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The ACA's Dependent Coverage Mandate: An Investigation of its Effects on Mortality with Regard to Race

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Abstract

I add to literature investigating the effects of the Affordable Care Act's (ACA) dependent coverage mandate (DCM). I examine how the mandate, which increased health insurance coverage for 19 to 25 year-olds, impacted short-run mortality rates for the affected age group. Unlike previous research, I examine if and how young adult mortality was affected differentially by race. I use data from the CDC's "WONDER" database to conduct difference-in-difference analysis to assess the effects of the policy change on mortality. I find that the DCM had a significant negative impact on mortality rates for the affected age group as a whole, but that African Americans and Asians and Pacific Islanders missed out on the effect. I then briefly investigate what might have driven these racially disparate impacts, but do not produce a conclusive explanation. During that investigation, I also offer a causally-identified estimate of the magnitude of the DCM's effect on health coverage.

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1. Introduction

The "dependent coverage mandate" (DCM) or "young adult provision," is a section of the Affordable Care Act (ACA) that requires private health insurers to allow policyholders' children to remain covered by their parents' plans until reaching age 26. Before the DCM's implementation in late 2010, most insurers followed the IRS definition of "dependent" and bumped children off of parental plans at age 19, or at age 24 if enrolled in higher education (Heim, Lurie, & Simon, 2018, A). Accordingly, young adults aged 19 to 25 stood to gain health insurance coverage from the mandate.

I make three contributions to the literature regarding the DCM. First, I find that the DCM significantly reduced short-run[1] mortality rates for the targeted age group as a whole. Second, I find that the DCM's effects on short-run mortality rates were concentrated among "Whites" and "American Indians or Alaskan Natives," and were not experienced by "African Americans" or "Asians or Pacific Islanders." Finally, in the course of investigating why the DCM had these racially disparate effects, I submit a causally-identified estimate for the magnitude of the DCM's impact on health coverage for people aged 19 to 25.

As far as I know, I am the first to find that the DCM had a significant effect on mortality for the affected group overall. I am also the first to look at the DCM's effects on total mortality by race, an investigation motivated in part by the fact that one of the ACA's goals was to equalize access to insurance across racial groups (O'Hara & Brault, 2013). My estimate of the DCM's effect on health coverage contributes to a considerable, but inconclusive, literature seeking to quantify the increase.

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[1] Throughout this paper, when discussing mortality rates I am referring to them in a short-run context, as in the long-run mortality rates are, of course, ultimately 100%. Accordingly, what I refer to here as a "reduction" or "decrease" in mortality rates is, in reality, a *delay* in mortality.

To produce each finding, I employ a technique of statistical analysis known as "difference-in-difference" (DD), which consists of creating a quasi-experiment to test the impact of an exogenous "treatment" on observational data. To conduct DD, "comparison" and "treatment" groups are created, and time is divided intro "pre" and "post" treatment periods. The treatment's effect on some dependent variable is estimated by comparing the average change in that variable between periods for the treatment group to the average change of that variable between periods for the comparison group.

For my analysis of mortality effects, I utilize data from the United States Centers for Disease Control (CDC). My data set is comprised of 512 observations, each containing the mortality rate of a unique national[2] demographic group based on sex, year, age, and race. I look at the 16 years from 2003 to 2018, the most recent year for which data are available. In the interest of comparing young adults that are most alike, I restrict my age range to 23 to 27: two years on each side of the provision's impact. This restriction aids in justifying the assumption necessary for DD that the two groups are identical aside from the exogenous shock of the DCM. Finally, I use four racial categories: White, African American, Asian or Pacific Islander, and American Indian or Alaskan Native. For simplicity's sake, from here on I will refer to Asian or Pacific Islander as simply "Asian," and to American Indian or Alaskan Native as simply "Native."

I use two equations in my investigation of the DCM's effects on mortality. First, to look at young adults as a whole, I use a simple model that includes the variables necessary for DD as well as all available demographic controls. To assess whether the DCM had differential effects

[2] I am forced to conduct my primary analysis at the national level, as research involving the mortality rates of narrow demographics, as I conduct here, is made difficult to conduct at the state level by the fact that any death count for a demographic that is less than 10 is suppressed for privacy reasons.

on mortality by race, I then employ a second, more complex, equation in which I interact binary race variables with each other variable.

Using the first equation, I find that, at the 1% significance level, the DCM lowered average annual mortality rates for 22 to 25 year-olds by 4.118 deaths per 100,000. This finding remains significant at the 1% level under all specifications that I use. Using the second equation, I find that, at the 1% significance level, the DCM lowered mortality rates for Whites and Natives by 5.84% and 20.32% of their average pre-implementation mortality rates, respectively. I also find, however, that African Americans and Asians missed out on this benefit, as their mortality rates were not significantly affected. I then support my results with placebo testing and a robustness check that controls for state as well as possible. My results for Natives and Asians, and particularly the magnitude of the effect on Natives, are dubious, however, as those groups had miniscule population sizes relative to Whites and African Americans.

I conclude with a brief investigation into the cause of these disparate impacts. I first examine whether the leading causes of death differed between racial groups, causing increased coverage to have different effects. My analysis produces no evidence that this was the case.

Next, I examine whether the DCM simply had a smaller effect on health coverage for African Americans and Asians than for Whites and Natives. Once again, I utilize DD, this time using American Community Survey (ACS) health coverage data. I use the simple DD model to estimate that, for Americans aged 19 to 25 as a whole, the DCM increased the average annual number of insured individuals aged 19 to 25 by 1,390,006.87. I then employ the interacted model to divide this effect by race. I find no evidence that African Americans and Asians experienced smaller health coverage increases from the DCM than Whites and Natives, but my regression carries an R-squared value of just 0.038.

2. Background Information

2.1. The Dependent Coverage Mandate

The DCM, officially titled Section 2714 of the Patient Protection and Affordable Care Act, was one of the first rules of the ACA to go into effect, with an implementation date of September 2010 (Patient Protection and Affordable Care Act of 2010). The rule applies to all private insurance plans that offer dependent child coverage and to all children regardless of marriage, education, or residence status. Insurers were required to abide by the rule for all policy renewals after September 22, 2010, but many began following it early, as the ACA was approved six months prior. Policy renewals are annual, so the latest possible date for an insurer to have abided by the rule was September 22, 2011. The mandate, therefore, was rolled out incrementally from the ACA's signing on March 23, 2010, to September 22, 2011.

The provision was designed to address young adults' relative lack of health insurance, as immediately before the ACA 29.7% of Americans aged 19 to 25 were uninsured, a significantly higher proportion than the overall uninsurance rate of 16.3% (DeNavas-Walt, Proctor, & Smith, 2009). Uninsurance rates were particularly high for this age group relative to young Americans in general, as in 2008 18% of people aged 12 to 18 and 25% of those aged 26 to 35 were uninsured (Akosa Antwi, Moriya, & Simon, 2013).

The law change was not entirely clear-cut, as different states had different existing laws regarding dependent insurance coverage prior to the ACA. Nearly 40 states already had a young adult coverage provision of some kind, although cut-off and eligibility specifics differed widely (Heim, Lurie, & Simon, 2018, A). These existing laws, in combination with the nature of the insurance at issue, mean that the ACA's new federal rule did not categorically increase insurance coverage for all Americans aged 19 to 25. Instead, the provision extended a coverage opportunity to people in that age range who: 1) had a parent holding a private insurance policy with dependent coverage, and 2) did not already have coverage independently or through a parent as the result of existing regulations in their state.

Nonetheless, the consensus is that the provision significantly increased insurance coverage for 19 to 25 year-olds, though different papers have produced estimates of the magnitude of the increase. At the low end of the range is an estimate of 700,000 newly insured young adults from 2009 to 2011 produced using ACS data (Rodean, 2012). Other papers, however, have found much larger increases: analysis of the National Health Interview Survey (NHIS) produced an estimated increase of 3 million (Sommers, 2012), and Survey of Income and Program Participation (SIPP) data suggested an increase of 2.06 million (Akosa Antwi et al., 2013).

In terms of uninsurance, CPS data show a drop in the uninsurance rate of the targeted age group of 2.4 percentage points (Cantor, Monheit, DeLia, & Lloyd, 2012), but the NHIS data demonstrate a drop of 10.4 points (Sommers, 2012). The exact causes of these wide ranges are not clear, but they are likely at least partially driven by differences in data sources, cut-off dates, and specifications. In Section 8.2, I contribute to the literature seeking to estimate the DCM's impact on health coverage by producing a causally-identified estimate of my own.

Embedded in each of these estimates is the fact that decreases in other forms of insurance offset some of the increase in private coverage under parental health plans (Akosa Antwi et al., 2013). This is the result of young adults switching onto parental plans from other, independent sources of health coverage. One study estimated a seven percentage point increase in coverage through parental plans was more than half counteracted by a 3.9 percentage point decrease in independently-held coverage among young adults (Akosa Antwi et al., 2013). In any event, all research assessing the DCM that I am aware of has linked the policy to a statistically significant increase in insurance coverage among young adults aged 19 to 25.

2.2. Health Insurance and Mortality

The goal of any policy aiming to increase health insurance is, of course, to enhance health and ultimately decrease morbidity. Accordingly, there is a good amount of research analyzing the relationship between health insurance coverage and mortality (McClellan, 2017). The first part of this connection is the question of whether or not increased health insurance coverage actually expands health care utilization. Recent work on the subject aligns with intuition and suggests that the two do in fact have a positively correlated relationship. Anderson, Dobkin, & Gross (2012) utilize pre-ACA aging-out of dependents from parental plans at age 19 to look at the impact of the coverage drop on the use of care. The authors conclude that the resulting reduction in coverage significantly reduced both emergency department visits and inpatient hospital admissions. Other research looking at this relationship has produced similar findings, including examinations of Medicare (Card, Dobkin, & Maestas, 2008) and the Oregon health insurance experiment (Finkelstein et al., 2012).

In addition to this work suggesting that the DCM, if effective in its goal of increasing coverage, would have increased care utilization, research has linked increased health care utilization with lower, or at least delayed, mortality. Like coverage and utilization, this relationship is intuitive but is also backed up by the recent literature. A deep-dive into the short-run impacts of recent health policy reforms in Massachusetts (Sommers, Long, & Baicker, 2014) linked increased care usage due to state policy changes with a drop in deaths per 100,000 of 8.2.

In sum, the research suggests that if the dependent coverage mandate were successful, it should have resulted in fewer deaths per 100,000 for young adults aged 19 to 25. I must note, however, that mortality is by no means the only outcome that should be used to assess the success of the DCM. Improvements in other measures of health or even simply a decline in uninsurance might reasonably be used to demonstrate the policy's success as well. My focus here, however, is on mortality, as the DCM's effect on this outcome is relatively unexplored.

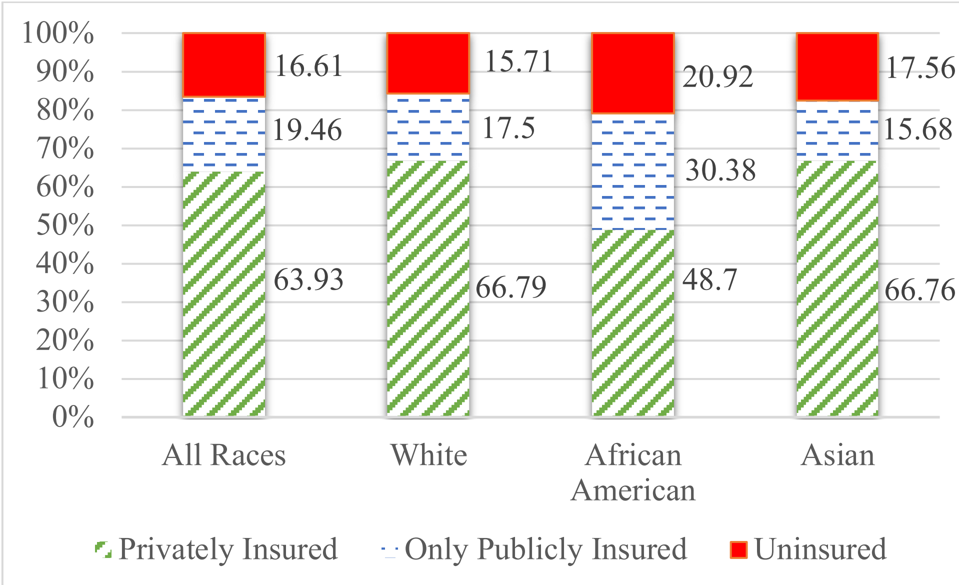
Given the already-low mortality rates of 19 to 25 year-olds, even if the DCM achieved its intended impact, only a small change in mortality could reasonably be expected. Furthermore, mortality rates only would have fallen for young adults with parents who possess private family health plans. Accordingly, the impact was likely different for different demographic groups based on differences in the groups' existing health coverage. Several demographic breakdowns might reveal differential impacts, such as sex or household income. My focus here is on race, and whether different racial groups experienced the effects of the DCM differently.

2.3. Health Insurance and Race

Analysis of the DCM's effects on different racial groups is especially relevant given the fact that one of the goals of the ACA was to close racial gaps in health insurance coverage, health care utilization, and ultimately health outcomes (O'Hara & Brault, 2013). These existing differences in coverage by race before the ACA provide key context for this paper's analysis of racial disparities in the DCM's effects.

The Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) provides economic characteristics of a representative sample of the American population, including health insurance status. The data from the 2009 Supplement provide a useful baseline for how health coverage differed by race before the implementation of the Affordable Care Act.

Graph 1: 2009 Health Insurance Status by Race



Graph 1, above, displays the ASEC data for all Americans. Unfortunately, the published ASEC data regarding health insurance breaks down race into just three categories: White, African American, and Asian. Accordingly, this background does not include the insurance coverage breakdown for Natives. The data show that, in 2009, uninsurance was 5.21 percentage points higher for African Americans (20.92%) than for Whites (15.71%), and 1.85 percentage points higher for Asians (17.56%) than for Whites (2009 Current Population Survey). These data suggest that prior to the ACA uninsurance was a more significant issue for non-Whites than Whites, and that it was a particularly significant problem for African Americans.

In addition to uninsurance, the proportion of each racial group holding private health plans is highly relevant to my research, as the DCM increased coverage only for those with parents possessing private coverage. Graph 1 shows that just 48.7% of African Americans were covered by private insurance in 2009, a much lower proportion than that of Whites (66.79%) and Asians (66.76%). This discrepancy suggests that, relative to other racial groups, a narrower proportion of African Americans were likely to be affected by the DCM. Conversely, the fact that a higher proportion of African Americans were uninsured suggests that that group had more room for increased coverage. These competing possibilities provide essential context for my work and motivate my inquiry.

3. Literature Review

3.1. The Dependent Coverage Mandate

With my focus on impact on mortality by race, I add a new wrinkle to a growing literature assessing the effects of the dependent coverage mandate. Given the opportunity it presents as a natural experiment that increased health insurance access for a distinct group, the DCM has been used to look at how increased health coverage affects a wide variety of outcomes.

As discussed in Section 2.1., several papers have sought to quantify the impact the provision had on its most naturally associated outcome: health insurance coverage. The consensus has been that the provision significantly increased insurance coverage for 19 to 25 year-olds, but researchers have produced estimates of the magnitude of the increase ranging from 700,000 (Rodean, 2012) to 3 million (Sommers, Buchmueller, Decker, Carey, & Kronick, 2012) newly insured young adults from 2009 to 2011. Similarly, estimates of the law's effect on the young adult uninsurance rate range from a decline of 2.4 percentage points (Cantor et al., 2012) to a drop as significant as 10.4 percentage points (Sommers, 2012). This ambiguity prompted me to try to quantify the increase myself.

Building on the literature examining the DCM's impact on coverage, work has also sought to assess the mandate's effect on health care utilization. McClellan summarizes that literature well: "… the work to date shows mixed effects on utilization. Studies using survey-based measures show little evidence of increases in general health care utilization, but higher utilization of mental health treatment after the dependent care coverage expansion" (McClellan, 2017). McClellan is referring to findings suggesting that: the DCM made young adults more likely to have a primary care doctor, but did not affect overall use of preventative care (Barbaresco, Courtemanche, & Qi, 2015); the DCM did not impact care utilization (Chua & Sommers, 2014); and that the DCM increased utilization of mental health care for its targeted group (Saloner & Le Cook, 2014).

Recent research has also examined areas of health less-obviously linked to increased insurance access. For example, two 2019 papers look at the DCM's impact on opioid use. Wettstein (2019) examines opioid mortality as it relates to the mandate, and finds "that 1 percentage point more coverage [as a result of the dependent coverage mandate] reduced opioid mortality among [young adults] by 2.5/100,000 or 19.8%." Carrillo (2019) uses the National Survey of Drug Use and Health to look at opioid misuse and abuse. He associates the DCM with decreases of 11.2% and 25% of prescription opioid misuse and general opioid abuse, respectively, for the target age group.

At least two papers have looked at the mandate as it relates to childbirth. Heim, Lurie, & Simon (2018, B) utilize tax records to examine the mandate and associated its implementation with a small decrease in births for the affected age group compared to those slightly older. Daw & Sommers (2018) link the DCM to increases in private insurance payments related to births, early prenatal care, and adequate prenatal care, and a decrease in premature births.

The literature has also advanced beyond looking at strictly health-related outcomes of the DCM. For example, labor market outcomes have received significant attention. Heim, Lurie, & Simon (2018, A) again use tax data to assess how the rule impacted employment status, job characteristics, and education. They are unable to associate the DCM with significant impacts on any of these outcomes and hypothesize that this is because health insurance is simply not an important driver of labor-related decision making for young adults. Bailey & Chorniy (2016) look at the DCM's impact on job lock among young adults, but also fail to establish a significant link. The only paper that has demonstrated the DCM having a significant effect on a labor market outcome is Bailey (2017), who finds that the mandate increased entrepreneurial activity, although his results do not pass placebo testing.

3.2. The Dependent Coverage Mandate and Mortality

Despite the wide breadth of existing literature on the DCM, the mandate's short-run effect on mortality remains mostly unexplored: only two papers have examined the relationship. This shortage is likely at least partially due to the inherently low mortality rates of young adults, which can make analyzing changes in and differences between those rates difficult (Barbaresco et al., 2015). In the first of the two existing papers on the DCM and mortality, Scott, Sommers, et al. (2015) focus on "trauma" deaths, and find no significant effect. The authors expected to find a reduction in trauma mortality corresponding with the mandate's implementation, but the lack of such a finding is not entirely surprising given the nature of trauma-related issues and their lower amenability to health coverage than other causes of death.

The other paper looking at the mandate's effect on deaths takes a different approach. McClellan (2017) is primarily concerned with "disease" mortality: a category of morbidity likely to respond to changes in health insurance. McClellan briefly looks at overall mortality and finds that the DCM has no significant effect. For disease-related deaths, however, he finds "a 6.1% decline in monthly disease-related mortality," which amounts to "30 averted deaths per month…, or 357 per year" nationally (McClellan, 2017, p. 516). These findings provide important context for my research. To date, no link between the DCM and overall mortality has been established, but some work suggests the provision significantly reduced at least some types of deaths.

3.3. The Dependent Coverage Mandate and Race

I am not the first to include race in an examination of the DCM, and the existing work that involves race leaves mortality as the natural next outcome to look into. While McClellan does not assess total mortality by race, he does do so for disease-related deaths. He finds larger reductions in all-disease mortality for African Americans than for Whites, although he finds that the drop for African Americans is driven entirely by a severe drop in cardiovascular mortality. He also finds that the DCM had no significant effects on races other than White and African American.

That paper seems to be the only study that has found a differential impact of the DCM by race on mortality of any type. Other work, however, has looked at whether or not the provision's impact on insurance coverage was homogenous across racial groups. The results are decidedly mixed. Scott, Salim, et al. (2015) find that coverage increases from the DCM were significant for all groups, but more than twice as high for Whites than African Americans. Akosa Antwi et al. (2013) find a similar disparity in impact on parental insurance, but puzzlingly find a nearly identical increase between races in insurance coverage in general.

Two other papers have found no statistically significant difference in effect on coverage between racial groups: O'Hara & Brault (2013), who use ACS data, and Shane, Ayyagari, & Wehby (2016), who use data from the Medical Expenditure Panel Survey. Finally, Sommers et al. (2012) analyze the National Health Interview Survey, and actually observe a significantly larger increase in coverage for non-Whites than for Whites. Despite this relatively large body of work, no consensus on the DCM's differential impacts by race has been reached. Accordingly, I had no prior expectation for what my look at the mandate's effects on mortality rates by race might reveal.

I also contribute to the wider literature examining differential effects by race of government health insurance policies in general. For example, several studies have looked at how other portions of the ACA impacted different races differently. Artiga, Orgera, & Damico (2019) examine the effects of the ACA as a whole by race, and find that since the law's implementation, coverage has increased substantially among non-Whites, but significant gaps between Whites and other racial groups persist. Yue, Rasmussen, & Ponce (2018) focus specifically on the ACA's Medicaid Expansion and find that Hispanics lagged behind other groups in coverage increases, but they find no other statistically significant changes in disparities between groups.

Other health policies have been looked at through a racial lens as well. Sommers et al. (2014) include race in their examination of how the effects of Massachusetts' 2006 health policy changes varied between subgroups, and find that effects on mortality were greater for Latinos and non-Whites than for non-Latino Whites. I add to the literature assessing health policy outcome disparities between races by doing so for the DCM's effect on mortality, which has not yet been done.

4. Data

Like several of the papers just discussed, I utilize the natural experiment created by the DCM to conduct a DD analysis of the mandate's impact. Using the age group affected by the provision (19 to 25 year-olds) and the timing of its implementation (2011), I construct a treatment group of those who experienced increased access to insurance and compare it to a comparison group of people who were close in age but not impacted by the mandate.

I pulled the data for this analysis from the CDC's "WONDER" database (Centers for Disease Control and Prevention). I used the database's data request website to create a set of demographic groupings based on sex, year, age, and race over the sixteen years from 2003 to 2018. Each year contains 32 unique categories: one for each combination of 4 ages (24 to 27), 2 sexes, and 4 races (White, African American, Asian, and Native). This makes for 512 observations (16 x 4 x 2 x 4 = 512).

In addition to variables for each demographic distinction, each observation includes a deaths-per-100,000 mortality rate. The CDC produces this rate using its database of death certificates (compiled from the records of the fifty states and the District of Columbia) along with U.S. Census Bureau annual population estimates, each broken down into the 512 demographic groupings specified above (Centers for Disease Control and Prevention). Race classifications on death certificates are reported by the coroner or funeral director tasked with creating the certificate, but Census population estimates are based on self-reported race. Additionally, the CDC database does not include multiple races, and instead assigns individuals to one of the four racial categories. Accordingly, there are likely some discrepancies in this process of racial classification, resulting in imperfect data, but the direction of the resulting bias is unclear. The CDC database includes a Hispanic ethnicity variable, but the reporting for those data are especially unreliable, as Hispanic ethnicity is significantly underreported on death certificates relative to the Census Bureau's self-reported population figures (Centers for Disease Control and Prevention). As a result, I decided not to include Hispanic ethnicity in my analysis.

I present summary statistics of my data set in Table 1 on the following page. The table displays the average annual population size of each of the treatment and comparison groups followed by each group's average annual deaths per 100,000 for the pre and post-implementation periods and the percent change between them. A preliminary look at the raw numbers suggests that the DCM reduced mortality rates of the treatment group for Whites and Natives but not for

Asians and African Americans. This is shown in the percent change column: if the percent change is lower or more negative for the treatment group than the comparison group, as is true for "All Races," Whites, and Natives, it is an indication that the DCM had a mortality-reducing effect on that group. While this initial evaluation lacks proof of causality, it indicates that my statistical analysis is likely to produce interesting results.

Table 1: Deaths per 100,000 by Age Group, Race, and Time Period

Group	Average Population	2003-2010	2011-2018	Percent Change
All Races 24-25	8,695,400.8	96.8	98.6	+1.86%
All Races 26-27	8,550,036.9	98.5	105.2	+6.8%
White 24-25	6,634,218.6	91.4	95.5	+4.49%
White 26-27	6,533,354.3	92.5	103	+11.35%
African American 24-25	1,344,554.1	148.9	137.5	-7.66%
African American 26-27	1,281,050.5	158.6	145	-8.58%
Asian 24-25	578,120.4	36.7	38.7	+5.45%
Asian 26-27	602,966.6	35.3	36.2	+2.55%
Native 24-25	138,507.7	118.3	112.1	-5.24%
Native 26-27	132,665.6	116.2	138.8	+19.45%

To conduct that analysis, I constructed three dummy variables. The first was assigned a 1 for the "post" period, and a 0 for the "pre" period. Although the DCM's implementation did not happen all at once, the majority of the provision's impact began at the beginning of 2011. This is because most health plans are renewed in January (Cronin, 2012), so, for most policyholders, the first renewal after September 22, 2010 occurred in January 2011. This treatment date is not perfect, because, as mentioned, the DCM affected plans as early as March 2010 and as late as

September 2011, but setting 2011 as the treatment year captures the majority of the DCM's impact.

In addition to the timing dummy, I created a dummy variable equal to 1 for observations in the treatment group and 0 for observations in the comparison group. As with the other dummy, this variable is not perfect. First, while the DCM was the first federal rule to extend dependent coverage, a majority of states already had a mandate of some sort of their own (National Conference of State Legislatures, 2010). Accordingly, 24 and 25 year-olds already granted parental coverage due to their state's laws were not affected, but I was forced to treat them as though they were. Furthermore, only young adults who actually had parents possessing private health plans with dependent coverage would have been impacted, but I could not account for this nuance.

I then created a final dummy equaling the product of the first two, resulting in a posttreatment variable equal to 1 for observations 25 and younger in 2011 or later, and otherwise equal to 0. In the full data set, there are 128 observations for which this variable is equal to 1, forming the treatment group for my analysis.

5. Methodology

5.1. Specifications

For my initial inquiry—whether or not the DCM resulted in lower mortality rates for 24 to 25 year-olds as a whole—I used Equation 1, shown below in condensed form.

Equation 1

$$Deaths\ per\ 100{,}000_{mayr} = \beta + \beta_1\, male + \beta_2\, age + \beta_3\, year + \beta_4\, race + \beta_5\, posttreatment + \varepsilon$$

In addition to the variables necessary for the DD, I utilized all available controls. Given the data set, these were sex, age, year, and race. I captured sex with a binary variable titled "male" set equal to 1 for male and 0 for female, and age with a continuous variable numbered 1 through 4 for the ages 24 to 27. To control for year, I created binary variables for each year from 2003 to 2018, and excluded 2003 to serve as the baseline. Similarly, I created dummy variables for each race and left out White to serve as the baseline. I used ordinary least squares (OLS) regression, clustered standard errors at the age/year level, and weighted my observations by population size.

To incorporate race into my analysis of the DCM's effects, I used Equation 2, shown below in condensed form.

Equation 2

$$\textit{Deaths per 100,000}_{mayr} = \beta + \beta_1\, male + \beta_2\, age + \beta_3\, year + \beta_4\, race +$$

$$\beta_5\, race \times male + \beta_6\, race \times age + \beta_7\, race \times year +$$

$$\beta_8\, posttreatment + \beta_9\, race \times posttreatment + \varepsilon$$

The variables are the same as in Equation 1, but each race dummy is interacted with each of the controls and with "posttreatment" to reveal how the effect of each differs between races. Once again, I excluded White to serve as the baseline. Aside from these new variables, I ran the regression in the same manner as Equation 1.

5.2. Difference-in-Difference Analysis

The DD approach is well-suited for looking at policy implementations like the DCM, as it allows for the creation and comparison of treatment and comparison groups within observational data. Any discrepancy in the size of the change over time between the two groups (hence "difference-in-difference") is expressed in the coefficient on an interaction term between dummy variables for the treatment group and post-implementation period, which here is titled "posttreatment."

In Equation 2, I took this a step further, utilizing a "difference-in-difference-in-difference." I interacted the dummy variables for each race with each of the other variables in the model. I once again used Whites as the baseline and accordingly left the variable for White race out of the specification. I included dummy variables for African American, Asian, and Native, however, and interacted them with each of the controls as well as the posttreatment variable. Under this approach, the coefficients on the standalone variables ("male," each year dummy, "age," and "posttreatment") represent the model's output for Whites. The coefficients on the interaction terms of each race with each variable represent how the output for that race differs from the baseline White output. The four posttreatment variables, then, are those of most interest, as they will show whether there was a significant difference in the change in mortality rates over time between the treatment and comparison groups, and if so, whether that difference was equal for the four racial groups.

Two primary assumptions are necessary for this statistical approach to be accurate. First, a fundamental assumption for any DD analysis is that the treatment and comparison groups were, absent the exogenous shock, on parallel trends. In other words, the approach assumes that if the DCM had not been implemented, mortality would have changed (or not changed) in the same manner for the group of 24 to 25 year-olds and the group of 26 to 27 year-olds. This assumption is best illustrated by considering a scenario in which it is not met: if, unrelated to the DCM, mortality rates were going to fall for the younger group but not for the older group over the same time period, the analysis would falsely produce a result suggesting that the mandate had curbed mortality rates when, in fact, it had not. The validity of this assumption is evaluated graphically in Section 6.

Second, it must be assumed that the binary variables used to construct the policy change correctly capture the policy and not some other change. In the setup used here, this assumption is impossible to fully prove, but is well-supported. For it to be inaccurate, there must have been another change occurring in 2011 that impacted 24 to 25 year-olds but not 26 to 27 year-olds, and specifically affected their mortality rates. While such a change is not impossible, there is no evidence that one occurred, and much research has been conducted under the assumption that the DD approach used here accurately captures the effects of the DCM.

6. Results

I present the key results from Equation 1, which I used to look at all racial groups together, below in Table 2. The full regression outputs are included in Appendix I as Table 6. To produce Column 1, I ran Equation 1 on the full data set. The posttreatment coefficient of -4.118, significant at the 1% level, represents my first contribution to the literature: it indicates that the DCM curbed average annual mortality rates for 24 to 25 year-olds by 4.118 deaths per 100,000. This figure corresponds to 4.25% of the average annual mortality rate of 24 to 25 year-olds from 2003 to 2010 of 96.8 (Centers for Disease Control and Prevention).

Table 2: Partial DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Basic Model	(2) Excluding Age 26	(3) Excluding 2011
posttreatment	-4.118***	-6.329***	-4.620***
	(0.924)	(1.238)	(1.005)
Observations	512	384	480
R-squared	0.916	0.917	0.916

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In addition to the primary model, I ran the same specification with certain observations excluded to test some of the assumptions of the model. To produce Column 2, I dropped observations aged 26 to eliminate some of the fuzziness caused by those who turn 26 and thereby lose dependent coverage in the middle of a year. Similarly, for Column 3 I excluded the year 2011 to remove some of the ambiguity surrounding the implementation of the DCM.

These exclusions altered the magnitude of the posttreatment coefficient but did not significantly change the results. The results were also robust to placebo testing, which I discuss fully in Section 7.

The DCM's potential to affect different races differently suggests that including race in the analysis might produce a more complete picture of the provision's effects on mortality. Table 3, below, displays the key coefficients produced by incorporating race into my assessment by using Equation 2. The full regression outputs are included in Appendix I in Table 7.

Table 3: Partial DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Interacted Model	(2) Excluding Age 26	(3) Excluding 2011
posttreatment	-5.339***	-8.384***	-5.980***
	(1.197)	(1.461)	(1.311)
blackposttreatment	7.624***	12.03***	8.085***
	(2.586)	(2.732)	(2.712)
asianposttreatment	6.627***	9.343***	7.411***
	(1.875)	(2.408)	(2.061)
nativeposttreatment	-18.70***	-18.10**	-18.56***
	(4.574)	(6.751)	(4.869)
Observations	512	384	480
R-squared	0.988	0.988	0.987

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

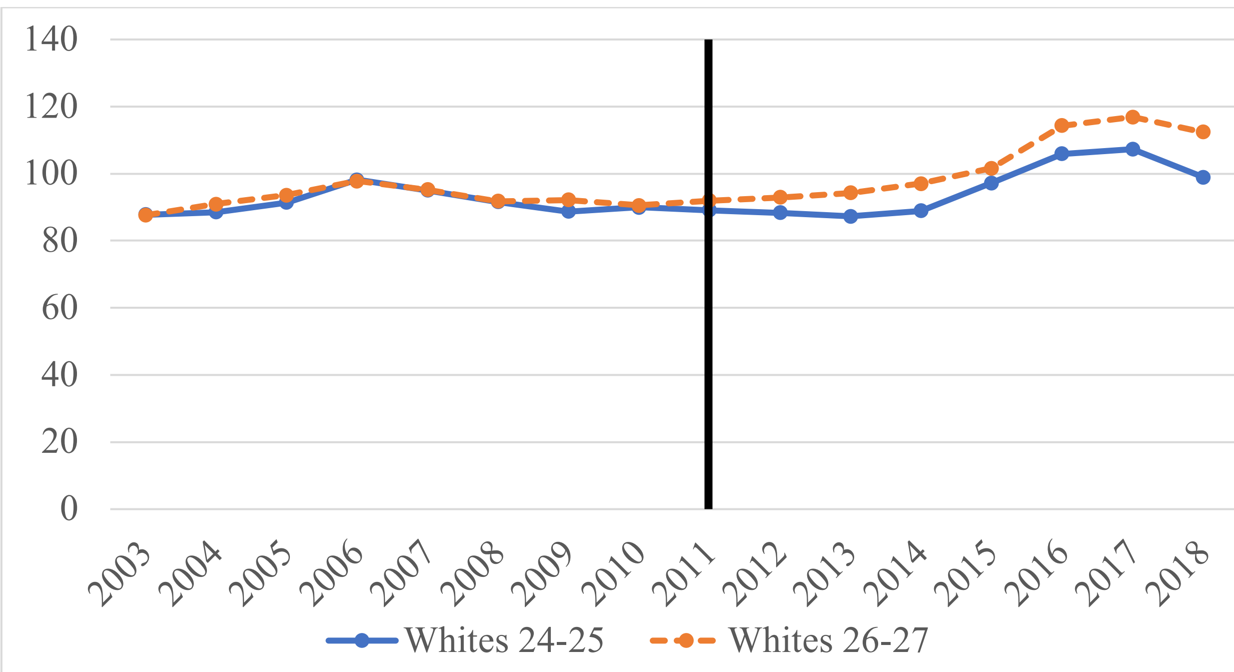
To produce Column I, I ran the interacted model on the complete data set. The -5.339 coefficient on posttreatment suggests that, for Whites, the DCM resulted in 5.339 fewer average annual deaths per 100,000 for 24 to 25 year-olds relative to 26 to 27 year-olds, significant at the 1% level. This figure makes up 5.84% of the average annual mortality rate of White 24 to 25 year-olds from 2003 to 2010 of 91.4. The other posttreatment coefficients, however, suggest that this effect was not experienced equally by all racial groups.

The negative coefficient of -18.70 on nativeposttreatment indicates that Natives were affected to an even greater degree than Whites. The model indicates that the DCM lowered average annual deaths per 100,000 for Natives by 24.039 (-5.339-18.7). This value constitutes 20.32% of the average annual mortality rate of Native 24 to 25 year-olds from 2003 to 2010 of 118.3, which is nearly four times greater than the corresponding figure for Whites (5.84%). This result does not seem reasonable, and could be the result of the very limited size of the Native population.

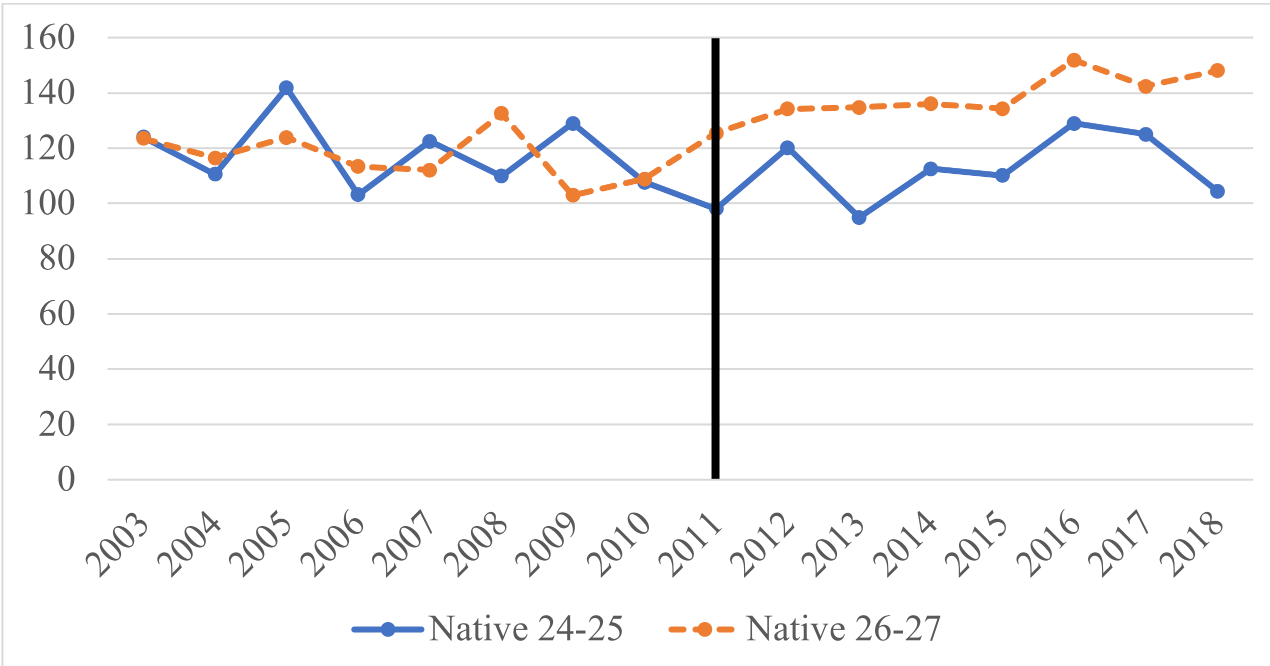
The blackposttreatment and asianposttreatment coefficients present my second, and perhaps most significant, contribution to the literature: at the 1% significance level, the DCM had a smaller impact on average annual mortality rates for African Americans and Asians than for Whites. In fact, the model suggests that the DCM had no significant effect on African American or Asian mortality at all. This is indicated by the Column 1 posttreatment and blackposttreatment coefficients, which together produce an estimate of 2.285 (-5.339 + 7.624) for African Americans, and 1.288 (-5.339 + 6.627) for Asians. Given the standard errors, these values are not statistically significantly different from 0. Columns 2 through 3 display the results of running the model with the same exclusions utilized previously. Once again, neither the age 26 or year 2011 exclusion significantly altered the results. I find, therefore, that the DCM statistically significantly lowered average annual mortality rates for Whites and Natives in the targeted age group, but did not do so for African Americans or Asians. I must note that, though not as small as that of Natives, the Asian group's population is modest relative to Whites and African Americans, and therefore that the result for that group is comparatively imprecise.

In Graphs 2 through 5, below, I present these results visually by displaying the mortality rate trend lines over time of the 24 to 25 year-old group and the 26 to 27 year-old group for each race.

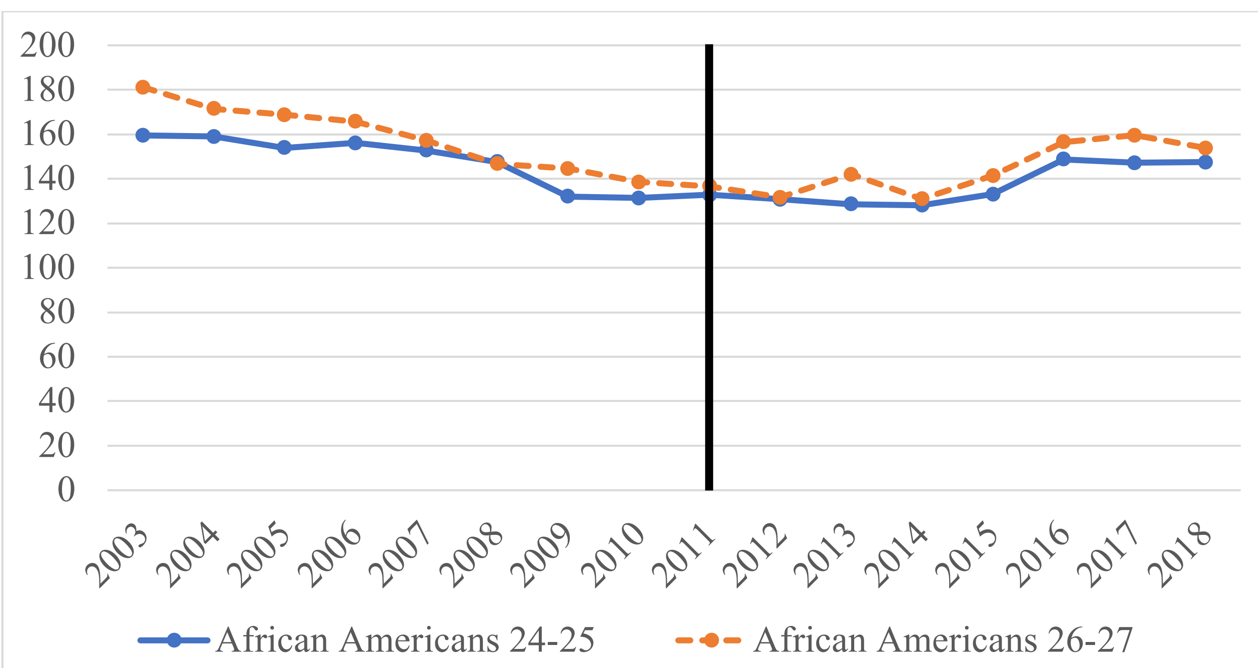
Graph 2: White Deaths per 100,000 Over Time by Age Group



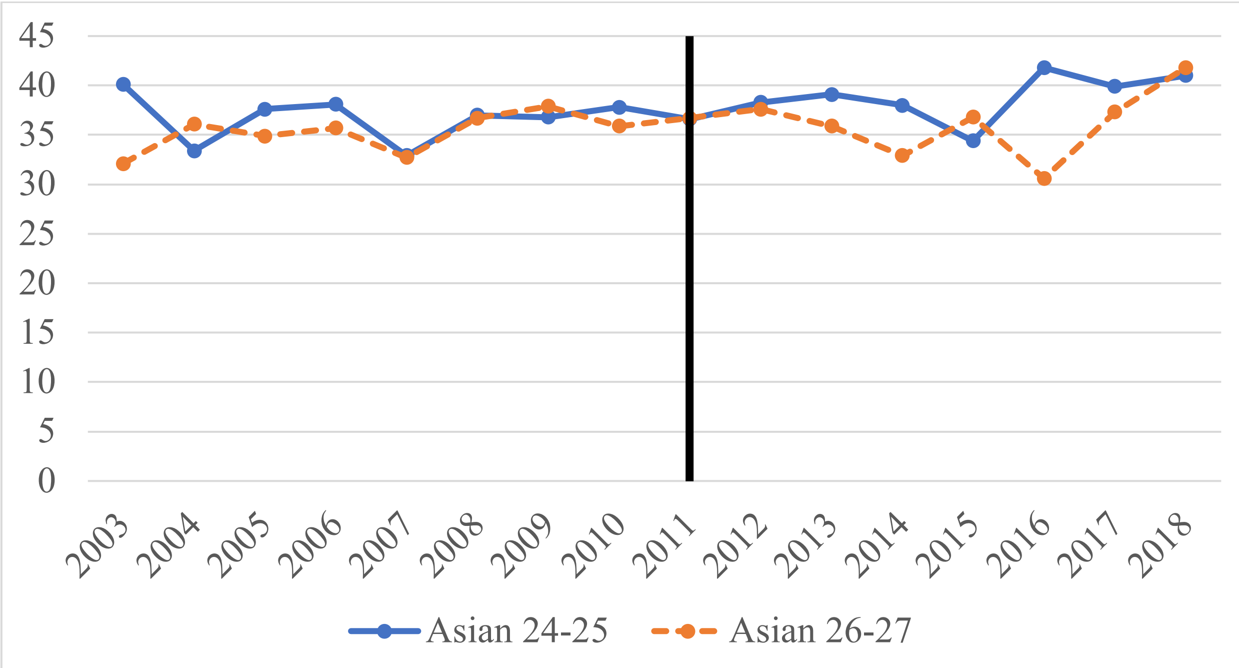
Graph 3: Native Deaths per 100,000 Over Time by Age Group



Graph 4: African American Deaths per 100,000 Over Time by Age Group



Graph 5: Asian Deaths per 100,000 Over Time by Age Group



Graphs 2 and 3 show that for Whites and Natives, following the DCM's implementation in 2011 the 26 to 27 year-old groups experienced increases in mortality that were not felt to nearly the same extent for the 24 to 25 year-old groups. Accordingly, gaps between the two age groups appear. Graphs 4 and 5, however, do not demonstrate any change in the gap between age groups for African Americans and Asians after 2011, matching Table 3's results suggesting African Americans and Asians aged 24 to 25 were not affected by the DCM. For Whites and African Americans, the parallel trends assumption appears to hold up well, bolstering my DD results for those groups. Graphs 3 and 5, however, demonstrate the highly variable nature of the Native and Asian groups due to their small populations, further suggesting my results for those group are not especially reliable.

7. Robustness Checks

To support my regressions, I ran two robustness checks. The first was a placebo test to verify the assumption that my results were caused by the DCM's implementation rather than unrelated trends or some other factor. I used data from 2003 to 2010 and a false implementation date of 2007. Otherwise, the specifications are identical to those used to produce Column 1 of Tables 2 and 3. Table 4, below, displays the key coefficients of the placebo tests. The complete regression output is included in Appendix I in Table 8.

Table 4*: *Partial Placebo DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Basic Model	(2) Interacted Model
fakeposttreatment	-0.376	-1.237
	(1.085)	(1.005)
blackfakeposttreatment		6.202*
		(3.652)
asianfakeposttreatment		-0.587
		(1.835)
nativefakeposttreatment		5.950
		(9.067)
Observations	256	256
R-squared	0.910	0.989

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Although the blackfakeposttreatment coefficient in the placebo interacted model is statistically significant, when combined with the baseline fakeposttreatment coefficient, the resulting value (4.965) is not significantly different from 0 given the standard error of 3.652. This lack of significance bolsters the assumption that the treatment group and 2011 implementation date in my primary regressions accurately capture the effects of the DCM. If the effects shown in Tables 2 and 3 were the result of something other than the DCM, or merely the result of random trends, similar false effects could likely be found using a randomly assigned implementation date and treatment group. As shown in Table 4, however, I found no such false effects.

For my second robustness check, I attempted to verify that its lack of geographical specificity did not significantly hinder my primary dataset. As mentioned, it was impossible to conduct my full analysis at the state level, as the CDC suppressed death counts below ten for privacy reasons. Given my fine demographic groupings and the low death rates of my target age group, many groups had death counts under ten and were therefore censored. For that reason, I chose to keep my primary analysis at the national level. With certain sacrifices, however, I was able to conduct a limited state-level regression as a robustness check. To do so, I expanded my age range to 22 to 29, did not divide by single-year age or sex, and limited my analysis to Whites and African Americans.

I was left with complete data for 31 states, but still had to drop 19 states[3] and the District of Columbia due to suppressed data. I then generated the necessary variables and used Equations 1 and 2 on my new data set to test my results for Whites and African Americans. I controlled for state using dummy variables and left out Alabama to serve as the state baseline. Otherwise, my regressions matched my primary ones, but with no control for sex, only two age groups (22 to 25 and 26 to 29), and only two races (White and African American), making for 1,984 observations. My results, shown in Table 9 in Appendix I, indicate that the DCM significantly reduced White mortality rates, but did not significantly affect African American mortality rates. These results match those of my primary regressions and bolster my findings of the DCM's significant effect on White mortality but lack of an effect on African American mortality.

[3] Dropped states were Alaska, Delaware, Hawaii, Idaho, Iowa, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, South Dakota, Utah, Vermont, West Virginia, and Wyoming.

8. Discussion

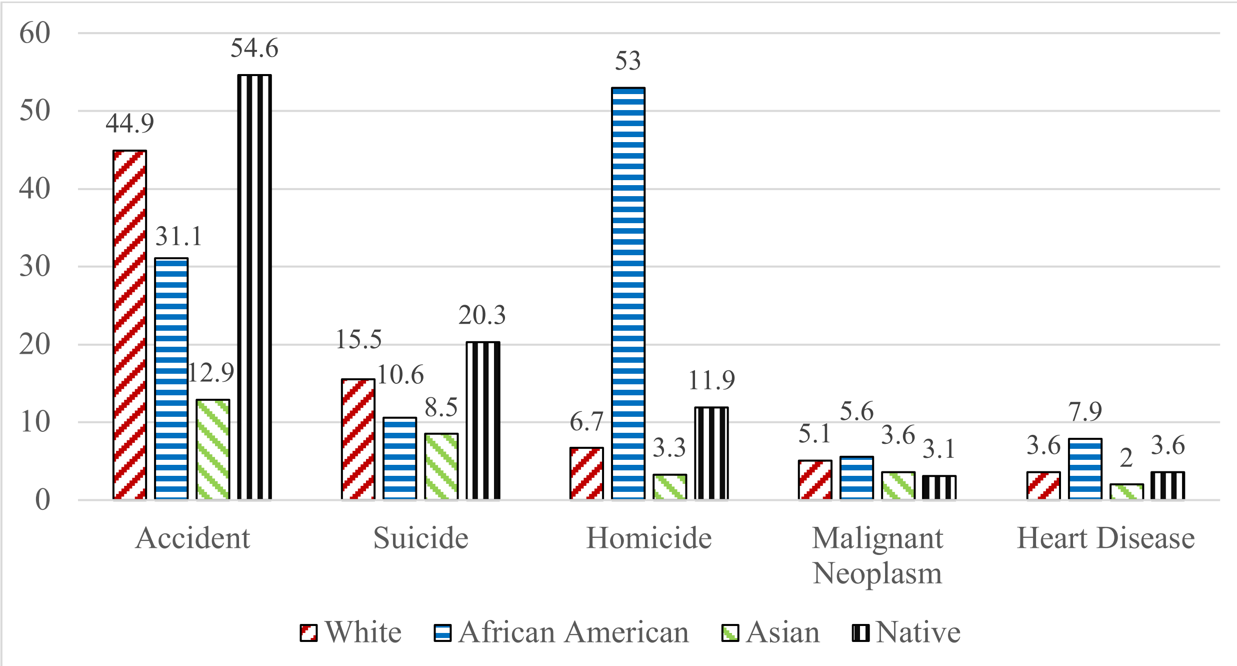
After I found that the DCM statistically significantly lowered mortality rates for Whites and Natives, but did not do so for African Americans and Asians, my natural next line of inquiry was to try to explain these disparities in outcomes. I considered two main possible explanations for the difference in effect. First, even if the DCM affected health insurance coverage identically for all racial groups, increased coverage might have, for whatever reason, affected racial groups differently. The other possibility, however, is that the coverage increase itself differed between racial groups. I conducted brief investigations of each of those possibilities.

8.1. Differences in Causes of Death

Some types of deaths are less amenable to health care, and accordingly to health insurance, than others (McClellan, 2017). If different racial groups are affected by non-amenable causes at different proportions, it follows that the groups' mortality rates would react differently to similar increases in health coverage. To assess whether racial differences in causes of death might have contributed to racial differences in the impact of the DCM on mortality, I used CDC mortality records to find and compare the five leading causes of death for young adults aged 24 to 25 (Centers for Disease Control and Prevention). I present the top five causes of death in mortality rate terms for each race in Graph 6 on the following page.

The graph displays several disparities between races in most common causes of death, but the most striking discrepancy is in homicides. For African Americans aged 24 to 25 from 2003 to 2018, the homicide mortality rate was 53 deaths per 100,000. This figure is more than four times greater than the homicide mortality rate for any other race.

Graph 6: Five Leading Causes of Death as Deaths per 100,000 by Race



The CDC records show that of these African American homicide deaths, more than 90% were the result of the discharge of a firearm. Given the highly lethal nature of firearm-related wounds, it is unlikely that increased health insurance would have a significant effect on such deaths. Accordingly, I hypothesized that this high homicide proportion of African American deaths for those aged 24 to 25 might at least partially explain why the DCM did not significantly lower mortality rates for the group. I tested this hypothesis by running the same specification that produced the discrepancy in mortality effects (Equation 2), but with homicides excluded from the mortality rate figures. My results were not meaningfully affected (shown in Table 10 in Appendix I), indicating that high numbers of homicides were not the reason why the DCM did not lower mortality rates for African Americans.

Graph 6 also shows that Whites and Native, the two groups whose mortality rates the DCM significantly affected, have high accident death rates relative to the other two groups. To test whether this discrepancy might somehow be linked with the DCM's differential effects, I ran my regressions once again, this time excluding accidents. As with homicides, my results were not significantly altered (shown in Table 11 in Appendix I). As a result, my inquiry produced no indication that racial differences in causes of death were the cause of the DCM's racially disparate effects on mortality.

8.2. Differences in Coverage Increases

A second, perhaps simpler, explanation for the discrepancies in the DCM's effects on mortality would be analogous discrepancies in the DCM's effects on coverage. In other words, if Whites and Natives experienced greater coverage gains from the DCM than African Americans and Asians, it follows that White and Native mortality rates would also be more significantly affected. Racial differences in the DCM's coverage effects were possible and perhaps even likely given the disparities in existing levels of private health coverage—which was necessary to benefit from the DCM—shown in Graph 1. As discussed in Section 3.1., potential discrepancies in the DCM's effect on coverage by race have been investigated before, but no consensus has been established. With that in mind, I contribute to that research with my own look at how the DCM affected coverage by race.

I used similar methods to those I utilized in my examination of the DCM's effects on mortality. O'Hara & Brault (2013) use DD and ACS data to look at coverage effects by race, but they only use data through 2011. Given the uncertainty in the literature, I decided to revisit the topic with updated data and a different specification. I pulled individual health insurance data for people aged 24 to 27 from the ACS for the ten years from 2008 to 2018 (American Community Survey). Once again, I created a posttreatment dummy variable equal to 1 for those affected by the DCM, and 0 otherwise. Table 5 on the following page displays the key results and the complete regression output is included in Table 12 in Appendix I.

For Column 1, I used Equation 1, but the ACS data were at the individual level, and the dependent variable was a dummy equal to 1 if the individual possessed health coverage and 0 otherwise. For Columns 2 through 4, I made the same changes to Equation 2. Even though the DCM applied only to private insurers, I included all coverage types, as looking at private coverage alone would overcount by including those who switched from public to private health plans due to the provision. I clustered standard errors at the age/year level and used person weights provided in the data set.

Table 5: Partial DD Estimates of Effect on Health Coverage

VARIABLES	(1) Basic Model	(2) Interacted Model
posttreatment	0.0444***	0.0463***
	(0.00363)	(0.00397)
blackposttreatment		-0.00403
		(0.00596)
asianposttreatment		-0.0137
		(0.00298)
nativeposttreatment		-0.0183
		(0.0152)
Observations	1,451,768	1,451,768
R-squared	0.037	0.038

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1 displays the primary output of the Basic Model looking at the effect of the DCM on 24 to 25 year-olds as a whole, and represents my contribution to the literature seeking to quantify the impact of the DCM on health coverage for young adults. The posttreatment coefficient of 0.0444, significant at the 1% level, indicates that the DCM made young adults in the treatment group 4.44% more likely to possess health coverage. Given the average annual 24 to 25 year-old population from 2011 to 2018 of 9,064,116.12 (Centers for Disease Control and Prevention), this corresponds to an increase of 402,446.756 in average annual insured individuals aged 24 to 25. Expanding to the full target population of 19 to 25 year-olds, which averaged 31,306,461 annually from 2011 to 2018 (Centers for Disease Control and Prevention), this result suggests that the DCM increased the average annual number of insured individuals aged 19 to 25 by 1,390,006.87.

This result fits squarely within the range of estimates produced by the existing literature, and stood up to a placebo test run using data from 2013 to 2018 and a fake implementation date of 2016 (shown in Table 13 in Appendix I). However, the R-squared value is just 0.037, suggesting that the included variables explain little of the variation of the health coverage variable.

Column 2 of Table 5 presents the key results of the primary interacted model (Equation 2). The four posttreatment coefficients do not match what would be expected given my findings in Section 6. They collectively suggest that each racial group experienced an increase in coverage, and that there was no statistically significant difference in the size of that increase between groups. These results passed placebo testing (shown in Table 13 in Appendix I), but the R-squared value is just 0.038. As a result, I do not find that the DCM's differential effects on mortality by race were due to corresponding differential effects on coverage, but my results are not definitive. My investigation, then, falls short of producing a satisfying explanation for the DCM's racially disparate effects on mortality.

9. Conclusion

I set out to investigate whether the ACA's dependent coverage mandate had a significant effect on mortality among young adults and whether that effect differed between racial groups. Using CDC WONDER data to conduct difference-in-difference analysis, I found that the answer to both questions was yes. The DCM reduced mortality rates for young adults as a whole, but the reduction was concentrated among Whites and Natives and was not experienced by African Americans or Asians. Given the small population sizes of Asians, and particularly of Natives, however, my results for those two groups must be taken with a grain of salt.

I then briefly examined two potential causes for these racially disparate effects. I hypothesized that the DCM's lack of an effect on African American and Asian mortality might have been the result of racial disparities in causes of death. I tested this possibility by repeating the primary difference-in-difference analysis with each of the most widely differing causes of death excluded from the data, but these exclusions did not significantly alter my results.

It was also possible that differences in mortality outcomes were the result of the DCM having had differential effects on health coverage by race. I tested this possibility using difference-in-difference analysis and did not find racially disparate increases in coverage aligning with what would be expected if this were the cause of the differential mortality effects. Along the way, I produced a causally-identified estimate of the DCM's effect on health insurance coverage among young adults.

In sum, my findings suggest that government policies aimed at increasing health insurance coverage can have different effects on different racial groups, and specifically that some racial groups can miss out on the benefits of these policies. I was unable to explain the DCM's differential effects on mortality, however, and future research might aim to produce a concrete explanation.

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Appendix I

Table 6: Complete DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Simple Model	(2) Excluding Age 26	(3) Excluding Year 2011
male	89.62***	89.97***	90.07***
	(0.981)	(1.143)	(1.014)
age	1.558***	1.237***	1.547***
	(0.280)	(0.230)	(0.281)
y2004	0.694	0.548	0.693
	(0.933)	(1.191)	(0.932)
y2005	2.832***	2.566**	2.832***
	(0.973)	(1.112)	(0.969)
y2006	6.748***	7.846***	6.748***
	(1.364)	(1.273)	(1.361)
y2007	3.381**	3.443*	3.381**
	(1.520)	(1.849)	(1.514)
y2008	-0.163	0.366	-0.163
	(1.041)	(1.123)	(1.035)
y2009	-2.537*	-3.455***	-2.538*
	(1.279)	(1.105)	(1.278)
y2010	-3.333**	-3.712**	-3.334**
	(1.348)	(1.488)	(1.342)
y2011	-1.210	0.805	
	(1.340)	(2.144)	
y2012	-1.455	0.204	-1.208
	(1.306)	(2.193)	(1.394)
y2013	-1.014	0.630	-0.765
	(1.010)	(1.286)	(1.013)
y2014	-0.204	1.081	0.0469
	(1.153)	(1.697)	(1.172)
y2015	5.735***	8.452***	5.985***
	(1.554)	(1.400)	(1.619)
y2016	16.42***	19.23***	16.67***
	(1.922)	(1.814)	(1.953)
y2017	17.92***	19.95***	18.16***
	(1.647)	(1.934)	(1.626)
y2018	12.77***	14.65***	13.00***
	(2.108)	(2.638)	(2.062)
black	52.74***	52.31***	53.16***
	(1.850)	(2.140)	(1.959)
asian	-57.53***	-57.01***	-57.86***
	(1.235)	(1.500)	(1.298)
native	24.99***	23.67***	25.31***
	(1.878)	(2.270)	(1.939)
posttreatment	-4.118***	-6.329***	-4.620***
	(0.924)	(1.238)	(1.005)
Constant	43.31***	43.86***	43.06***
	(1.367)	(1.467)	(1.379)
Observations	512	384	480
R-squared	0.916	0.917	0.916

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Complete DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Interacted Model	(2) Excluding Age 26	(3) Excluding Year 2011
male	84.25***	84.64***	84.73***
	(1.069)	(1.269)	(1.105)
age	1.080***	0.648**	1.057***
	(0.321)	(0.258)	(0.321)
y2004	2.018	1.695	2.019
	(1.233)	(1.496)	(1.226)
y2005	4.983***	4.547***	4.984***
	(1.266)	(1.370)	(1.255)
y2006	10.50***	10.93***	10.50***
	(1.411)	(1.409)	(1.398)
y2007	7.482***	7.241***	7.484***
	(1.722)	(2.018)	(1.710)
y2008	4.022***	3.983***	4.023***
	(1.281)	(1.461)	(1.270)
y2009	2.688*	1.773	2.689*
	(1.359)	(1.346)	(1.354)
y2010	2.492	2.090	2.493
	(1.541)	(1.685)	(1.528)
y2011	5.349***	8.061***	
	(1.724)	(2.581)	
y2012	5.342***	7.463***	5.661***
	(1.461)	(2.393)	(1.566)
y2013	5.516***	7.586***	5.839***
	(1.265)	(1.730)	(1.310)
y2014	7.799***	9.214***	8.125***
	(1.434)	(1.887)	(1.431)
y2015	14.29***	18.21***	14.61***
	(2.526)	(1.631)	(2.629)
y2016	24.91***	28.05***	25.23***
	(2.412)	(2.434)	(2.432)
y2017	26.79***	29.68***	27.11***
	(2.359)	(2.451)	(2.357)
y2018	20.40***	22.32***	20.71***
	(2.502)	(3.077)	(2.424)
black	38.70***	34.30***	38.05***
	(5.834)	(5.395)	(5.851)
asian	-16.29***	-16.65***	-15.63***
	(3.452)	(3.593)	(3.453)
native	33.38***	31.65***	35.14***
	(7.910)	(9.488)	(7.999)
blackmale	59.85***	59.43***	59.99***
	(2.238)	(2.579)	(2.379)
asianmale	-54.25***	-54.56***	-54.88***
	(1.205)	(1.490)	(1.202)
nativemale	1.663	1.494	0.994
	(3.293)	(3.951)	(3.453)
blackage	4.064***	4.666***	4.200***
	(0.827)	(0.842)	(0.836)
asianage	-1.546***	-1.171**	-1.618***
	(0.489)	(0.441)	(0.494)
nativeage	0.160	0.457	-0.157
	(1.628)	(2.007)	(1.655)
blacky2004	-7.152	-5.274	-7.151
	(4.513)	(5.122)	(4.448)
blacky2005	-14.11***	-13.61***	-14.11***

	(4.525)	(4.683)	(4.447)
blacky2006	-20.22***	-14.53**	-20.23***
	(6.482)	(6.460)	(6.416)
blacky2007	-23.47***	-20.74***	-23.48***
	(4.185)	(4.565)	(4.126)
blacky2008	-28.21***	-23.38***	-28.22***
	(5.135)	(5.107)	(5.111)
blacky2009	-35.89***	-36.30***	-35.90***
	(5.049)	(5.356)	(5.011)
blacky2010	-38.96***	-38.00***	-38.97***
	(4.366)	(4.857)	(4.314)
blacky2011	-43.66***	-46.97***	
	(5.229)	(5.385)	
blacky2012	-47.61***	-49.17***	-47.85***
	(4.773)	(5.559)	(4.741)
blacky2013	-44.12***	-45.76***	-44.36***
	(5.263)	(7.111)	(5.233)
blacky2014	-52.31***	-52.38***	-52.55***
	(4.617)	(5.354)	(4.561)
blacky2015	-51.67***	-55.83***	-51.91***
	(5.652)	(5.323)	(5.678)
blacky2016	-47.28***	-48.18***	-47.52***
	(4.677)	(5.852)	(4.630)
blacky2017	-48.90***	-52.97***	-49.15***
	(5.640)	(5.347)	(5.665)
blacky2018	-45.59***	-45.13***	-45.84***
	(4.883)	(5.570)	(4.814)
asiany2004	-3.298	-5.319	-3.301
	(2.923)	(3.599)	(2.926)
asiany2005	-4.730	-3.716	-4.734
	(3.420)	(3.460)	(3.430)
asiany2006	-9.626***	-11.76***	-9.628***
	(2.615)	(3.083)	(2.615)
asiany2007	-10.67***	-12.67***	-10.67***
	(2.466)	(2.914)	(2.467)
asiany2008	-3.162	-4.679	-3.161
	(2.415)	(2.971)	(2.420)
asiany2009	-1.373	-1.918	-1.372
	(2.420)	(2.903)	(2.425)
asiany2010	-1.723	-2.684	-1.723
	(3.189)	(4.085)	(3.202)
asiany2011	-5.487*	-8.625*	
	(3.263)	(5.098)	
asiany2012	-4.244	-7.904**	-4.632
	(2.703)	(3.888)	(2.854)
asiany2013	-4.927*	-7.984**	-5.321**
	(2.469)	(3.319)	(2.495)
asiany2014	-9.236***	-11.28***	-9.636***
	(2.663)	(3.316)	(2.626)
asiany2015	-15.58***	-22.04***	-15.98***
	(4.448)	(3.534)	(4.616)
asiany2016	-25.82***	-29.29***	-26.21***
	(4.478)	(6.046)	(4.374)
asiany2017	-25.24***	-29.08***	-25.61***
	(3.397)	(4.264)	(3.361)
asiany2018	-16.04***	-18.22***	-16.42***
	(2.771)	(3.542)	(2.713)
nativey2004	-12.32	-9.930	-12.32
	(7.391)	(9.660)	(7.486)
nativey2005	4.662	7.948	4.654
	(12.21)	(16.26)	(12.19)
nativey2006	-26.14***	-26.38***	-26.14***

	(4.901)	(6.592)	(4.945)
nativey2007	-13.73***	-11.33**	-13.73***
	(3.996)	(5.059)	(3.843)
nativey2008	-6.655	-7.951	-6.651
	(7.067)	(9.203)	(7.193)
nativey2009	-10.03	-2.877	-10.03
	(9.012)	(9.050)	(8.876)
nativey2010	-17.76***	-19.56**	-17.75***
	(6.434)	(8.427)	(6.345)
nativey2011	-5.196	-4.062	
	(5.027)	(7.668)	
nativey2012	10.12**	11.64*	10.07**
	(4.233)	(6.635)	(4.144)
nativey2013	-2.719	-0.923	-2.779
	(6.973)	(10.30)	(7.202)
nativey2014	4.886	5.422	4.818
	(4.348)	(6.874)	(4.277)
nativey2015	-3.598	-9.410	-3.667
	(9.060)	(9.147)	(9.149)
nativey2016	4.214	5.098	4.151
	(3.613)	(5.900)	(3.620)
nativey2017	-4.213	-1.391	-4.265
	(5.631)	(7.737)	(5.557)
nativey2018	-4.904	-6.106	-4.952
	(4.327)	(6.838)	(4.530)
posttreatment	-5.339***	-8.384***	-5.980***
	(1.197)	(1.461)	(1.311)
blackposttreatment	7.624***	12.03***	8.085***
	(2.586)	(2.732)	(2.712)
asianposttreatment	6.627***	9.343***	7.411***
	(1.875)	(2.408)	(2.061)
nativeposttreatment	-18.70***	-18.10**	-18.56***
	(4.574)	(6.751)	(4.869)
Constant	41.96***	42.92***	41.77***
	(1.651)	(1.637)	(1.660)
Observations	512	384	480
R-squared	0.988	0.988	0.987

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8*: *Complete Placebo DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Simple Model	(2) Interacted Model
male	90.06***	83.69***
	(1.215)	(1.234)
age	0.952***	0.152
	(0.311)	(0.308)
y2004	0.667	2.008*
	(0.992)	(1.160)
y2005	2.796***	4.971***
	(0.904)	(0.964)
y2006	6.714***	10.51***
	(1.279)	(0.940)
y2007	3.548***	8.125***
	(1.188)	(1.142)
y2008	0.00948	4.670***
	(0.978)	(0.919)
y2009	-2.372	3.332***
	(1.401)	(1.171)
y2010	-3.174**	3.137***
	(1.219)	(1.065)
black	64.36***	24.36***
	(2.607)	(7.134)
asian	-53.88***	-17.11***
	(0.868)	(3.988)
native	24.39***	29.42**
	(2.868)	(12.17)
blackmale		69.61***
		(3.696)
asianmale		-57.30***
		(1.595)
nativemale		5.300
		(4.759)
blackage		6.235***
		(1.208)
asianage		-1.026
		(0.607)
nativeage		0.619
		(2.567)
blacky2004		-7.134*
		(3.725)
blacky2005		-14.11***
		(3.465)
blacky2006		-20.29***
		(5.559)
blacky2007		-26.72***
		(3.708)
blacky2008		-31.49***
		(4.711)
blacky2009		-39.18***
		(5.460)
blacky2010		-42.25***
		(4.363)
asiany2004		-3.301
		(3.030)
asiany2005		-4.736
		(3.483)
asiany2006		-9.637***
		(2.718)

asiany2007		-10.43***
		(2.633)
asiany2008		-2.923
		(2.498)
asiany2009		-1.122
		(2.621)
asiany2010		-1.469
		(2.890)
nativey2004		-12.31
		(7.438)
nativey2005		4.669
		(12.53)
nativey2006		-26.15***
		(4.968)
nativey2007		-16.79***
		(5.432)
nativey2008		-9.699
		(9.035)
nativey2009		-13.04
		(9.044)
nativey2010		-20.75***
		(7.331)
fakeposttreatment	-0.376	-1.237
	(1.085)	(1.005)
blackfakeposttreatment		6.202*
		(3.652)
asianfakeposttreatment		-0.587
		(1.835)
nativefakeposttreatment		5.950
		(9.067)
Constant	42.71***	44.54***
	(1.581)	(1.407)
Observations	256	256
R-squared	0.910	0.989

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Complete State-Level DD Estimates of Effect on Deaths per 100,000

VARIABLES	(1) Simple Model	(2) Interacted Model
Arizona	-30.59***	-30.99***
	(1.859)	(2.148)
Arkansas	-5.339**	-9.553***
	(2.330)	(2.507)
California	-53.20***	-56.05***
	(2.170)	(2.135)
Colorado	-39.68***	-40.69***
	(2.273)	(2.443)
Connecticut	-45.75***	-42.80***
	(2.259)	(2.900)
Florida	-25.77***	-24.35***
	(1.738)	(2.139)
Georgia	-37.29***	-33.94***
	(1.816)	(2.126)
Illinois	-40.66***	-52.52***
	(2.349)	(2.529)
Indiana	-20.25***	-25.27***
	(2.563)	(2.834)
Kansas	-35.42***	-40.65***
	(2.365)	(2.396)
Kentucky	-5.887**	-5.618**
	(2.283)	(2.569)
Louisiana	4.563	-5.907**
	(2.942)	(2.866)
Maryland	-27.16***	-30.17***
	(2.891)	(3.851)
Massachusetts	-49.11***	-47.80***
	(3.294)	(4.059)
Michigan	-28.79***	-37.21***
	(2.347)	(2.471)
Minnesota	-65.47***	-66.83***
	(2.335)	(2.382)
Mississippi	-3.871	-4.901*
	(2.314)	(2.575)
Missouri	-15.80***	-26.87***
	(3.008)	(2.919)
New Jersey	-38.90***	-41.34***
	(2.390)	(3.218)
New York	-63.47***	-59.77***
	(2.093)	(2.473)
North Carolina	-32.01***	-30.99***
	(1.847)	(2.441)
Ohio	-26.77***	-30.73***
	(3.242)	(3.717)
Oklahoma	-15.56***	-18.10***
	(2.188)	(2.349)
Pennsylvania	-17.13***	-20.56***
	(2.954)	(3.712)
South Carolina	-20.63***	-21.36***
	(2.122)	(2.761)
Tennessee	-14.88***	-17.37***
	(1.497)	(1.885)
Texas	-39.92***	-39.97***
	(2.201)	(2.251)
Virginia	-49.71***	-51.24***
	(1.930)	(2.028)
Washington	-54.78***	-55.70***
	(2.560)	(2.712)
Wisconsin	-42.91***	-47.12***
	(2.489)	(2.722)
age	5.086***	2.496***
	(0.584)	(0.751)
y2004	-0.295	0.339
	(0.721)	(1.210)

y2005	2.416**	3.604**
	(1.005)	(1.343)
y2006	6.530***	8.259***
	(0.971)	(1.457)
y2007	4.732***	7.537***
	(0.798)	(1.094)
y2008	0.664	4.225***
	(0.794)	(1.098)
y2009	-2.544	1.805
	(1.792)	(1.984)
y2010	-3.769***	1.354
	(1.247)	(1.940)
y2011	2.330	8.517**
	(2.963)	(3.130)
y2012	1.748	8.472***
	(1.685)	(1.665)
y2013	0.964	7.699***
	(1.264)	(1.894)
y2014	2.110*	9.889***
	(1.179)	(1.440)
y2015	9.550***	16.63***
	(1.079)	(1.408)
y2016	20.74***	27.13***
	(1.338)	(1.764)
y2017	22.66***	29.38***
	(2.598)	(2.724)
y2018	16.93***	23.25***
	(2.579)	(2.969)
black	47.17***	72.24***
	(2.271)	(6.355)
blackArizona		-30.31***
		(6.209)
blackArkansas		-47.78***
		(6.407)
blackCalifornia		-12.50
		(8.536)
blackColorado		-15.47**
		(6.840)
blackConnecticut		-42.14***
		(6.502)
blackFlorida		-54.43***
		(8.495)
blackGeorgia		-39.32***
		(6.465)
blackIllinois		-38.38***
		(6.030)
blackIndiana		32.22***
		(5.036)
blackKansas		1.654
		(7.076)
blackKentucky		12.92
		(8.847)
blackLouisiana		-46.83***
		(7.012)
blackMaryland		-0.613
		(6.937)
blackMassachusetts		-21.00***
		(7.537)
blackMichigan		-53.90***
		(8.213)
blackMinnesota		11.58*
		(6.773)
blackMississippi		-31.20***
		(6.693)
blackMissouri		-26.33***
		(7.596)
blackNew Jersey		38.65***
		(6.394)
blackNew York		-21.94***
		(7.517)

blackNorth Carolina		-49.41***
		(6.870)
blackOhio		-35.47***
		(6.485)
blackOklahoma		-10.92*
		(5.949)
blackPennsylvania		-19.62**
		(9.123)
blackSouth Carolina		-14.28*
		(8.333)
blackTennessee		-27.89***
		(5.962)
blackTexas		-21.57***
		(5.627)
blackVirginia		-37.69***
		(5.690)
blackWashington		-25.90***
		(5.932)
blackWisconsin		-41.62***
		(6.697)
		-
blackage		15.85***
		(1.854)
blacky2004		-3.914
		(4.423)
blacky2005		-7.339***
		(2.515)
blacky2006		-10.59***
		(3.111)
blacky2007		-17.05***
		(2.418)
blacky2008		-21.56***
		(4.584)
blacky2009		-26.18***
		(2.384)
blacky2010		-30.69***
		(4.432)
blacky2011		-37.66***
		(2.652)
blacky2012		-40.60***
		(3.227)
blacky2013		-40.53***
		(4.215)
blacky2014		-46.00***
		(2.661)
blacky2015		-42.03***
		(2.682)
blacky2016		-38.31***
		(2.999)
blacky2017		-39.91***
		(2.638)
blacky2018		-37.85***
		(2.892)
posttreatment	-10.21***	-12.11***
	(1.338)	(1.476)
blackposttreatment		12.47***
		(2.126)
Constant	122.2***	124.0***
	(1.853)	(2.470)
Observations	1,984	1,984
R-squared	0.771	0.869

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: DD Estimates of Effect on Deaths per 100,000 Excluding Homicides

VARIABLES	(1) Simple Model	(2) Interacted Model
male	70.95***	76.87***
	(0.803)	(1.030)
age	2.072***	1.334***
	(0.244)	(0.305)
y2004	1.099	1.840
	(0.782)	(1.287)
y2005	3.078***	4.799***
	(0.743)	(1.317)
y2006	7.549***	10.75***
	(1.021)	(1.409)
y2007	4.347***	7.354***
	(1.216)	(1.621)
y2008	2.203**	4.800***
	(0.873)	(1.227)
y2009	1.047	4.074***
	(0.883)	(1.318)
y2010	0.252	4.253***
	(1.177)	(1.484)
y2011	2.384*	7.105***
	(1.215)	(1.749)
y2012	2.372**	7.190***
	(1.029)	(1.461)
y2013	3.431***	7.856***
	(0.793)	(1.273)
y2014	4.579***	10.20***
	(0.924)	(1.364)
y2015	9.306***	16.27***
	(1.532)	(2.507)
y2016	18.50***	26.13***
	(1.601)	(2.239)
y2017	20.68***	28.37***
	(1.685)	(2.369)
y2018	16.38***	22.10***
	(1.787)	(2.400)
black	7.647***	24.39***
	(1.497)	(4.928)
asian	-54.46***	-17.88***
	(1.203)	(2.679)
native	20.36***	28.61***
	(1.769)	(3.917)
blackmale		-15.71***
		(1.434)
asianmale		-49.81***
		(1.133)
nativemale		-4.685
		(3.092)
blackage		5.478***
		(0.784)
asianage		-1.385**
		(0.522)

nativeage	0.644
	(1.459)
blacky2004	-3.235
	(5.367)
blacky2005	-10.50**
	(5.124)
blacky2006	-15.67***
	(5.391)
blacky2007	-15.55***
	(4.820)
blacky2008	-16.82***
	(5.435)
blacky2009	-19.73***
	(4.886)
blacky2010	-25.12***
	(4.790)
blacky2011	-29.44***
	(5.797)
blacky2012	-32.61***
	(4.938)
blacky2013	-27.91***
	(5.313)
blacky2014	-34.64***
	(4.952)
blacky2015	-38.96***
	(5.630)
blacky2016	-38.97***
	(5.162)
blacky2017	-38.98***
	(5.139)
blacky2018	-30.56***
	(5.367)
asiany2004	-2.568
	(2.759)
asiany2005	-5.509
	(3.666)
asiany2006	-10.58***
	(2.833)
asiany2007	-10.36***
	(2.448)
asiany2008	-3.359
	(2.248)
asiany2009	-2.340
	(2.437)
asiany2010	-2.778
	(2.860)
asiany2011	-8.120**
	(3.283)
asiany2012	-5.935**
	(2.647)
asiany2013	-6.992***
	(2.491)
asiany2014	-10.57***
	(2.579)
asiany2015	-16.68***
	(4.392)

asiany2016		-26.20***
		(4.457)
asiany2017		-25.36***
		(3.497)
asiany2018		-16.72***
		(2.789)
nativey2004		-11.34*
		(6.152)
nativey2005		5.902
		(9.381)
nativey2006		-23.71***
		(4.727)
nativey2007		-11.88***
		(3.592)
nativey2008		-2.521
		(6.275)
nativey2009		-7.641
		(7.936)
nativey2010		-17.65***
		(4.243)
nativey2011		-0.225
		(3.977)
nativey2012		13.73***
		(4.313)
nativey2013		-0.822
		(7.155)
nativey2014		7.304*
		(4.334)
nativey2015		-1.456
		(7.419)
nativey2016		4.205
		(2.876)
nativey2017		-0.662
		(4.227)
nativey2018		-5.318
		(3.213)
posttreatment	-3.828***	-5.222***
	(0.832)	(1.137)
blackposttreatment		8.355***
		(2.208)
asianposttreatment		6.890***
		(1.897)
nativeposttreatment		-16.80***
		(4.275)
Constant	42.50***	37.47***
	(1.122)	(1.653)
Observations	512	512
R-squared	0.945	0.985

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: DD Estimates of Effect on Deaths per 100,000 Excluding Accidents

VARIABLES	(1) Simple Model	(2) Interacted Model
male	48.26***	38.58***
	(0.461)	(0.399)
age	1.913***	1.714***
	(0.202)	(0.199)
y2004	-1.260**	-0.459
	(0.487)	(0.313)
y2005	-0.741	0.804
	(0.718)	(0.537)
y2006	-0.324	2.131**
	(1.256)	(0.831)
y2007	-2.692***	-0.00681
	(0.706)	(0.685)
y2008	-3.720***	-0.879
	(0.751)	(0.614)
y2009	-2.969***	1.366***
	(0.680)	(0.268)
y2010	-5.170***	-0.543
	(0.975)	(0.939)
y2011	-4.657***	0.176
	(0.802)	(0.722)
y2012	-4.602***	0.643
	(0.859)	(0.698)
y2013	-5.087***	-0.191
	(0.974)	(0.767)
y2014	-5.508***	0.564
	(0.758)	(0.878)
y2015	-4.086***	1.565
	(0.763)	(1.119)
y2016	-0.573	4.412***
	(1.359)	(1.618)
y2017	-0.213	5.026***
	(0.682)	(0.872)
y2018	-2.120***	2.884***
	(0.795)	(0.763)
black	65.36***	51.40***
	(1.489)	(3.605)
asian	-25.05***	-10.88***
	(0.597)	(3.045)
native	15.65***	11.58
	(1.622)	(8.477)
blackmale		72.39***
		(1.731)
asianmale		-21.31***
		(0.791)
nativemale		10.13***
		(2.562)
blackage		2.227***
		(0.661)
asianage		-1.364***
		(0.458)

nativeage	-0.316
	(1.474)
blacky2004	-5.790*
	(3.031)
blacky2005	-10.37***
	(3.353)
blacky2006	-16.19***
	(5.133)
blacky2007	-18.03***
	(3.009)
blacky2008	-21.82***
	(3.357)
blacky2009	-31.29***
	(4.305)
blacky2010	-33.56***
	(3.480)
blacky2011	-33.84***
	(3.847)
blacky2012	-38.93***
	(3.326)
blacky2013	-36.21***
	(3.722)
blacky2014	-41.95***
	(3.449)
blacky2015	-38.40***
	(3.728)
blacky2016	-34.64***
	(3.385)
blacky2017	-35.62***
	(4.036)
blacky2018	-37.53***
	(3.296)
asiany2004	-0.549
	(3.568)
asiany2005	-0.847
	(3.397)
asiany2006	-0.554
	(3.013)
asiany2007	-2.358
	(2.804)
asiany2008	2.138
	(2.826)
asiany2009	0.856
	(2.636)
asiany2010	1.389
	(3.430)
asiany2011	-0.788
	(2.939)
asiany2012	0.179
	(2.929)
asiany2013	1.687
	(2.726)
asiany2014	-3.104
	(3.087)
asiany2015	-4.389
	(3.102)

asiany2016		-6.474*
		(3.381)
asiany2017		-6.168**
		(2.978)
asiany2018		-1.095
		(2.780)
nativey2004		0.254
		(7.791)
nativey2005		-6.558
		(10.07)
nativey2006		-14.30*
		(7.411)
nativey2007		-4.699
		(7.929)
nativey2008		1.506
		(9.837)
nativey2009		-1.305
		(9.422)
nativey2010		-0.684
		(10.18)
nativey2011		-3.421
		(9.975)
nativey2012		13.13*
		(7.690)
nativey2013		3.363
		(7.077)
nativey2014		7.506
		(7.282)
nativey2015		5.735
		(8.848)
nativey2016		17.82**
		(7.686)
nativey2017		10.73
		(7.245)
nativey2018		20.09***
		(7.231)
posttreatment	-1.685**	-2.093***
	(0.641)	(0.745)
blackposttreatment		3.403*
		(1.949)
asianposttreatment		3.444**
		(1.378)
nativeposttreatment		-15.73***
		(3.644)
Constant	24.64***	26.30***
	(0.825)	(0.591)
Observations	512	512
R-squared	0.846	0.989

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Complete DD Estimates of Effect on Health Coverage

VARIABLES	(1) Simple Model	(2) Interacted Model
male	-0.0785***	-0.0707***
	(0.00283)	(0.00285)
age	0.00809***	0.00765***
	(0.00140)	(0.00156)
y2009	-0.0122***	-0.00895**
	(0.00326)	(0.00433)
y2010	-0.0251***	-0.0222***
	(0.00389)	(0.00475)
y2011	-0.0277***	-0.0228***
	(0.00532)	(0.00575)
y2012	-0.0207***	-0.0146***
	(0.00386)	(0.00457)
y2013	-0.00885**	-0.00448
	(0.00429)	(0.00548)
y2014	0.0420***	0.0434***
	(0.00390)	(0.00456)
y2015	0.0866***	0.0863***
	(0.00363)	(0.00465)
y2016	0.109***	0.107***
	(0.00312)	(0.00398)
y2017	0.105***	0.104***
	(0.00292)	(0.00462)
y2018	0.106***	0.105***
	(0.00336)	(0.00397)
black	-0.0769***	-0.0441**
	(0.00274)	(0.0186)
asian	0.0336***	0.0136*
	(0.00247)	(0.00782)
native	-0.188***	-0.136***
	(0.00512)	(0.0268)
blackmale		-0.0637***
		(0.00341)
asianmale		0.0372***
		(0.00400)
nativemale		-0.0470***
		(0.0112)
blackage		0.00140
		(0.00261)
asianage		0.00462***
		(0.00134)
nativeage		-0.00529
		(0.00589)
blacky2009		-0.0147
		(0.0115)
blacky2010		-0.0197*
		(0.0102)
blacky2011		-0.0255***
		(0.00933)
blacky2012		-0.0296***
		(0.0104)
blacky2013		-0.0179*
		(0.00920)
blacky2014		-0.00428
		(0.00896)
blacky2015		0.00258
		(0.00987)

blacky2016		0.0154*
		(0.00912)
blacky2017		0.00762
		(0.0119)
blacky2018		0.00810
		(0.00985)
asiany2009		-0.0221***
		(0.00747)
asiany2010		-0.00638
		(0.00755)
asiany2011		-0.0206***
		(0.00754)
asiany2012		-0.0314***
		(0.00934)
asiany2013		-0.0281***
		(0.0104)
asiany2014		-0.0160**
		(0.00757)
asiany2015		-0.0123
		(0.00803)
asiany2016		-0.00662
		(0.00761)
asiany2017		-0.00767
		(0.00810)
asiany2018		-0.00573
		(0.00777)
nativey2009		0.00299
		(0.0116)
nativey2010		0.00849
		(0.0220)
nativey2011		-0.0203
		(0.0136)
nativey2012		-0.00168
		(0.0190)
nativey2013		-0.0130
		(0.0179)
nativey2014		-0.0125
		(0.0131)
nativey2015		0.0253
		(0.0278)
nativey2016		0.0114
		(0.0146)
nativey2017		0.0195*
		(0.0107)
nativey2018		6.30e-06
		(0.0220)
posttreatment	0.0444***	0.0463***
	(0.00363)	(0.00397)
blackposttreatment		-0.00403
		(0.00596)
asianposttreatment		-0.0137***
		(0.00298)
nativeposttreatment		-0.0183
		(0.0152)
Constant	1.730***	1.726***
	(0.00596)	(0.00732)
Observations	1,451,768	1,451,768
R-squared	0.037	0.038

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Placebo DD Estimates of Effect on Health Coverage

VARIABLES	(1) Basic Model	(2) Interacted Model
male	-0.0662***	-0.0585***
	(0.00253)	(0.00234)
age	-0.0113***	-0.0129***
	(0.00144)	(0.00159)
y2014	0.0509***	0.0479***
	(0.00813)	(0.00847)
y2015	0.0955***	0.0908***
	(0.00688)	(0.00721)
y2016	0.116***	0.110***
	(0.00749)	(0.00798)
y2017	0.112***	0.107***
	(0.00870)	(0.0100)
y2018	0.113***	0.108***
	(0.00759)	(0.00846)
black	-0.0688***	-0.0735***
	(0.00297)	(0.00539)
asian	0.0342***	-0.0238**
	(0.00307)	(0.00960)
native	-0.187***	-0.170***
	(0.00713)	(0.0179)
blackmale		-0.0574***
		(0.00416)
asianmale		0.0277***
		(0.00431)
nativemale		-0.0532***
		(0.0115)
blackage		0.00519***
		(0.00168)
asianage		0.0112***
		(0.00228)
nativeage		-0.00366
		(0.00756)
blacky2014		0.0139***
		(0.00264)
blacky2015		0.0204***
		(0.00362)
blacky2016		0.0275***
		(0.00517)
blacky2017		0.0199**
		(0.00908)
blacky2018		0.0206***

		(0.00543)
asiany2014		0.0121*
		(0.00698)
asiany2015		0.0158**
		(0.00711)
asiany2016		0.0194***
		(0.00661)
asiany2017		0.0182**
		(0.00781)
asiany2018		0.0201**
		(0.00734)
nativey2014		0.000306
		(0.0190)
nativey2015		0.0387
		(0.0260)
nativey2016		0.0304
		(0.0241)
nativey2017		0.0390*
		(0.0223)
nativey2018		0.0195
		(0.0314)
fakeposttreatment	0.00400	0.00194
	(0.00489)	(0.00578)
blackfakeposttreatment		0.0110
		(0.00704)
asianfakeposttreatment		0.00477
		(0.00581)
nativefakeposttreatment		-0.0121
		(0.0207)
Constant	1.801***	1.805***
	(0.00689)	(0.00715)
Observations	813,109	813,109
R-squared	0.027	0.028

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1