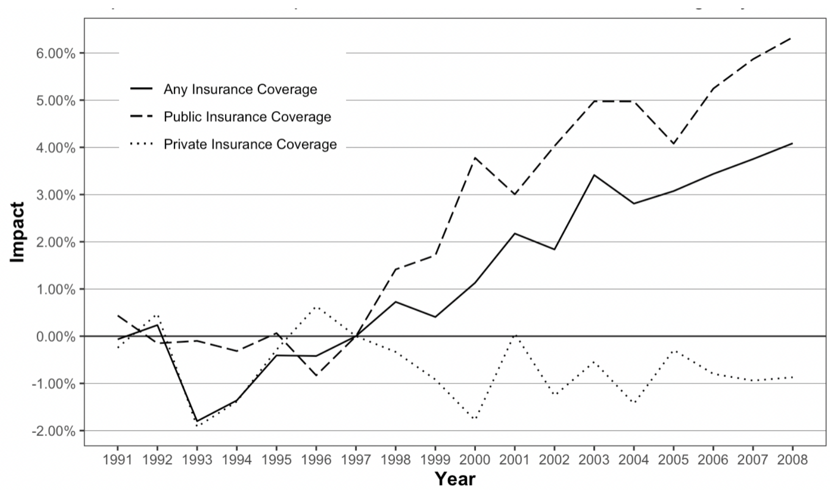

Figure 5: Replicated Results: Impact of SCHIP on Health Insurance Coverage, by Year

Again, the reformulated regression used for replication recovers the same yearly causal effects, as evidenced by the similarity between the paper results in Figure 4 and Figure **??**. In the plots, we see that public health insurance coverage for those under the age of 19 was roughly constant prior to 1997, but it then rose dramatically over the course of the following decade. However, there is no such trend private health insurance coverage, so the trend for any insurance coverage is similar to the trend for public coverage. The lack of any pre-existing differential trends and the growth in public insurance coverage after 1997 for those under the age of 19 supports the significant causal effects measured in for any insurance coverage and public insurance coverage in Table 3 and Table 4.

3.2 Extensions to Extended Parental Coverage Analysis

Extended Parental Coverage on the Likelihood of Living with Parents

To extend the parental coverage results, the original paper considers whether the extended parental coverage law affects the likelihood of one living with their own parents by invoking the following regression (which is very similar to the regression formulation for computing the main results of extended parental coverage):

$$WhetherLivingWithParents_{iast} = \alpha + \beta Law_{st} + \delta_a + \tau_t + \pi_s + \rho Unemp_{st} + X_{iast} + \varepsilon_{iast}$$

The outcome is now whether a certain observation is still living with his or her parents. While the remaining variables are the same as the main results. The standard errors are also clustered at a state level. The results of the regression above is shown in Table 9. It appears that there is a relationship between the parental coverage law and the likelihood of living with a parent. The statistical relationship between variables may be interpreted as causal, given that some states do require children to live with parents to be covered by the extended parental coverage. However, the causal relationship may also be due to unmeasured confounders that affects the state's tendency to pass the the law and the likelihood of living with the parents. Importantly, we also note that by running regression by switching treatment and outcome, we have verified and ruled out the possibility of reverse causation.

The results in Table 9 display similar results between the the original paper and our replication, at similar significance levels. The point estimates suggest that extended parental coverage laws appear to increase the likelihood of living with one's parents.

	Difference in Difference		Triple Difference
	Full Sample	Eligible Sample	Full Sample
Paper Results: Impact of Parental Coverage Law	0.012* (0.007)	0.019** (0.007)	0.038** (0.010)
Replicated Results: Impact of Parental Coverage Law	0.0115* (0.0066)	0.019** (0.0073)	0.0187** (0.0087)

Table 9: Extended Parental Coverage on the Likelihood of Living with Parents
* significant at 0.1 level
** significant at 0.05 level

Extended Parental Coverage Falsification Exercise: Reanalysis of 16-18 year-old subgroup

As an additional extension, we perform a falsification exercise by focusing on the 16-18 year-old always eligible subgroup is conducted. This age group would always be eligible for parental coverage, so in expectation, the size of the treatment effect should be smaller and less significant.

	Estimation results for dependent variable		
-	Any insurance coverage	Public insurance coverage	Private insurance coverage
Panel A. Full Sample			
DiD Full Sample N = 91,199	-5.02e-1** (2.22e-2)	-3.16e-2** (1.14e-2)	-2.88e-2 (2.64e-2)
Eligible Sample N = 89,309	-5.05e-2** (2.24e-2)	-3.08e-2** (1.16e-2)	-2.94e-2 (2.68e-2)
Panel B. Non-students			
DiD Full Sample N=9,633	-9.28e-2** (3.37e-2)	-6.94e-2** (2.05e-2)	-3.88e-2 (2.75e-2)
Eligible Sample N=8,485	-9.45e-2** (3.98e-2)	-6.61e-2 (2.42e-2)	-4.05e-2** (3.39e-2)

Table 10: Results of the Regression on Parental Coverage full sample
* significant at 0.1 level
** significant at 0.05 level

The results of the reanalysis are presented in Table 10 and Table 11. Though the authors claim that most coefficients are either insignificant in size of statistical significance, the simulation we have done had shown otherwise. The size of most coefficients are big relative to the corresponding cell in the 19-22 year old observations, and many are significant at a 0.1 or even 0.05 level. So under the premise that the model is correct, the conclusion that the policy shouldn't affect the 16-18 year old subgroup should be correct, but here, the negative coefficient indicate probable negative causation between the parental coverage law of a state and insurance coverage, which raises questions about the validity of the model.

	Estimation results for dependent variable		
-	Any insurance coverage	Public insurance coverage	Private insurance coverage
All Living with Parents (N=82,384)	-4.99e-1** 2.09e-2)	-2.87e-2** (1.2e-1)	-3.18e-2** (2.66e-2)
By income			
< 150 percent of poverty line N = 15,978	-1.13e-1** (2.79e-2)	-9.65e-2** (2.42e-2)	-4.55e-2* (2.33e-2)
150 - 300 percent of poverty line N = 29,651	-4.76e-2* (2.59e-2)	-2.55e-2* (1.5e-2)	-3.08e-2** (2.9e-2)
>300 percent of poverty line N = 36,755	-2.01e-2** (6.22e-3)	-6.08e-3 (5.74e-3)	-1.94e-2 (7.89e-3)
Parents' Insurance			
Group Coverage N=91,199	-5.01e-2** (2.19e-2)	-3.16e-2** (1.14e-2)	-2.87e-2 (2.61e-2)
And Firm Size < 100 N=14060	-2.18e-2* (1.1e-2)	-1.62e-2 (1.34e-2)	-1.27e-2 (1.91e-2)
And Firm Size > 100 N=45,461	-9.7e-3 (7.63e-3)	-6.66e-3 (7.94e-3)	-1.15e-2 (1.36e-2)
No Group Coverage (N=22,863)	-1.15e-1** (3.68e-2)	-9.59e-2** (2.7e-2)	-3.42e-2 (2.55e-2)

Table 11: Results of the Regression on Parental Coverage for the population living with parents
* significant at 0.1 level
** significant at 0.05 level

Sensitivity Analysis: E-Value Approach

We adopt the e-value metric to measure evidence for causation defined by Vanderweel and Ding [2]. E-value can be formulated as follows:

$$RR_x^{obs} + \sqrt{RR_x^{obs}(RR_x^{obs} - 1)}$$

where

$$RR_x^{obs} = \frac{\mathbb{P}(Y = 1|Z = 1, X = x)}{\mathbb{P}(Y = 1|Z = 0, X = x)}$$

The e-value can be interpreted as "the maximum value of the association between the confounder and the treatment and that between the confounder and the outcome to completely explain away an observed association"[2]. Here we calculate the e-value of the risk ratio point estimate and the e-value of the risk ratio confidence limit for certain subpopulations of interest. The subpopulation of interest includes non-student eligible sample, sample 150-300 percent of the poverty line and sample whose parents are in firm of firm size under 100. The selected subpopulations are the ones that the author pointed out to be more likely affected by the extended parental coverage laws.

We also set a benchmark to see the size of e-value suppose excluding the state FIPS variable and the marital status variable. These variables are used as benchmark since they are possible confounders affecting either the treatment or the outcome. One assumption for the benchmark to hold is that the strength of the confounding variables is stronger than that of the unmeasured confounders. The number in each cell is the corresponding e-value for the point estimator, while the number between the parentheses is the e-value for the confidence limit. The e-value indicated that the evidence for causal relationship is quite strong between the treatment and the outcome. The method we applied in Table 12 uses risk ratio, which is more widely used when considering rare outcome. Since the outcome is quite common other ratios such as the odd ratio should be further taken into consideration for further analysis to yield a more accurate result.

Subgroup of interest	Variable Omitted			Full Model
	Marriage Status	State FIP	Poverty Rate	
non-student eligible	1.8167 (1)	1.6776 (1)	1.7444 (1)	1.8220 (1)
150 - 300 percent of poverty line	1.7908 (1.4957)	1.5645 (1.2594)	1.7557 (1.4618)	1.7922 (1.4971)
firm size < 100	1.1460 (1)	1.2154 (1)	1.1479 (1)	1.1368 (1)

Table 12: E-values

4 Re-Analysis

The CPS data is observational data in nature, but the original paper utilizes regression techniques that are more typical of inference in randomized experiments. Thus, we re-analyze the estimated causal effect of the policies of interest by employing methods from both observational studies and randomized experiments.

4.1 Observational Studies Methods

In this section, we briefly introduce the methods that would be applied to reanalyze the coefficients of the data, assuming that the data came from an observational study.

First we define some notations, for observation i, and let

- i be the index for an observation
- X_i be the covariates
- Y_i be the outcome
- Z_i be the treatment
- $\hat{\mu}_0(X_i)$ be the regression estimate fitted by controlled units.
- $\hat{\mu}_1(X_i)$ be the regression estimate fitted by treated units.
- $\hat{e}(X_i)$ be the estimated propensity score

The causal effect derived through the outcome regression model can be stated as follows.

$$\tau^{reg} = \frac{1}{n}\sum_{i=1}^{n}\hat{\mu}_1(X_i) - \hat{\mu}_0(X_i)$$

where one limitation would be that the estimate would be sensitive to model specification. Thus, the propensity score model could be used instead. The Horvitz and Thompson estimator [3], denoted τ_{ht} is defined as

$$\hat{\tau}^{ht} = \frac{1}{n}\sum_{i=1}^{n}\frac{Z_i Y_i}{\hat{e}(X_i)} - \frac{1}{n}\sum_{i=1}^{n}\frac{(1-Z_i)Y_i}{1-\hat{e}(X_i)}$$

however, when $\hat{e}(X_i)$ is close to 0 or 1, the estimator tend to be unstable, hence, propensity score truncation or stratification is often used. To solve this issue, the reweighted hajek estimator [4] is proposed.

$$\hat{\tau}^{hajek} = \frac{\frac{1}{n}\sum_{i=1}^{n}\frac{Z_i Y_i}{\hat{e}(X_i)}}{\frac{1}{n}\sum_{i=1}^{n}\frac{Z_i}{\hat{e}(X_i)}} - \frac{\frac{1}{n}\sum_{i=1}^{n}\frac{(1-Z_i)Y_i}{1-\hat{e}(X_i)}}{\frac{1}{n}\sum_{i=1}^{n}\frac{(1-Z_i)}{1-\hat{e}(X_i)}}$$

However, the propensity score model would also have problems similar to the outcome model, where the estimate depends on the correctness of the propensity score model. Thus, the doubly robust estimator could be applied. The expected value of the outcome could be retrieved if either the regression imputation model or the propensity score model. The doubly robust estimator [5] could be formulated as follows

$$\hat{\tau}^{dr} = \sum_{i=1}^{n}\left[\frac{Z_i\{Y_i - \mu_1(X_i,\hat{\beta}_1)\}}{e(X_i,\hat{\alpha})} + \mu_1(X_i,\hat{\beta}_1)\right] - \sum_{i=1}^{n}\left[\frac{(1-Z_i)\{Y_i - \mu_1(X_i,\hat{\beta}_0)\}}{e(X_i,\hat{\alpha})} + \mu_0(X_i,\hat{\beta}_0)\right]$$

We also consider the matching estimator [6] used when controlled units out-numbers the treated units.

4.2 Randomized Experiment Methods

In this section, we briefly introduce the methods that would be applied to reanalyze the coefficients of the data, assuming that the data came from a randomized experiment. We note that the data used in this paper is inherently observational data by nature, but we will also pursue randomized methods for completeness. Additionally, the original paper conducts regression analysis to compute the causal effect as if the data was somewhat randomized, so randomized methods will be logical comparisons.

We will consider two methods for estimation under the randomized experiment framework: Neyman OLS (Ordinary Least Squares), and Lin's method of estimation. The Neyman OLS method does not adjust for covariates in the estimation process, and it uses the following regression:

$$Y_i \sim \beta_Z * Z + \epsilon$$

where we estimate the causal effect as $\tau_{Neyman} = \beta_Z$.

Lin's method for estimation does adjust for covariates in the estimation process, and it uses the following regression:

$$Y_i \sim \beta_Z * Z + \beta_X * X + \beta_{XZ} X * Z + \epsilon$$

where we estimate the causal effect as $\tau_{Lin} = \beta_Z$.

4.3 SCHIP Analysis

We re-analyze the relationship between treatment and outcome using techniques used in analyzing experiments as well as observational data. The five subpopulations analyzed are as follows. The full sample of observations ages $16-22$ ($n = 291,118$), the sample living with parents ($n = 218,818$), the sample whose family is < 150 percent of the poverty line ($n = 41,168$), the sample whose family is $150-300$ percent of the poverty line ($n = 77,562$), and the sample whose family is > 300 percent of the poverty line ($n = 100,088$). Within each of these groups, we estimate the causal effect on three different outcome variables: Any insurance coverage, public insurance coverage, and private insurance coverage.

We create tables for each subgroup, using both methods for experimental studies and observational studies (standard error estimates in parentheses). Then we compare the re-analyzed results to the original paper. The FRT p-value is computed using 100 Monte Carlo simulations. Table 13 displays the re-analyzed results for the causal effect of SCHIP eligibility for the full sample (see Table 15 and Table 16 in the Appendix for the re-analysis on sub samples).

	Point Estimates		
Method	Any insurance coverage	Public insurance coverage	Private insurance coverage
Randomized Experiments			
Neyman OLS	0.1223 (0.0015) $p_{FRT} = 0.00$	0.0530 (0.0014) $p_{FRT} = 0.00$	0.0820 (0.0018) $p_{FRT} = 0.00$
Lin's Estimator	0.0034 (0.0034) $p_{FRT} = 0.01$	0.0139 (0.0033) $p_{FRT} = 0.00$	-0.0072 (0.0038) $p_{FRT} = 1.00$
Observational Studies			
Regression Imputation	0.0034 (0.0038)	0.0139 (0.0033)	-0.0072 (0.0037)
Horvitz Thompson	-0.1550 (0.0020)	-0.0097 (0.0015)	-0.1470 (0.0016)
Hajek	0.0794 (0.0018)	0.0429 (0.0024)	0.0468 (0.0027)
Doubly Robust	0.0021 (0.0036)	0.0130 (0.0035)	-0.0074 (0.0040)
Paper Results			
	0.032 (0.005)	0.045 (0.005)	-0.004 (0.004)

Table 13: Reanalysis of Effect of SCHIP on Full Sample (n=291, 118)

In Table 13 above, we observe that for the causal effect of SCHIP eligibility on any insurance coverage, none of the alternate methods yield a similar estimate to the paper results. While all estimates except for the HT estimator have the same sign, the varying magnitudes should raise caution about being overconfident in the paper results.

For the causal effect of SCHIP eligibility on public insurance coverage, we notice that the Neyman OLS and Hajek estimators yield similar estimates to the paper results in terms of sign and magnitude. This is an interesting result as it highlights the hybrid experimental nature of the original paper, since both an observational method and a randomized method recovered the original result. While the data is observational in nature, the paper treats it as more of a randomized experiment (given their original regression formulation).

Lastly, there is little evidence for any type causal effect of SCHIP eligibility on private insurance coverage. Given that the original estimate was already very close to 0, we should note that Lin's Estimator, the Regression Imputation Estimator, and the Doubly Robust Estimator all yield very similar estimates for the causal effect, and these are relatively close to the original estimate. Thus, since multiple estimators agree, we can be fairly confident that there is little to no causal effect of SCHIP eligibility on private coverage.

4.4 Parental Coverage Analysis

We reanalyze the causal effect estimates with methods used to analyze randomized experiments and methods used to analyze observational data. The five subpopulations analyzed are as follows. The full sample living with parents, the full eligible sample, the non-student eligible sample, the sample whose family is 150-300 percent of the poverty line and the sample whose parents work for a firm of size under 100. The subpopulations are analyzed since they serve as evidence that the the extended parental coverage is effective in terms of increasing private insurance coverage.

We create tables for each subgroup, using both methods for experimental studies and observational studies. Then we compare the re-analyzed results to the original paper. The FRT p-value uses 100 Monte Carlo simulations. The outcome regression model assumes that all of the terms are linear, while the propensity score model is assumed to follow the logistic regression model.

	Eligible Sample		Eligible nonstudent Sample	
	Estimates		Estimates	
Method	Point Estimate	Standard Error	Point Estimate	Standard Error
Randomized Experiments				
Neyman OLS	9.74e-3 $p_{FRT} = 0.98$	4.77e-3	-0.0095 $p_{FRT} = 0.92$	0.00667
Lin's Estimator	3.57e-2 $p_{FRT} = 0$	2.56e-2	0.0629 $p_{FRT} = 0$	0.0354
Observational Studies				
Regression Imputation	0.00573	0.0063	0.0218	0.0133
HT	-0.185	0.003	-0.140	0.0069
Hajek	-0.005	0.003	0.0100	0.009
Doubly Robust	0.006	0.006	0.0220	0.0135
Matching	-0.022	0.005	-0.0242	0.008
True Result				
	0.0217	0.0124	0.0262	0.0108

Table 14: Reanalyse Eligible full sample and Eligible Nonstudent Sample

The results of re-analysis in the full sample and non-student groups are presented in Table 14 (see Table 17 and Table 18 in the Appendix for re-analysis results on other subgroups). The propensity score based estimates vary from estimates of outcome regression. Mostly, estimates that depended on outcome regression matches the results yielded by the paper more, in terms of sign and size. However, this observation implies that the conclusion of the research paper could be overturned supposing that the propensity score model is correct and when the outcome model is incorrect. Interestingly, the true estimate is very similar to that derived from the Lin's estimator, which shows again, that the paper starts off from a randomized experiment perspective, even though the nature of the data should be more observational-study oriented. Under natural experiments, we cannot guarantee that the SUTVA assumption is satisfied. It could be that observations who are related living in different states with different parental coverage laws are affected by whether one is insured or not, which causes spillover. Also, analyzing observational data under the randomization framework is likely to cause biasness towards the estimates of interest.

In particular, for the living with parent subgroup, the 150-300 poverty line subgroup and the firm size less than 100 subgroup, the estimates are more consistent with the results of the original paper compared to the eligible full sample subgroup and the eligible nonstudent subgroup. This is particularly true for methods that depend solely on the propensity score model, such as the HT estimator, Hajek and the Matching estimator. From the histogram of propensity score, the propensity score distribution is more skewed and sparse for the later group compared to the former subgroups, which is a possible reason for why the estimates differ so much across methods.

5 Conclusion

We have replicated the results of the paper, extended the main results, and reanalyzed the data using other observational studies methods that were not originally considered. We verified that the authors selection of the variables and implementation of methods are consistent with what is written by replicating the main results and getting back similar answers.

However, while the results of the paper were reproducible, the usage of methods typical of a randomized experiment are contestable, given that the data is observational by nature. The original paper's methods for analyzing data lean more toward the experimental approach as opposed to the observational approaches, and more towards outcome regression model as opposed to the propensity score model. Randomized experiments (generally speaking), make the strong assumption that the treatment is randomly assigned, but that is certainly not the case with CPS data as individual observations were put into various treatment groups on the basis of several conditions, such as their age and home state.

Still, despite the model-specific framework used in the original paper, the large sample size and convergence of several estimators in the re-analysis section do provide evidence that the original estimates are fairly robust. Given the expansion of computational resources and the emergence of non-parametric inferential methods (which are suited specifically for observational studies) in the years since the paper was published, a future direction for comparison would be to employ one or more non-parametric approach towards estimation, such as graphical models or the targeted learning framework.

Overall, through the reanalysis of data, and reviewing the key assumptions, we consider the authors approach to be valid, but very parameter and model dependent. Further re-analysis with less parameter- and model-dependent approaches would provide a parallel comparison to the reported estimated that would challenge the assumptions made in the paper's framework.

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Appendix

Figure 6 shows the correlation matrix between important covariates, treatment variables, and response variables.

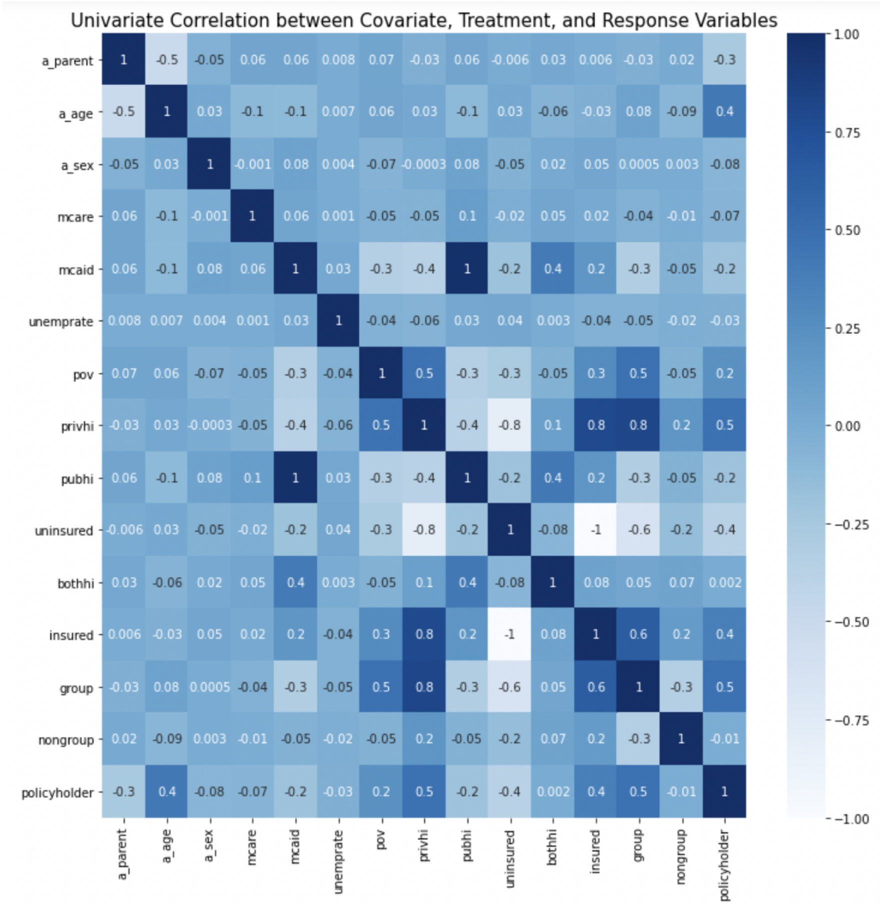

Figure 6: Correlation Matrix of Individual Variables

Method	Point Estimates: Any insurance coverage	Public insurance coverage	Private insurance coverage
Randomized Experiments			
Neyman OLS	0.0842 (0.0016) $p_{FRT} = 0.00$	0.0564 (0.0015) $p_{FRT} = 0.00$	0.0397 (0.0019) $p_{FRT} = 0.00$
Lin's Estimator	0.0292 (0.0029) $p_{FRT} = 0.00$	0.0126 (0.0029) $p_{FRT} = 0.00$	0.0209 (0.0033) $p_{FRT} = 0.00$
Observational Studies			
Regression Imputation	0.0126 (0.0017)	0.0139 (0.0033)	0.0209 (0.0030)
Horvitz Thompson	-0.0650 (0.0020)	0.0139 (0.0015)	-0.0763 (0.0018)
Hajek	0.0579 (0.0018)	0.0443 (0.0019)	0.0234 (0.0027)
Doubly Robust	0.0027 (0.0011)	0.0120 (0.002)	0.0198 (0.0033)
Paper Results			
	0.029 (0.010)	0.037 (0.005)	0.001 (0.008)

Reanalysis of Effect of SCHIP on Sample living with parents (n=218, 818)

Method	Point Estimates: Any insurance coverage	Public insurance coverage	Private insurance coverage
Randomized Experiments			
Neyman OLS	0.1415 (0.0046) $p_{FRT} = 0.00$	0.1240 (0.0049) $p_{FRT} = 0.00$	0.0344 (0.0046) $p_{FRT} = 0.00$
Lin's Estimator	0.0428 (0.0085) $p_{FRT} = 0.00$	0.0269 (0.0090) $p_{FRT} = 0.00$	0.0228 (0.0084) $p_{FRT} = 0.00$
Observational Studies			
Regression Imputation	0.0428 (0.0087)	0.0269 (0.0086)	0.0228 (0.0076)
Horvitz Thompson	0.0308 (0.0078)	0.0354 (0.0061)	0.0004 (0.0042)
Hajek	0.0939 (0.0090)	0.0801 (0.0089)	0.0245 (0.0054)
Doubly Robust	0.0356 (0.0109)	0.0197 (0.0107)	0.0201 (0.0088)
Paper Results			
	0.067 (0.016)	0.084 (0.016)	-0.004 (0.006)

Reanalysis of Effect of SCHIP on Sample <150 percent poverty (n=41, 168)

Table 15: Reanalysis of Effect of SCHIP sample living with parents and sample <150 percent of poverty line

Method	Point Estimates: Any insurance coverage	Public insurance coverage	Private insurance coverage
Randomized Experiments			
Neyman OLS	0.1034 (0.0028) $p_{FRT} = 0.00$	0.0660 (0.0025) $p_{FRT} = 0.00$	0.0541 (0.0032) $p_{FRT} = 0.00$
Lin's Estimator	0.0424 (0.0052) $p_{FRT} = 0.00$	0.0323 (0.0047) $p_{FRT} = 0.00$	0.0203 (0.0059) $p_{FRT} = 0.00$
Observational Studies			
Regression Imputation	0.0424 (0.0058)	0.0323 (0.0028)	0.0203 (0.0054)
Horvitz Thompson	-0.0384 (0.0043)	0.0242 (0.0014)	-0.0564 (0.0031)
Hajek	0.0778 (0.0048)	0.0542 (0.0021)	0.0393 (0.0046)
Doubly Robust	0.0407 (0.0065)	0.0323 (0.0023)	0.0193 (0.0060)
Paper Results			
	0.043 (0.004)	0.058 (0.018)	0.001

Reanalysis of Effect of SCHIP on Sample 150-300 percent poverty line (n=77, 562)

Method	Point Estimates: Any insurance coverage	Private insurance coverage	Public insurance coverage
Randomized Experiments			
Neyman OLS	0.0554 (0.0016) $p_{FRT} = 0.00$	0.0076 (0.0012) $p_{FRT} = 0.00$	0.0525 (0.0017) $p_{FRT} = 0.00$
Lin's Estimator	0.0169 (0.0031) $p_{FRT} = 0.01$	-0.0004 (0.0025) $p_{FRT} = 0.63$	0.0175 (0.0034) $p_{FRT} = 1.00$
Observational Studies			
Regression Imputation	0.0169 (0.0029)	-0.0004 (0.0019)	0.0175 (0.0031)
Horvitz Thompson	-0.1335 (0.0034)	-0.0022 (0.00122)	-0.1327 (0.0036)
Hajek	0.0400 (0.0016)	0.0049 (0.0017)	0.0382 (0.0017)
Doubly Robust	0.0173 (0.0029)	-0.0005 (0.0024)	0.0183 (0.0028)
Paper Results			
	0.002 (0.005)	0.005 (0.001)	-0.002 (0.006)

Reanalysis of Effect of SCHIP on Sample >300 percent poverty line (n=100, 088)

Table 16: Reanalysis of Effect of SCHIP on Samples 150-300 and >300 percent poverty line

Method	Point Estimate	Standard Error
Randomized Experiments		
Neyman OLS	-0.0077 $p_{FRT} = 0.91$	0.0058
Lin's Estimator	0.0287 $p_{FRT} = 0$	0.0307
Observational Studies		
Regression Imputation	0.01697	0.00736
HT	-0.1936	0.0066
Hajek	0.0063	0.0062
Doubly Robust	0.01766	0.00790
Matching	-0.0073	0.0107
True Result		
	0.0291	0.00728

With Parents

Method	Point Estimate	Standard Error
Randomized Experiments		
Neyman OLS	0.005 $p_{FRT} = 0.33$	0.010
Lin's Estimator	0.023 $p_{FRT} = 0.02$	0.0635
Observational Studies		
Regression Imputation	0.0505	0.024
HT	-0.179	0.0085
Hajek	0.0210	0.0147
Doubly Robust	0.0481	0.0247
Matching	-0.0124	0.0178
True Result		
	0.0511	0.0142

With Parents 150-300 Poverty Line

Table 17: Reanalysis of Effect of extended parental coverage laws on samples With Parent full Sample and sample 150-300 of the Poverty Line

Method	Point Estimate	Standard Error
Randomized Experiments		
Neyman OLS	0.0195 $p_{FRT} = 0.12$	0.0129
Lin's Estimator	0.0161 $p_{FRT} = 0.09$	0.0901
Observational Studies		
Regression Imputation	0.0045	0.0169
HT	-0.1830	0.0186
Hajek	0.008	0.0149
Doubly Robust	0.0067	0.0172
Matching	0.046	0.0210
True Result		
	0.0552	0.0199

Table 18: Reanalysis of Effect of extended parental coverage laws on Sample with Firm size < 100